This second volume of James Clerk Maxwell's correspondence and manuscript papers begins in July 1862 with his first referee reports for the Royal Society, and concludes in December 1873 shortly before the formal inauguration of the Cavendish Laboratory at Cambridge. The volume documents his involvement with the wider scientific community in Victorian Britain, and the period of his scientific maturity. In the years 1862–73 Maxwell wrote the classic works on statistical molecular theory and field physics, including the *Treatise on Electricity and Magnetism*, which established his special status in the history of science. His letters and drafts of this period provide unique insight into this work, which remains fundamental to modern physics. Few of the manuscripts reproduced here have received prior publication in other than truncated form; and the volume includes Maxwell's correspondence with G. G. Stokes, Kelvin and P. G. Tait. The edition is annotated with a full historical commentary.

THE SCIENTIFIC LETTERS AND PAPERS OF
JAMES CLERK MAXWELL

$$
\left.\begin{aligned}
k\,\nabla^2\mu\alpha &= 4\pi\mu\frac{d^2}{dt^2}\mu\alpha\\
k\,\nabla^2\mu\beta &= 4\pi\mu\frac{d^2}{dt^2}\mu\beta\\
k\,\nabla^2\mu\gamma &= 4\pi\mu\frac{d^2}{dt^2}\mu\gamma
\end{aligned}\right\}\quad(69)
$$

If we assume that α β γ are functions of $lx + my + nz - Vt = w$ the first equation becomes

$$k\mu\frac{d^2\alpha}{dw^2} = 4\pi\mu^2 V^2\frac{d^2\alpha}{dw^2} \qquad (70)$$

or

$$V = \pm\sqrt{\frac{k}{4\pi\mu}} \qquad (71)$$

The other equations give the same value for V so that the wave is propagated in either direction with a velocity V

This wave consists entirely of magnetic disturbances, the direction of magnetization being in the plane of the wave. No magnetic disturbance whose direction of magnetization is not in the plane of the wave can be propagated as a plane wave at all.

Hence magnetic disturbances propagated through the electromagnetic field agree with light in this, that the disturbance at any point is transverse to the direction of propagation, and such waves may have all the properties of polarized light.

The only medium in which experiments have been made to determine the value of k is air, in which $\mu = 1$ and therefore by equation (46)

$$V = v \qquad (72)$$

By the electromagnetic experiments of M. M. Weber & Kohlrausch

$$v = 310\ 740\ 000 \quad \text{meters per second}$$

is the number of electrostatic units in one electromagnetic unit of electricity and this according to our result should be equal to the velocity of light in air or vacuum

Velocity of light in air by M. Fizeau's† experiments

$$V = 314.858\ 000$$

Leipzig Trans V (1857) p 260 or Poggendorff's Annalen ... 1856 p 10

† Comptes Rendus vol XXIX (1849) p 90

The electromagnetic theory of light. From ‘A dynamical theory of the electromagnetic field’, October 1864 (Number 239).

THE SCIENTIFIC LETTERS AND PAPERS OF

JAMES CLERK MAXWELL

VOLUME II
1862–1873

EDITED BY

P. M. HARMAN

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For Tim and Rosie

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PREFACE

This second volume of James Clerk Maxwell's scientific letters and papers documents the period of his maturity. These manuscripts provide substantive evidence of the process in which the brilliant innovations of his scientific youth were transformed into the Maxwellian physics transmitted to posterity. The volume covers the years 1862–73, when Maxwell wrote the classic works on statistical molecular theory and field physics, including the *Treatise on Electricity and Magnetism*, which established his unique status in the history of science. The volume begins with his first referee reports for the Royal Society, signalling his involvement with the wider scientific community, and ends shortly before the inauguration of the Cavendish Laboratory at Cambridge.

Only a small number of the manuscripts reproduced in this volume have received prior publication in other than truncated form. Letters received by Maxwell, of which few are now extant, are reproduced on a selective basis, and third-party correspondence is also included. All the letters from his major correspondents – George Gabriel Stokes, William Thomson (later Lord Kelvin) and Peter Guthrie Tait – are reproduced complete, the correspondence with Tait being of especial interest.

This volume prints all of Maxwell's extant autograph letters from the period 1862–73. While these and other manuscripts provide profuse documentation of the evolution of Maxwell's science, his intellectual development, and the course of his public career, little information can be gleaned about his private affairs. Campbell and Garnett's *Life of Maxwell* contains no more than a very sparse selection of the documents, still extant at the time of his death, which related to his personal life in this period.

Much of the work on this volume has been carried out in the Cambridge University Library and the Harvard College Library. I am grateful to the President and Fellows of Clare Hall, Cambridge, and the Department of the History of Science, Harvard University, for hospitality in providing facilities for this work.

The edition owes much to the support of Cambridge University Press. Sir Alan Cook has taken a kind interest in the progress of my work on behalf of the Syndics. Richard Ziemacki, Simon Capelin, James Deeny and Fiona Thomson have been generous with advice and assistance. I am very grateful to Alan Winter and Richard Schermerhorn: the completion of this volume owes much to the help they kindly proffered. It is a pleasure to thank Susan

Bowring for her meticulous contribution as copy-editor; and to gratefully acknowledge the work of Pauline Ireland and of the draughtsmen and typesetters at the Press in the presentation of the edition in such an elegant form.

I have benefited from the helpfulness of archivists and librarians in many libraries. I thank especially Godfrey Waller of the Manuscripts Room of the Cambridge University Library and Alan Clark of the Library of the Royal Society, for their kindness and efficiency over many years.

I am grateful to many friends and colleagues who have been helpful in providing information about the location of manuscripts, in offering advice, and in fostering my work by various acts of kindness. I thank especially Jed Buchwald, I. Bernard Cohen, Francis Everitt, Tom Fuller, Ivor Grattan-Guinness, Erwin Hiebert, Bruce Hunt, Lord Jenkin, Robert Kargon, Martin Klein, Anne Kox, Sir Brian Pippard, A. I. Sabra, Robert Schulmann, Sam Schweber, Alan Shapiro, Daniel Siegel, Thomas Simpson, John Stachel, Carlene Stephens, The Hon. Guy Strutt, Garry J. Tee, Paul Theerman, Charles Webster, Tom Whiteside, L. Pearce Williams and David Wilson.

I am grateful to Ethel Dunkerley for typing the entire manuscript (from my handwritten transcriptions) with such dedication; to Isabel Matthews for valuable assistance in drawing the figures; and to Keith Papworth of the Cavendish Laboratory for his enthusiasm in preparing photographs of Maxwell's experimental apparatus.

It would have proved impossible to prepare this volume for publication without the opportunity to spend extended periods working in a research library. I am grateful to the Royal Society for awarding me a succession of research grants, and I thank Norman Robinson and Sheila Edwards for their interest. I am especially grateful to the National Science Foundation for substantial grants which supported two years work at Harvard; to thank Ronald J. Overmann for his generous interest and kindness, I can only say that without such support this volume would not have been completed.

For permission to reproduce manuscripts I am grateful to Lord Rayleigh; Professor Sydney Ross; Miss Margaret Tait; the Syndics of the Cambridge University Library; the Master and Fellows of Trinity College, Cambridge; the Master and Fellows of Peterhouse, Cambridge; the Cavendish Laboratory, Cambridge; the Librarian of Glasgow University Library; St Andrews University Library; Edinburgh University Library; the Trustees of the National Library of Scotland; the Royal Society; the Royal Institution; Imperial College London Archives; King's College London Archives; the University of London Library and the Athenæum; the Institution of Electrical Engineers, London; the Greater London Record Office; the Bodleian Library, Oxford; the Royal Greenwich Observatory; The General

Electric Company, p.l.c.; The Queen's University of Belfast Library; Columbia University Library; The Johns Hopkins University Library; the Smithsonian Institution Libraries; Harvard University Archives; and the Akademie-Archiv, Berlin.

I am grateful to the Cavendish Laboratory and the Whipple Museum of the History of Science, Cambridge for permission to reproduce photographs of Maxwell's apparatus, and to Dr I. B. Hopley for use of his photograph of the governor; and to the Syndics of the Cambridge University Library, the Librarian of Glasgow University Library, and the Royal Society for permission to reproduce photographs of documents.

To end on a very personal note, my deepest thanks are to Juliet, Tim and Rosie for so good-humouredly tolerating my absorption in this work.

LIST OF TEXTS

EDITORIAL NOTE

The terms of reference of this edition of Maxwell's scientific letters and papers are described in the General introduction and Editorial note to the first volume. Maxwell's extant autograph letters and papers are supplemented by documents drawn from the *Life of Maxwell*, and by his shorter publications – letters, reviews, abstracts of contributed and published papers, and contributions to discussions – which were omitted from the memorial edition of his *Scientific Papers* published by Cambridge University Press in 1890. The texts are reproduced in chronological sequence, so far as can be determined. In the case of postcards, where there is generally no date written by Maxwell, the convention is adopted (in the absence of any other evidence) of dating the cards by their postmarks. In the case of abstracts of contributed and published papers the convention is adopted either of citing the date when the paper was read, or (in the case of papers read to the Royal Societies of London and Edinburgh) the date the paper was received by the Secretary.

The primary intention of this edition is the reproduction of an accurate text of all Maxwell's scientific letters and substantive manuscript papers. Manuscript fragments and jottings have been included on a selective basis. Special mention should be made of a series of six notebooks(1) that Maxwell kept during the years spanned by this volume. These notebooks, each of which was in use for two or three years, contain drafts of examination questions, journal references, calculations, and a miscellany of fragmentary jottings and notes. Some materials drawn from these notebooks have been reproduced as texts, and additional manuscript jottings have been included in the editorial annotations.

As already mentioned, the edition includes publications omitted from the *Scientific Papers*. Two classes of such published works have of necessity not been included: reports of the British Association Committee on electrical standards, of which Maxwell was a member(2) (the report for the year 1863

(1) There are three notebooks in the Maxwell Papers in the King's College London Archives; one in ULC Add. MSS 7655, V, k/9; one in the Cavendish Laboratory, Cambridge (of which there is a photocopy in ULC Add. MSS 7655, V, n/1); and one in ULC Add. MSS 7655, V, n/2).

(2) In the British Association *Reports* for 1863, 1864 and 1869; reprinted in Fleeming Jenkin, *Reports of the Committee on Electrical Standards appointed by the British Association for the Advancement of Science* (London, 1873).

including his paper, written with Fleeming Jenkin, on electrical units and dimensional relations);[3] and his series of questions set during five years as an examiner and moderator for the Cambridge Mathematical Tripos,[4] though some of these questions (being of special relevance) have been reproduced as texts or included in the editorial annotations.

Letters addressed to Maxwell are listed in the appendix. Because of the limited scope of these documents (as described in the General introduction to the first volume of the edition) these letters are reproduced on a selective basis. In the present volume all the letters written by his major correspondents, George Gabriel Stokes, Peter Guthrie Tait, and William Thomson, are printed *in extenso*. Other letters are reproduced complete, in extract, or are merely cited, as judged appropriate. Because of the generally patchy nature of the extant incoming correspondence, these letters are reproduced as annotations to the Maxwell texts. Third-party letters and other documents that contain information bearing on Maxwell's writings and work are included, generally in selective extract.

In accordance with the principles of modern scholarship the reproduction of the texts faithfully follows the manuscripts in spelling, punctuation, capitalization, and in preserving contractions; endpoints to sentences have been silently inserted. Where the texts are reproduced from printed sources the style of the original is followed. Trivial cancellations have been omitted without comment, but corrections deemed significant have been recorded. Minor deletions are placed within angle brackets ⟨...⟩ preceding the revised text; longer cancelled passages are reproduced by setting a double vertical bar against them in the left-hand margin. Appended passages are reproduced with a single vertical bar in the left-hand margin; appended phrases by corners ⌊...⌋ which enclose the added words. Annotations which were subsequently appended by Maxwell or by his correspondents are recorded. The name enclosed by brackets {...} denotes the annotator. The very few editorial insertions to the text, which have been introduced for the sake of clarity, are enclosed within square brackets.

In general I have attempted to preserve the layout of the manuscripts in their transformation to the printed page, but some necessary adjustments have been made for reasons of clarity. This applies especially to the reproduction of figures, which, like the transcription of handwriting, requires editorial interpretation. Some of Maxwell's figures are clearly drawn, but

(3) J. Clerk Maxwell and Fleeming Jenkin, 'On the elementary relations between electrical measurements', *Phil. Mag.*, ser. 4, **29** (1865): 436–60, 507–25; in Jenkin, *Reports of the Committee*: 59–96.

(4) In the *Cambridge University Calendar* for 1866, 1867, 1869, 1870 and 1873.

most are rough sketches. The aim has been to elucidate Maxwell's intentions as determined by study of both the figure and the corresponding text. The aim has been clarity rather than the precise reproduction of the originals. Figure numbers have been added; the captions are Maxwell's. In printing documents in the annotations to the texts, the convention is adopted of marking paragraph divisions and lines of poetry by a solidus.

The editorial commentary – the historical and textual notes and the Introduction – is intended to aid the reader in following Maxwell's arguments and his allusions to concepts, events and personalities. The Introduction provides a broad account of his intellectual development and career in the period covered by this volume, and an outline review of the texts here reproduced. In addition to clarifying obscurities in the texts, the historical notes seek to establish the context within which the documents were written, employing contemporary published as well as manuscript sources, including letters written to Maxwell, third-party correspondence, and fragmentary manuscript jottings not reproduced as texts. Reference to the first volume of the edition is made in the form 'Volume I: 438', meaning Volume I: [page] 438.

LIST OF PLATES

ABBREVIATED REFERENCES

Ann. Chim. Phys.	*Annales de Chimie et de Physique* (Paris).
Ann. Phys.	*Annalen der Physik und Chemie* (Leipzig).
Boase	*Modern English Biography containing Many Thousand Concise Memoirs of Persons who have Died since the Year 1850.* By Frederic Boase, 3 vols. and supplement (3 vols.) (Truro, 1892–1921).
Camb. & Dubl. Math. J.	*Cambridge and Dublin Mathematical Journal* (Cambridge).
Camb. Math. J.	*Cambridge Mathematical Journal* (Cambridge).
Comptes Rendus	*Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences* (Paris).
DNB	*Dictionary of National Biography.* Ed. L. Stephen and S. Lee, 63 vols. and 2 supplements (6 vols.) (London, 1885–1912).
Electricity	Michael Faraday, *Experimental Researches in Electricity*, 3 vols. (London, 1839–55).
Electrostatics and Magnetism	William Thomson, *Reprint of Papers on Electrostatics and Magnetism* (London, 1872).
Knott, *Life of Tait*	Cargill Gilston Knott, *Life and Scientific Work of Peter Guthrie Tait. Supplementing the two Volumes of Scientific Papers Published in 1898 and 1900* (Cambridge, 1911).
Larmor, *Correspondence*	*Memoir and Scientific Correspondence of the Late Sir George Gabriel Stokes, Bart.* Ed. J. Larmor, 2 vols. (Cambridge, 1907).
Larmor, 'Origins'	'The origins of Clerk Maxwell's electric ideas, as described in familiar letters to W. Thomson'. Communicated by Sir Joseph Larmor, in *Proc. Camb. Phil. Soc.*, **32** (1936): 695–750. Reprinted separately (Cambridge, 1937).
Life of Maxwell	Lewis Campbell and William Garnett, *The Life of James Clerk Maxwell. With a Selection from his Correspondence and Occasional Writings and a Sketch of his Contributions to Science* (London, 1882).
Life of Maxwell (2nd edn)	Lewis Campbell and William Garnett, *The Life of James Clerk Maxwell with Selections from his Correspondence and Occasional Writings*, new edition, abridged and revised (London, 1884).
Math. & Phys. Papers	William Thomson, *Mathematical and Physical Papers*, 6 vols. (Cambridge, 1882–1911).
Molecules and Gases	*Maxwell on Molecules and Gases.* Ed. Elizabeth Garber,

	Stephen G. Brush and C. W. F. Everitt (Cambridge, Massachusetts/London, 1986).
OED	*The Oxford English Dictionary*, 12 vols. (Oxford, 1970).
Papers	George Gabriel Stokes, *Mathematical and Physical Papers*, 5 vols. (Cambridge, 1880–1905).
Phil. Mag.	*Philosophical Magazine* (London).
Phil. Trans.	*Philosophical Transactions of the Royal Society of London* (London).
Proc. Camb. Phil. Soc.	*Proceedings of the Cambridge Philosophical Society* (Cambridge).
Proc. Roy. Soc.	*Proceedings of the Royal Society of London* (London).
Proc. Roy. Soc. Edinb.	*Proceedings of the Royal Society of Edinburgh* (Edinburgh).
Scientific Memoirs	*Scientific Memoirs, Selected from the Transactions of Foreign Academies of Science and Learned Societies, and from Foreign Journals.* Ed. Richard Taylor, 5 vols. (London, 1837–52).
Scientific Papers	*The Scientific Papers of James Clerk Maxwell.* Ed. W. D. Niven, 2 vols. (Cambridge, 1890).
Thomson and Tait, *Natural Philosophy*	Sir William Thomson and Peter Guthrie Tait, *Treatise on Natural Philosophy. Vol. 1* (Oxford, 1867).
Trans. Camb. Phil. Soc.	*Transactions of the Cambridge Philosophical Society* (Cambridge).
Trans. Roy. Soc. Edinb.	*Transactions of the Royal Society of Edinburgh* (Edinburgh).
Treatise	James Clerk Maxwell, *A Treatise on Electricity and Magnetism*, 2 vols. (Oxford, 1873).
ULC	Manuscripts in the University Library, Cambridge.
Venn	*Alumni Cantabrigienses. A Biographical List of all Known Students, Graduates and Holders of Office at the University of Cambridge, from the Earliest Times to 1900.* Compiled by J. A. Venn. Part II. From 1752 to 1900, 6 vols. (Cambridge, 1940–54).
Wiener Berichte	*Sitzungsberichte der Mathematisch-Naturwissenschaftlichen Classe der Kaiserlichen Akademie der Wissenschaften* (Vienna).
Wilson, *Stokes–Kelvin Correspondence*	*The Correspondence between Sir George Gabriel Stokes and Sir William Thomson, Baron Kelvin of Largs.* Edited with an introduction by David B. Wilson, 2 vols. (Cambridge, 1990).

INTRODUCTION

This second volume of James Clerk Maxwell's scientific letters and manuscript papers begins in mid-1862 with his first referee reports for the Royal Society, and concludes in December 1873 shortly before the formal inauguration of the Cavendish Laboratory. In the period encompassed by this volume Maxwell took his place among the élite of Victorian science. Though his sense of duty prompted him to accept university appointments and to shoulder responsibilities within the scientific community, his ultimate commitment was that of a natural philosopher. As Lewis Campbell, his lifelong friend and biographer expressed it, 'with sacred devotion [he] continued in mature life the labours which had been his spontaneous delight in boyhood'.(1)

Two topics dominated his research and writing in this period of his scientific maturity: the electromagnetic field and the electromagnetic theory of light, and statistical molecular physics, on which he wrote the classic works which established his unique status in the history of science. His search for rigorous analytical foundations was balanced by an enduring concern with geometrical representation and analogy; and the simple apparatus of his experiments on colour vision in the 1850s yielded to the precision instruments

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(1) *Life of Maxwell*: 431. There is a biographical study by C. W. F. Everitt, *James Clerk Maxwell: Physicist and Natural Philosopher* (New York, 1975), based on his article on 'Maxwell' in *Dictionary of Scientific Biography*, ed. C. C. Gillispie, 16 vols. (New York, 1970–80), **9**: 198–230. Several of the documents included in this volume shed light on Maxwell's early interests and career. A letter to J. J. Sylvester (Number 267) is informative about Maxwell's knowledge of the geometrical and optical properties of ovals at the time of his first work in mathematics in 1846–7 (Volume I: 35–62). Information about his candidacy for a fellowship at Trinity College, Cambridge in 1854 and 1855 is given in a letter to Lewis Campbell (Number 251). The significance of the two geometrical problems in the *Camb. & Dubl. Math. J.* in 1853 (Volume I: 230–6) is described in letters to Tait and James Thomson: the problem on the motion of particles in a circle (Number 398) and that on the path of rays of light in a medium of continuously variable index of refraction (Numbers 249, 380 and 421). In 1870 he commented on 'gorgeous entanglements of colour', the chromatic effects of polarised light on doubly refracting crystals (Number 343) which had fascinated him in 1848 (Volume I: 97–100). A brief review of J. D. Forbes' contribution to the theory of colours (Number 431) gives a recollection of his own introduction to the subject by Forbes (see Volume I: 300–3); he continued his experiments on colour vision in the 1860s (Numbers 202, 341, 358, 359 and 360).

fashioned for his measurement of electrical units and experiments on the viscosity of gases.

An important feature of his writings in this period is a concern with the philosophical problems generated by his physics. These include the disjunction between the reversible laws of mechanics and the irreversibility of natural processes; his expression of the essentially statistical nature of the second law of thermodynamics (as illustrated by the 'demon' paradox); and his discussion of problems of atomism, determinism and free will. Maxwell's letters and manuscript drafts afford glimpses of the breadth of scholarship which sustained his reflections on these philosophical issues; and more generally, provide substantive evidence of the process in which the brilliant innovations of scientific youth were transformed into the enduring mature achievement which established his pre-eminence in the 'classical' physics of the nineteenth century.

London scientific society

Maxwell was appointed Professor of Natural Philosophy at King's College, London in July 1860, and he held the post until April 1865. The move to London fostered his induction into the wider scientific community of Victorian Britain. In the 1850s his work had been shaped by the scientific culture of the two universities where he had received his education, Edinburgh and Cambridge. The decision to continue his academic career in London, after his enforced redundancy from Marischal College, Aberdeen and the failure of his quest to succeed James David Forbes at Edinburgh University, was capped by his election as a Fellow of the Royal Society and the award of the Society's Rumford Medal.(2) On his arrival in London in October 1860 he was soon introduced into metropolitan scientific society. In his journal entry of Sunday, 24 March 1861 Thomas Archer Hirst recorded his first acquaintance with Maxwell: '[I was] at an evening party at Dr Carpenters and was introduced to Helmholtz and Maxwell... the latter talkative with a Scotch brogue, he took great interest in my ripples'.(3) The

(2) On Maxwell's nomination for and award of the Royal Society's Rumford Medal in 1860, and election as a Fellow of the Royal Society on 2 May 1861, see *Minutes of Council of the Royal Society from December 16th 1858 to December 16th 1869*, **3** (London, 1870): 63, 72 and 85; and *Proc. Roy. Soc.*, **11** (1860–1): 19–21, 193. See Volume I: 647n.

(3) *Natural Knowledge in a Social Context: the Journals of Thomas Archer Hirst FRS*, eds. W. H. Brock and R. M. MacLeod [Mansell microform] (London, 1980): f. 1572 (Sunday, 24 March 1861). On Hirst see Number 369 note (6); the reference is to his paper 'On ripples, and their relation to the velocities of currents', *Phil. Mag.*, ser. 4, **21** (1861): 1–20, 188–98. William Benjamin Carpenter, FRS 1844, was Registrar of the University of London (*DNB*).

following May he delivered his first Friday evening discourse to the Royal Institution 'On the theory of the three primary colours'.(4)

During his early years in London Maxwell was burdened with teaching duties at King's College. The syllabus for his lectures in his first session in 1860 broadly followed the curriculum of his predecessor, Thomas Minchin Goodeve, but with some stronger emphasis on the coverage of fundamental principles. For his elementary course for first-year students he offered lectures on mechanics, the properties of matter and heat, to be supplemented by a more detailed mathematical study of these topics; and the course concluded with some experimental demonstrations and lectures on light. For the lecture course for second- and third-year students, where Goodeve had discussed applied mechanics and astronomy, Maxwell announced an advanced course which was significantly more mathematical in scope; he proposed the study of rigid body dynamics, the motion of an incompressible fluid and its application to electricity and magnetism, of astronomy, and of waves and their application to sound and light. These lectures required Maxwell's attendance at King's College for three mornings a week during term, when he delivered one hour-long lecture to each of these two classes; and there was in addition the requirement to teach a separate course on experimental physics, an evening class which met once a week. In his second session in 1861–62 he abandoned the more mathematical lectures in the elementary class.(5)

This basic pattern of lecturing continued during Maxwell's subsequent sessions at King's College, though there was some modification in the content of the lecture courses. In 1862–63 the evening class was given on the subject of 'Sound, light, and radiant heat'; in 1863–64 on the 'Properties of bodies as affected by pressure and heat'; and in 1864–65 on 'Magnetism and electricity'.(6) In these two final sessions of his teaching at King's College, the demonstrations on optics were removed from the elementary course and included in the curriculum for the advanced class, where the lectures on mathematical physics were abandoned in favour of a more rigorous coverage of the basic topics taught in the elementary course.(7) An important development was the appointment of a lecturer in October 1861; having

(4) See Volume I: 675–9.

(5) *The Calendar of King's College, London for 1859–60*: 99–100; *Calendar for 1860–61*: 115–17, 268–70; *Calendar for 1861–62*: 113–15. See also C. Domb, 'James Clerk Maxwell in London 1860–1865', *Notes and Records of the Royal Society of London*, **35** (1980): 67–103.

(6) *The Calendar of King's College, London for 1862–63*: 288–90; *Calendar for 1863–64*: 294–6; *Calendar for 1864–65*: 299–301.

(7) *The Calendar of King's College, London for 1863–64*: 131–3; *Calendar for 1864–65*: 135–8.

noted that the duties of teaching were 'too heavy for one person properly to discharge'(8) the College Council appointed George Robarts Smalley (Numbers 204 and 208). On Smalley's resignation in July 1863 the post was filled by William Grylls Adams, who ultimately succeeded Maxwell as professor in April 1865.(9)

As early as October 1864 Maxwell signalled to William Thomson that it was his intention to resign from King's College, indicating his desire to pursue research unrestricted by academic duties (Number 235); and he declared subsequently that it was for this reason that he had resigned (Number 256). This was no doubt his motive: at the time of his resignation he was planning experiments to determine the ratio of electrical units (Numbers 242 and 243), and was very soon heavily engaged in measuring gas viscosity (Numbers 244, 245 and 246). Nevertheless, a resignation accepted on 10 February 1865 and which was intended to take effect in mid-session is curious, though the College Council minutes record Maxwell's readiness to 'continue his work until the appointment of his successor'.(10) The College Council proceeded to resolve the situation promptly: Adams was appointed to the professorship on 10 March 1865. On 21 March the Council abolished the lectureship, Adams having agreed to carry out all the teaching duties in natural philosophy for a salary enhanced from that advertised for the professorship;(11) the Council minutes record that this arrangement would result in a 'considerable saving' to the College.(12) The circumstances of Maxwell's resignation from King's College and of Adams' rapid succession to his post are not entirely clear.(13)

Maxwell remained in academic retirement until his return to Cambridge in 1871, although he was a candidate in 1868 for appointment as Principal of

(8) King's College London Archives, King's College Council Vol. I, minute 42, 11 October 1861.

(9) See Numbers 209 note (2) and 256 esp. note (4).

(10) According to the terms of Maxwell's letter of resignation as recorded in the minutes of the meeting of the King's College Council on 10 February 1865 (King's College London Archives, King's College Council Vol. I, minute 410); see Number 235 note (20). The post was advertised to take effect from 25 April 1865 (King's College London Archives, Special Committees Nº 2, f. 305, 15 February 1865).

(11) King's College London Archives, King's College Council Vol. I, minutes 415, 423.

(12) The advertised salary for the post of Professor of Natural Philosophy was four guineas per student *per annum* (King's College London Archives, Special Committees Nº 2, f. 305, 15 February 1865). Adams' salary as lecturer had been two guineas per student *per annum* (King's College Council Vol. I, minute 373, 18 November 1864). On the abolition of the lectureship he agreed to undertake all the teaching duties in natural philosophy for five guineas per student *per annum*, with the consequence that a 'considerable saving would be effected' (King's College Council Vol. I, minute 423, 21 March 1865).

(13) See also Domb, 'James Clerk Maxwell in London': 92–5, 101–3.

the United Colleges of St Andrews (Numbers 311 to 315). Despite his resignation from King's College, during the 1860s he continued to live in London for some of the winter months. As a Fellow he attended meetings of the Royal Society and acted as a referee on papers submitted for publication in the *Philosophical Transactions*. His most substantial memoirs of the period were published in the Society's *Transactions*: 'A dynamical theory of the electromagnetic field' (Numbers 238 and 239); 'On the viscosity or internal friction of air and other gases', the Bakerian Lecture for 1866 (Number 252); 'On the dynamical theory of gases' (Number 263); and 'On a method of making a direct comparison of electrostatic with electromagnetic force' (Number 289). He read several shorter papers at meetings of the Royal Society; these included papers on the dynamo (Number 268), on governors (Numbers 280, 283 and 297: Appendix), and on Arago's rotating disc (Numbers 400, 404 and 405). In April 1864 he was elected a member of the select Philosophical Club of the Royal Society (Number 226), which had been founded in 1847 to 'promote...the scientific objects of the Royal Society'.(14) His attendance is recorded at eight of the monthly meetings of the Philosophical Club in the years 1864–73; and the minutes record that on 18 December 1873 'Professor Clerk Maxwell exhibited an instrument for applying polarized light to detect the state of strain in a moving viscous fluid', this being the subject of a paper which he read to a meeting of the Royal Society the same day.(15)

In April 1867 Maxwell was elected a member of the newly formed London Mathematical Society,(16) presenting papers (see Numbers 279, 311, 318 and 345) and participating in discussions (Numbers 330 and 331), most notably when he raised questions on the condition for the stability of governors in January 1868 (Number 280) and on the convention regarding spatial relations in May 1871 (Number 370). His involvement in London scientific life was capped by two Friday evening discourses at the Royal Institution: 'On colour vision' in 1871 (Number 360) and 'On action at a distance' in 1873 (Numbers 437 and 438), the easy social relations prevailing on these occasions being recorded by Thomas Archer Hirst.(17) Maxwell was ac-

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(14) See Number 226 note (2).

(15) T. G. Bonney, *Annals of the Philosophical Club of the Royal Society* (London, 1919): 193; and see Number 259 note (12).

(16) *Proceedings of the London Mathematical Society*, **2** (1867): 26.

(17) *Journals of Thomas Archer Hirst*: f. 1896 (24 March 1871): 'At Spottiswoode's to dinner; present Clerk Maxwell (lectured at Royal Inst.) Tyndall, Odling, Mr and Mrs Birkbeck. After the lecture on Colour Maxwell, Odling and Sir F. Pollock smoked a pipe in Tyndall's rooms.'; and *Journals of Thomas Archer Hirst*: f. 1969 (21 February 1873): 'After the lecture...Sir F. and

quainted with W. R. Grove,[18] a senior figure in London science (Numbers 285 and 313), and in 1869 he was nominated by Grove and Hirst for election to the Athenæum Club.[19] While he was not aloof to Tait's urging[20] to contribute papers to the Royal Society of Edinburgh (Numbers 334 and 395), the London scientific milieu, where he established relations with the astronomer William Huggins (Numbers 271, 309 and 406) and with the chemistry community (Number 270), proved more compelling. He was not however an intimate of the vocal circle of metropolitan scientists which included Hirst, Thomas Henry Huxley and John Tyndall[21] (Numbers 226, 297 and 477). He was hostile to their secularist outlook; and his opposition to the formation of the Physical Society of London (Number 484) suggests his lack of sympathy with their cultivation of the role of the scientific professional.

Standards of electrical resistance

At the 1861 meeting of the British Association for the Advancement of Science Latimer Clark and Sir Charles Bright called for the establishment of a system of units of electrical potential, current and resistance.[22] William Thomson urged the formation of the British Association Committee on standards of electrical resistance; its aim was to determine the most convenient unit of resistance and to discover the most suitable material for the standard unit.[23] Maxwell was co-opted as a member of the Committee in 1862, and in May and June 1863 he joined Fleeming Jenkin and Balfour

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Lady Pollock... and others collected in Tyndall's rooms'. William Spottiswoode, FRS 1853, was President of the London Mathematical Society and Treasurer of the Royal Society in 1871 (*DNB*).

(18) William Robert Grove, FRS 1840, judge and physical scientist, leading member of the Royal Society Philosophical Club (*DNB*).

(19) *Journals of Thomas Archer Hirst*: f. 1843 (16 March 1869), an unsuccessful proposal; *Journals of Thomas Archer Hirst*: f. 1845 (13 April 1869), Maxwell elected to the Athenæum Club.

(20) Peter Guthrie Tait to Maxwell, 6 and 13 December 1867; see Number 277 notes (2) and (22).

(21) John Tyndall, Professor of Natural Philosophy at the Royal Institution, FRS 1852 (*DNB*). See R. M. MacLeod, 'The x-club: a social network of science in late-Victorian England', *Notes and Records of the Royal Society of London*, **24** (1970): 305–22.

(22) Latimer Clark and Sir Charles Bright, 'On the formation of standards of electrical quantity and resistance', *Report of the Thirty-first Meeting of the British Association for the Advancement of Science* (London, 1862), part 2: 37–8.

(23) 'Provisional report of the Committee appointed by the British Association on standards of electrical resistance', *Report of the Thirty-second Meeting of the British Association for the Advancement of Science* (London, 1863): 125–63. See Crosbie Smith and M. Norton Wise, *Energy and Empire. A Biographical Study of Lord Kelvin* (Cambridge, 1989): 687–94.

Stewart in an accurate measurement of electrical resistance in absolute units, employing a method devised by William Thomson.[24]

These experiments were carried out at King's College, London (Numbers 210 to 216). By 'absolute' units was meant reference to 'fundamental' units of time, mass and space, so that 'all the units form part of a coherent system', and hence avoiding 'useless coefficients in passing from one kind of measurement to another'. The determination of electrical units was based on the 'natural relations existing between the various electrical quantities, and between these and the fundamental units of time, mass and space'. The inspiration for this approach came from the classic experiments of Wilhelm Weber; and the 'natural' relations included Ohm's law and Joule's law of the work performed by a current in a circuit. These relations would yield an 'electrostatic' system of units; but the 'chief applications of electricity are dynamic, depending on electricity in motion, or on voltaic currents with their accompanying electromagnetic effects'. The expression of electrical quantities in 'electromagnetic' units would require the introduction of the effects of electricity in motion, such as the force exerted on the pole of a magnet by a current. Measurement of work and force would have required complex experiments; and Thomson therefore devised a method in which the resistance of a rotating coil was calculated from the measurement of the deflection of a magnet placed at its centre, the resistance of the coil being compared with that of an arbitrary standard.[25]

In working with precision apparatus designed by William Thomson these experiments embodied a distinct advance in experimentation over Maxwell's work on colour vision in the 1850s. The technique which was employed in the electrical experiments – 'studying oscillations of magnets by aid of mirrors' – had important implications, for it suggested, as Maxwell remarked to Stokes in June 1863, 'the determination of gaseous friction by means of a disc oscillating in a gas' using the same method (Number 212). His subsequent

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(24) 'Report of the Committee appointed by the British Association on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association for the Advancement of Science* (London, 1864): 111–76; see Number 210 note (2). Balfour Stewart, Edinburgh University 1845, was Director of the Kew Observatory 1859–70 (*DNB*). Fleeming Jenkin (P. G. Tait's class-mate at the Edinburgh Academy, where Maxwell was his senior) was an engineer in partnership with William Thomson (*DNB*); see Smith and Wise, *Energy and Empire*: 698–702, and William Thomson, 'Note on the contributions of Fleeming Jenkin to electrical and engineering science', in *Papers Literary, Scientific, &c by the Late Fleeming Jenkin*, ed. S. Colvin and J. A. Ewing, with a Memoir by Robert Louis Stevenson, 2 vols. (London, 1887), **1**: clv–clix.

(25) 'Report of the Committee appointed by the British Association on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association*: 111–16.

measurement of the viscosity of gases depended on the application of this experimental arrangement (Number 252).

The centrifugal governor designed by Fleeming Jenkin, which was used to control the speed of the revolving coil in the experiments on electrical standards, also sparked off Maxwell's imagination. He immediately began to investigate the dynamical principles that regulate the operation of governors, reporting his conclusions to Thomson (Numbers 214 and 219). The mathematical method he suggested to establish the conditions for the stability of governors is identical in form to the argument that he had used to determine the conditions of stability of the rings of Saturn in his Adams Prize memoir *On the Stability of the Motion of Saturn's Rings.*(26) He followed this procedure in his 1868 paper 'On governors' (Number 280). This interest in the stability of governors forms a strand in his continuing concern with stability problems, including that of the motion of Saturn's rings (Number 224).

A third important development from the work on electrical standards was a paper written in collaboration with Fleeming Jenkin, 'On the elementary relations of electrical quantities', which was included in the Committee's 'Report' in 1863.(27) Here Maxwell introduced the dimensional notation, which was to become standard, expressing dimensional relations as products of powers of Mass, Length and Time. For every quantity the ratio of the two absolute definitions (of the electrostatic unit based on forces between electric charges and the electromagnetic unit based on forces between magnetic poles) is a power of a constant with dimensions $[LT^{-1}]$ and magnitude very nearly the velocity of light.(28) The argument established a phenomenological link between electromagnetic quantities and the velocity of light, which no doubt fostered the strategy of his Royal Society paper 'A dynamical theory of the electromagnetic field' (Numbers 238 and 239). In this paper he set out a formulation of his 'Electromagnetic Theory of Light'(29) where the theory was detached from the mechanical ether model in which it had been embedded in his paper 'On physical lines of force' (Numbers 232 and 240).

The study of dimensional relations also revealed the different classes of experiments from which the ratio of electrostatic and electromagnetic units of electricity, and hence the velocity of propagation of electromagnetic waves,

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(26) See Number 219 note (17); and O. Mayr, 'Maxwell and the origins of cybernetics', *Isis*, **62** (1971): 425–44, esp. 428–9.

(27) See Number 218 note (3).

(28) See the *Treatise*, **2**: 239–43 (§§620–7); and Everitt, *James Clerk Maxwell*: 100–1.

(29) An expression first used in 'A dynamical theory of the electromagnetic field'; see Number 238 and *Scientific Papers*, **1**: 577.

could be obtained (Number 218). His correspondence with William Thomson in the autumn and winter of 1864–65 is filled with discussion of various experiments which could be deployed to determine this ratio (Numbers 233, 234, 235, 241, 242 and 243). This interest culminated in the experiments on the direct comparison of electrostatic and electromagnetic forces which he carried out with Charles Hockin in 1868 (Number 289); these measurements, which formed the capstone to his work as a member of the Committee on electrical standards in the 1860s, established a value for the velocity of propagation of electromagnetic waves, a key element in his theory of the electromagnetic field. Maxwell's work with the Committee on electrical standards was marked by his appointment as President of Section A (mathematics and physics) of the British Association in 1870 (Number 344).

The electromagnetic theory of light and the ether

Maxwell's heady statement in the third part of his paper 'On physical lines of force' in 1862, that '*light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena*',[(30)] stated the unification of optics and electromagnetism in terms of a mechanical theory of the ether. His concern to explore the implications of an ether which had optical and electromagnetic correlates led him to highlight two critical problems: the effect of motion in the ether, and the explanation of optical reflection and refraction by means of the electromagnetic theory. In the course of writing his paper 'A dynamical theory of the electromagnetic field' (Numbers 238 and 239) in 1864 he considered both of these questions.[(31)]

In April 1864 he set up an 'Experiment to determine whether the Motion of the Earth influences the Refraction of Light' (Number 227). This investigation was suggested by the experiments of Hippolyte Fizeau on the detection of the ether wind.[(32)] Maxwell suggested that the Fresnel drag (of the ether) could affect the refraction of light by a glass prism; but in calculating the displacement arising from the drag he ignored the compensating change in the density of the transparent medium. According to Fresnel's theory, the ether and refractive medium satisfy a continuity equation at their boundary; this has the consequence that the retardation due to the medium is not affected by the motion of the earth.[(33)] Stokes drew

(30) *Scientific Papers*, **1**: 500. See Daniel M. Siegel, *Innovation in Maxwell's Electromagnetic Theory: Molecular Vortices, Displacement Current and Light* (Cambridge, 1991): 120–43.

(31) See Everitt, *James Clerk Maxwell*: 114, 118–23.

(32) See Number 227 notes (4) and (7).

(33) See Number 227 note (5).

Maxwell's attention to the error when he submitted his paper to the Royal Society (Number 228). Indeed, in 1846 Stokes had himself considered this problem, and had concluded that the motion of the ether would have no effect on the refraction of light.[(34)]

Maxwell withdrew the paper in response to Stokes' criticism of its argument, but he gave an account of his experiment in a letter written in 1867 to the astronomer William Huggins (Number 271). Here he reported the result of his aborted 1864 paper, that his experiment had failed to find any effect of the motion of the earth on the refraction of light; but he now pointed out that Stokes had already established this conclusion. Maxwell's subsequent discussion of the problem – notably in a letter to the American astronomer David Peck Todd[(35)] – led A. A. Michelson in the 1880s to undertake his famous experiments to attempt to detect ether drag.

On writing to Stokes in May 1864 Maxwell remarked that 'I am not inclined and I do not think I am able to do the dynamical theory of reflexion and refraction on different hypotheses' (Number 228), alluding to his paper in preparation on 'A dynamical theory of the electromagnetic field'. But he did make an attempt to derive the laws of optical reflection and refraction from his electromagnetic theory of light; a fragmentary draft (Number 236) records his substitution of electromagnetic analogues for the elastic variables employed in theories of the luminiferous ether. In the event this derivation remained abortive: writing to Stokes in October 1864 (Number 237) he criticised the selectivity of the physical assumptions and boundary conditions which were required, an endemic difficulty of dynamical ether theories. Thus he told Stokes that 'I have written out so much of the theory as does not involve the conditions at bounding surfaces'; and his paper 'A dynamical theory of the electromagnetic field', which he was about to forward to the Royal Society, does not include discussion of optical reflection and refraction.

It might have been anticipated that he would broaden the scope of the electromagnetic theory of light in his *Treatise on Electricity and Magnetism*, to encompass an electromagnetic theory of optical reflection and refraction. But he did not do so, explaining (in February 1879) that 'In my book I did not attempt to discuss reflexion at all. I found that the propagation of

(34) See Number 228 note (4). Maxwell subsequently set a Mathematical Tripos question on this problem: see Number 228 note (5).

(35) Maxwell's letter of 19 March 1879 to D. P. Todd (to be reproduced in Volume III) was published posthumously as 'On a possible mode of detecting a motion of the solar system through the luminiferous ether', *Proc. Roy. Soc.*, **30** (1880): 108–10 (= *Nature*, **21** (1880): 314–15); and see Number 271 note (17).

light in a magnetized medium was a hard enough subject'.(36) The inherent complexity of the problem, aggravated by his lack of easy familiarity with ether dynamics, led him to restrict the scope of his explication of the electromagnetic theory of light.

Maxwell's limitation of the range of his electromagnetic theory of light as a general theory of optics is paralleled by his apparent failure to consider the question of the production of electromagnetic waves.(37) He conceived the generation of light to be a mechanical rather than an electromagnetic process, a phenomenon of molecular motion in the ether. This was consonant with his interpretation of the Faraday magneto-optic effect in the *Treatise* (Numbers 434, 441 and 468): that the magneto-optic rotation implied the rotation of vortices in the ether. Writing to Thomson in January 1873, shortly before the publication of the *Treatise*, he noted that 'Faradays twist of polarized light will not come out without what the schoolmen call local motion' (Number 434). The relation between optics and electromagnetism was expressed in terms of the molecular connection between ether and matter. This was therefore, as he explained in February 1879, a 'hybrid theory' based on 'the bodily motion of the medium', not a 'purely electromagnetic hypothesis'.(38)

This approach to optics is manifest in several drafts which relate to an examination question he set for the 1869 Cambridge Mathematical Tripos, where he explores the implication for physical optics of the mechanical relation between ether and matter. He supposes that the forces acting on the particles of matter originate in the ether, these particles being assumed to be independent of each other. This would, he noted in a draft in 1868, entail an 'irregularity of refraction' (Number 300). Returning to the subject in 1873, after the public discussion of anomalous dispersion,(39) he concluded that his

(36) ULC Add. MSS 7656, M 439, printed in part in Larmor, *Correspondence*, **2**: 40–3 (to be reproduced in Volume III).

(37) See Bruce J. Hunt, *The Maxwellians* (Ithaca/London, 1991): 28–30; and Thomas K. Simpson, 'Maxwell and the direct experimental test of his electromagnetic theory', *Isis*, **57** (1966): 411–32.

(38) See note (36). On Maxwell's theory of molecular vortices and the Faraday effect see Siegel, *Innovation in Maxwell's Electromagnetic Theory*: 29–84, 144–67; and Ole Knudsen, 'The Faraday effect and physical theory, 1845–1873', *Archive for History of Exact Sciences*, **15** (1976): 235–81, esp. 248–61.

(39) See Number 460 note (15); Jed Z. Buchwald, *From Maxwell to Microphysics. Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago/London, 1985): 233–7; and E. T. Whittaker, *A History of the Theories of Aether and Electricity*, 2 vols. (London, 1951–3), **1**: 261–5.

model of ether interacting with matter would yield an explanation of this phenomenon (Numbers 460 and 461).

The kinetic theory of gases and molecular physics

In November 1857, while engaged in revising for publication his Adams Prize essay *On the Stability of the Motion of Saturn's Rings*, Maxwell remarked to William Thomson that 'the general case of a fortuitous concourse of atoms each having its own orbit & excentricity is a subject above my powers at present'. He amplified the point in the memoir: 'When we come to deal with collisions among bodies of unknown number, size, and shape, we can no longer trace the mathematical laws of their motion with any distinctness'.[40] Concern with the problem of calculating the trajectories of particles in a complex dynamical system helped to alert his interest in the kinetic theory of gases. On reading a paper on the subject by Rudolf Clausius in 1859 he was led to a study of the collisions of particles as a means of establishing the properties of gases. The statistical method of his theory of gases provided a means of describing the complex pattern of the motion of gas molecules. The problem of stability (of the rings of Saturn) was transformed into a problem of molecular regularity represented by a statistical law. The success of his statistical method in the theory of gases led him in 1864 to a vain attempt to apply the method to compute the collisions of the particles constituting Saturn's rings, so as 'to throw some light on the theory of a confused assemblage of jostling masses whirling round a large central body' (Number 224).

In his paper 'Illustrations of the dynamical theory of gases' Maxwell had advanced on Clausius' use of a statistical argument to calculate the probability of a molecule travelling a given distance (the 'mean free path') without collision. He had introduced a statistical function for the distribution of velocities among the gas molecules.[41] This description of physical processes

(40) Maxwell to William Thomson, 14 November 1857 (Volume I: 555); J. Clerk Maxwell, *On the Stability of the Motion of Saturn's Rings* (Cambridge, 1859): 53 (= *Scientific Papers*, **1**: 354). See P. M. Harman, 'Maxwell and Saturn's rings: problems of stability and calculability', in *The Investigation of Difficult Things. Essays on Newton and the History of the Exact Sciences in Honour of D. T. Whiteside*, ed. P. M. Harman and Alan E. Shapiro (Cambridge, 1992): 477–502.

(41) Maxwell's and Clausius' papers are cited in Number 207 notes (5) and (7). On Maxwell's theory of gases see especially the introductory essay by Elizabeth Garber, Stephen G. Brush and C. W. F. Everitt, 'Kinetic theory and the properties of gases: Maxwell's work in its nineteenth-century context', in *Molecules and Gases*: 1–63; Stephen G. Brush, *The Kind of Motion We Call Heat: a History of the Kinetic Theory of Gases in the Nineteenth Century*, 2 vols. (Amsterdam/New York, 1976); and Everitt, *James Clerk Maxwell*: 131–63.

by a statistical function was a major innovation in the science of physics. However his treatment of the conduction of heat in gases was criticised by Clausius, who pointed out that Maxwell had disregarded the additional kinetic energy associated with motion in the direction of the temperature gradient, and had assumed an isotropic distribution function.(42) In attempting to meet Clausius' critique Maxwell introduced a variable path length (depending on the position of a particle between collisions), so as to consider particle collisions where the properties of the gas vary from place to place; but his attempt to draft a paper on these lines (Number 207) proved inconclusive, and was abandoned.

This failure to resolve Clausius' criticisms may have prompted Maxwell to a decision to undertake an investigation of transport phenomena. His experimental study of gas viscosity was suggested by the apparatus used in the determination of the standard of electrical resistance, using magnets to vibrate discs suspended in gases and measuring the oscillations by mirrors attached to the suspension (Number 212). He apparently began some experiments in late 1863;(43) but his correspondence indicates that systematic work, using the apparatus described in his Royal Society paper on gas viscosity (Number 252), was established in spring 1865 (Numbers 240, 242 and 244 to 249). The elastic-sphere model for gas molecules that he had assumed in his paper 'Illustrations of the dynamical theory of gases' implied that viscosity would vary as the square root of the temperature;(44) but he found that the viscosity was a linear function of the absolute temperature (Numbers 249 and 252). The hypothesis that gas molecules could be represented as colliding elastic spheres was therefore in question; and in drafting his paper 'On the dynamical theory of gases' he replaced this model by the concept of considering gas molecules as centres of force subject to a law of repulsion (Number 250).

The experiments on gas viscosity thus initiated a radical reconstruction of Maxwell's kinetic theory of gases. He computed the motions of molecules travelling in complicated trajectories by using the methods of orbital dynamics (Number 259). To describe gas viscosity he abandoned the notion of a 'mean free path' and in its place introduced the concept of the 'time of relaxation' of stresses in the gas.(45) He found that if the force law of repulsion between the molecules was an inverse fifth-power law, the viscosity varied directly with temperature, a result which was in agreement with his experiments and consistent with his definition of viscosity in terms of the

(42) See Number 207 notes (9) and (39).

(43) See Number 212 note (11).

(44) See Number 246 note (7).

(45) See Number 259 note (12), and Everitt, *James Clerk Maxwell*: 143–4.

relaxation time. This theory was developed in his paper 'On the dynamical theory of gases' (Number 263), where he presented a new derivation of the statistical distribution law, demonstrating that the velocity distribution would maintain a state of equilibrium unchanged by collisions. The equilibrium distribution provided the basis for calculating the properties of gases.

In drafting 'On the dynamical theory of gases' Maxwell considered the question of the equilibrium of temperature in a vertical column of gas under gravity (Number 259). He found that, according to his theory, the temperature of the gas would diminish as the height increased: the condition of final equilibrium would therefore be one of 'mechanical instability', and 'the energy thus developed could be transferred to machinery so as to convert the invisible agitation of the gas into any other form of energy and thus form a perpetual motion'. Maxwell's theory seemed to have the consequence that energy could be abstracted from a gas acted on by gravity while the gas cooled, a result which he found 'directly opposed to the second law of Thermodynamics'. On writing to William Thomson in February 1866 he emphasised the conclusion which he had drawn: 'by means of material agency mechanical effect is derived from the gas under gravity by cooling it below the temperature of the coldest of the surrounding objects' (Number 260), explicitly evoking the language of Thomson's own statement of the second law of thermodynamics.

By the time Maxwell submitted the paper to the Royal Society in May 1866 he had discovered one of the errors in his analysis, a mistake in mathematical reasoning;[46] but this partial correction led to the conclusion that the temperature would increase with the height (Number 263). Thomson reviewed the paper for the Royal Society, but was unable to detect the error that still compromised its argument. However, in December 1866 Maxwell reported to Stokes (the Secretary of the Royal Society) that he had located the error in his statistical reasoning: 'I now make the temperature the same throughout' (Number 266), a revision incorporated in an 'addition' which was appended to the paper prior to its publication in the *Philosophical Transactions*.[47]

Maxwell returned to this subject in 1873 (Numbers 457 and 481) in response to correspondence in the journal *Nature* on his statement in his *Theory of Heat* that gravity had no effect on the temperature of a column of gas. He re-examined the question (Numbers 472 and 473), confirming the conclusion presented in the 'addition' to his paper 'On the dynamical theory

(46) See Number 259 note (33).

(47) See Number 266 note (8).

of gases'. He reformulated the presentation of the statistical foundations of his theory of gases, employing a method derived from a paper by Ludwig Boltzmann on the general Maxwell–Boltzmann distribution law for complex molecules in the presence of an external field of force (Number 474).[48]

Maxwell's kinetic theory of gases, which provided an explanation of the transport properties of gases, had other important implications for molecular physics. One of its consequences was his derivation of 'Avogadro's hypothesis' (Number 259).[49] In a discussion following Benjamin Collins Brodie's presentation of his symbolic calculus of chemical operations at a meeting of the Chemical Society in June 1867, Maxwell emphasised that the derivation of Avogadro's hypothesis from the kinetic theory of gases provided important support for the molecular theory of matter (Number 270). His concern with understanding the nature of matter led also to an interest in molecular forces. This is apparent in his construction of apparatus to measure the surface tension of liquids, a study of the physics of capillary action (Numbers 292 and 293).

In 1873 he made a further advance in the application of the kinetic theory of gases to molecular physics. He developed Loschmidt's attempt to gain an estimate of molecular diameters from a study of the diffusion of gases, and obtained values which were in agreement with calculations based on viscosity (Numbers 469, 470 and 471). He gave a broad summary of his work on gases, on molecular physics and his method of statistical representation, in a lecture on 'Molecules' delivered to the meeting of the British Association for the Advancement of Science in September 1873 (Number 478). His application of mathematical methods to the study of gases had led him to a major innovation in mathematical physics, and to important results in molecular science. The lecture on 'Molecules' provided a suitable capstone to this work, which established Maxwell's international reputation in the 1860s.

Maxwell's text on the *Theory of Heat* was an outgrowth from his work on the theory of gases. The circumstances under which he undertook the commission from Longmans are not clear, and its writing led to an intermission in his work on the *Treatise on Electricity and Magnetism*. His letters written in late 1869 (Numbers 332 and 333) show that this work was well advanced at that time, but was apparently only resumed in the following autumn (Number 346). It seems that he wrote the *Theory of Heat* in 1870: in a letter to Thomson in April

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(48) See Number 472 note (12). See also Stephen G. Brush, 'Foundations of statistical mechanics, 1845–1915', *Archive for History of Exact Sciences*, **4** (1967): 145–83 (= Brush, *The Kind of Motion We Call Heat*, **2**: 335–85).

(49) See Number 259 notes (13) and (14).

1870 concerned with the conduction of heat he states that 'I am boiling all this down for my chapter on Conduction' (Number 339). It is in this letter that he first signed himself *dp/dt*, his thermodynamic signature.[(50)]

The origins of his interest in writing a text on heat may perhaps lie in his friend Peter Guthrie Tait's request in December 1867 that he read draft chapters of Tait's *Sketch of Thermodynamics*. His immediate response (Number 277) and the course of reviewing Tait's work (Numbers 278 and 284) may have helped to foster a more general interest in thermodynamics, apparent in his letters of April 1868 to Mark Pattison (Numbers 286 and 287). Tait's partialities did however have an adverse effect on the argument of the *Theory of Heat*, leading Maxwell to become confused about the significance of Clausius' contributions to thermodynamics, notably over the meaning of 'entropy'.[(51)] The concepts that Clausius had introduced to explicate his thermodynamic theory – entropy, disgregation, ergal and virial – were for Maxwell the butt of jocular barbs (Numbers 356, 402 and 403). But as he finally came to realise (Number 483), his confusion over the meaning of 'entropy' had disfigured the text of his book.

By April 1871 Maxwell was beginning to correct the proofs of the *Theory of Heat*. On writing to Tait in April and May 1871 (Numbers 366, 367, 372, 373 and 374) he mentioned points which arose during his proof-reading of the early chapters of the book; this also prompted a letter to C. W. Siemens on thermo-electricity (Number 378). Two letters to James Thomson (Numbers 381 and 382), bearing on the account in the *Theory of Heat* of Thomas Andrews' work on the continuity of the liquid and gaseous states, raised questions that Maxwell was anxious to resolve during the correction of proofs. He was concerned to establish the conditions determining the pressure at which gas and liquid can coexist at equilibrium. The problems about molecular forces raised by Andrews' and Thomson's work on the continuity of the gas–liquid transition were central to Maxwell's endeavour to understand the phenomena of molecular physics.

The paradoxes of dynamics and thermodynamics

Maxwell's initial discussion in early 1866 of the equilibrium of temperature in a column of gas had led him to a 'paradox': 'a collision between Dynamics & thermodynamics' (Number 260). He confessed in 1873 that until he had

(50) See Number 339 note (17).

(51) See Number 483 notes (19) to (22); Martin J. Klein, 'Gibbs on Clausius', *Historical Studies in the Physical Sciences*, **1** (1969): 127–49; and Edward E. Daub, 'Entropy and dissipation', *ibid.*, **2** (1970): 321–54.

resolved it, the problem 'nearly upset my belief in calculation' (Number 457). The paradox highlighted the issue of the relation between Maxwell's statistical theory of molecular motions and the second law of thermodynamics. This law had been first stated by Clausius and Thomson in the early 1850s, and its interpretation – as a law denoting the tendency of heat to pass from warmer to colder bodies – was still under debate.

It is likely that reflection on the problem of the equilibrium of temperature in a column of gas led Maxwell to consider the bearing of his statistical theory of the distribution of velocities among gas molecules on the interpretation of the second law of thermodynamics. It was in immediate response to Tait's request to read draft chapters of his *Sketch of Thermodynamics* that Maxwell first formulated the famous 'demon' paradox in December 1867 (Number 277).(52) The purpose of Maxwell's 'finite being' was to 'pick a hole' in the second law of thermodynamics, by exposing the problem of explaining the irreversible flow of heat from warmer to colder bodies in terms of the statistical regularities that describe the motion of gas molecules. As he later explained to Tait, his purpose was 'To show that the 2nd law of Thermodynamics has only a statistical certainty'.(53) Because of the statistical distribution of molecular velocities in a gas at equilibrium there will be spontaneous fluctuations of individual molecules, fluctuations that take heat from a cold body to a hotter one. While it would require the action of the 'finite being' to select individual molecules and produce an observable flow of heat from a cold body to a hotter one, this process occurs spontaneously at the molecular level. If it were possible to manipulate the molecules individually then the second law of thermodynamics could be violated; this law is therefore a statistical law which applies to systems of molecules, not to the fluctuations of individual molecules.

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(52) The term 'demon' was apparently first used by William Thomson. But see William Thomson, 'The kinetic theory of the dissipation of energy', *Nature*, **9** (1874): 441–4, on 442n (= *Math. & Phys. Papers*, 5: 12n), where he defines 'a "demon", according to the use of this word by Maxwell'. The term 'demon' did not receive Maxwell's approbation. In his letter of 6 December 1870 to J. W. Strutt (Number 350, and see note (55) below) he described his 'finite being' as a 'self-acting' device; and in an undated note to P. G. Tait headed 'Concerning Demons' (ULC Add. MSS 7655, V, i/11a, to be published in Volume III; the MS is printed in part in Knott, *Life of Tait*: 214–15) he declared: 'Call him no more a demon but a valve', observing: 'Who gave them this name? Thomson', unequivocally ascribing the term 'demon' to Thomson.

(53) In the MS 'Concerning Demons': see note (52). For analysis see Martin J. Klein, 'Maxwell, his demon, and the second law of thermodynamics', *American Scientist*, **58** (1970): 84–97; and also Stephen G. Brush, 'The development of the kinetic theory of gases. VIII. Randomness and irreversibility', *Archive for History of Exact Sciences*, **12** (1974): 1–88 (= Brush, *The Kind of Motion We Call Heat*, 2: 543–654).

Maxwell subsequently amplified his argument to highlight the disjunction between the laws of dynamics and the second law of thermodynamics. In a letter to William Thomson in 1857[(54)] he had first considered the dynamical implications of time-reversal on the motion of particles; writing to Mark Pattison in April 1868 he brought together his discussion of the perfect reversibility of particle motions with his understanding of the essential irreversibility of natural processes. Dynamical laws allow that everything could 'happen backwards', he told Pattison, but 'our experience of irreversible processes... leads to the doctrine of a beginning & an end instead of cyclical progression for ever' (Number 286). He illustrates irreversible processes by the jumbling together of black and white balls in a box, where 'the operation of mixing is irreversible' (Number 287).

He conjoined his arguments on the perfect reversibility of the laws of dynamics, the irreversibility of natural processes, and the statistical interpretation of the second law of thermodynamics, in a letter to John William Strutt (later Lord Rayleigh)[(55)] in December 1870. He argued that the perfect reversibility allowed for by the laws of mechanics is constrained by the irreversibility of natural processes. The 'finite being' – now described as a 'mere guiding agent', a 'self-acting' device which could select molecules – was introduced to illustrate his statistical interpretation of irreversible processes as described by the second law of thermodynamics, which 'has the same degree of truth as the statement that if you throw a tumblerful of water into the sea you cannot get the same tumblerful of water out again' (Number 350).

Maxwell's insistence on the disjunction between the laws of mechanics and the second law of thermodynamics led him to be severely critical of attempts, notably by Boltzmann and Clausius, to reduce the second law of thermodynamics to a theorem in dynamics. The second law of thermodynamics was a fundamentally statistical law; to attempt to give it a purely mechanical interpretation, he joked to Tait in December 1873, was in the realm of 'cloud-cuckoo-land' where the 'German Icari flap their waxen wings', doomed to catastrophic disappointment in the pursuit of an illusion (Number 483).[(56)]

Maxwell's contrast between the reversible laws of mechanics and the

(54) Maxwell to William Thomson, 24 November 1857 (Volume I: 561–2).

(55) John William Strutt, Trinity 1861, senior wrangler 1865, third Baron Rayleigh 1873 (*DNB*).

(56) See Klein, 'Maxwell, his demon, and the second law of thermodynamics'; and Günter Bierhalter, 'Von L. Boltzmann bis J. J. Thomson: die Versuche einer mechanischen Grundlegung der Thermodynamik (1866–1890)', *Archive for History of Exact Sciences*, **44** (1992): 25–75, esp. 28–48.

irreversibility of natural processes bore on the problems of materialism and determinism that exercised his circle in this period.(57) As he explained to Mark Pattison, a 'strict materialist believes that everything depends on the motion of matter'; and while the perfect reversibility of particle motions was consistent with the doctrine of materialism, 'in the present dispensation there remain a number of irreversible processes' (Number 286). The second law of thermodynamics, an irreducibly statistical law, was inconsistent with the supposition of a wholly mechanical universe.

In his lecture on 'Molecules' to the British Association in 1873 (Number 478) he appealed to Lucretius 'who attempted to burst the bounds of Fate by making his atoms deviate from their courses at quite uncertain times and places, thus attributing to them a kind of irrational free will'.(58) At the time of writing 'On the dynamical theory of gases' in February 1866 he had sought to clarify the meaning of Lucretius' atomism, making specific allusion to the swerve of the atoms (Number 257); and he quoted the passage in *De Rerum Natura* on the swerve of atoms and free will in his memorandum written for William Thomson in 1871 on the history of the 'kinetic theory of gases' (Number 377).(59) This theme is explored in some detail in his 1873 essay on 'the progress of Physical Science...and the Freedom of the Will' (Number 439). Here he argued that the universe was fundamentally causal yet not deterministic, citing the swerve of Lucretian atoms in illustration of the instability of a dynamical system at 'singular points'. Such instabilities were not uncaused but were incalculable; there were therefore limits to the perfect predictability of the Laplacian deterministic universe, which contrasted with 'a world like this, in which the same antecedents never again concur, and nothing ever happens twice'.(60)

His critique of materialism included scrutiny of the concept of matter. He explained to Pattison in April 1868 that he found it unacceptable to define matter as 'that which is perceived by the senses' or to conceive inertia as 'metaphysical passivity' (Number 287). These definitions, intended to provide foundations for the science of dynamics, had been stated by Thomson and Tait in their recently published *Treatise on Natural Philosophy*. Provoked

(57) See Theodore M. Porter, *The Rise of Statistical Thinking 1820–1900* (Princeton, 1986): 194–208; and Smith and Wise, *Energy and Empire*: 621–33.

(58) *Scientific Papers*, **2**: 373; and see Number 439 note (19).

(59) See Numbers 257 esp. note (10) and 377 esp. note (5).

(60) In a letter to Francis Galton of 26 February 1879 (to be reproduced in Volume III) he refers to the 'bifurcation of path' of a mechanical system at a point of singularity, referring to Joseph Boussinesq, *Conciliation du Véritable Déterminisme Mécanique avec l'Existence de la Vie et de la Liberté Morale* (Paris, 1878), where the phrase 'lieux de bifurcation' is used (on p. 50). See Harman, 'Maxwell and Saturn's rings: problems of stability and calculability': 496–501.

by their discussion (Number 294: Appendix), he cited a passage from Torricelli's *Lezioni Accademiche* in support of his contention that, in physics, matter should be defined in relation to energy and momentum; the concept of a material substratum endowed with inertia did not provide the basis for the science of dynamics.(61) In the *Treatise* he refers to the 'fundamental dynamical idea of matter', which he defines as being 'capable by its motion of becoming the recipient of momentum and of energy'.(62) He maintained that speculations about matter as a passive entity defining substances belonged to metaphysics rather than to the science of dynamics (Numbers 287 and 437: Appendix). Maxwell thus aimed to establish the conceptual basis of his dynamical theory of the electromagnetic field, and to refute the claim that the doctrines of materialism were sanctioned by the science of physics.

Maxwell endeavoured to present his physics in terms which evoked continuity with classical debates in the philosophy of science. An important example of this style of argument, where he sought to give his ideas the pedigree of historical tradition, is in his presentation of the theory of the electromagnetic field. In a lecture in early 1873 (Number 437) he stressed that the theory had as its aim the resolution of the enigma in Newton's natural philosophy, the problem of action at a distance. He had indeed sought to broaden his field theory to embrace the explanation of gravity, but without success (Numbers 238 and 287). The analogy of the tails of comets led him to envision lines of gravitational force spreading through space (Numbers 217 and 309), a representation of gravity that he had suggested to Michael Faraday in 1857;(63) but the imagery, though powerful, merely remained suggestive.

Geometry, mechanics and optics: the principle of duality

As a boy, Maxwell's scientific imagination had been aroused by geometry, leading to his first mathematical papers on the description of ovals and on the theory of rolling curves. The study of geometrical problems remained a

(61) On Torricelli see Numbers 287, esp. note (12); 294: Appendix, esp. note (29); and 437: Appendix, esp. note (48). On inertia, the definition of matter and Thomson and Tait's *Natural Philosophy*, see Numbers 287, esp. note (10); 294: Appendix, esp. notes (27) and (31); and 437: Appendix, esp. note (47).

(62) *Treatise*, 2: 181 (§550). See P. M. Harman, *Metaphysics and Natural Philosophy. The Problem of Substance in Classical Physics* (Brighton, 1982): 127–50; Harman, 'Newton to Maxwell: the *Principia* and British physics', *Notes and Records of the Royal Society of London*, **42** (1988): 75–96, esp. 88–92.

(63) Maxwell to Michael Faraday, 9 November 1857 (Volume I: 550–1).

lifelong concern (Numbers 320, 407 and 409), and included an abiding interest in understanding the mathematical properties of Cartesian ovals (Number 267). In the 1850s the analogy between the geometry of lines and surfaces and the imagery of Faraday's lines of force had shaped the geometrical expression of his field theory in his paper 'On Faraday's lines of force'. The commitment to geometrical representation was incorporated into his *Treatise on Electricity and Magnetism*, where topological ideas form an important element in the work's mathematical style.

Geometrical analogy continued to play a significant role in shaping his physics, especially in the application of contemporary methods of projective geometry. He first used these ideas in applying graphical analysis to the theory of frameworks; this theory formed an important element of his work in this period. He lectured on the theory of engineering structures to his class at King's College, London (Number 203), leading to a paper 'On reciprocal figures and diagrams of forces';[(64)] and he subsequently extended this theory of reciprocal diagrams in statics (Numbers 273 and 334). This work drew on projective geometry (termed the 'geometry of position' or 'modern geometry'),[(65)] especially the concept of geometrical correspondence between reciprocal figures. This method, as he explained in applying it to geometrical optics, was based on the 'principle of duality... the leading idea of modern geometry' (Number 480). He conceived this to be a general geometrical method, and by the process that he later termed the 'cross-fertilization of the sciences',[(66)] he applied the principle of reciprocity to the study of electrical potential (Number 274); to the depiction of the velocities of molecules in a gas by means of Hamilton's geometrical representation of the paths of particles, the hodograph (Number 472);[(67)] and to geometrical optics, where the 'object and image were homographic figures' (Number 480).[(68)]

Maxwell had been interested in optical instruments and geometrical optics

(64) See Number 203 note (4). See T. M. Charlton, *A History of the Theory of Structures in the Nineteenth Century* (Cambridge, 1982): 58–66; and Erhard Scholz, *Symmetrie, Gruppe, Dualität. Zur Beziehung zwischen theoretischer Mathematik und Anwendungen in Kristallographie und Baustatik des 19. Jahrhunderts* (Basel/Boston/Berlin, 1989): 181–201.

(65) For Maxwell's use of these terms see Numbers 472 § 1, 480 and 482. The term 'geometry of position' was also applied to topology: see Number 373, and note (82).

(66) *Scientific Papers*, **1**: 744.

(67) On the hodograph see Number 472 note (6); and Thomas L. Hankins, *Sir William Rowan Hamilton* (Baltimore/London, 1980): 327–33.

(68) See Number 273 note (2); and also Lorraine J. Daston, 'The physicalist tradition in early nineteenth-century French geometry', *Studies in History and Philosophy of Science*, **17** (1986): 269–95; and Joan L. Richards, 'Projective geometry and mathematical progress in mid-Victorian Britain', *ibid.*: 297–325.

in the 1850s, when he had expounded a theory of optical instruments in terms of geometrical relations divorced from consideration of the dioptrical properties of lenses. In 1867 he constructed a real image stereoscope (Number 272), which he used to project stereograms of surfaces (Numbers 274, 275, 277 and 279); and the following year he developed the popular zoetrope or 'wheel of life', which he used to illustrate Helmholtz's vortex rings and motion in a fluid (Numbers 306, 307 and 310). His application of the principle of duality to geometrical optics enlarged on his earlier work, enabling him to present the subject within a general mathematical framework, as he announced to the meeting of the British Association for the Advancement of Science in September 1873 (Number 480). As he subsequently explained to Lord Rayleigh, 'I am getting more light on Geometrical Optics', having grasped that the 'geometry of the subject is the geometry of position' (Number 482). The principle of duality served as a method of geometrical analogy, linking the graphical analysis of frames, geometrical optics, electrical circuits, and the kinetic theory of gases.

Reports for the Royal Society

Following his election as a Fellow of the Royal Society in 1861 Maxwell began to be asked to referee papers submitted for publication in the *Philosophical Transactions*. By the 1860s the Royal Society had regularised the process of reviewing papers.(69) Many of the papers read at meetings of the Society were submitted for publication in the *Transactions*. One of the Secretaries of the Society (in 1862, William Sharpey and George Gabriel Stokes, Sharpey being succeeded by Thomas Henry Huxley in 1872) would secure reports by (generally) two Fellows judged to have appropriate expertise. These reports were then considered by the Committee of Papers. If the paper was accepted, publication in the *Transactions* would follow; if rejected, its manuscript would be preserved in the archives of the Society. Abstracts of the papers read at meetings were printed in the Society's *Proceedings* (where short papers were also included), but publication in the *Transactions* was subject to the reviewing process.(70)

(69) Marie Boas Hall, *All Scientists Now. The Royal Society in the Nineteenth Century* (Cambridge, 1984): 68.

(70) The MSS of papers accepted for publication in the *Phil. Trans.* were sometimes, for years up to 1865, preserved in the archives of the Royal Society: see Numbers 200 note (7), 206 note (5), 212 note (6), 223 notes (9) and (15), and 239. An example of the form letter issued by the Secretaries of the Royal Society to invite a report from a referee is reproduced in Wilson, *Stokes–Kelvin Correspondence*, **1**: 323–4, this example being the paper 'On the viscosity of gases by M^r J. C. Maxwell'; for Thomson's report on Maxwell's paper see Number 252 note (3).

Maxwell's referee reports cover a wide range of subjects: optics, mechanics, hydrodynamics, elasticity, thermodynamics, gas theory, electricity, magnetism, electro-physiology, and visual perception. This breadth of coverage no doubt demonstrates Stokes' opinion of his competence and critical acuity over the entire range of physics. These reports are generally substantive essays offering significant commentary on the papers reviewed. The reports are informative about Maxwell's grasp of the topics reviewed; and in many cases the request to referee papers elicited his comments on problems not discussed elsewhere in his writings. He consistently maintained a commitment to the intellectual value of the refereeing process, to standards of rigorous and informative comment. Even when his conclusions are wholly negative, for example in his severely critical report on Robert Moon's theory of elastic impact (Number 291), his comments are substantive and suggestive, not briefly dismissive. When a paper strikes him as in some way confused, for example Frederick Guthrie on the electrical properties of hot bodies (Number 442) and Henry Moseley on glacier motion (Number 319), his comments are nevertheless informative, clearly intended to be helpful to the author.

On some occasions, as in his reviews of Des Cloiseaux on the optical properties of crystals (Number 290) and Bland Radcliffe on animal electricity (Number 342), he mentions his lack of specialised knowledge of these fields of research. The boundaries of his expertise are interesting in their own right. Thus his report on Samuel Haughton's paper on the reflection of polarised light (Number 199) clearly indicates his lack of familiarity with the intricacies of the wave theory of light (Number 200), a limitation that is also apparent in his attempt to discuss the motion of the earth through the ether (Numbers 227 and 228) and to elaborate an electromagnetic theory of optical reflection and refraction (Numbers 236 and 237). Several of the reports bear on topics that had been of major interest to him earlier in his career: on Jago on vision (Number 447), Everett's experiments on elasticity (Numbers 261, 269 and 282), Airy on the theory of elasticity (Numbers 205, 206 and 212), and Ferrers on the theory of rotating bodies (Number 325). His reports on these papers provided him with an arena for the expression of his current outlook on problems on which he would not otherwise have written so substantively, and his comments are often informative about his early work on these topics.

Many of the reports are on fields of research which connected more directly with his current interests and writing: on Chambers on the sun's magnetism (Number 220), Rankine on potential theory and hydrodynamics (Numbers 223 and 337) and on the thermodynamics of waves (Number 338), Airy on magnetism (Numbers 410 and 453), Latimer Clark on a standard of electromotive force (Numbers 415 and 462; and see Numbers 416, 418 and 420), M'Kichan on the ratio of electrical units (Number 455), Strutt

(Rayleigh) on potential theory and acoustics (Numbers 354 and 355), Stokes on wave propagation in a gas (Number 298) and Graham on the absorption of gases (Number 264). These reports provide insight into the development and scope of his physics, supplementing his letters and manuscript drafts.

There are several reports on papers concerned with experimental topics that have a less immediate relevance to Maxwell's own current work: on papers by Stokes, Robinson and Miller on spectra (Numbers 197, 198, 199 and 201), and by Tyndall on radiant heat (Numbers 255 and 258). Here his discussion reveals his grasp of the contemporary literature and his understanding of broader developments in physical science. His report on papers by Bashforth, Longridge and Merrifield on the motion of projectiles (Number 288) demonstrates his command over a traditional problem in dynamics; while his review of Tarn's paper on the stability of domes (Number 265) illustrates his knowledge of the subject of vaulted structures and elastic systems, an interest which forms a strand of his work in this period (Numbers 221 and 392).(71) On occasion the task of reviewing papers may have directly stimulated his own research, as in his development (Number 334) of Airy's function of stress, a concept that he had encountered and discussed in the course of writing a report (Number 205).

The *Treatise on Electricity and Magnetism*

Maxwell's correspondence with Tait, late in 1867, provides the first indication of his intention to write a treatise on electricity and magnetism.(72) In a letter to William Thomson in February 1868 (Number 281) he emphasised from the outset the mathematical style of his work, specifically mentioning potential theory (Green's theorem and spherical harmonic analysis). It is likely that the project was suggested by the publication of Thomson and Tait's *Treatise on Natural Philosophy* in 1867. Late that year he stated an interest in the application of potential theory to electrostatics (Number 274), and in spherical harmonics and in the symbolism appropriate to the representation of the Laplacian operator (Number 277); and it may be surmised that he had commenced work on the *Treatise* at this time.

His correspondence with William Thomson and Peter Guthrie Tait in the period 1868–71 enables his progress in writing the *Treatise* to be charted. The

(71) See Charlton, *History of the Theory of Structures*: 77–81; and Edoardo Benvenuto, *An Introduction to the History of Structural Mechanics*, 2 vols. (New York/Berlin/Heidelberg/London, 1991), **2**: 499–507.

(72) P. G. Tait to Maxwell, 27 November 1867 (Number 276 note (2)) and 6 December 1867 (Number 277 note (2)).

topics discussed in these letters (and from 1871 postcards, taking advantage of the new halfpenny postage) reflect Maxwell's perception of his correspondents' interests and special competence. The letters to Thomson in 1868–69 are dense with discussion of electrostatics and magnetism; while the correspondence with Tait (especially after November 1870) focuses especially on mathematical methods. While only a few of Thomson's letters to Maxwell are extant, the Maxwell–Tait correspondence is much more complete, and the nature of their relationship can be readily gauged. For Tait, Maxwell served as an inexhaustible fount of knowledge and critical insight across the range of physics; while Maxwell drew upon Tait's expertise in spherical harmonics and quaternions, mathematics that he sought to apply in the *Treatise*.

Maxwell's relationship with Thomson at this time was more complex. Thomson was currently engaged in preparing his *Reprint of Papers on Electrostatics and Magnetism*. One problem which he was re-working for the volume, on the distribution of electricity on the surface of a spherical bowl – 'the most remarkable problem of electrostatics hitherto solved', as Maxwell later acclaimed it(73) – is discussed in some detail (Numbers 310, 326 and 327); and the tone of Maxwell's letters makes abundantly clear his respect for Thomson's mastery of the mathematical theory of electrostatics. But Maxwell's general limitation of his correspondence with Thomson to these topics suggests his perception of the likely boundary of Thomson's interests and expertise. Correspondence between Tait, Thomson and Maxwell in July 1868 is suggestive of Thomson's view of Maxwell's conceptual acumen and command of mathematical physics. Thomson wrote to Tait puzzling over problems in the theory of vortex motion,(74) and suggested that Tait forward his letter to Maxwell in the hope that Maxwell might be able to shed light on the problems with which he was grappling unavailingly. Maxwell responded with an extraordinarily rich analysis (Number 295), expounding the mathematical analogy between vortex motion in a fluid and electromagnetism. Thomson apparently felt an element of competition with Maxwell at this time, describing one instance of their separate endeavours, on the axis of a magnet (Number 383), as a 'race'.(75)

Maxwell did on occasion communicate to Thomson his ideas on some of the deeper issues lying at the foundations of his theory of the electromagnetic field. A crucial feature of Maxwell's field theory, which differentiates the

(73) *Scientific Papers*, **2**: 303 (in his review of *Electrostatics and Magnetism*); see Number 310 note (2).

(74) William Thomson to P. G. Tait, 5 July 1868 (Number 295 note (2)).

(75) William Thomson to P. G. Tait, 21 August 1871 (Number 383 note (1)).

theory from continental electrodynamics (based on the motion of point charges), is his concept of charge as the manifestation of the electromagnetic field. The final enunciation of this theory in the *Treatise*[(76)] was the product of considerable conceptual development and clarification during the 1860s;[(77)] his conceptualisation of the field theory of electric charge (in a form preliminary to that expounded in the *Treatise*) is described in a letter to Thomson in June 1869 (Number 322). This theory of electric charge is based on the concept of a 'displacement of electricity' leading to the manifestation of charge; on the analogy between electricity and the flow of an incompressible fluid; and on electromotive force as the cause of the polarisation of dielectrics.

Maxwell described the oddities of Carl Neumann's theory of the 'transmission of Potentials' in a letter to Thomson in October 1869 (Number 327). But it was for Tait that he drew pointed contrast between his own theory of the electromagnetic field and the various versions of action at a distance electrodynamics proposed by German physicists: on Riemann (Number 284) and on Helmholtz's critique of Weber (Number 389). In the *Treatise* he presented his theory of the electromagnetic field as fulfilling the programme bequeathed to Wilhelm Weber by Carl Friedrich Gauss: to form a 'consistent representation' (Maxwell's translation of Gauss' *construirbare Vorstellung*) of the propagation of electrodynamic forces, which were 'not instantaneous, but propagated in time, in a similar manner to that of light'.[(78)]

Maxwell seems to have written the *Treatise* in the sequence of the four-part structure of its published text. His letters to Tait and Thomson in July 1868 give evidence of work on spherical harmonics (Numbers 293, 294 and 295) which was basic to his application of potential theory in Part I of the text, on 'Electrostatics'. In letters to Thomson in September and October 1868 he discussed the theory of electrostatic instruments and problems in potential theory (Numbers 302, 303, 306, 310 and 311). By May 1869 he reported to Thomson that he was completing this first part of the text, and described his use of Thomson's method of electric images in explaining the distribution of electricity on the surfaces of intersecting spheres (Number 321). The

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(76) See Number 322 note (8); and Buchwald, *From Maxwell to Microphysics*: 20–40.

(77) Siegel, *Innovation in Maxwell's Electromagnetic Theory*: 85–119; Joan Bromberg, 'Maxwell's electrostatics', *American Journal of Physics*, **36** (1968): 142–51.

(78) *Treatise*, **2**: 435 (§861), translating Gauss' letter to Weber of 19 March 1845 in *Carl Friedrich Gauss Werke*, **5** (Göttingen, 1867): 627–9, on 629; see P. M. Heimann, 'Maxwell and the modes of consistent representation', *Archive for History of Exact Sciences*, **6** (1970): 171–213.

expression of the potential of an electrified grating was discussed in letters to Stokes and Thomson in the summer of 1869 (Numbers 323, 324 and 326); and in May 1870 he gave John William Strutt an account of his treatment of the potential between parallel electrified plates (Number 340), both these problems arising in the theory of electrostatic instruments.

The work of writing Part II of the *Treatise*, on 'Electrokinematics', had also progressed. He described his chapter on electric conduction to Thomson in August 1868 (Number 301), and a year later this part of the text was also nearing completion (Numbers 322 and 326). By October 1869 he was able to report on his work in writing Part III, on 'Magnetism' (Number 327); a month later he was raising points in the theory of electromagnetism (Numbers 330 and 332). This rapid progress in writing is confirmed in a letter to Tait in December 1869, where he states that he was 'at the 4$^{\text{th}}$ of the 4 parts of my book namely Electrodynamics' (Number 333). The few extant preliminary drafts of this part of the *Treatise* (Number 335) were probably written at this time, as was an outline of the contents of the book (Number 329), which lists the chapters of its first three parts in detail, and terminates with the introductory chapters of Part IV, on 'Electromagnetism'.

It would seem likely that, by the time he abandoned work on the *Treatise* to write the *Theory of Heat* sometime early in 1870, he had progressed further in drafting the fourth part of the book. His later correspondence and publications establish that sections of its text, including his treatment of the mutual induction between coils (Numbers 395 and 396) and of Arago's rotating disc (Numbers 400, 404 and 405), were first written subsequent to his resumption of work on the *Treatise* in late 1870. But this correspondence does not suggest that he was then engaged in writing the substantive part of his account of 'Electromagnetism'. In his letters to Tait in November 1870 (Numbers 346 and 348), which signal his resumption of work on the *Treatise*, he declares his intention to introduce quaternions into his exposition of electromagnetic theory (Number 347). Judging by his subsequent correspondence with Tait, his major concern was with the revision and amplification of the mathematical argument of the book.

A key feature of the *Treatise* is the style of mathematical physics that pervades it, a style that emphasises the mathematical expression of physical quantities freed from their direct representation by a mechanical model. While this method preserved the Lagrangian analytical mechanics of his paper 'A dynamical theory of the electromagnetic field' (Number 238) rather than the physical mechanics of 'On physical lines of force', he did not adopt the algebraic form of Lagrange's *Mécanique Analytique* but emphasised the physical interpretation of the symbols of the Lagrangian calculus. The mathematical style of the *Treatise* also incorporates the physical geometry of his paper 'On Faraday's lines of force', where he had grounded his theory of lines of force on the geometrical analogy of lines of flow of an incompressible fluid. The analytical and geometrical style of the *Treatise* draws together and enlarges upon his own earlier methods, and embraces four fundamental mathematical ideas: quaternions (vector concepts), integral theorems, topology, and the Lagrange–Hamilton method of analytical dynamics.(79)

Topological arguments and integral theorems provided more rigorous expression for his representation of the electromagnetic field in geometrical terms. In 'On Faraday's lines of force' he had formulated field equations in terms of relations between electric and magnetic 'quantities' (acting through surfaces) and 'intensities' (acting along lines), making informal use of Stokes' theorem, which transforms line into surface integrals.(80) In the *Treatise* he gave these concepts analytical expression; and wrote to Stokes in January 1871 (Number 351) and to Tait the following April (Number 366) to inquire about the provenance of this theorem, published by Stokes in his Smith's Prize examination of February 1854, in which Maxwell had been placed equal Smith's Prizeman.(81)

Maxwell discussed the topology of knots and its electromagnetic analogue in two letters to Tait in November and December 1867 (Numbers 275 and 276), at the outset of embarking on the writing of the *Treatise*. His interest in topology, which he terms the 'geometry of position',(82) had been stimulated by Thomson's work on vortex motion, which drew upon Helmholtz's classic study on the subject. There use had been made of Riemann's classification of surfaces by their topological connectivity, a treatment of the properties of surfaces which he had developed in the course of his work on complex function theory.(83) Maxwell noted the use of integral theorems to express the topological properties of surfaces, and emphasised their application to theories of electromagnetism and the motion of fluid vortices (Numbers 276

(79) See P. M. Harman, 'Mathematics and reality in Maxwell's dynamical physics', in *Kelvin's Baltimore Lectures and Modern Theoretical Physics*, ed. Robert Kargon and Peter Achinstein (Cambridge, Mass./London, 1987): 267–97.

(80) Volume I: 257–8, 365, 371–5.

(81) See Numbers 351 note (3) and 366 note (3); and J. J. Cross, 'Integral theorems in Cambridge mathematical physics, 1830–55', in *Wranglers and Physicists. Studies on Cambridge Physics in the Nineteenth Century*, ed. P. M. Harman (Manchester, 1985): 112–48, esp. 139–45.

(82) See Numbers 276 note (8), 304 note (3), and 373. The term 'geometry of position' was also used to denote projective geometry: see note (65) and Number 373 esp. note (10); and Johann Benedict Listing, 'Vorstudien zur Topologie', in *Göttinger Studien. 1847. Erste Abtheilung: Mathematische und naturwissenschaftliche Abhandlungen* (Göttingen, 1847): 811–75, esp. 813–14.

(83) See Numbers 304 note (4) and 305 note (8).

and 305). He explored the relation between Helmholtz's theorems of vortex motion and the theory of electromagnetism (Numbers 295 and 296) and discussed the topology of curves and surfaces (Numbers 304, 305, 306, 308 and 317).

In February 1869 he gave the London Mathematical Society an account of Johann Benedict Listing's 'Der Census räumlicher Complexe', a study of the topology of geometrical figures (Number 318); and he applied these ideas to a study of topographical geometry (Number 345). In clarifying the mathematical argument of the *Treatise* he drew upon Listing's 'Vorstudien zur Topologie' to define the convention specifying the direction of linear and rotational motions, a problem crucial to understanding the relation between lines of force and electrical circuits (Number 385). He discussed the issue with Tait in May 1871, and raised it for discussion at a meeting of the London Mathematical Society (Numbers 368, 369, 370 and 371). In the *Treatise* he made explicit the enlargement of his physical geometry to include the topological treatment of lines and surfaces.(84)

In November 1870 he wrote to Tait, signalling a keen interest in quaternion ideas, methods and notation (Numbers 346 and 348). These letters give the first indication of his resumption of work on the *Treatise* and of an intention to remould its mathematical argument. His correspondence with Tait at this time also provides evidence of his first serious interest in quaternions. He aimed to demonstrate the application of vectors to the mathematics of electromagnetism (Number 347). William Rowan Hamilton had developed his calculus of quaternions from his work on algebra. In his study of complex numbers he sought to extend the complex number system to three dimensions, and in 1843 he invented 'quaternions', hypercomplex numbers with one real and three (imaginary) complex parts. He interpreted the three imaginary numbers as 'vectors' directed along three mutually perpendicular lines in space; the real part of the quaternion was the 'scalar'.(85) In the *Treatise* Maxwell emphasises the conceptual role of vectors as a means of representing physical quantities geometrically. This method provides a direct representation of electrical quantities congruent with their 'physical meaning'; it is a 'mode of contemplating geometrical and physical quantities' which is 'more primitive and more natural' than the method of Cartesian coordinates.(86)

His enthusiasm for quaternions was encouraged by Tait's declaration, in his paper 'On Green's and other allied theorems', of the 'promise of

(84) *Treatise*, **1**: 16–27 (§§18–24).

(85) See Number 346 notes (2) and (5). See Hankins, *Hamilton*: 283–325; and Michael J. Crowe, *A History of Vector Analysis. The Evolution of the Idea of a Vectorial System* (Notre Dame/London, 1967).

(86) *Treatise*, **1**: 8–9 (§10).

usefulness in physical applications'[87] of Hamilton's operator ∇ (Number 346). In this paper, which Maxwell eulogised as 'really great' (Number 349), Tait expressed Green's and Stokes' theorems in quaternion form, and emphasised the 'simplicity and expressiveness of quaternions' in establishing the 'mutual relationship' of the properties of the 'analytical and physical magnitudes which satisfy... Laplace's equation'.[88] Maxwell's rapid grasp of the advantages in applying quaternion concepts to electricity and magnetism is revealed in his correspondence with Tait in early 1871 (Numbers 352, 353 and 356).

From the first Maxwell placed great emphasis on the value of the 'ideas of the calculus of quaternions... [as] distinguished from its operations and methods' (Number 347),[89] an opinion that he later repeated in his review of Kelland and Tait's *Introduction to Quaternions* (Number 485). His representation of the 'Vector Functions of the Electromagnetic Field' (Number 347) was the immediate consequence of his incorporation of ideas drawn from quaternions. He assured Tait that 'the value of Hamiltons idea of a Vector is unspeakable'; and it was in placing emphasis on the separate vector and scalar parts of Hamilton's quaternion that he sought to 'leaven my book with Hamiltonian ideas without casting the operations into a Hamiltonian form' (Number 348).[90] In pursuit of this objective he continued to raise questions of quaternion expression and notation for Tait's appraisal, as the *Treatise* proceeded to publication (Numbers 396, 401, 422, 423, 443 and 465).

The mathematical argument of the *Treatise* also encompasses a development of the generalised Lagrangian theory of the electromagnetic field (Numbers 408, 414, 417, 419 and 430) as first presented in 'A dynamical theory of the electromagnetic field'. There he had expounded the theory from analytical equations of mechanical systems, deploying the Lagrangian formalism of dynamics without reference to a specific mechanical model of the ether. As he explained to Tait in December 1867, the ether model of

(87) P. G. Tait, 'On Green's and other allied theorems', *Trans. Roy. Soc. Edinb.*, **26** (1870): 69–84, on 69; and see his letter to Maxwell of 13 December 1867 (Number 277 note (22)) and his card of 5 April 1871 (Number 366 note (5)).

(88) Tait, 'On Green's and other allied theorems': 70.

(89) See also the *Treatise*, **1**: 9 (§ 10).

(90) In adopting this approach he was encouraged by Bartholomew Price, Secretary of the Delegates of the Clarendon Press, Oxford. In a letter to Maxwell of 4 January 1871 (ULC Add. MSS 7656, P 659; and see note (95)) he argued: 'Quaternion Methods and Quaternion Notation are only just beginning to be used in this place: and the exclusive use of them in your book would therefore much curtail its usefulness, so I think you had better always express the analysis in the ordinary Cartesian form, and repeat it when desirable to do so in the Quaternion form.'

his paper 'On physical lines of force' had been 'built up to show that the phenomena are such as can be explained by mechanism', but that the 'nature of this mechanism is to the true mechanism what an orrery is to the Solar System' (Number 278).[91] In the *Treatise* he maintained this preference for abstract rather than concrete representation, and emphatically refrained from speculating about a hypothetical ether model which could be invoked as a mechanical explanation: 'The problem of determining the mechanism... admits of an infinite number of solutions'.[92]

In postcards to Tait in May and June 1872 (Numbers 408 and 414) he indicated the scope of the revision of his application of analytical dynamics. He proposed to follow the method adopted by Thomson and Tait in their *Natural Philosophy*, deriving the generalised equations of motion from impulsive forces, scorning the algebraic Lagrangian approach where physical concepts are 'supplanted by symbols'[93]. He stressed the importance of the link between the mathematical formalism of dynamics and the physical reality depicted. In a draft he explained his preference for the form of the equations of motion as given by Hamilton over that by Lagrange. The Hamiltonian form of the equations was based on the concept of momentum rather than (as with Lagrange) on velocity; and Maxwell suggested that Newton's second law of motion would thus determine the meaning of the dynamical theory of the electromagnetic field (Number 419). As Maxwell explained in the *Treatise*, it was his aim to translate the mathematical symbols 'from the language of the calculus into the language of dynamics', so that this language should express 'some property of moving bodies'.[94]

Having commenced the revision of the text of the *Treatise* in autumn 1870, Maxwell wrote to Bartholomew Price, Secretary of the Delegates of the Clarendon Press, Oxford, to report his progress. Price's response (in early January 1871) indicates that the work was sufficiently advanced for the Press to 'begin printing'.[95] A contract was not however signed until 10 May 1871,[96] Maxwell having reported to Tait that 'I have been at the Clarendon & they are to go a head' (Number 367). Shortly afterwards he was struck by the need to revise the convention on spatial relations that he had adopted (Numbers 368, 369 and 370), sending Tait a revise of *Treatise* §23 with the

(91) On Maxwell's likely source for this analogy see Number 278 note (13).

(92) *Treatise*, **2**: 417 (§831).

(93) *Treatise*, **2**: 194 (§567).

(94) *Treatise*, **2**: 185 (§554).

(95) Price to Maxwell, 4 January 1871 (note (90)): 'I am pleased to hear that you are so far on with the work that we may begin printing.' (and see Number 367 note (3)).

(96) See Number 367 note (3).

urgent request: 'Will this do? Tell me that I may print' (Number 371). Type-setting commenced promptly; the first proof sheets were available in June 1871, and by the following August this work was well advanced,[97] with Maxwell continuing to revise his account of spherical harmonics (Numbers 387 and 388).

Tait and Thomson (Number 390) assisted in the correction of proofs, their 'many valuable suggestions made during the printing' being acknowledged in the Preface to the *Treatise*.[98] Thomson was currently engaged in publishing the reprint of his papers on *Electrostatics and Magnetism*, and Maxwell repaid the service by reading his proofs (Numbers 383, 402, and 420). From spherical harmonics Maxwell's correspondence with Tait turned towards Tait's current interest in thermo-electricity (Numbers 393, 394, 396 and 401), and then to his own new application of Thomson's theory of electrical images to the explanation of Arago's rotating disc (Numbers 399, 400, 403, 404 and 405), both topics being discussed in the *Treatise*. The errors of Weber and Carl Neumann were duly noted (Numbers 411 and 428). As the book proceeded through the press during 1872, Maxwell raised questions for Tait's comment on quaternions (Numbers 401, 422 and 423) and on Lagrangian dynamics (Numbers 408, 414 and 419). Proofs of the final sections of the text are dated January 1873,[99] and the Preface is dated 1 February 1873; the book was published the following March.[100]

The *Treatise* was reviewed (anonymously) by Tait in the journal *Nature*.[101] Describing Maxwell as having 'a name which requires only the stamp of antiquity to raise it almost to the level of that of Newton', Tait placed emphasis on the power and novelty of Maxwell's mathematical physics. He drew attention to Maxwell's use of quaternion concepts and notation, to the role of the Lagrange–Hamilton formalism of dynamics in his dynamical theory of the electromagnetic field, and to the treatment of spherical harmonics in his exposition of the mathematical theory of electrostatics. Tait declared however that the main object and achievement of the work was in expounding a transformation in the very foundations of physical theory, 'simply to upset completely the notion of *action at a distance*'; and this had led Maxwell to demonstrate 'the connection between radiation and electrical phenomena'.[102] The formulation of the concept of the electromagnetic field

(97) See Numbers 383 note (5) and 386 note (2).

(98) *Treatise*, **1**: x note.

(99) See Number 434 note (7).

(100) See Number 448 esp. note (5).

(101) Confirmed by Knott, *Life of Tait*: 356. See also David B. Wilson, 'P. G. Tait and Edinburgh natural philosophy, 1860–1901', *Annals of Science*, **48** (1991): 267–87.

(102) [P. G. Tait,] 'Clerk-Maxwell's Electricity and Magnetism', *Nature*, **7** (24 April 1873): 478–80.

and the expression of the electromagnetic theory of light were thus highlighted as the major features of the *Treatise.*

Cambridge and the Cavendish Laboratory

In 1849 the Board of Mathematical Studies at Cambridge had recommended that 'the Mathematical Theories of Electricity, Magnetism, and Heat, be not admitted as subjects of examination',[(103)] a reform that was in line with current practice, though questions on these topics were set in the papers for the Smith's Prizes for which the high wranglers in the Mathematical Tripos competed. During the 1860s there was a movement for reform of the Tripos, with advocacy of the introduction of physical subjects into the examination. This process of reform of the Mathematical Tripos was gently fostered by Maxwell (Number 316) on his appointment as an examiner and moderator in 1866, 1867, 1869 and 1870 (Numbers 253, 254 and 300); and he served as additional examiner in 1873 (Numbers 412 and 436). Maxwell introduced a few questions on electricity, magnetism and heat into the examination, these topics being the major fields of research in mathematical physics since the 1840s; and he also suggested that questions on physical problems be set for the Adams Prize competition (Number 362).

George Biddell Airy wrote to the Vice-Chancellor of the University in 1866 urging reform of the Mathematical Tripos. Subsequent discussion by the Board of Mathematical Studies led to the appointment of a Physical Sciences Syndicate, which in its report of 27 February 1869 recommended 'providing public instruction in Heat, Electricity and Magnetism', these subjects to be added to the examination for the Mathematical Tripos in 1873. The Syndicate recommended that a new professorship be established for offering lectures in these subjects, and also urged the foundation of a physical laboratory. The University's scientific professors had already concluded that a new professorship would be required to provide effective teaching in the new subjects.[(104)] These recommendations had uncomfortable financial implications; but the Chancellor of the University, the Duke of Devonshire, intervened with an offer 'to provide the funds required for the building and apparatus, as soon as the University shall have in other respects completed its arrangements for teaching Experimental Physics, and shall have approved

(103) Report of the Board of Mathematical Studies, 19 May 1849, ULC, Cambridge University Archives, Minute V, 7. See David B. Wilson, 'Experimentalists among the mathematicians: physics in the Cambridge Natural Sciences Tripos, 1851–1900', *Historical Studies in the Physical Sciences*, **12** (1982): 325–71.

(104) *Cambridge University Reporter* (16 November 1870): 93–7.

the plan of the building'.[105] This had immediate effect: a Professorship of Experimental Physics was established, the regulations to which it was to be subject approved, and the post was advertised on 14 February 1871, the election being announced for 8 March.[106]

William Thomson was invited to stand for election; and after declining he was prompted by Stokes (as Lucasian Professor of Mathematics) to write to Helmholtz in an unavailing attempt to arouse his interest.[107] Maxwell was then invited by Stokes and by E. W. Blore (of Trinity), and urged by Strutt, to offer himself for election to the professorship. Blore and Stokes assured him that Thomson would not come forward, and Stokes asserted that 'I think you would most likely be elected'.[108] Expressing tentative initial interest (Number 357) Maxwell decided to stand and was duly elected, 'on the understanding', according to Lewis Campbell, 'that he might retire at the end of a year, if he wished to do so'.[109] Maxwell's position as third choice for the professorship, after Thomson and Helmholtz, may well reflect a ranking as judged in 1870; but his resignation from King's College, London may have led his Cambridge colleagues to surmise his likely disinterest in an academic post. Campbell's statement indicates that there was some uncertainty about his future commitment; but whatever his private doubts, Maxwell began immediately to draw up plans for the design of the laboratory and the acquisition of apparatus (Numbers 364 and 365). He drew on the expertise of Thomson at Glasgow and Tait at Edinburgh (Numbers 362, 365, 366, 367 and 374), and on R.B. Clifton's recent experience in planning a physical laboratory at Oxford.[110] Work on the construction of the laboratory soon began to proceed (Numbers 397, 425 and 449).[111]

(105) The (seventh) Duke of Devonshire to the Vice-Chancellor, John Power, 10 October 1870, ULC, V. C. Corr. I. 2; printed in the *Cambridge University Reporter* (19 October 1870): 13.

(106) *Cambridge University Reporter* (30 November 1870): 125–6; *ibid.* (8 February 1871): 175; *ibid.* (15 February 1871): 188. See Number 357 note (3) and D. A. Winstanley, *Later Victorian Cambridge* (Cambridge, 1947): 194–8.

(107) S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 563–6.

(108) G. G. Stokes to Maxwell, 16 February 1871 (Number 357 note (3)).

(109) *Life of Maxwell*: 348; on Maxwell's election see Number 357 note (5).

(110) Number 365 note (4). See R. Sviedrys, 'The rise of physical science at Victorian Cambridge', *Historical Studies in the Physical Sciences*, **2** (1970): 127–51; Sviedrys, 'The rise of physics laboratories in Britain', *ibid.*, **7** (1976): 405–36.

(111) See 'The new physical laboratory of the University of Cambridge', *Nature*, **10** (1874): 139–42. The doors of the 'Cavendish Laboratory' (as the laboratory came to be known: see Number 463 notes (3) and (4)) bear the inscription 'Magna opera Domini exquisita in omnes voluntates ejus', the Vulgate version of Psalm 111, v. 2, 'The works of the Lord are great, sought out of all them that have pleasure therein' (Authorised Version). The inscription was placed

Maxwell delivered his inaugural lecture at Cambridge on 25 October 1871,(112) and immediately commenced regular lecturing duties. Stokes had informed him in February 1871 that his lectures would be 'subject to the approval of the board of Mathematical Studies'; it was anticipated that candidates for the Mathematical Tripos who opted for physical subjects, as well as candidates for the Natural Sciences Tripos, would attend.(113) Maxwell's correspondence with Tait indicates that he envisaged his lectures as being addressed to candidates reading for the Mathematical Tripos; thus in October 1872 he declared his intention 'to sow [quaternion] seed at Cambridge' (Number 423). In fulfilling the requirement that his professorship meet the scope of the Mathematical Tripos when the new regulations came into force in 1873, Maxwell lectured on 'Heat' in Michaelmas term 1871, on 'Electrostatics and Electrokinematics' in Lent term 1872, and on 'Electromagnetism' in Easter term 1872, the titles of these courses being modified slightly in subsequent years.(114) In Michaelmas term 1871 he recorded the attendance of 19 students (including Horace Lamb, W. W. Rouse Ball and W. M. Hicks), with attendance swelling to 26 in Lent term 1872 but falling to 10 in the Easter term. In Michaelmas term 1873 he recorded the attendance of a mere eight students, though these included George Howard Darwin (who had graduated as second wrangler in 1868 and was a Fellow of Trinity) and George Chrystal.(115) With very few exceptions, these students were candidates for the Mathematical Tripos.

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there on the initiative of the architect William Milner Fawcett; see his letter of 16 December 1873 to the Vice-Chancellor, H. W. Cookson (ULC, CUR, 55.2 (192)): 'I am sorry to hear any adverse criticism on the Inscription on the door of the Cavendish Laboratory. / I am solely responsible for it and was careful to refer to an old Vulgate that there might be no mistake, but that the Latin might be correct. / I did feel a difficulty in the translation in our authorized English but a friend recommended me that the Vulgate Latin tho' not good had a certain authority and I would not be wrong in quoting from it. / It is unfortunate that the Hebrew is not correctly rendered, but we are not responsible for the Vulgate translation and the text seems to me so appropriate that I hope no serious objection will be taken to it.'

(112) J. Clerk Maxwell, *Introductory Lecture on Experimental Physics*, October 25, 1871 (London/Cambridge, 1871) (= *Scientific Papers*, **2**: 241–55). The MS of the lecture is preserved in ULC Add. MSS 7655, V, h/7.

(113) Stokes to Maxwell, 18 February 1871 (Number 357 note (4)).

(114) *Cambridge University Reporter* (18 October, 1871): 16–17. In 1872–3 the lectures were on 'Heat and Elasticity', 'Electrostatics and Electrokinematics', and 'Electromagnetism'; see *Cambridge University Reporter* (16 October 1872): 8; *ibid.* (24 January 1873): 29; and *ibid.* (29 April 1873): 22. In 1873–4 the lectures were on 'Heat and Elasticity', 'Electricity and Magnetism', and 'Electromagnetism'; see *Cambridge University Reporter* (14 October 1873): 23; *ibid.* (14 January 1874): 185; and *ibid.* (14 April 1874): 315.

(115) In a notebook, ULC Add. MSS 7655, V, n/2.

G. H. Darwin's notes on Maxwell's lectures on 'Heat and Elasticity', delivered in the new lecture room of the Cavendish Laboratory in Michaelmas term 1873, and his more fragmentary notes on the lectures on 'Electricity and Magnetism' delivered in Lent term 1874,[116] have been preserved.[117] These notes show that Maxwell lectured on mathematical physics, and that his presentation was at an advanced level. In the lectures on 'Heat and Elasticity' he began by reviewing some of the topics described in his *Theory of Heat*, on liquid–gas exchanges, the Carnot cycle, Joule's paddle wheel experiment, Maxwell's thermodynamic relations, isothermal surfaces and Fourier's theory of the conduction of heat. His presentation was however at a deeper level of mathematical sophistication than that adopted in his text. On turning to more complex topics in mathematical physics he discussed the analogy between electricity and heat based on the mathematics of potential theory, and introduced the application of spherical harmonic analysis, taxing Darwin's comprehension.[118] His presentation of the kinetic theory of gases is of special interest, as the lectures followed the argument of his most recent work on the subject (Number 472). The severely analytical presentation was softened by the introduction of the geometrical analogy of the hodograph to represent the distribution of molecular velocities in space. His discussion of the statistical law of the distribution of velocities among molecules led to an account of his recent derivation of the distribution law for complex molecules in the presence of an external field of force, to which Darwin made reference.[119] Maxwell was an examiner for the Natural Sciences Tripos in 1873 (Number 488), but his lectures were not directed at the concerns of students about to sit the Cambridge examination in physics.[120]

As the new Cavendish Laboratory[121] neared completion early in 1873 (Number 449) Maxwell gave attention to the equipment which would be needed, and drew up a list of laboratory fittings and apparatus to be acquired (Numbers 463 and 464). The direction of the Laboratory and the encouragement of research by Cambridge Fellows, as in urging Strutt (Rayleigh) to write a book on the 'Theory of Sound' (Number 458), and his review of a dissertation (Number 489), were to be important duties during the last years of his life. Other dimensions of the final part of his career were broached in 1873: the planning and writing of articles for the ninth edition

(116) See note (114).
(117) ULC, DAR. 210.22.
(118) See Number 482 note (5).
(119) See Number 472 note (4).
(120) Coutts Trotter (see Number 361 note (2)) delivered lectures on physics attended by candidates for the Natural Sciences Tripos; see Wilson, 'Experimentalists among the mathematicians': 343–6.
(121) See note (111).

of the *Encyclopaedia Britannica* (Number 432); an interest in communicating his ideas to a wider public (Numbers 486 and 487), especially as a book reviewer for the journal *Nature* (Numbers 450 and 485); the agreement to undertake an edition of Henry Cavendish's manuscripts on electricity (Numbers 435 and 459); and his participation in the work of the Cambridge Philosophical Society (Number 490). The initiation of correspondence with Henry Rowland (Numbers 466 and 467), a member of the younger generation of physicists beyond Cambridge, whose work became deeply imbued with Maxwell's electromagnetic theory,[122] heralded the spread of Maxwellian theory and its dominance of 'classical' physics in the latter part of the nineteenth century.

(122) See Buchwald, *From Maxwell to Microphysics*: 73–7.

TEXTS

LETTER TO WILLIAM SHARPEY[1]

8 JULY 1862

From the original in the Library of the Royal Society, London[2]

Glenlair House
Dalbeattie
N.B.[3]
8 July 1862

Dear Sir

I now return Professor Stokes' paper 'On the Long Spectrum of Electric Light'.[4] I have read it and beg to report to the Committee of Papers that I find in it so much new information respecting the invisible rays, which constitute so large a part of the radiation from the electric spark and their relation to various substances[5] that I consider the Paper well worthy of being published in the Philosophical Transactions.

By the discovery of a substance capable of rendering visible the highly refrangible rays,[6] Professor Stokes has been enabled to make observations

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(1) Professor of Anatomy and Physiology at University College, London; Secretary of the Royal Society 1853–72 (*DNB*).

(2) Royal Society, *Referees' Reports*, **4**: 263.

(3) North Britain (Scotland).

(4) G. G. Stokes, 'On the long spectrum of electric light', *Phil. Trans.*, **152** (1862): 599–619 (= *Papers*, **4**: 203–33). The paper was received by the Royal Society and read on 19 June 1862; see the abstract in *Proc. Roy. Soc.*, **12** (1862): 166–8 (= *Papers*, **4**: 203–4).

(5) In a paper 'On the prismatic decomposition of electrical light', *Report of the Fifth Meeting of the British Association for the Advancement of Science* (London, 1836), part 2: 11–12, Charles Wheatstone had reported his discovery that the spectra of electric sparks contain bright lines determined by the nature of the electrodes. The subject had been of some recent interest: see A. J. Ångström, 'Optical researches', *Phil. Mag.*, ser. 4, **9** (1855): 327–42; Ångström, 'On the Fraunhofer lines visible in the solar spectrum', *ibid.*, **24** (1862): 1–11; Julius Plücker, 'Ueber die Constitution der electrischen Spectra der verschiedenen Gase und Dämpfe', *Ann. Phys.*, **107** (1859): 497–539, 638–43; Gustav Kirchhoff, 'Untersuchungen über das Sonnenspectrum und die Spectren der chemischen Elemente', *Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin* (Aus dem Jahre 1861): 63–95, esp. 67–74, (trans. by Henry E. Roscoe) *Researches on the Solar Spectrum, and the Spectra of the Chemical Elements* (Cambridge/London, 1862): 8–12.

(6) Stokes recalled that following the experimental researches described in his paper 'On the change of refrangibility of light', *Phil. Trans.*, **142** (1852): 463–562 (= *Papers*, **3**: 267–409), he had anticipated that an electric spark would emit 'rays of much higher refrangibility than were found in the solar spectrum'; but that in February 1853 he had been 'perfectly astonished on subjecting a powerful discharge from a Leyden jar to prismatic analysis with quartz apparatus, to find a spectrum extending no less than six or eight times the length of the visible spectrum'. Stokes projected 'a spectrum formed by a prism and lens of quartz on a piece of uranium glass';

with the eye on these rays and has thus extended our knowledge of –

– the bright rays due to metals[7]

– the absorption of these rays by various media, and the unequal absorption of different parts of the invisible spectrum by media which do not absorb visible light, a phenomenon having the same relation to the 'long spectrum' that colour has to the visible spectrum, and, affording the same kind of assistance in the discrimination of these substances.[8]

These researches may also afford the means for a more exact knowledge of the electric spark itself,[9] and when compared with the photographic results obtained by Dr Miller,[10] may render the proof of the absolute identity of the cause of all the effects produced by these radiations more convincing to those who do not yet believe it.

I remain
Yours truly
J. Clerk Maxwell

Dr Sharpey
Sec R.S.[11]

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reporting that on 'changing the metals between which the spark passed, we found that the lines were changed, which showed clearly that they were due to the particular metals'. See Stokes, 'On the long spectrum of electric light': 599–600.

(7) Stokes reported observations of the spectra of platinum, palladium, gold, silver, mercury, antimony, bismuth, copper, lead, tin, nickel, cobalt, iron, cadmium, zinc, aluminium and magnesium, and exhibited in a figure the principal lines of aluminium (which 'stands at the head of the above metals for richness in rays of the very highest refrangibility') together with those of zinc and cadmium for comparison. See Stokes, 'On the long spectrum of electric light': 603–6.

(8) A point made by Stokes in discussing the 'highly characteristic' absorption by alkaloids and glucosides; see 'On the long spectrum of electric light': 607.

(9) As discussed by Stokes in his paper 'On the long spectrum of electric light': 615–19.

(10) In his paper 'On the long spectrum of electric light': 601–2, Stokes refers to a paper by William Allen Miller (see Number 199 note (12)), to which his own paper was a 'supplement'. See Miller's letters to Stokes of 2 March, 25 and 29 April, and 23 August 1862 in Larmor, *Correspondence*, **1**: 159–61.

(11) Stokes' paper was also refereed by Charles Wheatstone, in a report dated 16 July 1862 (Royal Society, *Referees' Reports*, **4**: 262). Wheatstone declared the paper to be 'a valuable addition to the numerous memoirs which have been recently published relating to the prismatic decomposition of electric light, especially as showing how the author's own original and important experiments on fluorescence can be applied to the investigation and determination of the positions of the invisible rays'. See notes (5), (6) and (10); and Numbers 198 note (2) and 199 note (12).

198

LETTER TO GEORGE GABRIEL STOKES

14 JULY 1862

From the original in the Library of the Royal Society, London(1)

Glenlair
Dalbeattie
July 14, 1862

Dear Stokes

I now return to you Dr Robinsons Paper 'On Spectra of Electric Light as modified by the nature of the Electrodes and the Media of Discharge'(2) and beg to report to the Committee of Papers that I have read it and consider the investigations there described as worthy to be recorded in the Philosophical Transactions.

The subject is well worthy of study, notwithstanding the number of observations that must be made and the changes of circumstance that must be tried in order to ascertain the dependence or independence of the lines upon the circumstances under which they are produced.

The results tabulated by Dr Robinson(3) will no doubt be discussed by himself in the second part of his paper,(4) and the modifications of selected lines traced through all the changes of conditions. For me to attempt this from the tables without the experience gained by making the observations would be nearly useless, and as I have not attempted it I cannot properly appreciate the evidence of the connexion of particular phenomena with particular conditions.

The conditions which are at our disposal relate to the Spark to the Electrodes and to the Medium. The Spark is a transient electric current which may vary in 'mean strength' and in 'duration' the total quantity of electricity which passes being the product of these quantities.

Weber has shown how to ascertain by the magnetic galvanometer and electric dynamometer the strength and duration of a uniform current

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(1) Royal Society, *Referees' Reports*, 5: 218.

(2) T. R. Robinson, 'On the spectra of electric light, as modified by the nature of the electrodes and the media of discharge', *Phil. Trans.*, **152** (1862): 939–86, esp. 939–74. The paper was received by the Royal Society and read on 19 June 1862; see the abstract in *Proc. Roy. Soc.*, **12** (1862): 202–5.

(3) Thomas Romney Robinson, astronomer at Armagh Observatory, was Stokes' father-in-law (see Larmor, *Correspondence*, **1**: 14–15; and *DNB*). On Stokes' own contemporary related work see Number 197; and for the broader context see Number 197 note (5).

(4) Robinson, 'On the spectra of electric light': 974–86; and for Maxwell's report see Number 201.

equivalent to the actual spark both in quantity and in heating effect,[(5)] and we can calculate from this the actual strength and variation of the spark at any part of its duration by a rough theory. This investigation would not be necessary in spectrum observations but it may serve to direct our attention to what we may know about the spark.

The path of the current is known to us by its luminosity. This arises from something in rapid vibration, whether particles of gas or metal or ether. The amount of energy spent in producing these movements is directly as the whole quantity and inversely as ⟨the square⟩ of the duration. These movements constitute heat at the place where they exist and if they are propagated elsewhere they are called radiant heat, light or invisible rays according as they are received on a thermometer, the eye, or anything else. If the vibrations were quite irregular we should have a continuous spectrum but we find bright lines indicating a tendency of the vibrations to have particular periods. Is this tendency to a particular period of vibration an inalienable property of the molecules of elementary substances or can it be modified by the action of other molecules or changes of amplitude in the vibrations.

The action of other molecules of the same substance certainly alters the effects, see the CP spectra, the R spectra and the transition spectra,[(6)] but we must remember that cæteris paribus the rarification diminishes the resistance and alters the nature of the spark, and the heat produced in its path.

The action of bodies chemically combined might be expected to be more powerful but it certainly does not always change the character of the lines.

As far as I am aware no change in position of a line has been observed due either to change of power of spark or chemical action, which seems to indicate that the vibrating systems are very elementary. I am not aware of lines belonging to compound bodies.

To settle disputes with regard to lines selected for their apparent variation they should first be examined with a strong train of prisms to find out whether they are broad faint lines or narrow bright ones ⌊see Kirchhoff⌋.[(7)] Two sparks taken under different circumstances should then be compared simultaneously by reflecting both through different parts of the same slit. The

(5) Wilhelm Weber, 'Elektrodynamische Maassbestimmungen', *Ann. Phys.*, **73** (1848): 193–240, esp. 215–18; (trans.) 'On the measurement of electro-dynamic forces', *Scientific Memoirs*, ed. R. Taylor, **5** (London, 1852): 489–529, esp. 506–9.

(6) Spectra obtained from gases at common pressure ('CP'), on rarefying the gas in which discharges were made ('R'), and '*transition*' spectra produced during rarefaction; see Robinson, 'On the spectra of electric light': 946, 948.

(7) Gustav Kirchhoff, *Researches on the Solar Spectrum, and the Spectra of the Chemical Elements*, trans. by Henry E. Roscoe (Cambridge/London, 1862); see Number 197 note (5).

electric circumstances may be made the same by making the same current pass through both sparks or different by drawing off part of the current from one spark &c. Electrodes may be got rid of by using a strong induction coil and a vessel with partitions or perhaps this would answer.

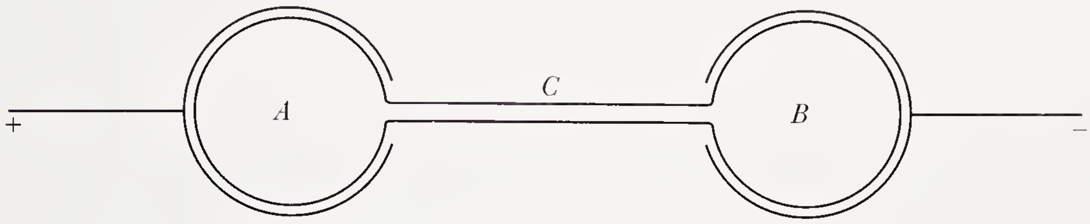

Figure 198,1

Two large globes *A*, *B* connected by a fine tube *C*. The balls of thin glass & covered externally with tin foil and the tin foil connected with the poles of the coil.

There would then be no metallic electrodes and the current would be a reciprocating one due to static induction, faint in the globes, but strong in the tube.

I do not understand how a line can be seen to be distinctly narrower than the image of the slit.(8) I have seen D double when I thought the slit too wide to show it. That the relative brightness of different lines in the same medium should alter is to be expected but may lead to further knowledge. That the position of a line should alter would be very remarkable and would require a readjustment of theories and probably would produce a still greater revolution in science than if the permanence of all lines were demonstrated.

Yours truly
J. Clerk Maxwell

Professor Stokes
Sec R.S.(9)

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(8) See Robinson's comments in 'On the spectra of electric light': 947.
(9) For Wheatstone's report on Robinson's paper see Number 201 note (8).

LETTERS TO GEORGE GABRIEL STOKES

16 JULY 1862

From the originals in the University Library, Cambridge[1]

Glenlair
Dalbeattie
July 16, 1862

My dear Stokes

I have read Professor Haughton's paper 'On the reflexion of polarized light from polished surfaces, transparent and metallic'[2] and I find that it contains many valuable observations, important to the theory of reflexion and I think the paper such as should be published in the Society's Transactions and beg to report accordingly to the Committee of Papers.[3]

M^r^ Haughtons experiments and observations determine for various incidences of plane polarized light on different surfaces

1^st^ the difference of phase of the components of the reflected light, in and perpendicular to, the plane of reflexion. (A)

2^nd^ The ratio of the amplitudes of these components, the components of the incident light being equal. (B)[4]

He has determined by experiment the angle of incidence at which the difference of phase is 90° (Principal Incidence) and the ratio of the components of the incident light so that the reflected light shall be circularly polarized.[5] The tangent of the Principal Incidence he calls 'the Coefficient

(1) ULC Add. MSS 7656, M 418.

(2) Samuel Haughton, 'On the reflexion of polarized light from polished surfaces, transparent and metallic', *Phil. Trans.*, **153** (1863): 81–125. The paper was received by the Royal Society on 9 June 1862, and read on 19 June 1862; see the abstract in *Proc. Roy. Soc.*, **12** (1862): 168–70.

(3) Maxwell subsequently qualified this judgment: see Number 200.

(4) On this ratio see Number 200 note (10).

(5) The terms in which Haughton's paper are couched follow those of Fresnel's theory of reflection: see A. J. Fresnel, 'Mémoire sur la loi des modifications que la réflexion imprime à la lumière polarisée', *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, **11** (1832): 393–433. Fresnel obtains amplitude ratios for oscillations in and perpendicular to the plane of incidence (tangent and sine laws). He supposes that on total reflection light is shifted in phase from the incident wave; and discusses the conditions in which the light is circularly polarised, as in the 'Fresnel rhomb'. See George Biddell Airy, *Mathematical Tracts on the Lunar and Planetary Theories, the Figure of the Earth, Precession and Nutation, the Calculus of Variations, and the Undulatory Theory of Optics* (Cambridge, $_3$1842): 342–55, esp. 349, 354; where he notes that in the case of 'reflection at the surfaces of metals, the reflected ray appears to possess properties similar to those of light totally reflected within glass'; and that in 'Fresnel's rhomb' the 'effect of the two reflections...will be to accelerate the phases of vibration in the plane...more than those perpendicular to that plane by 90°'.

of Refraction' (not the Index) and the ratio of components the Coefficient of Reflexion.(6)

If Fresnels theory(7) were correct Brewsters law(8) would hold(9) and the Coefficient of refraction would be the Index of Refraction(10) and the Coefficient of Reflexion would be zero.(11) It appears that the Coefficient is less than the Index of refraction and the Coeff^t of Reflexion has values up to nearly unity.

The different series of experiments on the same substance at the various incidences will all give independent values of (A) and (B) for each incidence. A comparison of these series first with each other, and then with theory would give first a measure of the value of the observations, and then an indication of the truth of the theory. As far as I can see the author has restricted himself to determining the two coefficients which are distinguishing marks of the media and quite independent of theory, though they may be made numerical constants in a theory afterwards. They may be found to throw light on the optical character of metallic and other surfaces, and from the observations they appear to be capable of sufficiently accurate determination.

I have also read Professor Miller's Paper 'On the Photographic Transparency of Bodies &c'(12) and beg to report to the Committee of Papers that I think it should be published in the Transactions.(13)

(6) See Haughton, 'On the reflexion of polarized light from polished surfaces': 84.

(7) See note (5).

(8) That the condition of maximum polarisation is that the tangent of incidence is equal to the index of refraction. See David Brewster, 'On the laws which regulate the polarisation of light by reflexion from transparent bodies', *Phil. Trans.*, **105** (1815): 125–59, esp. 127; Brewster, 'On the phenomenon and laws of elliptic polarization, as exhibited in the action of metals upon light', *Phil. Trans.*, **120** (1830): 287–326, esp. 324; and Brewster, *A Treatise on Optics* (London, 1831): 169, 229–30.

(9) The relation between Fresnel's theory and Brewster's law is discussed in Airy's *Mathematical Tracts*: 343–4, 355n.

(10) See note (8).

(11) This follows from Fresnel's tangent law for oscillations in the plane of incidence when the angles of incidence and refraction are complementary; Haughton, 'On the reflexion of polarised light from polished surfaces': 87. See Fresnel, 'Mémoire sur la loi des modifications que la réflexion imprime à la lumière polarisée': 402; and Airy, *Mathematical Tracts*: 343–4, who notes, following Fresnel, that this condition gives $\tan i = \mu$ (Brewster's law) 'which defines the polarizing angle'.

(12) W. A. Miller, 'On the photographic transparency of various bodies, and on the photographic effects of metallic and other spectra obtained by means of the electric spark', *Phil. Trans.*, **152** (1862): 861–87. The paper was received by the Royal Society and read on 19 June 1862; see the abstract in *Proc. Roy. Soc.*, **12** (1862): 159–66.

(13) For Stokes' reference to Miller's paper see Number 197 note (10). Miller was Maxwell's colleague, as Professor of Chemistry, at King's College London; see Volume I: 662n. In his paper

By taking a number of photographs on the same scale D^r^ Miller has enabled us to compare at our leisure the spectra of the different metals and the effects of absorption on them. While the method of using a fluorescent screen (as you do)[14] ensures seeing all that is to be seen, and enables the observer to adjust his apparatus by the help of the eye, the photographic method, when once perfected, though each observation takes longer time and is done in the dark gives us permanent records of the facts without the labour of measurement or the uncertainty of memory.

At the same time the comparison of the photographs with the appearances on the fluorescent screen will afford proof that the same vibrations produce both effects.

D^r^ Miller has used different electrodes different media for the spark to traverse & different media for the light of the spark to traverse. I know no other variable except the substance used as the sensitive screen. As D^r^ Millers researches already have great optical value any extension of them with varied sensitive media would have immense importance in Photography. I do not think most photographers are sufficiently aware that the ordinary media are hardly sensitive till near the line *G*.

I think there is perhaps an error at the top of Page 39 where D^r^ Miller says your screen was made of Uranium GLASS.[15][a] I understood it to be a modified phosphate of Uranium.[16]

I remain
Yours truly
J Clerk Maxwell

Professor Stokes
Secretary of the Royal Society[17]

(a) {Stokes} I used sometimes one & sometimes the other. G.G.S.[18]

'On the photographic transparency of various bodies': 864n he thanked Maxwell for help with some calculations.

(14) See G. G. Stokes, 'On the long spectrum of electric light', *Phil. Trans.*, **152** (1862): 599–619; and for Maxwell's report see Number 197.

(15) As mentioned by Stokes in his paper 'On the long spectrum of electric light': 600; see Number 197 note (6).

(16) The substance used as a screen whose preparation is described in detail by Stokes in 'On the long spectrum of electric light': 602–3.

(17) In a report on Miller's paper, dated 23 August 1862 (Royal Society, *Referees' Reports*, **5**: 157), Charles Wheatstone declared that: 'The great number of metallic substances in which he has determined the photographic lines from the spectra by prismatic analysis, renders it of especial value, and though the positions of these lines are only roughly ascertained, his experiments will furnish most valuable indications to the physicist who may hereafter undertake to determine accurately the index of refraction of the invisible rays.'

(18) In the published text of his paper 'On the photographic transparency of various bodies': 882, Miller refers to Stokes' use of 'a screen of uranium glass, or of a particular phosphate of uranium'.

Dear Stokes

The accompanying papers only arrived here yesterday and D[r] Millers photographs are still on their way. The reason is that when I sent for the papers from London they were mistaken for another parcel which I did not wish forwarded.

Yours truly
J C. Maxwell

LETTER TO GEORGE GABRIEL STOKES

21 JULY 1862

From the original in the Library of the Royal Society, London(1)

Glenlair
Dalbeattie
1862 July 21

Dear Stokes

I quite concur in your report on Mr Haughton's paper.(2) I have never read M. Jamins paper(3) though I was aware of its existence, and that he had constructed an instrument to analyze elliptically polarized light.(4) I also knew that you had constructed an instrument with a selenite plate for a similar purpose,(5) but as I had not the means of getting up the subject within the time, I assumed that Mr Haughton,(6) living in Dublin and working for

(1) Royal Society, *Referees' Reports*, **5**: 103.

(2) This letter was written in supplement to Maxwell's report on Haughton's paper 'On the reflexion of polarized light from polished surfaces, transparent and metallic' (Number 199). To obtain agreement between the two referees of the paper Stokes had sent Maxwell his own report, dated 30 June 1862 (Royal Society, *Referees' Reports*, **5**: 106), on receiving Maxwell's letter of 16 July 1862. Stokes was severely critical of Haughton's paper, concluding with the following recommendations: 'On the whole, I do not think the paper ought to be printed without some modifications. / 1st It would be well if the author were to modify the opening sentence, in which he appears to claim as a discovery a very simple consequence of the theory of transverse vibrations applied to the leading features of metallic reflection.... / 2nd The error or oversight of giving two different definitions of the same expression "coefficient of reflection" should be corrected. / 3rd Reference should be made to the experiments of Jamin.... / 4th Further information should be afforded as to the mode of conducting the experiments.... / 5th The author should be requested to reconsider his result that J/I depends on the azimuth of the polarizer.... / 6th It might be desirable to give at the end a comparison of the constants as determined respectively by the author and M. Jamin.... / ... I am prepared, in case the other referee be decidedly favourable to the publication, to recommend that the paper be printed subject to slight modification, provided that, after communication with the author the referees should be satisfied that the numerical results were sufficiently trustworthy....'.

(3) Jules Jamin, 'Mémoire sur la réflexion à la surface des corps transparents', *Ann. Chim. Phys.*, ser. 3, **29** (1850): 263–304. Two earlier papers by Jamin were also relevant to Haughton's work: 'Mémoire sur la réflexion métallique', *ibid.*, **19** (1847): 296–342, and 'Mémoire sur la couleur des métaux', *ibid.*, **22** (1848): 311–27.

(4) Jamin, 'Mémoire sur la réflexion à la surface des corps transparents': 270–86. The phenomenon had been discussed by David Brewster, 'On the phenomenon and laws of elliptic polarization, as exhibited in the action of metals upon light', *Phil. Trans.*, **120** (1830): 287–326.

(5) G. G. Stokes, 'On a new elliptic analyser', *Report of the Twenty-first Meeting of the British Association for the Advancement of Science* (London, 1852), part 2: 14 (= *Papers*, **3**: 197–9).

(6) Haughton was a Fellow of Trinity College, Dublin, FRS 1858.

years on the subject had made the necessary inquiries and placed his experimental results before the Society simply as an addition to the mass of facts observed by various experimenters.

The opening statement claiming the discovery that plane polarized light could be made into circularly polarized light by reflexion at proper incidence and azimuth(7) must be so expressed as to be understood as a mere deduction, tolerably obvious, from the fact that the two components have a difference of phase varying with the incidence and extending sufficiently to allow of one value being $\frac{1}{4}$ undulation, while neither component vanishes.(8)

I understood the definition of the coeffts to relate to the conditions of this single experiment only, for of course it is possible that when the difference of phase is 90°,(9) $\frac{J^{(10)}}{I}$ may not be a minimum, and the second definition therefore should not stand side by side with the first(11) till their identity is proved.

From the very brief manner in which the mode of observation is described I suspected that the author had already described it but as there was no reference to any more complete description, I think that the observations lose much of their value for want of some statement of the way in which each result was obtained.(12) In a case like that of this paper, where several series

(7) Comparison of the manuscript of Haughton's paper 'On the reflexion of polarized light from polished surfaces, transparent and metallic' (Royal Society, PT. 68.4) with its printed text in *Phil. Trans.*, **153** (1863): 81–126, on 83, shows that Haughton left this statement unaltered. See also note (15) on Haughton's revision.

(8) On the phase difference of 90° between components, a retardation of $\frac{1}{4}$ of a wave-length, see Number 199 esp. note (5). In his report (note (2)) Stokes had made reference to the constant that 'the author calls the coefficient of refraction...defined as the tangent of the principal incidence (or that at which the difference of phase is a quarter of an undulation) and accordingly for transparent substances would agree with the index of refraction on the supposition of the exactitude of the formula of Fresnel.' See Number 199 esp. notes (8) and (11).

(9) See Number 199.

(10) In Haughton's paper I and J denote the amplitudes of the components of the reflected waves polarised in and perpendicular to the plane of incidence, respectively. These symbols had been introduced by A. L. Cauchy, 'Mémoire sur la polarisation des rayons réfléchis ou réfractés par la surface de séparation de deux corps isophanes et transparents', *Comptes Rendus*, **9** (1839): 676–91, esp. 687–91; and subsequently employed by Jamin, 'Mémoire sur la réflexion à la surface des corps transparents': esp. 274–5. See Number 199 where Maxwell denotes J/I as relation (B).

(11) See Stokes' second point in his critique of Haughton's paper (note (2)). Earlier in his report he had noted that Haughton seemed to define the 'coefficient of reflexion...at one place as the value of J/I at the principal incidence, in another as the minimum value of J/I... regarded as a function of the angle of incidence'.

(12) See Stokes' fourth point in his critique of Haughton's paper (note (2)). Earlier in his

of observations are taken under slightly different circumstances (azimuth of polarizer) which ought to give identical series of values of the retardation and $\frac{J}{I}$ for each incidence, I think the reader ought to have the means placed before him of judging the degree and reliance to be placed on each observation with deductions as to the accuracy of each element of the apparatus (polarizer compensator analyzer & graduated circles)[13] and finally a comparison of general results with different modifications of the theory showing whether the differences from the theory can be explained by errors of observation.

The variation of $\frac{J}{I}$ with the azimuth must either be erroneous or may arise from the action of the quartz of the compensator.[14] A few experiments with the compensator should have been given to prove that no rotation of plane polarization takes place.

I therefore agree with you that the author should be requested to point out

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report he had criticised Haughton with some vehemence: 'we are not told whether the observations were taken right and left of the plane of incidence at equal azimuths, or if not in what manner the index errors of the polarizer and analyzer were determined, or whether they were determined at all. We do not know whether or not the precaution was taken of reversing in succession the polarizer and analyzer (i.e. turning them through 180°) and taking the mean of the results in the 4 different positions. We are not informed whether the results given were got by single observations, or how many they were the mean of.' See Haughton, 'On the reflexion of polarized light from polished surfaces': 83–4 for a brief account of his experimental procedure.

(13) Haughton explained that he had 'employed the quartz compensator described by M. Jamin, for the purpose of converting the elliptically-polarized reflected light into plane-polarized light, before allowing it to pass through the analyser'; Haughton, 'On the reflexion of polarized light from polished surfaces': 83; and see Jamin, 'Mémoire sur la réflexion à la surface des corps transparentes': 271–2.

(14) See Stokes' fifth point in his critique of Haughton's paper (note (2)). Earlier in the report he had stated: 'Yet on the strength of these experiments, so many important details respecting which are omitted, we are expected to believe that the ratio of J/I at a given angle of incidence changes with the azimuth of the polarizer.... But neither Fresnel's formula for reflexion nor any others are involved in the ratio of J to I when nothing but the azimuth of the polarizer is changed.... A variation in the ratio of J to I with a variation in the azimuth of the polarizer could not be accepted as a true physical result without the most rigorous scrutiny of every step of the process. In default of such we should unhesitatingly attribute it to some disturbing cause vitiating in a regular manner the result of observation.' In a document 'Remarks on M^r Stokes' Report...' (Royal Society, *Referees' Reports*, **5**: 105), accompanying a letter to Stokes of 6 November 1862 (Royal Society, *Referees' Reports*, **5**: 104), Haughton responded to this critique: 'With regard to J/I varying with the azimuth, while the incidence remained the same, these variations were observed, after making all due precautions against experimental error, & I cannot therefore consent to alter that portion of my paper.'

the claims of his paper to publication and to state in what relation it stands to M. Jamin's observations, whether as more correct or otherwise.[15]

If the observations were properly reduced and some proof given of their accuracy the results would be of value, though not the first of their kind, but I imagine from what you say that in the case of mere verifications and repetitions of researches the Society would not recommend publication in the Transactions.

Yours truly
J. Clerk Maxwell

(15) Comparison of the manuscript of Haughton's paper with its printed text (see note (7)) shows that Haughton responded to Stokes' report by prefacing his paper with a resumé of Jamin's work and appending tables comparing his own results with those of Jamin. See Haughton, 'On the reflexion of polarised light from polished surfaces': 81–3, 123–5.

REPORT ON A PAPER BY THOMAS ROMNEY ROBINSON ON THE SPECTRA OF ELECTRIC SPARKS[1]

10 SEPTEMBER 1862

From the original in the Library of the Royal Society, London[2]

REPORT ON DR ROBINSONS 'CONTINUATION OF PAPER ON ELECTRIC SPECTRA'[3]

In this paper, the author, by comparing the results of the observations already described and tabulated,[4] arrives at the general conclusions to which his experiments point.

He wishes to ascertain in what circumstances connected with the production of an electric spark the position and intensity of the bright lines in its spectrum depend. He has therefore varied the electric conditions the electrodes and the medium through which the discharge takes place.

If the lines were due entirely to the independent action of the elementary bodies in the region of the spark and if each elementary body produces a group of lines each of them distinct from any line due to any other element then with very little trouble we should arrive at an ultimate analysis of all bodies. But if many lines are common to many elements, and if the lines due to compounds and mixtures differ by excess or defect from the sum of the lines due to the compounds then we have the prospect of a wider field of investigation before we reach the ultimatum of science.

It appears from the observations that a large number of lines are seen in many different cases, when both the electrodes and the gaseous medium are changed. In these cases there is either an exact coincidence between certain of the modes of vibration of different elements, or there is an unknown element common to the supposed elements or the lines do not arise from gross matter but from certain mechanical properties of an 'etherial' medium which is always present.

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(1) See also Number 198.

(2) Royal Society, *Referees' Reports*, 5: 219.

(3) T. R. Robinson, 'On the spectra of electric light, as modified by the nature of the electrodes and the media of discharge', *Phil. Trans.*, **152** (1862): 939–86, esp. 974–86.

(4) See Number 198 note (2).

A perfect coincidence between the periodic times of vibration of perfectly different and independent bodies would be unexpected and would require rigid proof, both with respect to the degree of coincidence and the want of anything in common between the bodies. Exact coincidence even of single lines, much more of groups whether near each other or in different parts of the spectrum would be a very strong argument in favour of identity of cause. Whether we have reason to trust to spectrum observations in attempting to reduce the number of elementary bodies, or whether we should expect rather to discover some properties of an ethereal medium, we must make use of comparisons such as those in this paper and at the same time we should study the mathematical theory of the vibrations of compound systems.

If a system is performing compound vibrations the component vibrations belonging to a given series, then the presence of another system capable of performing a series of vibrations having various relations to the first series may greatly modify the actual vibrations. If the mechanical connexion between the two systems is not very close, then only those vibrations will be much affected which are common to both systems and these will be affected in intensity but not in period.

If the connexion between the two systems is of a more intimate kind we may expect slight changes in the period as well as the intensity of the vibrations.

If any connexion could be made out between the results of a mechanical theory and the observed effects of condensation and rarefaction mixture and combination we should obtain even more insight into nature than if we had established a theory of lines peculiar to each element, and unalterable.

The weakening of a line is as remarkable a phenomenon as its intensification and is free from the suspicion which we may often feel of a line being due to an inappreciable quantity of a foreign body. I think it would add to the value of the paper if the history of a few of the more remarkable lines were exhibited in as many tables, the table for each line consisting of columns for the gases and horizontal lines for the electrodes and the body of the table indicating by marks of magnitude, as *, n.b &c,[(5)] the intensity for the gas and the metal corresponding. The reader would then see at one view the effect of different circumstances on the brightness of a line, without tracing it through the tables of metals in the 1st part.

The paper indicates a large field of inquiry which it would take long to work over but while it agrees with former researches in making catalogues of

(5) As he explained in 'On the spectra of electric light': 949, Robinson denoted by a * 'the lines which are far transcendent in brilliancy, and are not less broad than the image of the slit'.

lines and pointing out the most conspicuous as characteristic of particular substances, it shows the importance of a careful study of particular lines as influenced by the density of the medium, its composition, and that of the electrodes and the electric circumstances of the spark. The lines most worth examining are those which are visible in many cases but have variable intensity and perhaps those most important of all may be those which are broad and apparently ill defined. These would probably be most altered by change of circumstances and would therefore give most information as to the conditions of alteration.

When a few remarkable lines have been selected and examined with a powerful train of prisms it might be worth trying whether the position of a line cannot be altered by change of temperature in the medium, substitution of flame for electric spark or different combinations of the element.

By viewing a standard case and another example at once through the same slit the coincidence or defect of coincidence might be very accurately observed.

I think that Ways mercury light[(6)] would be a good subject for experiment because we have mercury for electrodes and nearly pure mercury vapour (very dense) for medium and we can vary the intensity of the electric current continuously and we may even by connecting the electrodes with those of an induction machine have sparks in the same place with the continuous current and simultaneously.

In the same way we might use sparks passing through an ordinary flame, to compare the effects of heat and electricity.

I do not understand however whether a molecule can be said to have a temperature of its own. If its parts have relative motion, that may constitute its selfcontained temperature, while its motion as a whole may be the condition of its temperature as observed. The first kind of motion probably is that on which the lines depend and may possibly be different when excited by electricity than when excited by communication of heat.

I do not know whether visible light can be produced by the sudden compression of a gas. Radiant heat is so produced (see Tyndall)[(7)] and if we could get flashes of light in this way in gases of great density, without

(6) Way's mercury light is described by J. H. Gladstone, 'On the electric light of mercury', *Phil. Mag.*, ser. 4, **20** (1860): 249–53, on 249.

(7) John Tyndall, 'On the absorption and radiation of heat by gaseous matter', *Phil. Trans.*, **152** (1862): 59–98, esp. 75–80.

electricity or combustion it might be an aid in eliminating the effects of particular circumstances.

JAMES CLERK MAXWELL(8)

Sept 10, 1862

(8) In a report dated 22 October 1862 (Royal Society, *Referees' Reports*, 5: 220), Charles Wheatstone declared Robinson's paper to be 'a very valuable contribution to that new department of optics and chemistry which has been designated "Spectral Analysis". The influence which pressure in gases has on the development of the spectral lines; and the proof that lines appear in compound gases which do not exist in their elementary components, while others which occur in the elements disappear in the compound are quite new and contrary to the prevalent opinion.'

LETTER TO GEORGE GABRIEL STOKES

10 SEPTEMBER 1862

From the original in the University Library, Cambridge(1)

Glenlair,
Dalbeattie
1862 Sept 10

Dear Stokes

I send you reports on Dr Robinson's papers.(2) I did not know that the reciprocating discharge in a closed glass vessel had been tried and found to exhibit the lines of the constituents of glass.

I have been comparing the results of colour observations by different eyes in order to see whether they can be reduced to a single diagram by altering the mode of projection, that is taking different units of the standard colours.(3)

I find that I get consistent results in the three cases I have tried but that the numbers by which the coordinates must be multiplied vary from 1 to 3 so that if we suppose one person to see blue 3 times stronger relative to green than another person, we get consistent results. I find that the centre and circumference of my retina differ so that white appears bluer to the retina generally than to the centre and therefore the colour complementary to red is of higher refrangibility for the retina than for its centre. I cannot perceive any difference in merely looking at white paper, but I suppose any constant difference in different parts of the retina can be discovered only by reason as it would not be an object of perception.(4) By choosing a standard eye, or taking an average I could express the facts of colour vision in two diagrams.

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(1) ULC Add. MSS 7656, M 419. First published in Larmor, *Correspondence*, **2**: 22–3.

(2) Numbers 198 and 201.

(3) In his paper 'On the theory of compound colours, and the relations of the colours of the spectrum', *Phil. Trans.*, **150** (1860): 57–84, esp. 68–9 (= *Scientific Papers*, **1**: 424–5), Maxwell took red, green and blue as standards of spectral colour, marking the positions of the standard colours on the scale of his colour box. See Volume 1: 619, 635, 638. Maxwell had corresponded with Stokes on the subject in 1859–60; see Volume I: 619–22, 632, 640, 645–53, 657–8. In a letter to C. J. Monro of 18 February 1862 Maxwell described a new instrument, based on an experimental arrangement described by Newton, for experiments on colour vision: see Volume I: 709. There are some experimental results in colour vision *c.*1862 among Maxwell's manuscripts (ULC Add. MSS 7655, V, b/13).

(4) See Maxwell's paper 'On colour-vision at different points of the retina', *Report of the Fortieth Meeting of the British Association for the Advancement of Science; held at Liverpool in September 1870* (London, 1871), part 2: 40–1 (= *Scientific Papers*, **2**: 230–2). See also Maxwell's letter to Monro of 6 July 1870 (Number 341).

1st a triangle of colour giving relations in the quality of colours and the same for all eyes.(5)

2nd a curve representing the intensity of colour at each point of the spectrum.(6) This curve is different for different eyes and for different parts of the same eye. These differences may arise from absorption of certain rays before they reach the retina. Irregularities in the curve extending over small spaces are probably of this sort. There may also be differences in the sensibility of the nerves to particular sensations. This would account for inequalities extending over entire regions of colour in a regular manner.

The fact that all colours lie nearly in two straight lines makes it impossible to assign the exact position of the sensation defective in the colour blind.(7) It lies in the production of the line joining green with the extreme red but its exact position differs according to the normal person with whom the colourblind eye is compared, and colour blind eyes differ as much with regard to intensity of different colours as normal eyes do.

Yours truly
JAMES CLERK MAXWELL

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(5) Compare Volume I: 620; and Maxwell, 'On the theory of compound colours': 74 and Fig. 4 opposite 84 (= *Scientific Papers*, **1**: 431, 444).

(6) Compare Volume I: 650–1; and Maxwell, 'On the theory of compound colours': Figs. 6, 7 and 9, opposite 84 (= *Scientific Papers*, **1**: 444).

(7) Compare Maxwell, 'On the theory of compound colours': 81 and Fig. 10 opposite 84 (= *Scientific Papers*, **1**: 440, 444); 'The triangle of colours is reduced, in the case of dichromic vision, to a straight line'.

MANUSCRIPT ON DIAGRAMS OF FORCES(1)

circa NOVEMBER 1862(2)

From the original in the King's College London Archives(3)

MECHANICAL DIAGRAMS & DIAGRAMS OF FORCES(4)

A mechanical diagram is a figure of a structure in which lines are drawn between the different points of a structure indicating the *direction* of the forces which act between them.(5)

The forces which act between two points are either *Tensions* pulling them together or *Pressures* pushing them asunder. We reckon Tensions positive and Pressures negative. The action on the one point is always equal and opposite to that on the other. In the mechanical diagram each line of action is distinguished by a letter.

A Diagram of Forces is a figure in which every force in the mechanical diagram is represented in magnitude and direction by a line parallel to its line of action in the 1st Diagram and distinguished by the same letter.

Every system of forces in the mechanical diagram which $\left\{\begin{array}{l}\text{act at}\\ \text{pass through}\end{array}\right.$ one point is represented in the diagram of forces by a closed figure whose sides are parallel and proportional to the forces.

When the lines meeting at a point in either figure form a closed figure in the other the two diagrams are said to be *reciprocal*.

When a frame is loaded only at external points on one side or the other, so that no piece which forms a side of two closed figures is loaded at both ends a reciprocal figure can always be drawn.

All the loads are represented by consecutive parts of the same straight line.

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(1) Probably notes for a lecture to the class of applied mechanics at King's College London: see Number 334.

(2) The entries following this note in Maxwell's notebook (see note (3)), class exercises for King's College London, are dated November 1862.

(3) Notebook of James Clerk Maxwell (1), King's College London Archives.

(4) In his paper 'On reciprocal figures and diagrams of forces', *Phil. Mag.*, ser. 4, **27** (1864): 250–61, on 251 (= *Scientific Papers*, **1**: 515), Maxwell states that he was indebted to a discussion by W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 137–40, for a general statement of the method of diagrams of forces.

(5) See also Numbers 273 and 334.

LETTER TO JOHN WILLIAM CUNNINGHAM(1)

5 DECEMBER 1862

From the original in the King's College London Archives(2)

8 Palace Gardens Terrace
W
5 Dec 1862

Dear Sir

I am very anxious that the examination papers in Mechanics should be printed from type instead of from stone.

I find that the lithographic papers are printed so that even if everything is plain in perfect copies, uncertainties exist in other copies which are very apt to make the examination not quite a fair one.

Mr Smalley(3) has the M.S. and expects to give it in at the office today.

Yours truly
J. Clerk Maxwell

J W Cunningham Esqre

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(1) Secretary of King's College London.

(2) King's College London Archives, KA/IC/M 67. Previously published in C. Domb, 'James Clerk Maxwell in London, 1860–1865', *Notes and Records of the Royal Society*, **35** (1980): 67–103, on 79.

(3) George Robarts Smalley, St John's 1841, Mathematical Master at King's College School (Venn), was appointed to a College Lectureship in Natural Philosophy on 11 October 1861. The appointment was made on the grounds that the 'duties [were]... too heavy for one person properly to discharge' (King's College London Archives, King's College Council Vol. I, minute 42). Smalley had been a candidate for the Professorship of Natural Philosophy (to which Maxwell was appointed) in 1860; see Volume I: 662n. On his duties as lecturer see Number 209.

REPORT ON A PAPER BY GEORGE BIDDELL AIRY ON STRESS IN BEAMS

LATE DECEMBER 1862[1]

From the original in the Library of the Royal Society, London[2]

REPORT ON THE ASTRONOMER ROYAL'S PAPER ON THE STRAINS[3] IN THE INTERIOR OF BEAMS[4]

In this paper the Author investigates the conditions of equilibrium of the forces of tension and pressure acting in the interior of a heavy lamina, and applies his results to cases of beams supported and loaded in different ways.

The laws of the resolution and composition of internal pressures and tensions acting in the same plane are established, and then, by considering the equilibrium of a portion of the beam divided from the rest by an imaginary line of any form, the author arrives at a result, which, being treated by means of the Calculus of Variations, gives the equations of equilibrium of the vertical and horizontal pressures and shearing forces.

These equations are partial differential equations, one solution of which is, that these three forces are the three second differential coefficients of a single function of x and y.[5] In the general form of the solution, arbitrary functions of x and y, respectively, are added to the 2nd diffl coeffts with respect to x & y respectively. The author, however, considers that the effect of these terms is merely to express accidental distributions of pressure arising from the beam being originally in a state of strain. In this investigation which has reference to the *additional* stresses[6] due to the external applied forces, they are therefore disregarded. We shall see the effect of this hereafter.

The Author then assumes an expression containing a sufficient number of terms of powers and products of x and y as the form of the function to be found, and reduces it by the equations of condition, till it contains only three arbitrary constants.

(1) See Number 206. According to the Royal Society's *Register of Papers Received* Airy's paper was referred to Maxwell on 18 December 1863, and to Rankine on 31 December 1863.

(2) Royal Society's *Referees' Reports*, 5: 6.

(3) For Maxwell's comment on Airy's use of the term 'strain' see Number 206 esp. note (7).

(4) George Biddell Airy, 'On the strains in the interior of beams', *Phil. Trans.*, **153** (1863): 49–79. The paper was received by the Royal Society on 6 November 1862, and read on 11 December 1862; see the abstract in *Proc. Roy. Soc.*, **12** (1862): 304–6.

(5) See note (10).

(6) On Maxwell's use of the term 'stress' see Number 206 esp. note (7).

To determine these three quantities the original equations are not sufficient. (We must refer to this again.)(7) The Author therefore avails himself of the following hypothesis deduced from experiment by several writers on this subject (Young &c).(8)

That the middle point of any vertical section is a neutral point (having no horizontal pressure or tension) and that the horizontal pressures below this point are equal to the tensions at points equally distant above it.(9)

From this supposition he is enabled to deduce the form of the function F,(10) and from it the nature of the forces acting at any point. Tables are given, showing the values of the principal pressures at selected points and the angles they make with the vertical, and this is done for a beam projecting from a wall, a beam supported on two piers & unloaded, centrally loaded, and excentrically loaded, a beam fixed at both ends, and a beam fixed at one end and supported at the other. Diagrams are added,(11) showing the direction of the axes of stress at every point of the beam in each of these cases and it is easy to see from these diagrams the general character of the forces in a way which will be practically useful to all who wish to understand the subject.

I therefore regard this paper as a valuable one with regard to its subject and its results but I have some remarks to make upon the physical and mathematical principles by which these results are worked out.

The objection which I have to the method of investigation is that the conditions arising from the elasticity of the beam are not taken account of at all or even mentioned.(12)

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(7) See *infra* and note (21).

(8) W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 73–4. There is a general discussion of the elasticity of beams by Thomas Young, *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, 2 vols. (London, 1807), **1**: 135–52.

(9) Airy, 'On the strains in the interior of beams': 58.

(10) Maxwell subsequently developed the application of this function in his papers 'On reciprocal diagrams in space, and their relation to Airy's function of stress', *Proceedings of the London Mathematical Society*, **2** (1868): 58–60 (= *Scientific Papers*, **2**: 102–5); and 'On reciprocal figures, frames and diagrams of forces', *Trans. Roy. Soc. Edinb.*, **25** (1870): 1–40, esp. 27–31 (= *Scientific Papers*, **2**: 192–7), where he referred to Airy's 'important simplification of the theory of the equilibrium of stress in two dimensions' by means of the stress function. See Number 334. In his report on Airy's paper, dated 26 January 1863 (Royal Society, *Referees' Reports*, **5**: 1), W. J. M. Rankine observed that 'the introduction of that function F...leads to remarkably clear, simple, and certain methods of solving problems respecting the strains in the interior of beams'.

(11) Plates V, VI and VII in *Phil. Trans.*, **153** (1863).

(12) In a letter to Stokes of 22 February 1863 (Royal Society, *Referees' Reports*, **5**: 4), having been sent Maxwell's report, Airy responded: 'This remark astonishes me. The elasticity and its law, are the foundations of every one of my applications of the new theory.' In support of this

Now there are certain mechanical investigations in which it is of use to consider elasticity when the question relates altogether to forces. When we regard a beam as a mere line, and enquire into the whole moment of bending tending to break it across any given point, we obtain the result without requiring to take notice of the yielding of the beam.

Even when the question relates to a beam of finite depth as in the case before us, it seems likely, and is I believe demonstrable, that the final result, giving the value of the forces at any point, will be independent of the coefficients of elasticity, provided all parts of the beam are equally elastic. But I do not think that we can obtain the forces acting between the parts of any system without knowing the conditions under which the distance of those parts is variable, unless there are no more connexions between the parts than are just sufficient to fix each point. If there are more connexions we require to know something of the elasticity of the connexions, or the question is indeterminate.

In the present paper a system of forces is given which fulfils the conditions of internal equilibrium and the conditions of no pressure at the free surface. Such a system of forces might exist in a beam, if it could be produced in it by the straining of an elastic solid. But whether this is the system of internal forces which would be *produced* in a beam by the action of the given external forces is as yet undecided.

Conceive a beam free from weight, and projecting from a wall and having its interior in a state of strain but in equilibrium among themselves. Now let gravity act on the beam. It will produce an additional system of internal forces superimposed on the first. Will this additional system be the same as that which the author has obtained in his first example?[(13)] I think it is necessary to examine the question before we can decide.

Writing as in the paper $LM - Q$ for the pressure parallel to x, the shearing

contention he cites passages from his paper (see note (21) and also Airy, 'On the strains in the interior of beams': 49, 56, 58–9), concluding: 'in every instance, the value of the function [F] is found from a process which rests ENTIRELY on the theory of elasticity.' For Maxwell's response see Number 212, esp. note (2), Airy having acquiesced to Stokes' inquiry in a letter of 26 February 1863 (Royal Greenwich Observatory Archive, ULC, Airy Papers 6/392, 124R–125V): 'Would you have any objection to letting me forward to him your letter to me?...'. Stokes continued: 'I have not as yet myself read your paper, and therefore cannot fully enter into the report and your letter; but unless I greatly mistake I catch his meaning – that your investigation takes account of *systems of forces only* not entering into displacements; that your result is therefore necessarily FROM THE VERY PRINCIPLE OF THE PROCESS, indeterminate'.

(13) Airy, 'On the strains in the interior of beams': 57–60.

force and the pressure parallel to y ($p_{xx} p_{xy}$ and p_{yy} in Rankine)(14) then the eq^n of eq^m are

$$\frac{d}{dx}p_{xx}+\frac{d}{dy}p_{xy}=0$$

$$\frac{d}{dx}p_{xy}+\frac{d}{dy}p_{yy}+g=0$$

writing g for gravity that we may know what terms depend on it.(15)

Putting $M=p_{xy}=-\dfrac{d^2F}{dx\,dy}$ we find

$$L=p_{xx}=\frac{d^2F}{dy^2}+Y \text{ (a function of } y \text{ only)}$$

$$Q=p_{yy}=\frac{d^2F}{dx^2}+X-gy \quad (X \text{ a function of } x \text{ only}).^{(16)}$$

Let us suppose the beam isotropic in its elasticity then if $\xi\eta\zeta$ be the displacements in xyz and if μ & m coeffts of cubic & linear elasticity(17)

$$\frac{d\xi}{dx}=\left(\frac{1}{9\mu}+\frac{2}{3m}\right)p_{xx}+\left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_{yy}+p_{zz})^{(18)}$$

$$\frac{d\eta}{dy}=\left(\frac{1}{9\mu}+\frac{2}{3m}\right)p_{yy}+\left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_{zz}+p_{xx})$$

$$\frac{d\zeta}{dz}=\left(\frac{1}{9\mu}+\frac{2}{3m}\right)p_{zz}+\left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_{xx}+p_{yy})$$

$$\frac{d\xi}{dy}+\frac{d\eta}{dx}=\frac{2}{m}(p_{xy}).$$

(14) Rankine, *Applied Mechanics*: 89; in Rankine's symbolism p denotes 'intensity of a stress', and of the subscript letters he intended 'the first small letter to denote the direction perpendicular to the plane on which the stress acts, and the second to denote the direction of the stress itself'.

(15) In 'On the strains in the interior of beams': 54 Airy wrote $y-Q=O$, y being a vertical ordinate representing gravity.

(16) Compare Airy, 'On the strains in the interior of beams': 55, where he establishes that 'L, M, O are the three partial differential equations of the second order of a function F of x and y, such that $L=d^2F/dy^2$, $M=d^2F/dx\,dy$, $O=d^2F/dx^2$'. See notes (10) and (15).

(17) Maxwell uses the symbols for these coefficients – there termed the 'moduli of cubical and linear elasticity' – which he had introduced in his paper 'On the equilibrium of elastic solids', *Trans. Roy. Soc. Edinb.*, **20** (1850): 87–120 (= *Scientific Papers*, **1**: 30–73). See Volume I: 135.

(18) The form of these equations of elasticity is drawn from those of 'On the equilibrium of elastic solids': 90–5 (= *Scientific Papers*, **1**: 34–41). See Volume I: 157–63.

Now there are two cases we may consider. 1st a very thin lamina free from pressure along z, then $p_{zz} = 0$. 2nd a very thin plank unable to expand in z, then $\zeta = 0$ and

$$p_{zz} = -\frac{\dfrac{1}{9\mu}-\dfrac{1}{3m}}{\dfrac{1}{9\mu}+\dfrac{2}{3m}}(p_{xx}+p_{yy}) = -h(p_{xx}+p_{yy}).$$

Integrating we find

$$\xi = \overset{(1-h^2)}{\left(\frac{1}{9\mu}+\frac{2}{3m}\right)}\int p_{xx}\,dx+\overset{(1-h)}{\left(\frac{1}{9\mu}-\frac{1}{3m}\right)}\left(\frac{dF}{dx}+\int Xdx-gxy\right)$$

$$\eta = \overset{(1-h^2)}{\left(\frac{1}{9\mu}+\frac{2}{3m}\right)}\int p_{yy}\,dy+\overset{(1-h)}{\left(\frac{1}{9\mu}-\frac{1}{3m}\right)}\left(\frac{dF}{dy}+\int Ydy\right)$$

and the factors written above the line are to be used in case 2nd only. Differentiating and adding we find

$$\frac{d\xi}{dy}+\frac{d\eta}{dx} = \overset{(1-h^2)}{\left(\frac{1}{9\mu}+\frac{2}{3m}\right)}\left(\frac{d}{dy}\int p_{xx}\,dx+\frac{d}{dx}\int p_{yy}\,dy\right)$$

$$+\overset{(1-h)}{\left(\frac{1}{9\mu}-\frac{1}{3m}\right)}\left(2\frac{d^2F}{dx\,dy}-gx\right)$$

$$= \frac{2}{m}\frac{d^2F}{dx\,dy}.$$

$$\therefore \frac{d}{dy}\int p_{xx}\,dx-2p_{xy}+\frac{d}{dx}\int p_{yy}\,dy-\overset{\frac{1}{1-h}}{g}hx = 0.$$

Note. The value of h does not affect the distribution of forces.

This is the equation depending on the fact that the beam was unstrained before the weight began to act. Let us examine whether the solution in the present paper fulfils it.

We have at Page 11[19]

$$p_{xx} = \frac{d^2F}{dy^2} = \frac{3}{s^2}(r-x)^2(s-2y)$$

$$\int p_{xx}\,dx = -\frac{1}{s^2}(r-x)^3(s-2y)+Y'$$

$$\frac{d}{dy}\int p_{xx}\,dx = \frac{2}{s^2}(r-x)^3+\frac{dY'}{dy}$$

$$p_{xy} = -\frac{d^2F}{dx\,dy} = \frac{6}{s^2}(r-x)(sy-y^2)$$

$$p_{yy} = \frac{d^2F}{dx^2}-y = \frac{1}{s^2}(3sy^2-2y^3)-y$$

$$\int p_{yy}\,dy = \frac{1}{s^2}(sy^3-\tfrac{1}{2}y^4)-\tfrac{1}{2}y^2+X'$$

$$\frac{d}{dx}\int p_{yy}\,dy = \frac{dX'}{dx}.$$

The equation of condition now becomes

$$\frac{2}{s^2}(r-x)^3-\frac{12}{s^2}(r-x)(sy-y^2)+\frac{dX'}{dx}+\frac{dY'}{dy}-ghx = 0.$$

Now $\frac{dX'}{dx}$ is a function of x and constants and $\frac{dY'}{dy}$ is a function of y and constants. These may be so assumed as to make the equation true, whatever functions of x alone or y alone enter into it. But the second term is a function of x and y and therefore the author's solution does not satisfy this equation.

Let us now introduce the terms which he has neglected in the expressions for p_{xx} & p_{yy} namely Y & X. These terms will produce terms in the equation of condition of the form $x\frac{dY}{dy}+y\frac{dX}{dx}$ so that if we make

$$\frac{dY}{dy} = -\frac{12}{s^2}(sy-y^2) \quad \text{and} \quad Y = -\frac{2}{s^2}(3sy^2-2y^3)+Ay+B$$

the equation of condition will be satisfied. In this case

$$p_{xx} = \frac{3}{s^2}(r-x)(s-2y)-\frac{2}{s^2}(3sy^2-2y^3)+Ay+B$$

(19) Airy, 'On the strains in the interior of beams': 59. Using the symbols introduced by Airy, r and s are the length and depth of the beam, x the horizontal abscissa, and y the vertical ordinate.

and the other forces remain as before. This solution satisfies the condition that the beam will return to a state of no strain if its weight were abolished, but it does not satisfy the condition that $p = 0$ at the end of the beam. To make the solution complete we should require to calculate the effect of a distribution of pressure represented by $\frac{2}{s^2}(3sy^2 - 2y^3) + Ay + B$ on the end of a beam fixed at the other end and unaffected by gravity. I have not been able to do this. The condition of no longitudinal pressure on any part of the free end of the beam renders the exact determination of the forces much more difficult. The difference, however, between the exact solution and that of this paper will in the case of long beams be very small, and only sensible very near the end.

If we determine A and B so as to make the *total* longitudinal pressure $= 0$ and also the *total* moment of bending in the bounding vertical surface $= 0$ we find for the value of p_{xx}

$$\begin{aligned} p_{xx} &= \frac{3}{s^2}(r-x)^2(s-2y) + s\left(\frac{4y^3}{s^3} - \frac{6y^2}{s^2} + \frac{12}{5}\frac{y}{s} + \frac{1}{5}\right) \\ &= s\left(\frac{2y}{s} - 1\right)\left\{-3\frac{r^2}{s^2}\left(1 - \frac{x}{r}\right)^2 + 2\frac{y^2}{s^2} - 2\frac{y}{s} + \frac{1}{5}\right\}. \end{aligned}$$

This is the nearest approximation to the solution of Case 1st that I can obtain. It corresponds to the case of a beam infinite in length, acted on by its own weight and by other forces at a great distance on each side of the part considered, in such a way as to make the *total* longitudinal, bending and shearing forces at the section $(r = x)$ disappear. Case 2 – the beam resting on piers at its extremities(20) – may be modified in the same way by supposing it produced both ways and such vertical forces and couples applied at the extremities, that the moment of bending shall disappear at the points corresponding to the piers.

The actual case is complicated by the conditions relating to the two free surfaces at the ends and to the upward pressure of the piers, which will introduce terms depending on inverse powers of the distance from the points where the pressure is applied. These inverse powers of a distance would occur also in an exact solution of the cases in which weight is applied at a point of the beam.

If any one can work out the *exact* solutions, he will have performed a mathematical feat, but I do not think he will have added anything to our practical knowledge of the forces in a beam not near the ends or the points

(20) Airy, 'On the strains in the interior of beams': 60–3.

where pressures are applied. For all such points the formulæ obtained in this paper are quite satisfactory and as far as I know they are new.

I must also remark that the supposition made at page 9 about horizontal pressures and the position of the neutral line,[(21)] are not required, for if we assume $F = (ax^2+bx+c)y^2+(ex^2+fx+g)y^3$ [(22)] we can determine *all* the constants from the following conditions at the surfaces.

$$\frac{d^2F}{dx\,dy} = 0 \quad 1^{\text{st}} \text{ when } y = 0 \quad 2^{\text{nd}} \text{ when } y = s \quad 3^{\text{rd}} \text{ when } x = r$$

$$\frac{d^2F}{dx^2} - y = 0 \quad 1^{\text{st}} \text{ when } y = 0 \quad 2^{\text{nd}} \text{ when } y = s$$

$$\frac{d^2F}{dy^2} = 0 \quad \text{when } x = r.$$

The result is the same as that given in the paper.[(23)] I think therefore that this assumption may be dispensed with, and if some notice were taken of the fact that the beam is in some slight degree elastic the paper would be more valuable as a part of the Transactions.[(24)]

JAMES CLERK MAXWELL

(21) See *supra* and note (9). In his response to Maxwell's critique (see note (12)) Airy cited this passage of his paper, italicising a key clause: '..."the usual assumptions, namely, that there is a neutral point in the centre of the depth, that on the upper side of this neutral point the forces are forces of tension, and on the lower side are forces of compression, and that *these forces are proportional to the distances from the neutral point with equal coefficients on both sides*...". (These last words embody the whole ordinary theory of elasticity. They denote that the elastic forces put in play are proportional to the extension or compression of the material....)'.

(22) As in Airy, 'On the strains in the interior of beams': 57.

(23) Airy, 'On the strains in the interior of beams': 58–60.

(24) In contrast to Maxwell, Rankine raised no objections to Airy's paper in finding that the paper was 'theoretically interesting, and practically useful, in the highest degree, and well worthy of being published in the Transactions'. See also Number 206 note (7).

LETTER TO GEORGE GABRIEL STOKES

29 DECEMBER 1862

From the original in the University Library, Cambridge[1]

Glenlair,
Dalbeattie
29 Dec 1862

Dear Stokes

I enclose a report on the Astronomer Royal's paper on Strains in Beams.[2] I am not enough up in the literature of the subject to say whether it is quite new. I have not Lamés Leçons[3] to refer to and there may be something of the kind in the Journal de L'École Polytechnique.

The establishment of the eq^ns^ of equilibrium of stresses is very well done in Rankine especially for forces in one plane.[4]

But I do not believe that any solution can be other than indeterminate unless we assume a law for the *elasticity* of the beam. The simplest law is that it shall be uniformly elastic and by what I must regard as a mathematical accident the solution in the paper nearly coincides with this supposition.

It does not matter what the numerical value of the elasticity is provided it is uniform, but if it is not uniform or if the lamina be of variable thickness then the stiffest parts will support the greatest forces, no matter how small the actual deflexion may be.

There are two pieces of verbal criticism I have not put in the report because sometimes an author by the frequent use of a word establishes for it a peculiar meaning in his own writings.

The first is at p. 10 et passim 'momentum' plural 'momenta' used to signify the tendency of a force or forces to turn a body round a given point.[5]

English mathematicians generally use 'moment' plural 'moments' in this sense and reserve the other for what Newton calls 'Quantity of Motion'.[6]

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(1) ULC Add. MSS 7656, M 420.

(2) Number 205.

(3) Gabriel Lamé, *Leçons sur la Théorie Mathématique de l'Élasticité des Corps Solides* (Paris, 1852).

(4) W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 82–112, esp. 95–8.

(5) In the manuscript of his paper 'On the strains in the interior of beams' (Royal Society, PT. 68.3, on f. 10), Airy used the term 'momentum' and the expression 'equation of momenta' in the sense criticised by Maxwell. In the published text of 'On the strains in the interior of beams', *Phil. Trans.*, **153** (1863): 49–79, on 58, Airy corrected his usage to 'moment' and 'equation of moments'.

(6) Isaac Newton, *Principia*, Definition II; 'Quantitas motus'.

The second is a violation of a rule hardly yet established. It is very useful to have clear distinctions between words expressing forces – tensions – pressures – and words expressing their effects – extensions compressions. It is also useful to have some general word to signify the whole class of actions of each kind. Now a solid is said to be *strained* when its parts are no longer in their primitive relative positions. This is called a state of strain and the displacements themselves properly defined may be called strains.

The internal forces whether pressures tensions or shearing forces have been called by M^r^ Rankine Stresses[(7)] and I think that this distinction between strains and stresses ought to be encouraged and recognised as much as possible that our mathematical language may be as complete and accurate as is consistent with its being English.

I must also dissent from the statement that the stress-functions are always in positive integral powers of x & y.[(8)] They are often in negative powers of r.[(9)]

Yours truly
J. Clerk Maxwell

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(7) Rankine, *Applied Mechanics*: 58. This distinction between 'stress' and 'strain' had been proposed by Rankine in his paper 'On axes of elasticity and crystalline forms', *Phil. Trans.*, **146** (1856): 261–85, esp. 262; and subsequently modified by William Thomson, 'Elements of a mathematical theory of elasticity', *ibid.*: 481–98, esp 481. Maxwell had immediately adopted this usage; see Volume I: 489. In his report on Airy's paper (Royal Society, *Referees' Reports*, **5**: 1) Rankine did not object to Airy's use of the term 'strain'; see Number 205 note (10).

(8) Airy, 'On the strains in the interior of beams': 56–7.

(9) See Number 205 esp. note (19).

DRAFT PAPER ON THE CONDUCTION OF HEAT IN GASES[1]

circa SPRING 1863[2]

From the original in the University Library, Cambridge[3]

ON THE CONDUCTION OF HEAT IN GASES[4]

In the Philosophical Magazine for January and July 1860 I applied the theory of the motions and collisions of small elastic particles to the explanation of various properties of gases,[5] according to the analogies already pointed out by Daniel Bernoulli[6] and others and more recently by M. Clausius.*[7] That theory supposes that the particles of gases are in rapid motion, that they

* (7)

(1) This manuscript paper, written as if intended for publication (see Number 377 para. 12), constitutes Maxwell's considered response to criticisms of his 1860 theory of gases (see note (5)) advanced by Rudolf Clausius, 'Ueber die Wärmeleitung gasförmiger Körper', *Ann. Phys.*, **115** (1862): 1–56, to which he first alludes in a letter to Lewis Campbell of 21 April 1862 (Volume I: 711–12; and see also Volume I: 713–24).

(2) See Number 377, paras, 12 and 14, where Maxwell implies that this paper was written before his experiments in 1865 on the viscosity of gases (see Numbers 244, 245 and 246). He informed Stokes of his intention to undertake these experiments in a letter of 9 June 1863 (Number 212, and see esp. note (11)). It seems plausible to assume that he wrote the present paper before deciding to resort to an experimental test.

(3) ULC Add. MSS 7655, V, f/5. Previously published in *Molecules and Gases*: 339–47; see note (35).

(4) Compare the title of the English translation of Clausius' paper 'Ueber die Wärmeleitung gasförmiger Körper': 'On the conduction of heat by gases', *Phil. Mag.*, ser. 4, **23** (1862): 417–35, 512–34.

(5) J. C. Maxwell, 'Illustrations of the dynamical theory of gases', *Phil. Mag.*, ser. 4, **19** (1860): 19–32; *ibid.*, **20** (1860): 21–37 (= *Scientific Papers*, **1**: 377–409).

(6) Daniel Bernoulli, *Hydrodynamica, sive de Viribus et Motibus Fluidorum Commentarii* (Strasbourg, 1738): 200–2.

(7) Rudolf Clausius, 'Ueber die Art der Bewegung, welche wir Wärme nennen', *Ann. Phys.*, **100** (1857): 353–80; (trans.) 'On the kind of motion which we call heat', *Phil. Mag.*, ser. 4, **14** (1857): 108–27; and Clausius, 'Ueber die mittlere Länge der Wege, welche bei der Molecularbewegung gasförmiger Körper von den einzelnen Molecülen zurückgelegt werden; nebst einigen anderen Bemerkungen über die mechanische Wärmetheorie', *Ann. Phys.*, **105** (1858): 239–58; (trans.) 'On the mean length of the paths described by the separate molecules of gaseous bodies on the occurrence of molecular motion: together with some other remarks upon the mechanical theory of heat', *Phil. Mag.*, ser. 4, **17** (1859): 81–91. For Maxwell's account of work on the 'Kinetic Theory of Gases' see Number 377.

do not act on one another except within a very small distance, but that an exceedingly intense repulsive force comes into action when two particles come within this distance from each other. The particles therefore move in straight lines except when they come within the reach of the repulsive action of other particles which alters their motion with a suddenness like that of the impact of elastic bodies. The path of each particle is thus made up of a succession of straight lines and very sharp curves, and the mode in which the motion of one set of particles influences that of another will depend upon the average length of the straight part of the path as well as on the mass and velocity of the particles.

M. Clausius has recently published an investigation of the particular case of the conduction of heat through a gas†[(8)] which was very imperfectly treated by me in the paper referred to. He has pointed out several oversights in my calculation.[(9)] I have reexamined it and found some others the influence of which extends to other parts of my investigation.§[(10)] I shall therefore state here so much of my former results as will make the requisite corrections intelligible, and I shall retain the methods used in my former paper except when obliged to compare them with those of M. Clausius.

1 In my former paper I investigated the results of the collision of two elastic spheres and found that the velocity of each after the collision is resolvable into two parts, one of which is equal to the velocity of the centre of gravity of the two spheres before impact, and in the same

† Pogg Annalen Jan 1862[(8)] § Prop XXI[(10)]

(8) Clausius, 'Ueber die Wärmeleitung gasförmiger Körper'.

(9) Maxwell had erred, as Clausius pointed out, in that he had disregarded the additional kinetic energy associated with motion in the direction of the temperature gradient within the gas, and assumed an isotropic distribution function; see 'Illustrations of the dynamical theory of gases. Part II. On the process of diffusion of two or more kinds of moving particles among one another', *Phil. Mag.*, ser. 4, **20** (1860): 21–33 (= *Scientific Papers*, **1**: 392–405). Clausius commented ('Ueber die Wärmeleitung gasförmiger Körper': 13n): 'Maxwell hat in seiner oben erwähnten Abhandlung (*Phil. Mag.*, Vol. XX) bei der Bestimmung der Wärmeleitung den Umstand, dass die von einer Schicht ausgesandten Molecüle einen Ueberschuss an positiver Bewegungsgrösse haben, nicht berücksichtigt, sondern hat in seinen Rechnungen stillschweigend vorausgesetzt, dass die Molecüle nach allen Richtungen in gleicher Weise ausgesandt werden.' For Clausius' further criticism of Maxwell's treatment of the conduction of heat in gases see note (39). Clausius also drew attention to two oversights in his estimate of the ratio of the conductivities of air and copper; see 'Ueber die Wärmeleitung gasförmiger Körper': 54n, and Number 377 para. (11) and note (22).

(10) Prop. XXI of 'Illustrations of the dynamical theory of gases. Part II': 31–3 (= *Scientific Papers*, **1**: 403–5): 'To find the amount of energy which crosses unit of area in unit of time when the velocity of agitation is greater on one side than on the other'.

direction while the other is equal to the relative velocity of the sphere with respect to the centre of gravity and may, with equal probability be in any direction.

2 If a great many particles are in motion in the same vessel they will not all have the same velocity, but the average number of particles whose velocity lies within the limits v and $v+dv$ will be

$$N\frac{4}{\alpha^3\sqrt{\pi}}v^2 e^{-\frac{v^2}{\alpha^2}} dv \tag{1}$$

where N is the whole number of particles, and α a constant depending on the velocity. The velocities range through all possible values but more particles have a velocity $= \alpha$ than any other given velocity. The mean values of the different powers of v are found by integration to be

$$\frac{1}{v} = \frac{2}{\alpha\sqrt{\pi}}, \quad v = \frac{2\alpha}{\sqrt{\pi}}, \quad v^2 = \frac{3}{2}\alpha^2 \quad v^3 = \frac{4\alpha^3}{\sqrt{\pi}}. \tag{2}$$

Whenever we have to take the mean values of any power of v we must use the value here given, and not that got by raising the mean value of v to the given power.

3 The motion of the particles after a sufficient number of collisions will be compounded of a velocity V in a given direction, the same for all the particles, and a velocity v which may be in any direction and of which the values are distributed according to the law of eq^{n} (1). We shall call V the motion of translation and v the motion of agitation.(11)

4 If the velocities of agitation of two systems are distributed according to the law of eq^{n} (1) then the relative velocities of the particles, one in each system will be distributed according to the same law, but the mean relative velocity of agitation will be the square root of the sum of the squares of the mean velocities of agitation in the two systems.*(12)

* Note. In his original investigation of the length of path described by a particle M. Clausius has assumed the velocities of all the particles in each system to be equal, and in a communication to the Philosophical Magazine he has shown that on that supposition if v_1 and v_2 be the velocities in each system the mean relative velocity is not $\sqrt{v_1^2+v_2^2}$ but $v_1+\frac{1}{3}\frac{v_2^2}{v_1}$ where v_1 is the

(11) Maxwell, 'Illustrations of the dynamical theory of gases. Part I. On the motions and collisions of perfectly elastic spheres', *Phil. Mag.*, ser. 4, **19** (1860): 19–32, esp. 23–4 (= *Scientific Papers*, **1**: 381–2).

(12) See Maxwell's letter to Stokes of 30 May 1859 (Volume 1: 606–11).

greater of the two velocities. If however the velocities in each system follow the law of eq^{n} (1) then the mean relative velocity is $\sqrt{v_1^2+v_2^2}$.[13]

By the same method of demonstration it may be shown that if any quantity u is a linear function of several independent quantities xyz of the form

$$u = ax+by+cz \tag{3}$$

then if xyz are distributed according to the law of eq^{n} (1), u will also be distributed according to that law and the mean values of u, x, y & z will be connected by the equation

$$u^2 = a^2x^2+b^2y^2+c^2z^2. \tag{4}$$

5 The pressure of a gas is one third of the density multiplied by the mean of the square of the velocity.[14] Now if T be the absolute temperature[15] and p_0 ρ_0 the pressure and density when $T = T_0$ then we know by experiment that

$$p = \frac{p_0 \rho T}{\rho_0 T_0}\text{[16]}$$

whence we find $\overline{v^2} = 3\dfrac{p_0}{\rho_0 T_0}T$

or the velocity varies as the square root of the temperature.

6 When particles of different kinds are allowed to communicate their motion of agitation to each other the average *vis viva*[17] of the particles

(13) Clausius expressed the velocity of the gas molecules as an average velocity, while Maxwell introduced a statistical distribution function. Clausius had attempted to justify his method in a paper 'On the dynamical theory of gases', *Phil. Mag.*, ser. 4, **19** (1860): 434–6. Clausius' unconvincing objection to Maxwell's mathematical method in 'Illustrations of the dynamical theory of gases. Part I' drew no response from Maxwell: see his comments in Numbers 284 and 377; and W. D. Niven's note in *Scientific Papers*, **1**: 387.

(14) Maxwell, 'Illustrations of the dynamical theory of gases. Part I': 30 (= *Scientific Papers*, **1**: 389). See note (37) and Volume I: 607n.

(15) See William Thomson, 'On an absolute thermometric scale founded on Carnot's theory of the motive power of heat, and calculated from Regnault's observations', *Proc. Camb. Phil. Soc.*, **1** (1848): 66–71 (= *Math. & Phys. Papers*, **1**: 100–6); and Thomson, 'On the dynamical theory of heat. Part V. Thermo-electric currents', *Trans. Roy. Soc. Edinb.*, **21** (1854): 123–71, esp. 125 (= *Math. & Phys. Papers*, **1**: 235), where he states that 'the absolute values of two temperatures are to one another in the proportion of the heat taken in to the heat rejected in a perfect thermo-dynamic engine working with a source and refrigerator at the higher and lower of the temperatures respectively'.

(16) See W. J. M. Rankine, *A Manual of the Steam Engine and other Prime Movers* (London/Glasgow, 1859): 228.

(17) The term *vis viva* was still generally used, its usage becoming obsolete after Thomson and Tait introduced the term 'kinetic energy' in their *Treatise on Natural Philosophy* (Oxford, 1867): 195. For an account of energy terms see Volume I: 549–51n.

tends to become the same in both sets of particles, or if M_1 and M_2 are the masses of an atom in the two systems then when there is equilibrium of temperature

$$M_1 v_1^2 = M_2 v_2^2$$

or

$$\frac{M_1}{M_2} = \frac{v_1^2}{v_1^2} = \frac{p_2 \rho_1}{p_1 \rho_2}$$

so that when the pressures and temperatures of two gases are the same the atomic weights are proportional to the densities.(18)

7 By considering the effect of the collisions of bodies of any form not spherical it appears that the vis viva of rotation tends to become equal to that of translation so that the whole energy in unit of volume is not $\frac{1}{2}\rho v^2$ as in the case of perfect spheres, but ρv^2.(19) In a medium consisting partly of perfect spheres and partly of other bodies the energy will be $\frac{1}{2}\beta\rho v^2$ where

$$\beta = 1 + q$$

where q is the ratio of the mass of the non-spherical particles to the whole mass.(20)

If γ is the ratio of the specific heat under constant pressure to that under constant volume

$$\gamma = 1 + \frac{2}{3\beta}.$$

If the particles are all spherical with their centres of figure and mass coincident then $q = 0$, $\beta = 1$ and $\gamma = 1\frac{2}{3} = 1.\dot{6}$.

If none of the particles fulfil these conditions then $q = 1$ $\beta = 2$ $\gamma = 1\frac{1}{3} = 1.\dot{3}$.

These are the two extreme cases.

In the case of air $\gamma = 1.408$(21) $\beta = 1.634$ $q = .634$ $1 - q = .366$ so that according to the theory we are treating of we must suppose .634 of the weight of air to consist of non spherical particles and .366 of its

(18) Maxwell, 'Illustrations of the dynamical theory of gases. Part I': 30 (= *Scientific Papers*, **1**: 389–90).

(19) On the equipartition theorem of the distribution of kinetic energy among the translational and rotational motions of the particles, see Maxwell, 'Illustrations of the dynamical theory of gases. Part III. On the collision of perfectly elastic bodies of any form', *Phil. Mag.*, ser. 4, **20** (1860): 33–7 (= *Scientific Papers*, **1**: 405–9).

(20) β is 'the ratio of the whole *vis viva* to the *vis viva* of translation'; Maxwell, 'Illustrations of the dynamical theory of gases. Part III': 36 (= *Scientific Papers*, **1**: 409). See Clausius, 'Ueber die Art der Bewegung welche wir Wärme nennen': 377–80.

(21) The accepted contemporary value. See Rankine, *Manual of the Steam Engine*: 319–20. For discussion of this value see Volume I: 608–9n.

weight to consist of perfectly spherical particles having their centres of gravity at their centres.

In the case of Oxygen Hydrogen and Nitrogen the proportions must be supposed nearly the same as in air, but in Carbonic Acid we must suppose the proportion of spherical particles to be smaller.

In the case of Steam, if we admit the value of γ given at p. 320 of 'Rankine on the Steam Engine' is correct[22] we find

$$\gamma = 1.304 \quad \beta = 2.19 \quad q = 1.19 \quad 1-q = -.19$$

that is, we must suppose a negative quantity of spherical particles to exist, or in other words our theory fails to explain how the value of γ can be so low as 1.304.[23]

8 We come now to those properties of gases which depend on the distance which a particle travels between successive collisions. The distance depends on the number of particles in unit of volume and on the distance of the centres of two particles at the moment of collision.

If l_1 be the *mean* length of path of a particle of a gas whose density is ρ_1, mixed with other gases whose densities in the mixture are ρ_2 &c

$$\frac{1}{l_1} = A\rho_1 + B\rho_2 + \&c$$

where $$A = \sqrt{2}\frac{\pi s_1^2}{M_1}, \quad B = \sqrt{1+\frac{v_2^2}{v_1^2}}\frac{\pi s'^2}{M_2} \,\&c$$

where s_1 is the distance of centres at collision for two particles of the first kind and s' the same for a collision between one of the first and one of the second M_1 & M_2 the masses of particles of each kind and v_1 v_2 the velocities of agitation.

9 The actual length of the path described by a particle between successive collisions is not always the same, but the values of the actual paths are distributed according to the following law, as M. Clausius has shown in his former paper.*[24]

Let l be the mean length of path, then the proportion of the whole particles whose path exceeds nl is e^{-n}.[25]

* Phil. Mag. Feb. 1859[24]

(22) Rankine, *Manual of the Steam Engine*: 320.

(23) Compare Maxwell's comments in 'Illustrations of the dynamical theory of gases. Part III': 37 (= *Scientific Papers*, **1**: 409); and in his paper presented to the British Association in 1860 (Volume I: 659–60).

(24) Clausius, 'On the mean length of the paths': 85–7.

(25) Maxwell, 'Illustrations of the dynamical theory of gases. Part I': 27–8 (= *Scientific Papers*, **1**: 386–7).

10 The actual value of the length of path depends on the diameter of the particles and on the number in unit of volume, neither of which quantities are known, but if the internal friction of gases arises from the intermingling of particles from different layers of the moving gas then there will be a relation between l and the coefficient of internal friction, which may be determined by experiments on oscillating bodies[26] and on the passage of gases through long tubes.[27] I have shown that if μ is the tangential force on unit of area when the velocity parallel to that area increases by unity for every unit of length normal to the area

$$\mu = \tfrac{1}{3}\rho l v = \tfrac{1}{3}Av$$

since $\rho l = A$. This shows that μ is proportional to the square root of the absolute temperature, and independent of the density.[28]

From the value of μ given by Professor Stokes†[29] it appears that for air under the ordinary conditions, μ[30] $= \frac{1}{400,000}$ inch nearly.[31]

11 I now come to the question which I neglected to consider in my former paper. When the density and temperature of a gas or the composition of a system of mixed gases vary from one place to another, what is the proportion of particles, which, starting from one given place, arrive at another given place without a collision.

Let us suppose the gas to be a mixture of gases whose densities are ρ_1 ρ_2 &c, their velocities of agitation v_1 v_2 &c all these quantities being functions of x. Let s_1 be the distance of centres for collision between two particles of the first kind, s' for a particle of the first with one of the second.

Let λ_1 be the mean length of path which a particle of the first kind

† [29]

(26) O. E. Meyer, 'Ueber die Reibung der Flüssigkeiten', *Ann. Phys.*, **113** (1861): 55–86, 193–228, 383–425. See Maxwell's letter to Campbell of 21 April 1862 (see note (1)).

(27) Thomas Graham, 'A short account of experimental researches on the diffusion of gases through each other, and their separation by mechanical means', *Quarterly Journal of Science*, **28** (1829): 74–83. See Maxwell's letters to Stokes of 30 May 1859 and to Thomas Graham of 1 May 1865 (Number 248).

(28) Maxwell, 'Illustrations of the dynamical theory of gases. Part I': 32 (= *Scientific Papers*, **1**: 391); and see his letter to Stokes of 30 May 1859 (Volume I: 610).

(29) George Gabriel Stokes, 'On the effect of the internal friction of fluids on the motion of pendulums', *Trans. Camb. Phil. Soc.*, **9** part 2 (1851); [8]–[106], esp. 16–17. See Maxwell's letters to Stokes of 7 September 1858 and 30 May 1859 (Volume I: 597–8, 606–11).

(30) Read: l.

(31) On this value see Maxwell's discussion in his paper presented to the British Association in 1860 (see note (23)).

having a velocity v' differing slightly from v_1 would have if projected in a system for which ρ_1 ρ_2 v_1 v_2 are constant. We find by Prop IX[32]

$$\frac{1}{\lambda_1} = \pi s_1^2 N_1 \sqrt{1+\frac{v_1^2}{v'^2}} + \pi s'^2 N_2 \sqrt{1+\frac{v_2^2}{v'^2}}.$$

Putting $v_1 = v' + dv$ we get

$$\frac{1}{\lambda_1} = \sqrt{2}\,\pi s_1^2 N_1\left(1+\frac{1}{2}\frac{dv}{v_1}\right) + \pi s'^2 N_2 \sqrt{1+\frac{v_2^2}{v_1^2}}\left(1+\frac{v^2}{v_1^2+v_2^2}\frac{dv}{v_1}\right)$$

or
$$\frac{1}{\lambda_1} = A\rho_1\left(1+\frac{1}{2}\frac{dv}{v_1}\right) + B\rho_2\left(1+\frac{v_2^2}{v_1^2+v_2^2}\frac{dv}{v_1}\right).$$

λ_1 is a function of x, and when $v' = v_1$, $\lambda_1 = l_1$.

If r be measured in any direction and if particles are projected with velocity v' in this direction, then if u represent the number of particles wh: arrive at distance r, the proportion of these which will be stopped in the succeeding portion dr will be $\dfrac{dr}{\lambda_1}$ or in symbols

$$du = u\frac{dr}{\lambda_1}$$

or
$$u = N e^{-\int_0^r \frac{dr}{\lambda_1}}$$

if N is the whole number projected and u the number which reach a distance r. Since r must be small because it is the path of a particle we may write $r = n\Lambda$, where $\Lambda = \frac{1}{2}(l_1+\lambda_1)$ and then

$$u = N e^{-n}.$$

In all cases therefore, in which the properties of the gas vary from place to place, the number of particles which start from one place and pass through another, depends on the quantity Λ which is a mean distance between l_1 the length of path of the particles at the place where they started, and λ their length of path if they had been projected with their actual velocity at the other place.

By overlooking the differences between l_1 λ_1 & Λ, I have gone wrong

(32) Maxwell, 'Illustrations of the dynamical theory of gases. Part I': 26–7 (= *Scientific Papers*, **1**: 385–6); for two sets of particles 'to find the number of pairs which approach within a distance s in unit of time.' See also Prop. XI, 'In a mixture of particles of two different kinds, to find the mean free path of each particle.' Maxwell obtains $\dfrac{1}{l_1} = \sqrt{2}\pi s_1^2 N_1 + \pi\sqrt{1+\dfrac{v_2^2}{v_1^2}}\,s'^2 N_2$ where l_1 is the mean distance for a particle of the first kind; see 'Illustrations of the dynamical theory of gases. Part I': 28–9 (= *Scientific Papers*, **1**: 387–8).

in Props XIV & XVI of my former paper. I must therefore repeat the calculations of those propositions making the requisite corrections.

12 Prop XIV (corrected) In a system of particles whose density, velocity &c are functions of x, to find the quantity of matter transferred across the plane of yz due to the motion of agitation alone.(33)

If there is a motion of translation we must suppose the plane of yz to move with the same velocity so as to reduce the relative motion of translation to zero.

Let N = number of particles in unit of volume
M = mass of each particle
v = velocity of agitation at distance x
l = mean path of particle at distance x
v_0 = velocity of agitation at origin
λ = mean path of particle with velocity v among the particles at origin
$\Lambda = \frac{1}{2}(l+\lambda)$
q = quantity of matter transferred across unit of area in unit time.

Take a stratum whose thickness is dx and distance from origin x the area being unity. The number of collisions taking place in this stratum is

$$N\frac{v}{l}dx.$$

These particles will move in all directions with velocity v till they strike other particles and their lengths of path will be different in different directions, because the properties of the system are different in different places. We have only however to ascertain what proportion of these particles pass through the plane yz and we have shown that this depends only on the value of Λ.

We thus find for the number of particles projected from the stratum in unit of time whose paths are between $n\Lambda$ and $(n+dn)\Lambda$ and which pass through the plane yz in a positive direction

$$\frac{Nv(x\mp n\Lambda)}{2nl\Lambda}e^{-n}\,dx\,dn$$

where x must be between $\pm n\Lambda$ and the upper or lower sign is to be taken according to x is positive or negative.

(33) Maxwell, 'Illustrations of the dynamical theory of gases. Part II': 22 (= *Scientific Papers*, **1**: 393). There is a preliminary draft of the revise of this proposition in ULC Add. MSS 7655, V, c/45a.

We find by multiplying by M and integrating with respect to x from $x = -n\Lambda$ to $x = +n\Lambda$ and with respect to n from 0 to ∞

$$q = -\frac{1}{3}\frac{d}{dx}\left(\rho v \frac{\Lambda^2}{l}\right).$$

Since Λ is a mean between l & λ and these three quantities differ by very small quantities we may write

$$q = -\frac{1}{3}\frac{d}{dx}(\rho v \lambda)$$

as the most convenient expression for q. In differentiating λ we must remember that in the value of λ, ρ_1 ρ_2 v_1 v_2 are the values at the origin and therefore are to be regarded as constant, and that v' is the velocity at x, so that $v' = v_1 + \frac{dv_1}{dx}x$ and $dv = -\frac{dv_1}{dx}x$.

We find $$\frac{d\lambda_1}{dx} = l_1^2 \frac{1}{v_1}\frac{dv_1}{dx}\left(\frac{1}{2}A\rho_1 + \frac{v_2^2}{v_1^2+v_2^2}B\rho_2\right)$$

and $$q = -\frac{1}{3}\rho_1 v_1 l_1 \left\{\frac{1}{\rho_1}\frac{d\rho_1}{dx} + \frac{1}{v_1}\frac{dv_1}{dx}\left(1 + \frac{1}{2}Al_1\rho_1 + \frac{v_1^2}{v_1^2+v_2^2}Bl_1\rho_2\right)\right\}$$

which is the value of q for two mixed gases. For one gas, the expression becomes

$$q = -\frac{1}{3}\rho_1 v_1 l_1\left(\frac{1}{\rho}\frac{d\rho_1}{dx} + \frac{3}{2}\frac{1}{v_1}\frac{dv_1}{dx}\right).$$

This is the value to be employed in treating of the conduction of heat ⌞where $\rho = MN$ is the density v the mean velocity of agitation and l the mean length of path.

This result differs from that formerly obtained(34) as l does not come under the sign of differentiation.⌟(35)

13 Let us now apply similar corrections to Prop XVI.

(34) See note (35).

(35) These appended remarks are written on the verso of f. 5 of 'On the conduction of heat in gases'. Garber, Brush and Everitt, *Molecules and Gases*: 347, place these remarks at the end of the paper. But compare the conclusion of Prop. XIV of 'Illustrations of the dynamical theory of gases. Part II': 23 (= *Scientific Papers*, **1**: 394): 'We thus find for the quantity of matter transferred across unit of area by the motion of agitation in unit of time, $q = -\frac{1}{3}\frac{d}{dx}(\rho v l)$, where $\rho = MN$ is the density, v the mean velocity of agitation and l the mean length of path.'

Prop XVI (Corrected) To find the resultant dynamical effect of all the collisions which take place in a given stratum.[36]

We have to find the resultant momentum of all the particles which enter the stratum and strike there in unit of time.

Using the same symbols we have to find the momentum of particles which starting from a stratum dx lodge in a stratum whose thickness is α at the origin.

The number of particles projected from dx is as before

$$N\frac{v}{l}dx.$$

The proportion of these whose directions make an angle with x whose cosine lies between μ & $\mu+d\mu$ is

$$\frac{1}{2}d\mu.$$

The proportion of these which reach a distance $r = n\Lambda = \frac{1}{\mu}x$ is

$$e^{-n}.$$

The proportion of these which strike in the stratum α, that is between r and $r+dr$ where $\mu dr = \alpha$ is

$$\frac{\alpha}{\mu\lambda}.$$

The velocity of these particles resolved along the axes of x is $-v\mu$ and the resolved momentum is $-Mv\mu$.

Multiplying all these numbers together and remembering that $\frac{d\mu}{dn} = -\frac{x}{n^2\Lambda}$, we get for X the whole momentum

$$\alpha\rho X = \int_0^\alpha \int_{-n\Lambda}^{+n\Lambda} \frac{1}{2}\frac{\rho v^2\alpha}{l\lambda\Lambda n^2} x\, e^{-n}\, dx\, dn$$

$$= \frac{1}{3}\frac{d}{dx}\left(\frac{\rho v^2\Lambda^2}{l\lambda}\right)\alpha$$

and since Λ is a mean between l & λ this may be reduced to

$$\rho X = \frac{1}{3}\frac{d}{dx}(\rho v^2) = \frac{dp}{dx} \quad \text{by Prop XI.}^{(37)}$$

(36) Maxwell, 'Illustrations of the dynamical theory of gases. Part II': 23 (= *Scientific Papers*, **1**: 394).

(37) Read: Prop. XII of 'Illustrations of the dynamical theory of gases. Part I': 30

This result is the same as that which I obtained before, but the method of procedure is now rendered strict.

14 In applying these results to the case of the conduction of heat through a stratum of air from a hot surface to a cold one[38] we must introduce the conditions that the transfer shall be of heat only and not of matter, and that every intermediate slice of air shall be in equilibrium. In my former paper I paid little attention to this subject as I had no experimental data to compare with the theory, but the errors of principle into which I fell are worth correcting in order to compare the results of my method of calculation with those obtained by M. Clausius.[39]

The whole quantity of matter transferred across unit of area in unit of time is $Q = q + \rho V$ where q has the value given in Prop XIV and V is the velocity of translation. When there is no transfer of matter, $Q = 0$.

The resultant force of the collisions per unit of volume is

$$-\frac{dp}{dx}.$$

When there is no resultant force, p must be constant.

The quantity of energy in each particle depends partly on the velocity of its centre of gravity and partly on its velocity of rotation about that centre. As these velocities tend towards a constant ratio, we can assume that the energy of a particle is $\frac{1}{2}\beta M v^2$.[40]

The amount of energy which is transferred across unit of area in unit of time depends partly on the motion of translation, and partly on that of agitation. That depending on the motion of translation is $\frac{1}{2}\beta V\rho v^2$, and that depending on the motion of agitation may be found by the method of Prop XIV by substituting the energy instead of the mass of each

(= *Scientific Papers*, **1**: 389), where Maxwell obtains the result $p = \frac{1}{3}MNv^2$ for the pressure on unit area, where $MN = \rho$.

(38) Compare Prop. XXI of 'Illustrations of the dynamical theory of gases. Part II': 31–3 (= *Scientific Papers*, **1**: 403–5); see note (10).

(39) In his paper 'Ueber die Wärmeleitung gasförmiger Körper': 47–8n Clausius pointed out that Maxwell had derived his expression for the conduction of heat in gases (see note (41)) from his expression for the quantity of matter transferred by the motion of agitation of a gas (see note (35)); and that Maxwell therefore implied that a flow of heat is accompanied by and is partly the result of the motion of the gas molecules. Clausius comments: 'Sie steht also mit der Voraussetzung, welche wir machen müssen, wenn wir von Wärmeleitung sprechen, im Widerspruche, denn unter Wärmeleitung versteht man eine Fortbewegung der Wärme *ohne Fortbewegung der Masse*.'

(40) See note (20).

particle. The whole energy transferred being called E we find

$$E = \frac{1}{2}\beta V\rho v^2 - \frac{1}{3}\frac{d}{dx}\left(\frac{1}{2}\beta\rho v^3\lambda\right)$$

with the conditions

$$p = \text{constant} = \frac{1}{3}\rho v^2.$$

$$Q = V\rho - \frac{1}{3}\frac{d}{dx}(\rho v\lambda) = 0.$$

We find

$$E = -\frac{1}{12}\beta\rho l\frac{1}{v}\frac{dv}{dx}(v^2 . v + 3v^3)^{(41)}$$

where the mean values of v, v^2 and v^3 must be taken as shown in section 2 so that $v^2 . v + 3v^3 = 5v^2 . v$.

If q is the quantity of heat transferred in unit of time measured in ordinary units, $E = Jgq$(42) where J is the mechanical equivalent of heat.(43) If T be the absolute temperature

$$\frac{1}{v}\frac{dv}{dx} = \frac{1}{2}\frac{1}{T}\frac{dT}{dx}^{(44)}$$

$$\rho v^2 = 3p$$

$$v = \sqrt{\frac{8p_0 T}{\pi\rho_0 T_0}}.^{(45)}$$

(41) See '(12) Prop. XIV (corrected)' above. Compare Prop. XXI of 'Illustrations of the dynamical theory of gases. Part II': 32 (= *Scientific Papers*, **1**: 404) where he obtains the result, criticised by Clausius (see note (39)) $E = -\frac{1}{2}\frac{\beta v^2}{A}\frac{dv}{dx}$, where $A = \frac{1}{l\rho}$.

(42) g is the acceleration due to gravity.

(43) Following William Thomson, 'On the dynamical theory of heat, with numerical results deduced from Mr Joule's equivalent of a thermal unit, and M. Regnault's observations on steam', *Trans. Roy. Soc. Edinb.*, **20** (1851): 261–88, esp. 269 (= *Math. & Phys. Papers*, **1**: 186); 'J denote[s] the mechanical equivalent of a unit of heat'. Compare also Rankine, *Manual of the Steam Engine*: 299; 'The quantity above stated, 772 foot-pounds for each British thermal unit, is commonly called "*Joule's equivalent*", and denoted by the symbol J, in honour of Mr. Joule, who was the first to determine its value *exactly*. His... best set of experiments, from which the accepted value 772 is deduced, may be consulted in the *Philosophical Transactions* for 1850.'

(44) Maxwell equates the velocity gradient with the temperature gradient; see also Rankine, *Manual of the Steam Engine*: 258.

(45) See paras. (2), (5) above.

If l_0 be the value of l where $\rho = \rho_0$, then $l = l_0 \frac{\rho_0}{\rho}$ and E becomes

$$\mathcal{J}gq = -\frac{5}{4}\sqrt{\frac{2}{\pi}}\beta\, l_0\, p_0^{\frac{3}{2}}\, T_0^{-\frac{3}{2}} \rho_0^{-\frac{1}{2}}\, T^{\frac{1}{2}} \frac{dT}{dx}.$$ (46)

(46) Compare Maxwell, 'Illustrations of the dynamical theory of gases. Part II': 32, where he obtains the expression $\mathcal{J}gq = -\frac{3}{4}\beta plv\frac{1}{T}\frac{dT}{dx}$ (there is a misprint in *Scientific Papers*, **1**: 404).

LETTER TO JOHN WILLIAM CUNNINGHAM

24 MARCH 1863

From the original in the King's College London Archives[1]

8 Palace Gardens Terrace
24 March 1863

Dear Sir

I am in receipt of your letter of the 23rd. I think it is very right that Mr Smalleys remuneration should be increased,[2] and I am quite willing to agree to the arrangements for that purpose as stated in your letter.

I remain
Yours truly
J. CLERK MAXWELL

J. W. Cunningham Esqre
Secretary of Kings College
London

Turn Over[3]

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(1) King's College London Archives, KA/IC/M 71.

(2) On his appointment on 11 October 1861 (see Number 204 note (3)) Smalley's salary as Lecturer in Natural Philosophy had been fixed at a fee of 7s. per pupil per term, this sum to be deducted from the Professor's salary (King's College London Archives, King's College Council Vol. I, minute 42).

(3) The *verso* is blank.

LETTER TO GEORGE BIDDELL AIRY

14 MAY 1863

From the original in the Royal Greenwich Observatory Archive(1)

8 Palace Gardens Terrace
W
1863 May 14

Dear Sir

M^r G. R. Smalley has for two sessions given lectures on Mechanics to one division of the Department of Applied Sciences while I have lectured the men in the other division on the same subject, and have examined both divisions.

I believe M^r Smalley to possess the scientific knowledge and the habits of accuracy which would fit him for the work of an Observatory.

He has already had some experience as an observer at the Cape, and he is now desirous to obtain the situation which is vacant in the Observatory at Sydney.

From what I know of M^r Smalley during my intercourse with him at King's College, I consider that he would be steady, accurate and skilful in Observatory work.(2)

I remain
Yours truly
J. Clerk Maxwell

The Astronomer Royal

(1) Royal Greenwich Observatory Archive, ULC, Airy Papers 6/148, 318R–V.

(2) Smalley's resignation was accepted at a meeting of the King's College Council on 21 July 1863, on his appointment as Astronomer Royal for New South Wales (King's College London Archives, King's College Council Vol. I, minute 267).

LETTER TO WILLIAM THOMSON

29 MAY 1863

From the original in the University Library, Cambridge(1)

8 Palace Gardens Terrace
W
1863 May 29

Dear Thomson

Can you dine with us on Tuesday 9th June?

On Wednesday 27th we made three determinations which I have now reduced. I give you the details of the 1st so that you may see our present method.(2)

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(1) ULC Add. MSS 7342, M 101. First published in Larmor, 'Origins': 734–6.

(2) Maxwell here begins to report measurements of electrical resistance in electromagnetic absolute units which he undertook with Balfour Stewart and Fleeming Jenkin, 'according to the method devised by Professor W. Thomson', as stated in the 'Report of the Committee appointed by the British Association on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association for the Advancement of Science; held at Newcastle-upon-Tyne in August and September 1863* (London, 1864): 111–76, on 111. See the 'Description of an experimental measurement of electrical resistance, made at King's College' (by Maxwell, Balfour Stewart and Fleeming Jenkin), *ibid.*: 163–76. The apparatus and method is described in the following terms: 'Professor W. Thomson has designed an apparatus by which the resistance of a coil can be determined in electromagnetic measure by the observation of the constant deflection of a magnet, and his arrangement has been adopted for the experiments made by the Committee.... For convenience of description, the apparatus with which the experiments were made may be divided into five parts: 1°, the driving gear; 2°, the revolving coil; 3°, the governor; 4°, the scale, with its telescope, by which the deflections of the magnet were observed; 5°, the electric balance, by which the resistance of the copper coil was compared with a German-silver arbitrary standard.... The apparatus consisted of two circular coils of copper wire, about one foot in diameter, placed side by side, and connected in series; these coils revolved round a vertical axis, and were driven by a belt from a hand-winch, fitted with Huyghens' gear to produce a sensibly constant driving-power. A small magnet, with a mirror attached, was hung in the centre of the two coils, and the deflections of this magnet were read by a telescope from the reflection of a scale in the mirror. A frictional governor controlled the speed of the revolving coil.... By calculation it can be shown that when the coil revolves round a vertical axis... $R = L^2V/k^2 \tan d$, an equation from which the earth's magnetic force and the moment of the suspended magnet have been eliminated, and by which the absolute resistance (R) can be calculated in terms of the length, L, the velocity, V, the radius, k, and the deflection, d. The resistance thus calculated is expressed in electromagnetic absolute units, because [this] equation... is a simple consequence of... fundamental equations in the electromagnetic system. The essence of Professor Thomson's method consists in substituting, by aid of the laws of electromagnetic induction, the measurement of a velocity and a deflection for the more complex and therefore less accurate measurements of work and force, required in the simple fundamental equations.' (*ibid.*: 163–4, 118–19). The

(A) Determination of zero. Coil rotating + no current. 40 observations of extremities of oscillations

Mean of 1st ten (millimeters)	949.56
2nd—	950.55
3rd—	950.73
4th—	950.52
Mean	950.34

(B) Connexion made. Coil rotating + and 80 oscillations taken

Mean reading of 1st ten	1237.38
2nd	8.33
&c	9.65
	9.86
	8.25
	8.40
	9.35
	7.80
Mean Scale reading.	1238.6275

Time of 3000 revolutions		
0	to 3000	530.0
300	3300	530.0
600	3600	530.0
900	3900	530.0
Time of 100 rev.	$T=$	17.67 (3)

(C) Circuit broken rotation + zero reading again

	951.30
	50.87
	50.00
After exp. Mean	950.723
Mean zero before and after	950.504

apparatus is described in the 'Report': 163–8, and in a series of drawings, Plate VI, *ibid.*: facing 176. See also I. B. Hopley, 'Maxwell's work on electrical resistance. I. The determination of the absolute unit of resistance', *Annals of Science*, **13** (1857): 265–72.

(3) 'The speed of the coil was determined by observing on a chronometer the instant at which a small gong was struck by a detent released once in every hundred revolutions'; see 'Report': 120, and number 216, esp. note (5).

Mean reading due to current	1238.627
deflexion $= \delta_1$	288.123
Time of 100 revolutions $= T_1$	17.67
Product $T_1 \delta_1 =$	5091.13

The second exp. went wrong in the spinning.
The third experiment

	zero	957.00
negative rotation	scale reading	668.936
deflexion negative		288.064
Time of 100 revolutions		17.6284
Product $T\delta$		5078.11

Fourth Exp.	rotation + Scale	1247.11
	zero	957.00
	deflexion	290.11
	Time of 100 rev$^{\text{ns}}$	17.57
	Product $T\delta$	5097.1

Jenkin has the coils which he equated to the spinning coil between these exp$^{\text{s}}$.(4)

There was a change of zero which we must try to avoid, but I know when it took place.

Now for the measure of resistance(5)

length of wire	303.403 meters
number of turns	308
Distance of scale from mirror	3000 millimeters

(4) The 'resistance of the copper coil was compared with a German-silver arbitrary standard'; see 'Report': 164 and note (2). See also Number 214 notes (14) and (18).

(5) In the published 'Report' Maxwell (see 'Report': 163) wrote out an account of the 'Mathematical theory of the experiment' ('Report': 168–71), deriving an expression for the value of the resistance of the rotating copper coil (in electromagnetic units):

$$R = \frac{200\pi^2 Dnl \sin^3 \alpha}{T\delta}\{1 + \text{corrections}\},$$

where D is the distance of the scale from the mirror, n the number of windings of the coil, l the length of wire in metres, α the angle subtended at the axis by the radius of the coil, T the time of 100 revolutions of the coil in seconds, and δ the scale-reading deflection in mm.

(equal to a certain wooden rod which I shall measure again)
α = angle subtended by radius of coil at magnet 83° 11′
$\sin^3\alpha = .978912.$

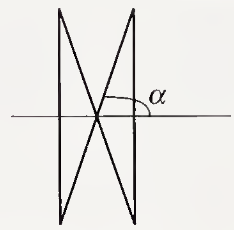

Figure 210,1

Corrections for thickness &c of coil .9996116[6]

$$R[=]\frac{200\pi^2 \times 3000 \times 303.403 \times 308 \times .978912 \times .9996116}{T\delta}\text{[7]}$$

$$+\text{correction.}$$

Correction for induction of magnet at centre	$+.00720$
for torsion of fibre	$-.00135$
Proportional correction	$+.00583.$
Correction for reduction of $\tan 2\phi$ to $\tan\phi$	$= 1.000\,\dfrac{\delta^2}{4D^2}$ [8]
for induction of coil on itself	$= -.7331\,\dfrac{\delta^2}{4D^2}$ [9]
total	$0.2669\,\dfrac{\delta^2}{4D^2}$

Final formula for R

$$R = \frac{544648{,}000{,}000}{T\delta} + 4037\,\frac{\delta}{T}.\text{[10]}$$

I have not divided out as I have no time.[11]
Here are former values of $T\delta$ on a colder day

5176
5105.

(6) The published 'Report' included 'an elaborate analysis of the corrections required ...made by Professor Maxwell' ('Report': 120); and see 'Report': 171–6.
(7) See note (5).
(8) ϕ is the angle between the axis of the magnet and the magnetic meridian, and $\tan 2\phi = \delta/D$; see 'Report': 170, 172, and note (5).
(9) Compare 'Report': 172.
(10) Compare the corrected value in the published 'Report': 173.
(11) But see Number 211.

Jenkin cannot come on Wednesday but I think Thursday will do for an experiment as Stewart, Jenkin & I can repeat what we have done and if you are engaged Thursday it does not so much matter as you could do us more good by advice when we are not spinning than when we are spinning. If you can come here[(12)] on the 9th I will ask Jenkin & Stewart.

Yours truly
J. C. Maxwell

(12) The experiments were made at King's College, London; see 'Report': 116, and Numbers 213 and 216.

LETTER TO WILLIAM THOMSON

JUNE 1863[1]

From the original in the University Library, Glasgow[2]

Dear Thomson

We had a spin yesterday with the following results.

Time (mean of series)	Scale readings	Time of 100 revolutions	
$1^h\ 36^m$	1016.40	∞ unknown no connexion	Zero
$1^h\ 48^m$	1306.09	17.3296	+
$2^h\ 40^m\ 50^s$	730.565	17.4625	−
3 21.30	1339.02	15.6715	+
3. 35^m	1020.325	no connexion	Zero

Values of $T\delta$[3]

from	1st & 2nd		5020.21
	2	3	5005.86
	3	4	5025.41
	4	5	4994.43

These are not good but if you observe 1st and 5th observations the zero has changed owing to variation of terrestrial magnetism.[4] I have not got the variations at each instant from Stewart[5] yet but if we suppose that there was

(1) See Number 213.

(2) Glasgow University Library, Kelvin Papers, M 13.

(3) See Number 210 esp. note (5).

(4) In the published 'Report of the Committee... on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association* (London, 1864): 171, Maxwell (see Number 210 note (6)) noted that: 'since the direction of the earth's magnetic action is continually varying, we must find the difference of *declination* between the times of the two readings, and calculate what would have been the undisturbed reading of the scale at the time when the deviation was observed.'

(5) In the published 'Report': 171, Maxwell explained that: 'In our experiments this correction was made by comparison with the photographic registers of magnetic declination made at Kew at the same time that our experiments were going on.' Balfour Stewart was Director of the Kew Observatory.

an increase of 3.925 in 2 hours and that the changes at intermediate times were in proportion to the time then we get corrections which being applied give our four values.

	5011.7
The last is doubtful owing to	5015.2
not knowing the mean time of	5017.1
zero reading exactly.	5004.5

So you see our principal desideratum is a knowledge of the variation of declination for when it is roughly applied it takes away most of the discrepancies. Stewart can give the Kew observations of corresponding times, for better corrections.(6)

By a new observation of the power of the small magnet

$$R = \frac{54512}{T\delta} \qquad +$$

$$= 108665000 + 64000$$

$$= 108729000 \frac{\text{meter}}{\text{second}}^{(7)} \qquad 4^{\text{th}} \text{ June.}$$

Value of R on 27th May 107,365,000.(8) The difference arises from change of temperature as will be shown by Jenkins observations and coils.(9)

The temperature of the coil increases by spinning owing to the induced currents.

If horizontal terrestrial force = 3.8 British and we go at 300 turns in 50 seconds I find that in 5000 turns (which is about the number in each series) the electrical work done by overcoming resistance is about 7.3 footpounds or about $\frac{1}{100}$ of British thermal unit. I do not know the weight of the coil or the capacity of copper but it all comes out easy now.

Jenkin can now compare numerically the resistance on different days(10) so we shall be able to work the observations into each other to get a mean.

If you come a little before 7 on Wednesday I shall have the mixture of colours in action.(11)

Yours truly
J. C. Maxwell

(6) See notes (4) and (5), and 'Report': 175.

(7) On the dimensions of resistance in electromagnetic units as $[LT^{-1}]$ see J. Clerk Maxwell and Fleeming Jenkin, 'On the elementary relations between electrical measurements' in the 'Report': 130–63, esp. 145, 159.

(8) See Number 210, where Maxwell reports measurements made on 27 May 1863.

(9) See Number 210 esp. note (4).

(10) See Number 210 note (4).

(11) See Number 202.

LETTER TO GEORGE GABRIEL STOKES

9 JUNE 1863

From the original in the University Library, Cambridge[1]

8 Palace Gardens Terrace
9 June 1863

Dear Stokes

I have received your letter and that of the Astronomer Royal.[2] Perhaps I ought to have explained more distinctly what I meant by the conditions arising from elasticity.[3]

There are three separate subjects of investigation in the theory of Elastic Solids.

1st Theory of Internal Forces or Stresses their resolution and composition and the conditions of equilibrium of an element.

2nd Theory of Displacements or Strains their resolution and composition and the equation of continuity (if required).

3rd Theory of Elasticity or the relations between systems of stresses and systems of strains in particular substances.[4]

Mr Airys conclusions are all deducible from the conditions of equilibrium of the *Forces* or *Stresses*[5] for although he has introduced into his calculation considerations arising from the observed uniformly varying strain and stress between the top & bottom of the beam (see top of p 3 of his letter and his paper art 15)[6] yet I have shown that on his own principles these assumptions

(1) ULC Add. MSS 7656, M 421.

(2) Airy's letter is his letter to Stokes of 22 February 1863 (Royal Society, *Referees' Reports*, **5**: 4; see Number 205 notes (12) and (21)). In a letter to Stokes of 27 February 1863 (copy in Royal Greenwich Observatory Archive, ULC, Airy Papers 6/392, 130R–131V) Airy wrote: 'Pray send my letter for Prof. Maxwell's reading, if you think there is nothing in it at which he can take the slightest umbrage.' In his reply dated 18 March 1863 (Airy Papers 6/392, 131A–132A) Stokes told Airy that 'I have not yet written to Prof.r Maxwell about your paper, because there was no hurry about it'.

(3) In his report (Number 205) on Airy's paper 'On the strains in the interior of beams', *Phil. Trans.*, **153** (1863): 49–79.

(4) On Maxwell's distinction, following Rankine and William Thomson, between 'stress' and 'strain' see Number 206, esp. note (7).

(5) See Numbers 205 and 206.

(6) See Number 205 note (21) for the passages in Airy's letter to Stokes, and in the paper 'On the strains in the interior of beams', to which Maxwell refers. Article (15) here referred to was subsequently re-numbered (16) in the manuscript of Airy's paper (Royal Society, PT. 68.3, on f. 9), a new §12 as in the printed text being inserted; see Airy, 'On the strains in the interior of beams': 55, 58.

are not required, for the results may be got from the conditions near the end of my report namely that the pressure all over the surface is zero. Now we know this without any theory of elasticity and any application of elastic principles which tells us no more than this may in a mathematical paper be treated as an episode an illustration or instructive consideration but not a necessary part of the investigation ⟨just as many mechanical experiments help us to see the truth of principles which we can establish otherwise⟩.

What I meant by the conditions arising from the elasticity of the beam may perhaps be more accurately described as 'Conditions arising from the beam having been once an unstrained solid free from stress'.

That is, the stresses must be accounted for by displacements of an elastic solid from a state in which there were no forces in action.

I think what you and the author intend is that I should state the result of the above assumption instead of that of the paper.(7) The mode of getting complete solutions I have only partially worked out. It depends on expanding the applied forces in Fouriers series the terms are of the form $A \sin (nx+b)e^{\pm ny}$. I shall send you the note or appendix when I can write it I hope before Thursday.(8)

I have not been out of town but have been busy with the standard of Electric resistance. We have now got no errors comparable with those produced by the change of magnetic declination during the experiment. These we shall shortly be able to eliminate by comparison with the photographic records at Kew.(9)

I have been studying oscillations of magnets by aid of mirrors(10) and I hope to apply the principle to the determination of gaseous friction by means of a disc oscillating in a gas and determining the log. decrement of oscillation and the time of oscillation.(11)

Yours truly
J. Clerk Maxwell

(7) In his letter to Stokes of 22 February 1863 (see note (2)) Airy concluded with the suggestion: 'If Professor Maxwell on further consideration should see reason for making other remarks, I shall be delighted to see them in the form of Appendix to the paper, if approved by the President and Council of the Royal Society.'

(8) Airy's paper as published does not contain an appendix by Maxwell. The paper had reached revise proofs by June 1863 (see Airy Papers 6/392, 145R).

(9) See Number 211 esp. notes (4) and (5).

(10) Suggested by the method used in the apparatus for measuring electrical resistance: see Number 210 note (2).

(11) See Numbers 244, 245, 246 and 252. A sheet of data among Maxwell's manuscripts (ULC Add. MSS 7655, V, f/4) records experiments on gas viscosity dated '6 Nov 1863'.

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LETTER TO JOHN WILLIAM CUNNINGHAM

27 JUNE 1863

From the original in the King's College London Archives(1)

8 Palace Gardens Terrace
W
27 June 1863

Dear Sir

We have now completed the first series of experiments(2) in the room down stairs. We may however require to repeat them in October. Can we have the stone &c left there till then? and can M^{r} Jenkin be allowed to go there occasionally during the vacation to work by himself? If you have any communication to make to him address

Fleeming Jenkin Esq^{re}
6 Duke Street
Adelphi W.C.

I shall be abroad till the end of July after which at Glenlair, Dalbeattie NB.

We did not find it necessary to make any experiments at night. We expect to be able to deduce excellent results from those made in the day time.

I remain
Yours truly
J. CLERK MAXWELL

J. W. Cunningham Esq^{re}

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(1) King's College London Archives, KA/IC/M 71.

(2) The British Association experiments on standards of electrical resistance were carried out at King's College: see Numbers 210 note (12), 214 esp. note (6), and 216.

LETTER TO WILLIAM THOMSON

31 JULY 1863

From the original in the University Library, Glasgow(1)

Address till 1st October
(Not known in Wigtonshire)

Glenlair
Dalbeattie
1863 July 31

Dear Thomson

I cannot tell you much about the action of the tides on the earth.(2) I have the old calculation at London somewhere. Date probably 1853. I have never printed on that subject. See however papers by Daniel Vaughan in Phil Mag 1862.(3) I know no old papers about it. People were always incredulous and told you that there was no such thing on account of Thales and his eclipse.(4) But the whole effect possible would not do any harm to any eclipse so recent as Thales.

Jenkin & I have finished up the 1st set of exp. We measured everything with a box wood meter of Becker's.(5) The wire was gently uncoiled and laid in a groove between two planks of the Museum floor 50 feet at a time and so measured straight but not stretched. Jenkin managed by this wrinkle to straighten the wire.(6)

I have been in Germany for a month and having borrowed from the schoolmaster Log. Tables I worked out the self-induction of the coil with accuracy.(7) For experiments on self-induction kicks with the galvanometer we must have a needle for which the 'deadening' due to all causes is small and of which the time of oscilln is long enough to take good observations of the

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(1) Glasgow University Library, Kelvin Papers, M 14. Previously published in A. T. Fuller, 'James Clerk Maxwell's Glasgow manuscripts: extracts relating to control and stability', *International Journal of Control*, **43** (1986): 1595–7.

(2) See Thomson's paper 'On the rigidity of the earth', *Phil. Trans.*, **153** (1863): 573–82 (= *Math. & Phys. Papers*, **3**: 312–36), and his letters to Stokes of 19 April 1862 and 8 July 1862 (ULC Add. MSS 7656, K 137, K 138; printed in Wilson, *Stokes–Kelvin Correspondence*, **1**: 292–3, 295).

(3) Daniel Vaughan, 'On the form of satellites revolving at small distances from their primaries', *Phil. Mag.*, ser. 4, **20** (1860): 409–18; Vaughan, 'Static and dynamic stability in the secondary systems', *ibid.*, **22** (1861): 489–97.

(4) On the story of Thales' alleged prediction of a solar eclipse see O. Neugebauer, *The Exact Sciences in Antiquity* (Providence, $_2$1957): 142–3.

(5) Carl Ludwig Christian Becker, of Elliott Bros. (Boase).

(6) Compare the 'Report of the Committee... on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association* (London, 1864): 111–76, esp. 120, on the measurement of the length of the copper wire in the Museum of King's College.

(7) See Number 210, esp. note (9).

ends of each swing. If the observer were to have command of the make & break key I think very good numerical results might be got by exp.

I have now got the whole theory of the inductive coefficient of one circle placed near and parallel to another, the distance between the circles being small compared with the radii.[8]

I take the electrotonic coeffts F, G, H[9] and express the equations of no electric current[10]

$$\frac{d^2F}{dx^2}+\frac{d^2F}{dy^2}+\frac{d^2F}{dz^2}=0 \quad \&c$$

first in cylindrical coordinates and then in annular coordinates (that is distance along a ring, distance from it, and angle between that distance and radius of ring).

In the case of a circular current let N be the electrotonic 'force' parallel to the ring at dist r from the ring[11]

$$\frac{d^2N}{dr^2}+\frac{1}{r}\frac{dN}{dr}+\frac{1}{r^2}\frac{d^2N}{d\theta^2}+\frac{1}{a+r\cos\theta}\left(\frac{dN}{dr}\cos\theta-\frac{dN}{d\theta}\sin\frac{\theta}{r}\right)-\frac{N}{(a+r\cos\theta)^2}=0.\text{[12]}$$

By this eq^n and by the principle that the inductive coeff of A on B is equal to that of B on A I get

$$N=\log\frac{r}{8a}\left\{1-\frac{1}{2}\frac{r\cos\theta}{a}+\frac{3}{16}\frac{r^2}{a^2}(1+2\cos^2\theta)-\frac{1}{32}\frac{r^3\cos\theta}{a^3}(9+5\cos^2\theta)+\&c\right\}$$

$$+2-\frac{1}{2}\frac{r\cos\theta}{a}+\frac{1}{32}\frac{r^2}{a^2}(11+6\cos^2\theta)-\frac{1}{32}\frac{r^3\cos\theta}{a^3}\left(\frac{39}{2}-\frac{5}{3}\cos^2\theta\right)\&c.$$

The inductive potential between two wires whose distance is r and radii a and $a+r\cos\theta$ respectively is

$$-2\pi a\gamma\gamma' N$$

where $\gamma\gamma'$ are the currents and N the quantity found above.

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(8) Compare 'Part VII. Calculation of the coefficients of electromagnetic induction' of Maxwell's paper 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 506–12 (= *Scientific Papers*, **1**: 589–97).

(9) On Maxwell's development of Faraday's concept of the electro-tonic state see his papers 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83, esp. 63–7, and 'On physical lines of force. Part II', *Phil. Mag.*, ser. 4, **21** (1861): 281–91, 338–48, esp. 289–91, 338–42 (= *Scientific Papers*, **1**: 203–9, 475–82). See Volume I: 371–5, 406–9, 688).

(10) Compare Maxwell, 'A dynamical theory of the electromagnetic field': 506 (= *Scientific Papers*, **1**: 589).

(11) Compare Maxwell, 'A dynamical theory of the electromagnetic field': 507–8 (= *Scientific Papers*, **1**: 591–3).

(12) a is the radius of the coil, θ the angle between r and the plane of the coil.

By integration I find the coefft of	
A on B and B on A to be	104153.295 meters
A on A and B on B	293521.736
Correction for want of homogeneity and for return currents in wire	+75.5
Total	397750 meters
By experiment June 16	398500 meters.(13)

I do not think much of the experiment as we could not determine well the time of oscillation owing to rapid diminution of swing.


Figure 214,1. Section of coil.

Here are the results of the last 3 days after we got good scales and cleared away iron.

They are corrected for variation of magnetic declinations for change of resistance observed directly and for scale errors.

T = time of 100 revolutions
δ = deflexion in millimeters
R = resistance Jenkins German silver(14) being unity.(15)
$T\delta R$ should be const.(16)

	$T\delta R$
June 16	5046.18
19	5075.77
23	5037.98
Mean	5053.32(17)

Resistance of Jenkins German Silver

$$R = 107620116 \frac{\text{meters}}{\text{second}}^{(18)}$$

(13) These are the values recorded in the 'Report of the Committee...on standards of electrical resistance': 172.

(14) German silver: an alloy of nickel, copper and zinc; see Henry Watts, *A Dictionary of Chemistry*, 5 vols. (London, 1863–9), 2: 51.

(15) See Number 210 esp. note (4).

(16) See the values for $T\delta$ reported in Numbers 210 and 211.

(17) These are the values recorded in the 'Report': 175.

(18) See the 'Report': 176; for the 'coil of German silver, marked June 4th...we find as the result of the experiments for the resistance of "June 4" in absolute measure 107620116 metres per second. Knowing the absolute resistance of "June 4" we may construct coils of given resistance by known measure'.

I think we might do better still.

Have you written any description of your Governor with a spring?(19) I have been doing the theory of Jenkins friction gov.(20) and what I suppose to be yours. They have both a diff^{l} eq^{n} of 4th order, reducible to 3rd by integration. Of the 3 solutions one is of the form e^{-nt} and the other two, $e^{\pm n't}\cos mt$. The condition is that n' in the last expression shall be negative. In Jenkins gov this must be got by putting a fan on the axle of the screw. In yours, by impeding by some kind of fluid friction the *radial* motion of the bob. I have a plan with mercury. Jenkins is the easiest to fulfil conditions. Yours is independent of the coefft of friction. Both yours and mine depend on a spring.(21)

Yours truly J. C. M.

(19) For further discussion see Maxwell's letter to William Thomson of 11 September 1863 (Number 219).

(20) The governor used in the experiments on standards of electrical resistance; see 'Report': 120, 166; and Number 219 esp. note (8).

(21) The governors Maxwell labels T and TJ, respectively, in his letter to Thomson of 11 September 1863; see Number 219 notes (7) and (9).

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LETTER TO JOHN WILLIAM CUNNINGHAM

10 AUGUST 1863

From the original in the King's College London Archives(1)

Glenlair
Dalbeattie
1863 Aug 10

Dear Sir

I enclose £10 for Kings College Hospital.

I shall be here till the end of the vacation. I think that is 2nd October.

Yours truly
J. CLERK MAXWELL

J. W. Cunningham Esqre
King's College
London WC.

(1) King's College London Archives, KA/IC/M 71.

LETTER TO ROBERT DUNDAS CAY[1]

21 AUGUST 1863

From the original in the Library of Peterhouse, Cambridge[2]

Glenlair
21 August 1863

Dear Uncle

I enclose Receipt for £5..2. We are very glad to hear of Charlies[3] success. It is very well to read at this time but he must keep himself cool in January. How are Aunt Jane & Uncle Albert? Johnnie was in good spirits when we saw him last. We have just been dining the Presbytery of Kircudbright who ordained M^r Sturrock minister of Corsock yesterday.[4] The Corsock district is now a parish quoad sacra and the congregation fills the church. I am finishing a report on electrical measurements for the British Asses.[5] I do not mean to go to Newcastle but the only place where the report would be intelligible at present is in the basement story of Kings College where all our instruments are set up.

We are to have four men to work there together in autumn, one man (the Secretary) to turn the driving wheel, another (the Astronomer) to take the time of a bell ringing every 100 turns[6] another (myself) to look at a scale through a telescope and a fourth to look at another scale through another telescope. We have to go on turning steadily taking times and viewing scales for $\frac{1}{2}$ hour then stop & reverse and so on.

Your aff^t nephew
J. Clerk Maxwell

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(1) Maxwell's uncle; see Volume I: 682.

(2) Peterhouse, Maxwell MSS (23).

(3) Charles Hope Cay: see Number 240.

(4) Rev. Geo. Sturrock, *Corsock Parish Church: Its Rise and Progress &c* (Castle-Douglas, 1899). See also *Life of Maxwell*: 329 on the endowment of the church.

(5) 'Report of the Committee...on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association for the Advancement of Science; held at Newcastle-upon-Tyne in August and September 1863* (London, 1864): 111–76. See Numbers 210, 211 and 214.

(6) See Number 210 note (3) and 'Report': 120; 'Mr. Balfour Stewart's skill in this kind of observation enabled...great accuracy' to be achieved.

LETTER TO GEORGE PHILLIPS BOND(1)

25 AUGUST 1863

From the original in the Harvard University Archives(2)

Glenlair House
Dalbeattie
Scotland
1863 Aug 25

Dear Sir

When your letter(3) arrived I had just gone abroad, and my letters were not forwarded to me so that I was not aware of your kindness in sending me such valuable books.(4) I hope that my being out of town has not put either you or Mr Parker(5) to inconvenience. I have asked Mr Parker if he has not already sent them to my house, to keep them till I return to London in October.

I shall study what you say about Saturn in your letter(6) when I see your

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(1) See notes (2) and (7).

(2) Bond MSS UAV. 630.6, Harvard University Archives, Pusey Library, Harvard University. Published in part in Edward S. Holden, *Memorials of William Cranch Bond, Director of the Harvard College Observatory 1840–1859, and of his Son George Phillips Bond, Director of the Harvard College Observatory 1859–1865* (San Francisco/New York City, 1897): 203–6.

(3) G. P. Bond to Maxwell, 9 July 1863 (holograph copy in Bond MSS UAV. 630.6). The letter is addressed from the 'Observatory of Harvard College, Cambridge Mass.'

(4) On Bond's acquaintance with Maxwell (whom he had met in May 1863) see note (7). In his letter of 9 July 1863 Bond wrote: 'I shall have the pleasure of forwarding to you, shortly, through Henry Tooke Parker Esq. 3 Ladbroke Gardens, Notting Hill, London a package containing: Annals of the Astronomical Observatory of Harvard College Vol. I (Parts I–II)/Vol. II Part I/III/IV Part I/ Observatory Reports for 1862 & 1863 and a collection of Memoirs and shorter articles. / By some accident your name was not inserted in its proper place on our list for distribution, & I am sorry on this account not to be able to furnish you with a full collection of our publications of many of which we have no copies remaining.'

(5) See note (4).

(6) Bond had written: 'Of one very remarkable fact, however, I am perfectly convinced. It is that during the time of the so-called disappearance of the ring, the bright protuberances seen upon the edge, like little satellites, have no sensible motion of rotation about the ball. / Among other curious subjects, the peculiar configuration of the shadow of the ball on the ring, & its visibility on *both* sides of the ball at the same time, are particularly noticeable. / Perhaps the suddenness of the illumination of the ring as its reappearance in Sept. 1848, may throw some light on the nature of the surface of the ring, as to its reflective quality, when it is considered at what a very small angle of elevation the sun was then shining upon it. Between the 4th & 13th of Sept. the breadth of the ring did not exceed 0″.25; still the amount of light reflected was such as to make it visible in a telescope of only 3in aperture. / I have never seen upon the ring any object by which the time of its rotation could be inferred. The protuberances at the "disappearance" are sufficiently prominent, *but they do not move*. I am aware that Sir William Herschel has derived a

drawings and observations.[7] I have no doubt that the time is coming when we shall know more about the heavenly bodies than that they attract each other from a distance.

In Saturns Rings we certainly have a very wonderful object to examine and when we come to understand it we shall certainly know more mechanics than we do now.

Your observations of comets' tails[8] go far to render them legitimate subjects of speculation and I think that when we have mastered the theory of these tails we shall know more about what the heavens are made of.[9]

I think the heavenly spaces are by no means empty since, as Thomson has

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time of rotation from them, but after considering all the data which he has collected, the telescopes which he used, & all the circumstances of the observations, I cannot think that the evidence is sufficient to outweigh the numerous observations made with our great refractor under very favourable circumstances. The latter, moreover, agree as to the main question, the immobility of the bright points, Schröters observations at Lilienthal, which were numerous & were made with large telescopes.' On William Herschel's observation of the period of rotation of Saturn's ring, see his paper 'On the satellites of the planet Saturn, and the rotation of its ring on an axis', *Phil. Trans.*, **80** (1790): 427–95, esp. 479; and see volume I: 443n. See also J. H. Schröter, 'Nachricht von merkwürdigen Beobachtungen über den Ring des Saturns', *Astronomisches Jahrbuch* (1806): 159–64.

(7) See W. C. Bond, *Observations of the Planet Saturn, made with the Twenty-three Foot Equatorial, at the Observatory of Harvard College, 1847–1857* (Cambridge, Mass., 1857), Volume II, Part I of *Annals of the Astronomical Observatory of Harvard College*; see note (4). In 1850 Bond had observed the dark 'obscure ring' interior to the two bright rings of Saturn; see G. P. Bond, 'Inner ring of Saturn', *Monthly Notices of the Royal Astronomical Society*, **11** (1851): 20–7. In *On the Stability of the Motion of Saturn's Rings* (Cambridge 1859): 3–4n (= *Scientific Papers*, **1**: 294n) Maxwell referred to Bond's paper 'On the rings of Saturn', *Astronomical Journal*, **2** (1851): 5–8, 9–10. See also his letter to William Thomson of 1 August 1857 (Volume I: 527–31) for allusions to Bond's papers on the rings of Saturn. Bond had visited London in May 1863; in his 'Diary' he recorded an entry on Tuesday 5 May 1863: 'I went this morning to Palace Garden Terrace for Professor Maxwell, and found him at home. At the Royal Society, Saturday evening, I saw his apparatus to illustrate the motions of a ring of thirty-six satellites about Saturn. He does not think the constitution of Satellites conforms with the aspect of the ring. He has discussed the subject of the ring being a disintegrated solid. He states that the loss of force by friction and heat would not be appreciable to observation, supposing there were perpetual collisions. So loss by friction of a fluid would be inappreciable. He doubts if a ring of satellites would satisfy the observed aspect. / He referred to the aspect of the moon at full having the rim brightest, as probably an indication of a rough surface of large blocks – not fine sand.' (Holden, *Memorials*: 129). Maxwell described his model illustrating the movements of the satellites constituting the rings of Saturn in his letter to William Thomson of 30 January 1858 (Volume I: 578–9 and Plate VII).

(8) George P. Bond, 'An account of Donati's comet of 1850', *Edinburgh New Philosophical Journal*, **10** (1859): 60–84; and G. P. Bond, *Account of the Great Comet of 1858* (Cambridge, Mass., 1862), Volume III of *Annals of the Astronomical Observatory of Harvard College* (see note (4)).

(9) See Number 309.

shown,[10] a cubic mile of sunlight, even at the earths distance is worth mechanically 12,050 foot-pounds and a cubic foot of space near the sun can contain energy equal to .0038 foot-pound *at least.*

This is under ordinary circumstances and gives an estimate of the amount of strain which the medium has been for ages subjected to without in any way giving way.

But we have no reason to believe that if the suns heat were increased 1000 fold, the medium would be unable to transmit it, or would break down under the forces applied.

We have therefore no knowledge of the ultimate *strength* of the heavenly medium but it is well able to do all that is required of it, whether we give it nothing to do but to transmit light & heat or whether we make it the machinery of magnetism and electricity also and at last assign gravitation itself to its power.[11]

If we could understand how the presence of a dense body could produce a linear pressure radiating out in straight lines from the body and keep up this kind of pressure continually, then gravitation would be explained on mechanical principles and the attraction of two bodies would be the consequence of the repulsive action of the lines of pressure in the medium.

For instance in the case of a body at a distance from the Sun the equation to the lines of force w^d be

$$P\cos\theta + r^2\sin^2\theta = C$$

where r is the distance from P and θ the angle which r makes with PS.

There are two sets of lines separated by the surface of revolution whose eq^n is got by making $C = P$

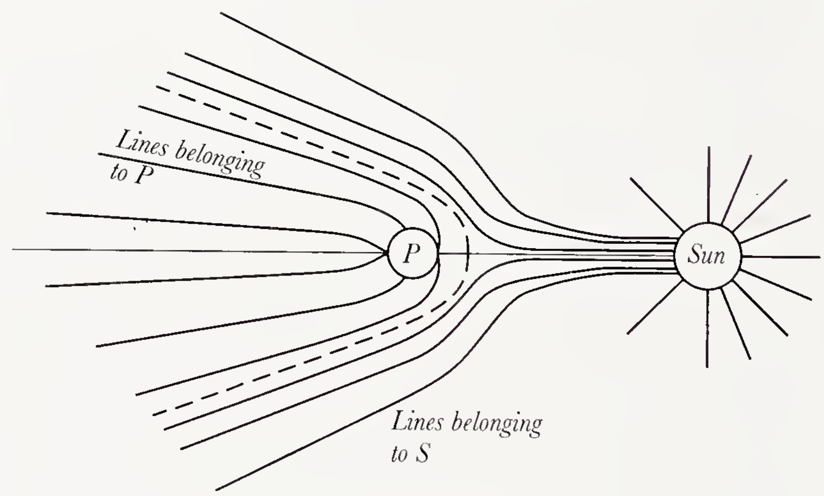

Figure 217,1

$$r^2 = \frac{a^2}{(1+\cos\theta)}.$$

(10) William Thomson, 'Note on the possible density of the luminiferous medium and on the mechanical value of a cubic mile of sunlight', *Trans. Roy. Soc. Edinb.*, **21** (1854): 57–61 (= *Math. & Phys. Papers*, **2**: 28–33).

(11) An issue discussed by Maxwell in his paper 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 492–3 (= *Scientific Papers*, **1**: 570–1); see Number 238.

This surface has the general shape of a paraboloid of rev^{n} but suggests the appearance of a comets tail being more like a catenary than a parabola near the head.

Is there anything about a comet to render its lines of force visible? and not those of a planet which are stronger.

I think that visible lines of gravitating force are extremely improbable, but I never saw anything so like them as some tails of comets.[(12)]

What Herschel says about the repulsive action of the Sun[(13)] leaves unexplained the fact that the motion of the nucleus is that of a body gravitating toward the Sun with a force neither more nor less than that of ordinary matter.

If there were at any time matter in the comet which was not gravitating, or not gravitating to the same extent as earthly matter, then the path of the comet would be less curved to the sun than if it were made of ordinary matter, and therefore calculations depending upon the common value of the Suns attractive power would not give the true path of comets.

I have nothing yet to send you,[(14)] but we are making a report on Electrical Measurements for the Brit. Ass.[(15)] which I will send you when I get copies, and if you will inform me of any electrical men in America I will bring forward their claims to have copies of the standard coil of electrical Resistance.

We have hopes of producing coils next winter the resistance of which is known to within a small fraction in electromagnetic units. Such coils may be employed in measuring electromotive forces, in determining the mechanical equivalent of Heat and in other researches.

The present measures of resistance in absolute units vary by 6 or 7 per cent but I think we are already safe within $\frac{1}{2}$ per cent and I see how to make determinations quite as exact as we can determine the size of our coil in meters.[(16)]

In the course of our work we have had to obtain a constant velocity of rotation. This was secured by means of a governor invented by M^{r} Fleeming Jenkin, but we propose to make a new governor combining the principles of

(12) Compare Maxwell's comments in his letter to Faraday of 9 November 1857 (Volume I: 550).

(13) In his lecture 'On comets', *Good Words* (1863): 476–82, 549–57, esp. 553 (= J. F. W. Herschel, *Familiar Lectures on Scientific Subjects* (London, 1867): 90–141, esp. 128–30), where he refers to Bond's observations of comets' tails (see note (8)). See also Number 309.

(14) In response to Bond's request, in his letter of 9 July 1863, for 'any publications which you can in future add to those which you had the kindness to give me when in London'.

(15) See Number 210 note (2). (16) See Number 214 esp. note (6).

Prof. W. Thomson and Mr Jenkin and we hope to get results comparable with clockwork.(17) I have been studying the mathematical principles of governors and I have been able to detect the sources of irregularities in the motion and, I hope, to correct them.(18) We mean to expose the new governor to severe tests by sudden variations of driving power, and if we find it answer I hope it will be taken into consideration in devising moving power for large equatoreals. The dynamics of governors is exceedingly interesting on account of the number of conditions which may be introduced by various arrangements of the machinery, and the different and sometimes opposite effects of these on the stability of the motion.

I am exceedingly obliged to you for your kindness in sending the books. I hope to be able to say so again when I have read the part about Saturn. I think the visibility of the ring under oblique sunshine shows that its surface is very rough, the roughness being not like that of paper or sandstone but like that of a wilderness of sharp rocks so that we being on the same side as the sun see nearly every spot of sunshine while most of the shadows are hid by their respective objects.

Aragos test of the solidity of a heavenly body by polarized light(19) supposes the solid body to be as smooth as a rough bar of iron if not actually polished whereas the smoothest part of our earth is a paved street and even the sea is generally too rough to polarize much light.

With much Respect
Yours truly
J. Clerk Maxwell

Prof. G. P. Bond

(17) See Maxwell's letter to William Thomson of 11 September 1863 (Number 219, esp. notes (7), (8) and (9)).

(18) See Number 219.

(19) Maxwell may have had in mind Arago's discussion in his *Popular Astronomy*, (trans.) W. H. Smyth and R. Grant, 2 vols. (London, 1855–8), **1**: 418. See also Herschel, 'On comets': 555.

A body is placed at F under the attraction of a very distant body S. The curve separating the lines of force which belong to F from those which belong to S has the equation $r^2 = \dfrac{a^2}{1+\cos\theta}$.

Construction. Draw the dotted lines at distance $\dfrac{a}{2}$ from the axis. Draw with any radius and centre F a circle AQP and make $QP = AQ$. P is a point on curve. There are two asymptotes at distance a on each side of the axis. When S is not at an infinite distance these meet in the line FS.

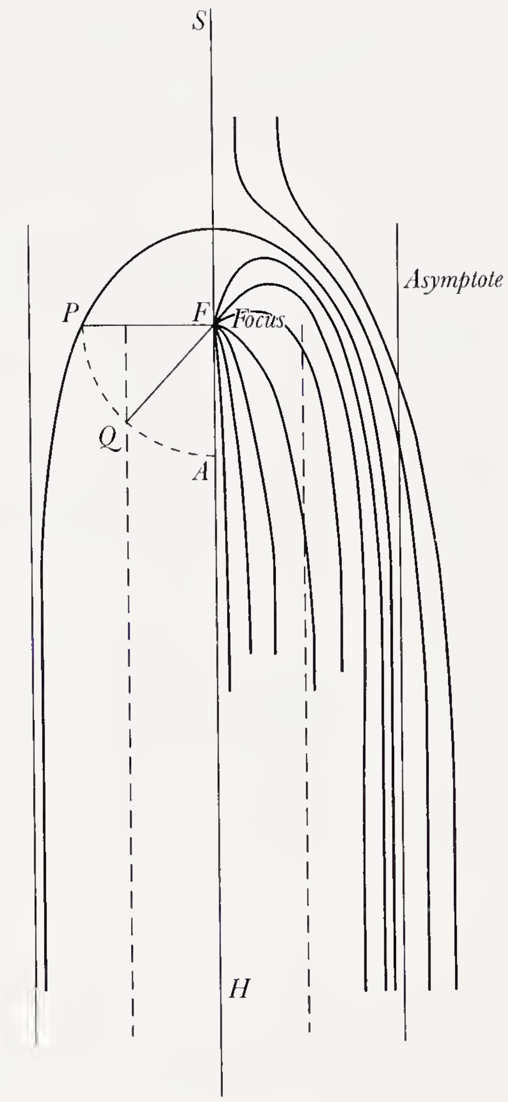

Figure 217,2

FROM A LETTER TO FLEEMING JENKIN[1]

27 AUGUST 1863

From Campbell and Garnett, *Life of Maxwell*[2]

27 Aug. 1863

To compare electromagnetic with electrostatic units:–[3]

1st, Weber's method – Find the capacity of a condenser in electrostatic measure (meters).[4]

Determine its potential when charged, and measure the charge of discharge through a galvanometer.

2d, Thomson's – Find the electromotive force of a battery by electromagnetic methods, and then weigh the attraction of two surfaces connected with the two poles.[5]

3d, (Not tried, but talked of by Jenkin). – Find the resistance of a very bad conductor in both systems –

(1) By comparison with (4th June),[6]

(2) By the log. decrement of charge per second.

All the methods require a properly graduated series of steps. The 1st and 2d determine V, a velocity = 310,740,000 meters per second.[7]

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(1) According to Lewis Campbell's account (*Life of Maxwell*: 317), a 'mass of correspondence, containing numerous suggestions made by Maxwell from day to day in 1863–4, has been preserved by Professor Jenkin'. The correspondence is no longer extant.

(2) *Life of Maxwell*: 336–7.

(3) Compare the discussion of the 'Experimental determination of the ratio, v, between electromagnetic and electrostatic measures of quantity' in the paper by J. Clerk Maxwell and Fleeming Jenkin, 'On the elementary relations between electrical measurements', included in the 'Report of the Committee...on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association* (London, 1864): 111–76, esp. 130–63, on 153–4 (reprinted, with corrections, in *Phil. Mag.*, ser. 4, **29** (1865): 436–60, 507–25, esp. 515–16).

(4) R. Kohlrausch and W. Weber, 'Elektrodynamische Maassbestimmungen insbesondere Zurückführung der Stromintensitätsmessungen auf mechanisches Maass', *Abhandlungen der Königlichen Sächsischen Gesellschaft der Wissenschaften*, (*math.-phys. Klasse*), **3** (1857): 219–92; W. Weber and R. Kohlrausch, 'Ueber die Elektricitätsmenge, welche bei galvanischen Strömen durch den Querschnitt der Kette fliesst', *Ann. Phys.*, **99** (1856): 10–25.

(5) William Thomson, 'Measurement of the electrostatic force produced by a Daniell's battery', *Proc. Roy. Soc.*, **10** (1860): 319–26 (= *Electrostatics and Magnetism*: 238–46).

(6) The German-silver arbitrary standard used in the experiments on electrical resistance; see Number 210, esp. note (4) and Number 214 note (18); and see 'Report': 174–6.

(7) Compare J. C. Maxwell, 'On physical lines of force. Part III', *Phil. Mag.*, ser. 4, **23** (1862): 12–24, esp. 21–2 (= *Scientific Papers*, **1**: 498–500); and his letters of 19 October 1861 to Michael Faraday and of 10 December 1861 to William Thomson (Volume I: 683–9, 692–8);

The 3d method determines V^2.

The first method requires a condenser of large capacity, and the measurement of this capacity and that of the discharge by a galvanometer.

I think this method looks the best; but I would use a much larger condenser than Weber, and determine its capacity by more steps.

The chief difficulty of Thomson's method is the measurement of a very small force and a very small distance. I think these difficulties may be overcome by making the force act on a comparatively stiff spring and magnifying optically the deflection.

On the third method we require a very large condenser indeed, also a series of resistances in steps between 4th June and that of the insulating substance of the condenser, and a galvanometer (or electrometer) to measure discharge (or tension).

'From the determination by Kohlrausch and Weber of the numerical relation...I have determined the velocity of propagation of transverse vibrations' (Volume I: 685).

219

LETTER TO WILLIAM THOMSON

11 SEPTEMBER 1863

From the original in the University Library, Glasgow(1)

Glenlair
Dalbeattie
1863 Sept 11

Dear Thomson

I have been working at the theory of the induction of currents on themselves(2) and have ascertained 1st what happens when a cylindrical conductor begins to conduct, 2nd when it has currents in it of the form $\sin nt$. The different shells of the cylinder have different currents at first and there is therefore a want of 'solidarity' about the electricity. Let $\frac{1}{2}L\gamma^2$ be the intrinsic energy of the current γ in a conductor of length l(3) calculated on the supposition that γ is uniform then in all cases of variable currents we must make $L' = L - \frac{1}{4}l$(4)
that is, the want of solidarity diminishes the 'mass' of the electricity by $\frac{1}{4}l$ just as any looseness in a thing diminishes its mass for instantaneous effects though not for prolonged ones.

I have also got the theory of circular coils of various sections and their values of L and the experimental determination of $\frac{L}{R}$ whence either L or R might be found.(5)

Do you know any publication on this subject.

(1) Glasgow University Library, Kelvin Papers, M 15. Previously published in A. T. Fuller, 'James Clerk Maxwell's Glasgow manuscripts: extracts relating to control and stability', *International Journal of Control*, **43** (1986): 1593–612, on 1597–600.

(2) Compare J. Clerk Maxwell, 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 508–12 (= *Scientific Papers*, **1**: 592–7). See also Maxwell's letter to Thomson of 31 July 1863 (Number 214).

(3) L is the 'coefficient of self-induction'; see 'A dynamical theory of the electromagnetic field': 508 (= *Scientific Papers*, **1**: 592).

(4) See Number 239 §8; and compare 'A dynamical theory of the electromagnetic field': 511 (= *Scientific Papers*, **1**: 595–6), where he writes $\frac{1}{4}\mu l$, where μ is the 'coefficient of magnetic induction for the substance of the wire'.

(5) R is the resistance of the wire.

Governors(6)

I have been working at the conditions of steady motion for your governor (T)(7) for Jenkins (J)(8) for yours & J^{s} in series TJ,(9) for T & J independent on

(6) The term was conventional; see John Robison, *A System of Mechanical Philosophy*, 4 vols. (Edinburgh, 1822), **2**: 152–9; and Henry Moseley, *The Mechanical Principles of Engineering and Architecture* (London, 1843): 390–4. Maxwell's discussion in this letter was preliminary to his paper 'On governors', *Proc. Roy. Soc.*, **16** (1868): 270–83 (= *Scientific Papers*, **2**: 105–20).

(7) Maxwell's drawing of Thomson's governor (Fig. 219, 1) is in accordance with his account of Thomson's device in his paper 'On governors': 273 (= *Scientific Papers*, **2**: 107), where he notes that 'the force restraining the centrifugal piece is that of a spring acting between a point of the centrifugal piece and a fixed point at a considerable distance, and the break is a friction-break worked by the reaction of the springs on the fixed point'. This governor differs in construction and mode of operation from the device described by Thomson in his paper 'On a new form of centrifugal governor', *Transactions of the Institution of Engineers and Shipbuilders in Scotland*, **12** (1868): 67–71; see Fuller, 'Maxwell's Glasgow manuscripts': 1604–5. The leaf spring mechanism (see Fig. 219, 1) is described by Fleeming Jenkin in a letter to Thomson of 8 August 1860 (Kelvin Papers J 38, Glasgow University Library).

(8) In the 1863 'Report of the Committee... on standards of electrical resistance' in the *Report of the Thirty-third Meeting of the British Association for the Advancement of Science* (London, 1864): 120, reference is made to a 'frictional governor of novel form, designed by Mr Jenkin for another purpose, and lent for the experiments'. This governor has been confused with the governor which Maxwell labels TJ. In his list of 'instruments belonging to the Committee of the British Association on Electric Standards' (*Cambridge University Reporter* (27 April, 1875): 354) Maxwell listed both 'Jenkin's governor with contact-breaker' and 'Thomson and Jenkin's governor' (on which see note (9)). For a reconstruction of Jenkin's governor see Fuller, 'Maxwell's Glasgow manuscripts': 1605–6; and see note (18).

(9) In his letter of 31 July 1863 (Number 214) Maxwell refers to this governor as 'mine'; and he labels the corresponding equation (M) *infra*. In 'On governors': 279 (= *Scientific Papers*, **2**: 115) he terms this the 'compound governor', stating that it had 'been constructed and used'. This may be the governor preserved in the Cavendish Laboratory, Cambridge, which is reproduced in Plate I and in I. B. Hopley, 'Maxwell's work on electrical resistance. I. The determination of the absolute unit of resistance', *Annals of Science*, **13** (1957): 265–72, Plate 13. This governor is presumably the instrument which Maxwell described in 1875 as 'Thomson and Jenkin's governor' (see note (8)); it incorporates the principles described for the governor TJ and for the 'compound governor' described in 'On governors', and thus embodies Maxwell's own development of Jenkin's governor. It incorporates a spring-loaded rod (now missing from the instrument, but shown in Plate I) which opposes the centrifugal force of the rotating fly-weights, in a manner similar to the spring in Thomson's governor (see note (7)). The governor in the Cavendish Laboratory is discussed by Fuller, 'Maxwell's Glasgow manuscripts': 1606–8, who reconstructs its mode of operation. This governor also incorporates a damper, apparently unlike Jenkin's governor as constructed in 1863 (see *infra* and note (18)). In the 1863 'Report of the Committee... on standards of electrical resistance': 120, 166, it is stated that 'better results are expected with a larger governor, made specially for the apparatus, on the joint plans of Professor Thomson and Mr Jenkin'; and that 'an improved governor on the same principle will be

the same axle T+J and for Siemens S.(10) T & J have eq^{ns} of 3rd order T+J & S 4th and TJ of the 5th order.(11) Here is TJ in which T is employed to turn the loose wheel of J and lay on friction so.(12)

Let angle described by main axle	$= \omega t + \theta$
Moment of inertia of main axle	$= M$
Power of damper or centrifugal friction break	$= X$
Driving power	$= L$
Angle described by loose Jenkin wheel	$= \psi$
Moment of inertia	$= C$
Power of damper to J	$= Y$
Power of Js friction beak(13)	$= J$.
Angle between Thomsons centrifugal piece & axes	$= \alpha + \phi$
$A' =$ difference of mom. inert.	$A = A' \sin 2\alpha\omega$
$B =$ mom about axis of suspension	
$T =$ power of Ts friction break(14) then	

$$\text{(M)} \qquad M\frac{d^2\theta}{dt^2} + X\frac{d\theta}{dt} + A\frac{d\phi}{dt} + T\phi + J\psi = L$$

$$\text{(J)} \qquad M\frac{d^2\psi}{dt^2} + Y\frac{d\psi}{dt} = T\phi$$

$$\text{(T)} \qquad B\frac{d^2\phi}{dt^2} = A\frac{d\theta}{dt}^{(15)}$$ when properly adjusted as to spring power.

By T it appears that whenever the centrifugal piece is in equilibrium the machine is not only at the right velocity but in the right place, so that the

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adopted in further experiments, in describing which an account of its construction will be given'. These experiments were carried out in the following year (see Number 222), but no mention of this governor (presumably the governor here denoted TJ by Maxwell) is made in the 'Description of a further experimental measurement of electrical resistance made at King's College', in the *Report of the Thirty-fourth Meeting of the British Association* (London, 1865): 350–1.

(10) The governor described by C. W. Siemens, 'On an improved governor for steam engines', *Proceedings of the Institution of Mechanical Engineers* (1853): 75–83, Plates 17 and 18.

(11) See Maxwell, 'On governors': 276, 278–9 (= *Scientific Papers*, **2**: 111, 114–15).

(12) In Maxwell's governor (TJ) a spring, employed as in Thomson's governor (see notes (7) and (9)), controls the fly-weights (of Jenkin's governor) which on rotation turn a horizontal wheel connected to a friction brake.

(13) See note (12).

(14) See note (7).

(15) Compare Maxwell, 'On governors': 278, equations (13) (= *Scientific Papers*, **2**: 115), where a term for friction is added to the equation corresponding to (T).

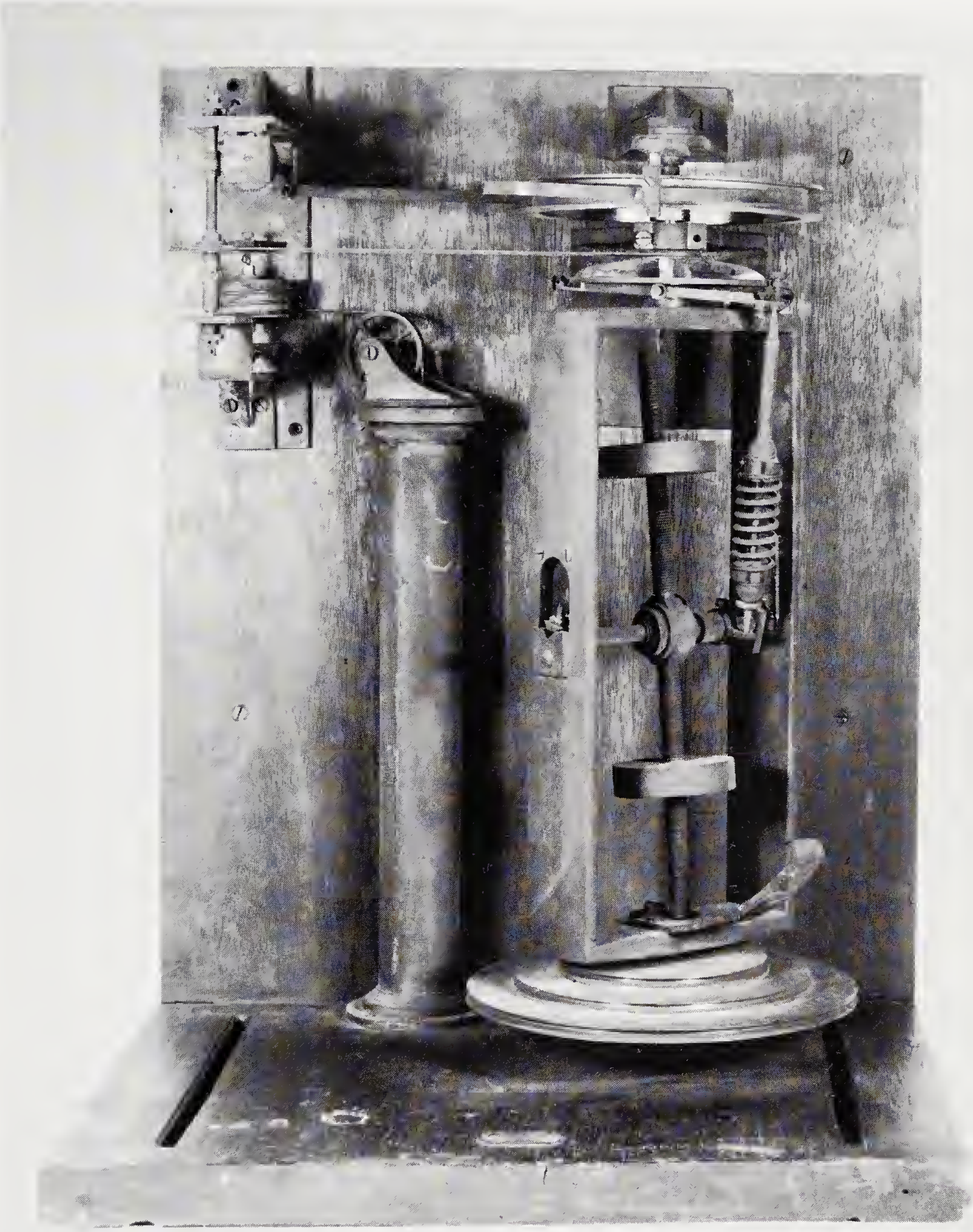

Plate I. The 'compound' governor (constructed after 1863) preserved in the Cavendish Laboratory, that Maxwell described as 'Thomson and Jenkin's governor'. This device incorporates Maxwell's modification (the spring-loaded rod, suggested by the spring opposing centrifugal force in Thomson's governor) of Jenkin's governor (Number 219).

effect of an increase of driving power is to produce oscillations after which clock error and rate are both as before.

Let $\quad \dfrac{Y}{C}=e \quad \dfrac{X}{M}=f \quad \dfrac{AT}{BM}=g \quad \dfrac{A^2}{BM}=h \quad \dfrac{J}{C}=j$

then if θ is of the form $P_1\epsilon^{x_1t}+P_2\epsilon^{x_2t}$ &c

$$x^5+(e+f)x^4+efx^3+(g+he)x^2+gex+gj=0.^{(16)}$$

The roots of this eq$^{\text{n}}$ are in this case of the form

$$a,\ b\pm\sqrt{-1}\,c,\ b'\pm\sqrt{-1}\,c'.$$

If either a b or b' is positive there will be destructive oscillations. Can you find the conditions of their being all negative?(17) Here are two necessary conditions

$$(e+f)ef>(g+he)$$
$$(e+f)e>j.$$

Here g is the power of Thomson $\quad j$ of Jenkin
$\quad f=$ damper of main axle $\quad e$ of Jenkin.
The 1$^{\text{st}}$ condition gives the necessary power of f
the 2$^{\text{nd}}$ ———— of e.
f is got either by a simple centrif. friction break like the Edin$^{\text{h}}$ Equatoreals clock or Jenkins without the screw part, or by some loose wheels fitting with various degrees of friction on the main axis.
e is got by making Jenkins scale pan go up & down in water or better by letting a wire from the bottom of it dip into Canada Balsam or Tar.(18) Can you tell me if there is any other condition than these two of a b b' being $-^{\text{ve}}$?

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(16) Compare Maxwell, 'On governors': 279, equation (16) (= *Scientific Papers*, **2**: 115).

(17) In his paper 'On governors' Maxwell linearises the equations of motion, which he reduces to a characteristic polynomial; the coefficients of this characteristic equation determine whether the governor is stable. He notes that this 'is mathematically equivalent to the condition that all the possible roots and the possible parts of the impossible roots, of a certain equation shall be negative'; 'On governors': 271 (= *Scientific Papers*, **2**: 106). This mathematical technique is the condition first introduced in his essay *On the Stability of the Motion of Saturn's Rings* (Cambridge, 1859): 10–11 (= *Scientific Papers*, **1**: 301–2), and see Volume I: 450–1. In his review of *Saturn's Rings* the Astronomer Royal, George Biddell Airy, 'commend[ed] these propositions to the study of the reader, as an interesting example of a beautiful method, applied with great skill to the solution of the difficult problems which follow'; G. B. Airy, 'On the stability of the motion of Saturn's rings', *Monthly Notices of the Royal Astronomical Society*, **19** (1859): 297–304, on 300.

(18) In 'On governors': 275 (= *Scientific Papers*, **2**: 111) Maxwell describes Jenkin's governor as having a damper: 'a weight is made to hang in a viscous liquid'. This was presumably a modification to Jenkin's governor as constructed in 1863.

This machine (TJ) is easily turned into T by clamping the loose wheels.[19] I think with a good spring T is an excellent time keeper. Spring short and not near the cent. piece but kept as near the axis as may be connected to C by a thin wire so that when AB is vertical $SC \perp CO$ and tension $= 0$.[20]

By keeping AB as free as possible and making the velocity considerable and the working angle of $AB = 45°$ you get very quick action and increased stability with the same power of dampers.

I send a photograph and shall be much obliged for one of you if you have some still. I shall be here till the 30th Sept. after which in London.

Yours truly
J. Clerk Maxwell

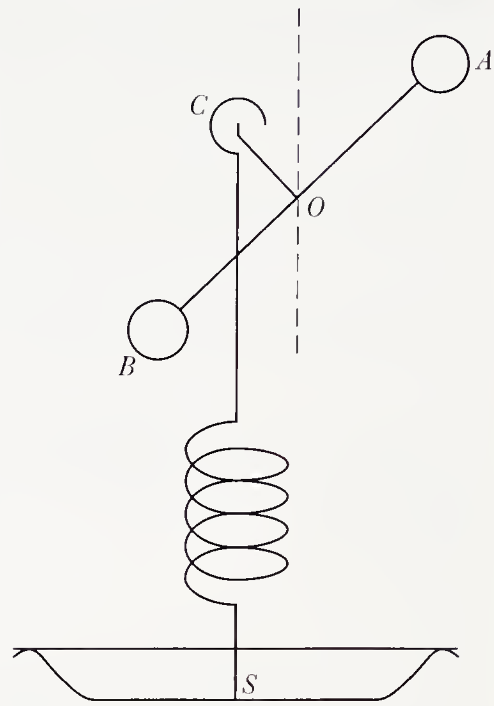

Figure 219,1

(19) The horizontal wheel of Jenkin's governor (see note (12)) had a loose fit on the vertical axle.

(20) See note (7). The lower end of the spring S is attached to a horizontal leaf spring.

REPORT ON A PAPER BY CHARLES CHAMBERS[1] ON THE MAGNETIC ACTION OF THE SUN

LATE OCTOBER 1863[2]

From the original in the Library of the Royal Society, London[3]

REPORT ON M^r C. CHAMBERS PAPER ON THE NATURE OF THE SUNS MAGNETIC ACTION UPON THE EARTH[4]

The author first shows (after Poisson)[5] that the three components of terrestrial magnetism due to the Sun's action as a magnet on the earth and the soft iron &c in it are linear functions of the components of the suns direct magnetic action.

From this it follows that the part of the disturbance due to the suns action as a magnet ought to be a harmonic variation whose period is one solar day, and that terms having periods of $\frac{1}{2}$ $\frac{1}{3}$ &c of a day are not due to the action of the Sun as a magnet.

On comparison with observation it appears that there are considerable variations dependent on the Suns hour angle but that those portions whose period is a fraction of a day are very considerable with respect to the term whose period is one day.

Hence the effect of the sun not due to action as a magnet is large compared to his effect as a magnet.[6]

Further it appears that on account of the Suns rotation the mean diurnal variation taken over a considerable time will be the same as if the sun were magnetized in the direction of his axis. Hence a law of the variations

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(1) Charles Chambers was an assistant at Kew Observatory from 1856–63.

(2) In a letter to Stokes of 29 October 1863 (ULC Add. MSS 7656, RS 419; printed in Wilson, *Stokes–Kelvin Correspondence*, **1**: 308), William Thomson wrote: 'Hearing from Maxwell that it was to him, not me, that it had been intended to send Chambers' paper I have given it to him. / I received it at the Royal Institution last June, with a note from you, so I suppose I was one of the referees, and I therefore leave a report for you upon it'. According to the Royal Society's *Register of Papers Received* Chambers' paper was approved for publication on 29 October 1863.

(3) Royal Society, *Referees' Reports*, **5**: 48.

(4) Charles Chambers, 'On the nature of the sun's magnetic action upon the earth', *Phil. Trans.*, **153** (1863): 503–16. The paper was received by the Royal Society on 30 April 1863, and read on 21 May 1863; see the abstract in *Proc. Roy. Soc.*, **12** (1863): 567.

(5) S. D. Poisson, 'Second mémoire sur la théorie du magnétisme', *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, **5** (1826): 488–533, esp. 533.

(6) Compare Maxwell's similar remark (referring to Chambers) in the *Treatise*, **2**: 125 (§471).

depending on the position of the earth with respect to the points where the plane of the suns equator cuts the ecliptic.

The numbers deduced from this rule do not correspond with the actual variations as is shown by observations at different places.

Hence the suns action as a magnet is very small compared with his action on the magnetism of the earth in other ways e.g. (1) by heating it (2) by attracting it (3) by directing its course through space (4) by producing internal pressures in its substance.

The manner in which the results of observation are compared with the results of the hypothesis of the Sun's being a magnet is very clear and well conceived and I consider that the paper would be valuable as an addition to the Societies Transactions.[7]

J. CLERK MAXWELL

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(7) In a brief report dated 28 October 1863 (Royal Society, *Referees' Reports*, **5**: 47) William Thomson also recommended publication, adding: 'I have long had strong reason for forming the same conclusion as that to which M^r Chambers has been led by a thorough examination of the results of observation. I add a short note (accompanying this report) which if approved by the Council, might be printed at the end of the paper.' Thomson's supplementary 'Note', affirming 'that no effect of the sun's action as a magnet is sensible at the earth', was printed as an addendum to Chambers' paper, in *Phil. Trans.*, **153** (1863): 515–16.

MANUSCRIPT ON THE EQUILIBRIUM AND STIFFNESS OF A FRAME(1)

circa JANUARY 1864(2)

From the original in the University Library, Cambridge(3)

TRIANGULAR ARCHED BRIDGE(a)(b)

This is a stiff unstrained frame before it is loaded. When loaded, it is strained first by the load and the upward pressure at the springs, and second by the horizontal thrust of the springs.

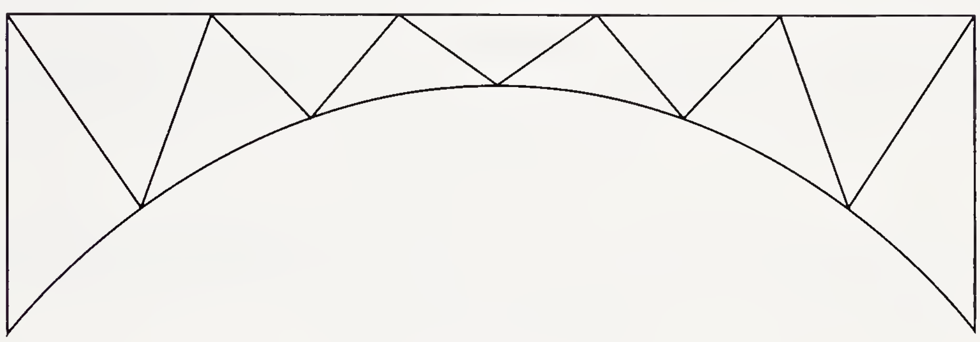

Figure 221,1

If the load is given it is easy to calculate the stress on each piece supposing the supporting forces at the springs vertical (as in a roof).

This may be done either by drawing a diagram or by arithmetical calculation.

Let p be the stress on one of the pieces on this supposition when the load is unity.

Next find the stress on each piece due to a tension unity between the spring

(a) {Endorsed} From Prof. Maxwell 26 January 1864 / Triangular Arched Bridge.

(b) {Headed} Diagram in office(4) / yield 1^{in} in 150^{ft} – this gives E / take e from Clark on Britannia Bridge(5) load unity.

(1) Compare Maxwell's paper 'On the calculation of the equilibrium and stiffness of frames', *Phil. Mag.*, ser. 4, **27** (1864): 294–9 (= *Scientific Papers*, **1**: 598–604).

(2) See the endorsement *infra*.

(3) ULC Add. MSS 7655, V, g/5.

(4) The manuscript may well have been written for Maxwell's cousin William Dyce Cay (son of Robert Cay), who was an engineer engaged in bridge-building: see Number 229, and Volume I: 410, 480, 575.

(5) Edwin Clark, *The Britannia and Conway Tubular Bridges*, 2 vols. (London, 1850).

of the arch. Let q be the stress on that piece for which the former stress was found.

Finally find the stress on each piece due to a single load equal to unity placed on a given joint A and let that on the selected piece be r (the supporting forces at the springs being vertical).

Then for a load L distributed as in the first case combined with a horizontal thrust H between the springs the stress on a piece is

$$pL+qH.$$

If e be the number of pounds weight required to produce unit elongation in the piece[(6)] then the actual elongation

$$=\frac{1}{e}(pL+qH).$$

The elongation of the line joining the springs due to the elongation of this piece only, the others remaining of invariable length, is by the principle of work[(7)]

$$\frac{1}{e}(pqL+q^2H).$$

Hence if we call the sum of all quantities of this kind with respect to each piece of the frame

$$\sum\left\{\frac{1}{e}(pqL+q^2H)\right\}$$

this will be the total elongation of the line joining the springs.

Let the springs be connected by a tie beam whose elasticity is E[(8)]

then
$$\frac{1}{E}H+\sum\frac{1}{e}pqL+\sum\frac{1}{e}q^2H=0$$

or
$$H=-L\frac{\sum\left(\frac{1}{e}pq\right)}{\sum\left(\frac{1}{e}q^2\right)+\frac{1}{e}}.\quad{}^{(9)}$$

---

(6) e is the 'elasticity' of a piece, 'the force required to produce extension-unity'; $\frac{1}{e}$ 'the extension produced in a piece by tension-unity', is its 'extensibility'; compare 'On the calculation of the equilibrium and stiffness of frames': 295 (= *Scientific Papers*, **1**: 599–600).

(7) In his paper 'On the calculation of the equilibrium and stiffness of frames': 294 (= *Scientific Papers*, **1**: 598) Maxwell states: 'The method is derived from the principle of Conservation of Energy, and is referred to in Lamé's *Leçons sur l'Elasticité*, Leçon 7$^{\text{me}}$, as Clapeyron's Theorem; but I have not yet seen any detailed application of it.' See Gabriel Lamé, *Leçons sur la Théorie Mathématique de l'Élasticité des Corps Solides* (Paris, 1852): 80–3.

(8) See note (6).

(9) Compare 'On the calculation of the equilibrium and stiffness of frames': 297 (= *Scientific Papers*, **1**: 601).

This gives us the tension of the tie beam in terms of L. If we make $E = \infty$ the springs become fixed. The elongation of the tie beam is

$$\frac{1}{E}H = -L\frac{\sum\left(\frac{1}{e}pq\right)}{\sum\left(\frac{1}{e}q^2\right)+1}.$$

If we put $E = 0$ we get the spread of the springs if there were no tie beam and they rested on a smooth horizontal plane.

To find the deflexion at the joint A due to the elongation (l) of the selected piece, the rest being rigid.

If the stress on A is 1 that on the piece is r therefore if the deflexion of A is d and the elongation of the piece l

$$d = rl$$

In the present case

$$d = \frac{1}{e}(rpL + rqH).$$

Hence the total deflexion at A is

$$L\sum\left(\frac{1}{e}rp\right) + H\sum\left(\frac{1}{e}rq\right)$$

or

$$D = L\left\{\sum\left(\frac{1}{e}rp - \frac{\sum\left(\frac{1}{e}pq\right)\sum\left(\frac{1}{e}rq\right)}{\sum\left(\frac{1}{e}q^2\right)+\frac{1}{E}}\right)\right\}.^{(10)}$$

This is the deflexion of any joint A due to the imposition of a load L in a given manner, $p\,q\,r$ are the stresses on a piece of elasticity e due to unity of the given load, unity of tension of tie beam and unity of load at A respectively and $\sum$ implies that all quantities of the kind are to be added. The tie beam whose elasticity is E is not included in the summation.

Note – the value of D is symmetrical with respect to p and r. Hence the deflexion produced at A by a load L at B is equal to the deflexion produced at B by L placed at A.

It will be best to make a table of the values of q once for all and two other tables one of x (the stress on each piece due to unity of vertical thrust on the left spring) and another of y (the same due to unit vertical thrust on the right spring). Then if the load divides the space in the ratio of n to $1-n$, p will lie $(1-n)x$ for the pieces to the left of the load and ny for those on the right.

(10) See note (9).

FROM A LETTER TO KATHERINE MARY CLERK MAXWELL

28 JANUARY 1864

From Campbell and Garnett, *Life of Maxwell*.(1)

We are going to have a spin with Balfour Stewart tomorrow.(2) I hope we shall have no accidents, for it puts off time so when anything works wrong, and we cannot at first find out the reason, or when a string breaks, and the whole spin has to begin again.... However, we hope to bring out our standards by September,(3) and Becker(4) makes them up excellently.

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(1) *Life of Maxwell*: 316.

(2) Further experiments on standards of electrical resistance: see Numbers 210, 211 and 214, and note (3).

(3) See the 'Description of a further experimental measurement of electrical resistance made at King's College' (by Maxwell, Balfour Stewart, Fleeming Jenkin and Charles Hockin), in the 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-fourth Meeting of the British Association for the Advancement of Science*; *held at Bath in September 1864* (London, 1865): 345–67, esp. 350–1.

(4) See Number 214 note (5).

REPORT ON A PAPER BY WILLIAM JOHN MACQUORN RANKINE ON FLUID MOTION

LATE FEBRUARY 1864[1]

From the original in the Library of the Royal Society, London[2]

REPORT ON D$^{\mathrm{r}}$ RANKINE'S PAPER 'ON PLANE WATER-LINES'[3]

This paper is partly theoretical and partly practical. The theoretical part discusses two kinds of lines of fluid motion.[4] The practical part shows how certain of these lines may be adapted to the drawing of the waterlines of ships.

The theoretical definition of a Plane Waterline is 'a curve which a particle of liquid describes in flowing past a solid body[5] when such flow takes place in plane layers'.[6]

The general theory of such lines is in many respects simpler than that of waterlines in general.[7] The velocity of the fluid at any point is given by the equations $u = \dfrac{dU}{dy} \quad v = -\dfrac{dU}{dx}$ which of themselves satisfy the equation of continuity. The equation of a waterline is then $U = \text{const.}$[8]

(1) According to the Royal Society's *Register of Papers Received* Rankine's paper was referred to Maxwell on 18 February 1864, and approved for publication on 17 March 1864; and see note (15).

(2) Royal Society, *Referees' Reports*, **5**: 216.

(3) W. J. M. Rankine, 'On plane water-lines in two dimensions', *Phil. Trans.*, **154** (1864): 369–91. The paper was received by the Royal Society on 28 July 1863, and read on 26 November 1863; see the abstract in *Proc. Roy. Soc.*, **13** (1863): 15–17.

(4) By 'lines of fluid motion', which Rankine terms 'water-lines', Maxwell here denotes the lines traced by the paths of particles in a current of fluid, curves which Rankine subsequently termed 'stream-lines'. See Number 337, esp. note (4). For cases of steady motion, with which Rankine's paper is concerned, the paths of particles (stream lines) coincide with lines drawn to indicate the direction of fluid motion. See Number 337, esp. notes (5) and (6); and Horace Lamb, *A Treatise on the Mathematical Theory of the Motion of Fluids* (Cambridge, 1879): 21–2.

(5) See note (4).

(6) Rankine, 'On plane water-lines in two dimensions': 369.

(7) Maxwell's statement of the theory of stream lines in the case of motion in two dimensions, x and y, follows Rankine, 'On plane water-lines in two dimensions': 370. He had himself given a similar exposition in a manuscript 'On the steady motion of an incompressible fluid...', dated 9 May 1855 (Volume I: 295, 297).

(8) U is the stream function.

The rotation of a fluid element is represented by

$$\frac{d^2U}{dx^2}+\frac{d^2U}{dy^2} = \chi$$

and since as has been shown by Stokes (lectures &c?)[9] and recently by Helmholtz (Crelle 1859)[10] this rotation remains constant for the same particles of a perfect fluid during their motion the condition of motion is χ = function of U.

In the present paper χ is always equal to 0.[11]

This is equivalent to $udx+vdy = d\phi$[12] and then, as has been shown by W. Thomson Cam & Dub Math Journal III p 286[13] the systems of curves defined by U = const and ϕ = const. are reciprocal that is if either is a system of water lines the other is a system of velocity function-curves.

(In the theory of Heat these would be lines of flow of heat and isothermals and in magnetism or electricity they would be lines of force and equipotential lines.)[14]

The waterlines here discussed are those due to the motion of an infinite sheet of water the distant parts of which move with uniform velocity C while the parts near the solid are disturbed in their course by having to flow past it.

It is manifest that the introduction of a solid of any form would generate a system of plane water lines of which the solid itself would be one. Physically considered it would be the first water line, but mathematically considered it would be one of a series any one of which might be substituted for the solid, and would produce the remaining waterlines.

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(9) On reviewing Rankine's paper (see note (15)) William Thomson appended the comment 'This very remarkable result due to Stokes?' to the MS of Rankine's paper (Royal Society, PT. 70.7, f. 6). See G. G. Stokes, 'On the steady motion of incompressible fluids', *Trans. Camb. Phil. Soc.*, **7** (1842): 439–53, esp. 441–6 (= *Papers*, **1**: 1–16).

(10) H. Helmholtz, 'Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen', *Journal für die reine und angewandte Mathematik*, **55** (1858): 25–55, esp. 33–7.

(11) The condition of irrotational motion of an incompressible fluid in two dimensions; see Volume I: 311, and Stokes, 'On the steady motion of incompressible fluids': 441.

(12) The function ϕ is the velocity potential; for Maxwell's use of this term (following Helmholtz) see Number 254. On the equation see Stokes, 'On the steady motion of incompressible fluids': 439–41.

(13) [William Thomson,] 'Note on orthogonal isothermal surfaces', *Camb. Math. J.*, **3** (1843): 286–8 (= *Math. & Phys. Papers*, **1**: 22–4).

(14) The argument is basic to Maxwell's paper 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83 (= *Scientific Papers*, **1**: 155–229). There he also makes reference to Thomson's paper on orthogonal surfaces; see Volume I: 361.

The cases considered by the author are those in which the lines are produced by a circular cylinder or by an oval of a peculiar form.

In both cases the mathematical expressions indicate a system of lines within as well as without the primitive curve, which do not belong to this question.

As I think these particular cases and the graphical method employed by the author[15] can be more easily explained in a physical way than by reference to the equations in the memoir I shall do so, observing that the author has already given several examples of such treatment in cases where it is not so easy to apply it as in the present case.

It follows from the nature of plane water lines that if two systems of water lines be drawn, so that the constant in each series increases by equal intervals, a third system of lines may be drawn diagonally through these and this system will also be a system of water-lines.

1 The waterlines corresponding to a uniform rectilinear flow form a system of parallel lines at equal intervals.

2 The waterlines corresponding to a flow outwards from a centre form a system of lines radiating from a point at equal angles.

3 The waterlines corresponding to a flow outwards from one focus and inwards towards another may be got by combining the two cases graphically. The result, however is more simply got by drawing a system of circles through the two points so that the tangents at those points shall be a series of lines at equal angles.

4 When the two points coincide, these circles become a system of circles all touching at the same point and having their radii in harmonic series.

5 By combining (1) & (4) we get the 'Cyclogenous Neoids'.[16]

6 By combining (1) and (3) we get the 'Oogenous Neoids'.[17]

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(15) See Plate VIII Figs. 1 and 2 in *Phil. Trans.*, **154** (1864) for Rankine's figures. In a letter to Stokes of 17 February 1864, reporting on Rankine's paper (Royal Society, *Referees' Reports*, **5**: 214; in Wilson, *Stokes–Kelvin Correspondence*, **1**: 317–19), William Thomson remarked that 'Maxwell I believe was the first to use the diagonal method of drawing, for lines of force & I think should be referred to'. In a brief supplementary note, published as an 'Appendix' to his paper 'On plane water-lines in two dimensions': 390, Rankine responded by citing Maxwell's method of constructing lines of force; see Volume I: 627–8, and the *Treatise*, **1**: 147–9 (§123), and Maxwell's paper 'On a method of drawing the theoretical forms of Faraday's lines of force without calculation', *Report of the Twenty-sixth Meeting of the British Association for the Advancement of Science* (London, 1857), part 2: 12 (= *Scientific Papers*, **1**: 241).

(16) Rankine, 'On plane water-lines in two dimensions': 373; '*Cyclogenous Neoïds*, that is, *ship-shape curves generated from a circle*'.

(17) Rankine, 'On plane water-lines in two dimensions': 373; water-line curves generated from '*Ovals, or Oögenous Neoïds*'.

7 By combining (1) and (2) we get the 'Parabologenous Neoids'[18] or the system of lines seen when a carriage wheel rolls behind a vertical railing.

8 By combining a sufficient number of cases of (2) with (1) we may get waterlines of any form and there will be one closed curve among them provided as much water flows out of one set of points as flows in at another.

Which of all these systems of waterlines including all forms whatever is best for shipbuilding must be decided by considering which gives the smoothest and simplest motion to the water beyond the vessel, and is least liable to generate waves and so waste energy.

On examining the waterlines of the Oogenous kind it appears that those near the centre have three points of minimum velocity (where they are furthest apart), and two points of maximum velocity (where they are closest together), but that those at a distance have two points of minimum and one of maximum velocity.

On the limiting waterline where the two maxima and one minimum coalesce there will be at that point a position of most uniform velocity and that waterline may be considered as the waterline of least variation of velocity. The author therefore selects it as the best waterline for practice.[19]

(It is impossible to have a system of waterlines of absolutely uniform velocity, for in that case both the water lines and their orthogonal system of curves must be equidistant, whence it follows by geometry that both systems must be straight lines.)

The waterlines finally selected are the portions of these lines of most uniform flow up to the point of minimum velocity. Such waterlines may be of any degree of bluffness or sharpness and the author shows that the discontinuity at the extremities does not interfere much with the ships motion.

Of course the external waterlines of such a ship will differ in some degree from the 'Oogenous Neoids' of this paper, but unless the water is thrown into eddies and waves the effect on the ships motion will be inconsiderable.[20]

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(18) Rankine, 'On plane water-lines in two dimensions': 377.

(19) Rankine, 'On plane water-lines in two dimensions': 380, 388; termed the 'water-lines of smoothest gliding, or *Lissoneoïds*'. In his letter to Stokes of 17 February 1864 (note (15)) Thomson emphasised Rankine's discussion of the 'trajectories of minimum & of max^{m} velocity of water relative to solid... the "lissenoid" core is very remarkable'.

(20) In his letter to Stokes of 17 February 1864 Thomson declared: 'But the application is not, as the author supposes, even approximate for a real ship. His statements on this subject would require *very decided correction* but this would leave the main substance of the paper untouched. / A much closer approx^{n} to real ship water lines would be had by taking a solid of revolution moving through a liquid instead of a case in which the motion is entirely in parallel planes.'

The author has also investigated the effect of the friction of the water against the surface of the ship, and has shown how to reduce to a mathematical form the problem of the waterline of least frictional resistance.

The friction here considered is that of the water against the solid side of the vessel and is assumed proportional to the square of the velocity of sliding. The work done against friction divided by the distance described by the vessel in the same time, gives the resistance to the motion.

This friction differs from the internal friction of the fluid the effects of which are proportional to the velocity simply.

The effect of the variations of velocity in producing inequalities of level near the vessel are investigated. These inequalities may become the origin of waves, which may dissipate the energy of the vessel over the ocean. This part of the paper appears from its bearing upon the resistance to be worthy of more extended investigation.

The paper as a whole is an instance of mathematical principles applied with skill to a practical subject which is beset with very great and necessary difficulties and I consider it worthy of a place in the Transactions.

DRAFTS ON THE THEORY OF SATURN'S RINGS

1864[1]

From the originals in the University Library, Cambridge[2]

[1] MATHEMATICAL THEORY OF SATURNS RINGS

We assume the Rings to consist of independent portions of solid matter of small dimensions compared with the thickness of the rings (100 miles or less) revolving about Saturn in orbits very nearly circular, the differences of the actual motion from that of a satellite in a circular orbit in the plane of the ring being small quantities of the first order. Let S be the Mass of Saturn r the distance from his centre then the linear velocity of the supposed satellite at distance r would be $\sqrt{S\frac{1}{r}}$ in the positive direction at right angles to r, and the actual motion of a particle may be resolved into velocities

u in the direction of r, away from Saturn $= \dfrac{dr}{dt}$

$\sqrt{S\frac{1}{r}}+v$ in the positive direction around Saturn $= r\dfrac{d\theta}{dt}$

w normal to the plane of reference $= \dfrac{dz}{dt}$.

Let z be the actual distance of the particle from the plane of reference.

First, for the motion in the plane of reference we have as usual[3]

$$\frac{1}{r} = \frac{S}{h^2}(1+e\cos(\theta-\alpha)),$$

$$\frac{1}{r^2}\frac{dr}{dt} = \frac{S}{h^2}e\sin(\theta-\alpha)\frac{d\theta}{dt},$$

$$h = r^2\frac{d\theta}{dt}.$$

(1) In his letter to G. B. Airy of 16 October 1872 (Number 424) Maxwell indicated that these drafts were written 'about the year 1864'.

(2) ULC Add. MSS 7655, V, a/7. Previously published (in different sequence) in S. G. Brush, C. W. F. Everitt and E. Garber, *Maxwell on Saturn's Rings* (Cambridge, Mass./London, 1983): 169–81, 183–9. See also A. T. Fuller, 'James Clerk Maxwell's Cambridge manuscripts: extracts relating to control and stability – IV', *International Journal of Control*, **39** (1984): 619–56. Brush, Everitt and Garber include a preliminary worksheet (on which see note (23)) and some supplementary notes, not reproduced here; see *Maxwell on Saturn's Rings*: 181–3, 190–4.

(3) The equation in polar coordinates for an ellipse of eccentricity e.

Whence $$u = \frac{S}{h} e \sin(\theta - \alpha) \quad v = \frac{1}{2}\frac{S}{h} e \cos(\theta - \alpha).$$

Let u_0 and v_0 be the values of u & v when $\theta = 0$

$$u = u_0 \cos\theta + 2v_0 \sin\theta$$
$$v = v_0 \cos\theta - \tfrac{1}{2}u_0 \sin\theta.$$

In these expressions v is the tangential velocity relative to that in a circle at the distance from the centre at which the particle is *at that instant* so that while $\sqrt{\frac{S}{r}}$ represents the average tangential velocity v represents the additional velocity due to agitation.

Let us suppose that R is the total mass of the ring contained within the radius r so that R is a function of r which is zero when r is less than the inner radius of the ring and equal to the whole ring when r is greater than the outer radius of the ring.

Then the quantity of matter in unit of area of the ring will be

$$\frac{1}{2\pi r}\frac{dR}{dr}.$$

The attraction of the ring on a particle at its surface, normal to the ring will be

$$\frac{1}{r}\frac{dR}{dr}$$

and if the particles of the ring be supposed uniformly scattered within a stratum whose thickness $= Z$ on either side of the plane of reference the attraction on a particle within it will be

$$\frac{1}{r}\frac{dR}{dr}\frac{z}{Z} \quad \text{towards the plane of reference.}$$

The equation of motion in z will therefore be

$$\frac{d^2z}{dt^2} = -\left(\frac{S}{r^3} + \frac{1}{rZ}\frac{dR}{dr}\right)z$$

whence if we make

$$1 + \frac{r^2}{SZ}\frac{dR}{dr} = n^2\mu^2 \quad \text{and} \quad n = \sqrt{\frac{S}{r^3}} = \frac{d\theta}{dt}$$

we shall have

$$z = z_0 \cos\mu\theta + \frac{w_0}{n\mu}\sin\mu\theta$$
$$w = w_0 \cos\mu\theta - z_0\, n\mu \sin\mu\theta.$$

z, the distance from the plane of reference and w the velocity perpendicular to that plane are connected together by the following equation which we obtain by eliminating θ from the two equations above

$$z^2n^2\mu^2+w^2 = z_0^2n^2\mu^2+w_0^2.$$

If z^2 represents the mean square of the distance of all the particles from the plane of reference and w^2 the mean square of the agitation normal to it it will be seen that in the case in which the mean thickness of the ring does not alter the value of w^2 will also be constant, that is, for the case of stability

$$z^2 = z_0^2 \quad \text{and} \quad w^2 = w_0^2.$$

[2] [THE STABILITY OF SATURN'S RINGS]

The existence of a thin flat and circular system of rings, surrounding, yet nowhere touching the planet Saturn has been known for 200 years but we have as yet little knowledge of their structure and little foundation for coming to a decision whether we may expect them to continue in their present state for a long time, or whether their total or partial destruction is likely to be witnessed by living astronomers.

Considerable changes have certainly taken place in their appearance as seen by successive astronomers(4) but we know that the earlier observers had more imperfect telescopes than those which are now directed to Saturn so that the evidence requires very careful examination before we can conclude in favour of an actual change of form or a development of new features.(5)

Mathematicians, however, while they accept the results of observation as to the changeableness or permanence of an astronomical phenomenon, are not at liberty to accept them as ultimate facts from which the future phases of the motion may be deduced by the principle of continuity. They cannot regard Saturns Rings as the result of some undiscovered law of generation and development prevailing among a class of planets. They must either explain on mechanical principles how they have continued to exist so long and whether they are subject to decay, or they must confess that a gap has

(4) Otto Struve had claimed that the rings had changed in form during the two centuries following their first observation; see Otto Struve, 'Sur les dimensions des anneaux de Saturne', *Mémoires de l'Académie Impériale des Sciences de Saint-Petersbourg*, ser. 6, **5** (1853): 439–75, esp. 473 (= *Recueil de Mémoires présentés à l'Académie des Sciences par les Astronomes de Poulkova*, **1** (1853): 349–85, esp. 383).

(5) A question considered by Maxwell in his essay *On the Stability of the Motion of Saturn's Rings* (Cambridge, 1859): esp. 67–8 (= *Scientific Papers*, **1**: 373–4), where he made reference to Struve's paper.

been discovered in celestial mechanics which may be perceived by the telescope but cannot be stopped up by the calculus.[6]

We must suppose the Rings to consist of matter in some of the states known to us and to be acted on by gravitation. Laplace was the first to show that a solid uniform ring is necessarily unstable and must fall on the body of the planet.[7] I have shown that a single thin ring loaded at one point of its circumference with a mass rather more than $4\frac{1}{2}$ times its own weight *might* permanently revolve about a central body. The adjustment of weight however must be very accurate it must not be less than .8159 or greater than .8279 of the whole.[8] The magnitude of such a weight would render it impossible to escape observation and the delicacy of adjustment would soon be impaired by the immense forces called into play tending to break or bend the ring – forces under which the strongest materials known to us would behave like sand or wax.

The hypothesis of a ring forming a solid mass is therefore untenable. That of the coexistence of many such rings has still less mechanical possibility. We are therefore obliged to regard the rings as consisting of matter the parts of which are not rigidly connected.

I have shown that it is possible for a ring consisting of a single row of unconnected particles to revolve permanently about the planet under certain conditions and that many such rings may revolve concentrically about the planet but that their mutual perturbations will gradually increase till some of them are thrown into a state of confusion.

The conclusion at which I arrived in my former paper was – 'that the only system of rings which can exist is one composed of an indefinite number of unconnected particles revolving round the planet with different velocities according to their respective distances. These particles may be arranged in a series of narrow rings, or they may move through each other irregularly. In the first case the destruction of the system will be very slow; in the second case it will be more rapid, but there may be a tendency towards an arrangement in narrow rings which may retard the process'.[9]

I was then of the opinion that 'When we come to deal with collisions among bodies of unknown number size and shape we can no longer trace the mathematical laws of their motion with any distinctness' (§(32)).[10] I

(6) Compare *Saturn's Rings*: 1 (= *Scientific Papers*, **1**: 290–1).

(7) P. S. de Laplace, *Traité de Mécanique Céleste*, 5 vols. (Paris, An VII [1799]–1825), **2**: 155–66; see *Saturn's Rings*: 2–4 (= *Scientific Papers*, **1**: 292–4), and Volume I: 440–2.

(8) Maxwell, *Saturn's Rings*: 15–16 (= *Scientific Papers*, **1**: 307–8); and see Volume I: 564.

(9) Maxwell, *Saturn's Rings*: 67 (= *Scientific Papers*, **1**: 373).

(10) Maxwell, *Saturn's Rings*: 53 (= *Scientific Papers*, **1**: 354). Compare his remark in his letter

propose now to take up the question at this point and to endeavour to throw some light on the theory of a confused assemblage of jostling masses whirling round a large central body.

I shall not enter into the theory of the *formation* of a ring (see papers by M^r^ Daniel Vaughan[(11)]). I shall suppose the rings already existing and find the conditions of their stability and the rate of their decay. In my former paper I restricted myself to cases in which no collisions take place and in which the mutual gravitation of the planet and the particles is the only force in action. It appears however that in this case that particles of each ring must be at a certain distance apart and each ring of particles must be at a considerable distance from the next so that even at the immense distance from which we view them we should see the discontinuity of their structure by their almost perfect transparency and the feebleness of their illumination. Whatever, therefore may be the condition of the dark inner rings, the outer rings are too substantial to have a constitution of this kind. The particles composing them must be so near together that they influence each other far more by collisions and jostling than by the attraction of gravitation. The individual masses, even if large compared with meteoric stones or even mountains are probably so small compared with planets that the effect of the mutual gravitation of two of them on the velocity or path of either may be entirely neglected. We have therefore to consider the motions of an immense number of small bodies occupying a space in the form of a flat ring or rings of which the thickness is less than a thousandth of the diameter and whirling round a planet in the centre with velocities nearly corresponding to their respective distances.

Collisions will occur between these bodies and after collision each body will be projected with a velocity which will carry it into some other part of the cloud of particles, where it will meet with other particles moving with a velocity different from its own. Another collision will thus occur and in this way the jostling of the particles once begun will be carried on throughout the

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to William Thomson of 14 November 1857: 'The general case of a fortuitous concourse of atoms each having its own orbit & excentricity is a subject above my powers at present' (Volume I: 555).

(11) Maxwell is alluding to a series of papers by the American astronomer Daniel Vaughan: 'On the form of satellites revolving at small distances from their primaries', *Phil. Mag.*, ser. 4, **20** (1860): 409–18; 'On the stability of satellites in small orbits, and the theory of Saturn's rings', *ibid.*, **21** (1861): 263–74; 'On phenomena which may be traced to the presence of a medium pervading all space', *ibid.*, **21** (1861): 507–15; and 'Static and dynamic stability in the secondary systems', *ibid.*, **22** (1861): 489–97. In his paper 'On phenomena which may be traced to the presence of a medium pervading all space': 508 Vaughan had discussed Maxwell's conclusions in the essay on *Saturn's Rings*; see Brush, Everitt and Garber, *Maxwell on Saturn's Rings*: 23.

system and kept up on account of the different velocities of the different parts of the system so as to produce a continual loss of energy and a decay in the motion of the rings.

The principles by which problems of this kind can be treated were first discussed by Prof^r Clausius in a paper 'on the nature of the motion which we call heat',*(12) and were applied to several cases in gaseous physics by myself in a paper on the Motions and Collisions of Perfectly Elastic Spheres.†(13) Professor Clausius‡ has since pointed out some mistakes in the latter parts of this paper(14) in his paper on the conduction of Heat in Gases.(15) I hope to be able to complete a correct investigation of diffusion and conduction of heat in gases and to establish the distribution of velocities among the particles in all cases,(16) but at present I must confine my attention to the effects of the collisions of rough imperfectly elastic bodies, in which case the complete theory is much more difficult and we must be satisfied with certain approximations. This is less to be regretted as we do not know the coefficient of elasticity for the collisions of the pieces of Saturns Rings and therefore we can expect only approximate numerical results.

We compare the motion of any particle at any instant with that which it would have if it revolved uniformly about the central body at that distance. We find that it differs from it by a small quantity which may be resolved into three components, a radial component (x) and a tangential component (y) and a component normal to the orbit (z). These components constitute the *velocity of agitation* of the particle as distinguished from the velocity of the ring at that point which is that corresponding to a circular orbit. If we trace this velocity of agitation for a single particle we find that it moves among the different concentric rings as if it described an ellipse in the same time as the time of revolution but in the opposite direction the major axis being in the direction of the radius vector and equal to twice the minor. It also oscillates

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(12) Rudolf Clausius, 'On the kind of motion which we call heat', *Phil. Mag.*, ser. 4, **14** (1857): 108–27; trans. from 'Ueber die Art der Bewegung welche wir Wärme nennen', *Ann. Phys.*, **100** (1857): 353–80.

(13) J. C. Maxwell, 'Illustrations of the dynamical theory of gases. Part I. On the motions and collisions of perfectly elastic spheres', *Phil. Mag.*, ser. 4, **19** (1860): 19–32 (= *Scientific Papers*, **1**: 377–91). See Volume I: 606–11.

(14) See Number 207.

(15) Rudolf Clausius, 'On the conduction of heat by gases', *Phil. Mag.*, ser. 4, **23** (1862): 417–35, 512–34; see Number 207 note (1).

(16) See Number 263.

perpendicularly to the plane of the ring in a period rather less than that of revolution.

This is what would take place if the particle did not meet with any other particle in its course, but we know that other particles exist and can easily calculate the chance of its not being struck for a given time. In this way we can deduce an equation connecting the velocity of agitation of particles at the instant of their projection with that which they have when they suffer their next collision. On account of the difference of mean motion of the different concentric rings the motion of agitation among the particles when they meet is greater than that with which each was originally projected in the ring from which it came so that if the particles were perfectly elastic the motion of agitation would increase continually till the rings were dispersed in a cloud. But if the particles are inelastic the velocities after collision are generally less than before it, and thus the increase in the motion of agitation due to the interpenetration of particles belonging to different rings, and the decay of the motion of agitation due to the imperfect elasticity of the striking particles, may produce a kind of equilibrium or steadiness of motion in certain cases, while in other cases the motion of agitation will continually increase or diminish, till some change is effected in the arrangement of the system.

In studying the nature of the motion of agitation we shall find that it does not consist of a system of disturbances in the motion distributed alike in all directions but that the directions and velocities are distributed differently in different directions. The measure of the agitation in a given direction which we shall adopt is found by multiplying half the mass of each particle by the square of the motion of agitation resolved in that direction. This we shall call the Energy of Agitation in that direction. It appears that the energy of agitation is greatest in one direction, least in a direction at right angles to this and of intermediate value in a direction normal to the plane of these two directions, and that its value in any other direction is found by the same rules as are used to determine 'moments of inertia' and other mathematical quantities having an arrangement similar to that of the diameters of an ellipsoid. The sum of the energies in three rectangular directions is always equal to the total energy of agitation and the absolute energy is equal to the total energy of agitation together with the total energy due to the motion of the particles in mass.

We have next to examine the effect of collisions on the distribution of the agitation. We find that the total energy of agitation is diminished in the ratio of p to 1 where p has a value depending on the elasticity and on the nature of the bodies. The difference of the energy of agitation in any two of the principal axes is also diminished in the ratio of q to 1. The investigation of the values of p and q must be attended to separately.

[3] [ON THE MOTION AND COLLISION OF PARTICLES]

To find the relations between the velocities and rotations of two bodies of any form before and after impact.

I have considered the case of the collisions of perfectly elastic bodies of any form in a paper on the Dynamical Theory of Gases, (Philosophical Magazine July 1860)[(17)] and have shown that the average vis viva of translation of every particle tends to become equal after many such collisions, and that the vis viva of rotation of each particle about each of its principal axes is equal and that the whole vis viva of rotation of each particle is equal to its vis viva of translation.[(18)] The equality of the vis viva of particles of different sizes leads to an explanation of Gay Lussacs law of atomic volumes of gases[(19)] and the relation between the vis viva of translation and rotation leads to the result that Bernoullis hypothesis[(20)] in its simplest form will not explain the relation between the specific heat of air at constant pressure and at constant volume.[(21)]

When the particles are not perfectly elastic, the motion of agitation cannot be kept up without some external cause and the investigation of the question becomes much more complicated than in the case where the motion of agitation if the particles are confined within an elastic vessel is self sustaining and capable of attaining a constant state. The external cause which sustains the motion of agitation in the case of Saturn's rings is the different velocities of contiguous portions of the rings and the energy which is lost by collision is made up by a supply drawn partly from the energy of motion of the rings round Saturn and partly from the potential energy of their gravitation towards him. As this source of energy is gradually diminished the form of the rings is gradually altered.

As we cannot investigate the effects of the collisions of inelastic bodies so easily as when the elasticity is perfect and as our only object is to obtain approximate numerical values for p & q we shall simplify the calculations by the following assumptions.

1st That the principal moments of inertia of each body are equal and represented by $M_1 k_1^2$ and $M_2 k_2^2$.

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(17) J. C. Maxwell, 'Illustrations of the dynamical theory of gases. Part III. On the collision of perfectly elastic bodies of any form', *Phil. Mag.*, ser. 4, **20** (1860): 33–7 (= *Scientific Papers*, **1**: 405–9).

(18) On the equipartition theorem see Number 207 para. 7.

(19) Viz. 'Avogadro's hypothesis': see Number 259 §4, esp. notes (13) and (14).

(20) On Daniel Bernoulli and the kinetic theory of gases see Numbers 257, 263 and 377.

(21) See Number 207 para. 7.

2nd That the points of the two bodies at which the impact takes place are at distances a_1 a_2 from their respective centres of gravity.
3rd That at a certain stage of the collision these two points are at rest relatively to each other on account of the action of the 'impulse of compression' which we may call R.
4th That the whole impulsive force acting between the two bodies consists of the impulse of compression R, and the impulse of restitution R' and that these two forces act in the same direction and are in the ratio of 1 to e where e is the coefficient of elasticity of impact for the two bodies.

It is this part of our assumption which is most precarious. It supposes that there is no slipping between the bodies and that the value of e is the same for normal and for oblique impact. It is probable that the tangential impulse, when much smaller than the normal is independent of it but when the normal impulse is small the tangential impulse will have a maximum value equal to the normal impulse multiplied by a coefficient of impulsive friction.

In an investigation like the present in which we know so little about the bodies in motion it would not be advisable to begin by the introduction of complicated conditions arising from laws of friction and collision which at best are empirical.

We therefore begin with two rough spherical masses M_1 & M_2 whose radii are a_1 and a_2 and radii of gyration k_1 & k_2. Let the direction cosines of the line drawn from M_1 and M_2 at collision be l, m, n.

Let the velocities of the centres of gravity resolved along the axes of x y z be

$$\begin{array}{lll} u_1\, v_1\, w_1 \quad \text{and} \quad u_2\, v_2\, w_2 & & \text{before impact} \\ \bar{u}_1\, \bar{v}_1\, \bar{w}_1 \quad \text{and} \quad \bar{u}_2\, \bar{v}_2\, \bar{w}_2 & & \text{at great compression} \\ u_1'\, v_1'\, w_1' \quad \text{and} \quad u_2'\, v_2'\, w_2' & & \text{after restitution.} \end{array}$$

Let the angular velocities of rotation about the axes x y z be denoted according to the same system of accentuation and suffixes by

$$p \;\; q \;\; r.^{(22)}$$

Let the components of the impulse of compression be

$$X \;\; Y \;\; Z$$

and those of the impulse of restitution according to our hypothesis

$$eX \;\; eY \quad \& \quad eZ,$$

those of the whole impulse will be

$$(1+e)\,X \quad (1+e)\,Y \quad \& \quad (1+e)\,Z.$$

(22) p and q are here used differently from usage *supra*; see *infra* where Maxwell uses p' and q' for p and q as used in §2.

The equations of motion are now easily written down, but as their number is great we may confine ourselves to those relating to the axis of x and to the instant of greatest compression.

$$\bar{u}_1 = u_1 + \frac{1}{M_1}X \quad \bar{p}_1 = p_1 + \frac{a_1}{M_1 k_1^2}(mZ-nY)$$

$$\bar{u}_2 = u_2 - \frac{1}{M_2}X \quad \bar{p}_2 = p_2 + \frac{a_2}{M_2 k_2^2}(mZ-nY).$$

To find the final velocities we must substitute an accent for the bar and multiply the impulse by $(1+e)$.

The velocities of the striking points in the direction of x are

$$u_1 - (mr_1 - nq_1)\,a_1 \quad \text{and} \quad u_2 + (mr_2 - nq_2)\,a_2.$$

At the instant of greatest compression these points have no relative motion, or

$$\bar{u}_2 - \bar{u}_1 + m(a_1\bar{r}_1 + a_2\bar{r}_2) - n(a_1\bar{q}_1 + a_2\bar{q}_2) = 0.$$

Substituting the values of these velocities we find

$$\left(\frac{1}{M_1}+\frac{1}{M_2}\right)X + \left(\frac{a_1^2}{M_1k_1^2}+\frac{a_2^2}{M_2k_2^2}\right)((m^2+n^2)\,X - lmY - nlZ)$$
$$= u_2 - u_1 + m(a_1r_1 + a_2r_2) - n(a_1q_1 + a_2q_2)$$

with two other equations related to y & z as this is to x. Multiplying the first by $\left(\frac{1}{M_1}+\frac{1}{M_2}+l^2\left(\frac{a_1^2}{M_1k_1^2}+\frac{a_2^2}{M_2k_2^2}\right)\right)$ the second by $lm\left(\frac{a_1^2}{M_1k_1^2}+\frac{a_2^2}{M_2k_2^2}\right)$ and the third by $nl\left(\frac{a_1^2}{M_1k_1^2}+\frac{a_2^2}{M_2k_2^2}\right)$ and adding we obtain the value of X

$$X = \frac{1}{\dfrac{a_1^2+k_1^2}{M_1k_1^2}+\dfrac{a_2^2+k_2^2}{M_2k_2^2}}\left\{u_2 - u_1 + \frac{\dfrac{a_1^2}{M_1k_1^2}+\dfrac{a_2^2}{M_2k_2^2}}{\dfrac{1}{M_1}+\dfrac{1}{M_2}}\,l\{l(u_2-u_1)\right.$$
$$\left. + m(v_2-v_1) + n(w_2-w_1)\} + m(a_1r_1+a_2r_2) - n(a_1q_1+a_2q_2)\right\}.$$

In the same way we find

$$mZ - nY = \frac{1}{\dfrac{a_1^2+k_1^2}{M_1k_1^2}+\dfrac{a_2^2+k_2^2}{M_2k_2^2}}\{m(w_2-w_1) - n(v_2-v_1)$$
$$-(m^2+n^2)\,(a_1p_1+a_2p_2) + lm(a_1q_1+a_2q_2) + ln(a_1r_1+a_2r_2)\}.$$

If we write $\dfrac{a_1^2+k_1^2}{M_1 k_1^2}+\dfrac{a_2^2+k_2^2}{M_2 k_2^2} = \dfrac{2}{A}$ and $\dfrac{\dfrac{a_1^2}{M_1 k_1^2}+\dfrac{a_2^2}{M_2 k_2^2}}{\dfrac{1}{M_1}+\dfrac{1}{M_2}} = B$

in these expressions and substitute them in the equations for the final velocities

$$u_1' = u_1+\frac{1+e}{M_1}X \quad p_1' = p_1+\frac{(1+e)\,a_1}{M_1 k_1^2}(mZ-nY)$$

(23)
$$u_1' = u_1+\frac{1+e}{M_1}\frac{A}{2}\{u_2-u_1+Bl(l(u_2-u_1)+m(v_2-v_1)+n(w_2-w_1)) \\ +m(a_1 r_1+a_2 r_2)-n(a_1 q_1+a_2 q_2)\}$$

$$p_1' = p_1+\frac{(1+e)\,a}{M_1 k_1^2}\frac{A}{2}\{m(w_2-w_1)-n(v_2-v_1)-(m^2+n^2)\,(a_1 p_1+a_2 p_2) \\ +lm(a_1 q_1+a_2 q_2)+ln(a_1 r_1+a_2 r_2)\}.$$

The other ten equations for the components of velocity and rotation may be easily written down from these

We have next to determine the relations between the energy of agitation before and after impact. For this purpose we suppose the coordinate axes to be taken so as to coincide with the principal axes of agitation. We then square the equations for u_1' and p_1' remembering that all terms containing products of different components of the velocities will disappear on summation, so that in obtaining mean values we retain only those terms containing squares of velocities. The following equations are therefore true of the mean squares of the quantities

$$u_1'^2 = u_1^2-\frac{1+e}{M_1}A\{1+Bl^2\}\,u_1^2+\frac{(1+e)^2A^2}{4M_1^2}\{(1+2Bl^2+B^2l^4)\,(u_1^2+u_2^2) \\ +B^2l^2m^2(v_2^2+v_1^2)+B^2l^2n^2(w_1^2+w_2^2)+m^2(a_1^2 r_1^2+a_2^2 r_2^2)+n^2(a_1^2 q_1^2+a_2^2 q_2^2)\}$$

$$p_1'^2 = p_1^2-\frac{(1+e)\,a_1^2}{M_1 k_1^2}A(m^2+n^2)\,p_1^2+\frac{(1+e)^2A^2a_1^2}{4M_1^2 k_1^4}\{m^2(w_1^2+w_2^2) \\ +n^2(v_1^2+v_2^2)+(m^4+2m^2n^2+n^4)\,(a_1^2 p_1^2+a_2^2 p_2^2) \\ +l^2m^2(a_1^2 q_1^2+a_2^2 q_2^2)+l^2n^2(a_1^2 r_1^2+a_2^2 r_2^2)\}.$$

Now $l\,m\,n$, the direction cosines of the line of centres at impact are independent of the velocities and by integrating over the surface of a sphere we find that

(23) Brush, Everitt and Garber, *Maxwell on Saturn's Rings*: 181–3 print a preliminary draft of the following paragraphs, concluding at '... l^2m^2 is $\frac{1}{15}$'. There is another draft folio in ULC Add. MSS 7655, V, c/45a.

the mean value of the square of a cosine such as l^2 is $\frac{1}{3}$, that of the fourth power as l^4 is $\frac{1}{5}$ and that of the square of a product as l^2m^2 is $\frac{1}{15}$.[(24)] The equations may therefore be written

$$u_1'^2 = u_1^2 - \frac{(1+e)\,A}{M_1}\left(1+\frac{1}{3}B\right)u_1^2 + \frac{(1+e)^2A^2}{4M_1^2}\left\{\left(1+\frac{2}{3}B+\frac{1}{5}B^2\right)(u_1^2+u_2^2)\right.$$

$$\left. +\frac{1}{15}B^2(v_1^2+v_2^2+w_1^2+w_2^2)+\frac{1}{3}(a_1^2r_1^2+a_2^2r_2^2+a_1^2q_1^2+a_2^2q_2^2)\right\}$$

$$p_1'^2 = p_1^2 - \frac{2}{3}\frac{(1+e)\,Aa_1^2}{M_1k_1^2}p_1^2 + \frac{(1+e)^2A^2a_1^2}{4M_1^2k_1^2}\left\{\frac{1}{3}(v_1^2+v_2^2+w_1^2+w_2^2)\right.$$

$$\left. +\frac{8}{15}(a_1^2p_1^2+a_2^2p_2^2)+\frac{1}{15}(a_1^2q_1^2+a_2^2q_2^2+a_1^2r_1^2+a_2^2r_2^2)\right\}$$

with ten other equations which may be written down from symmetry.

When, as in the case of Saturns rings, the motion of agitation is sustained by a cause which affects the motions of translation without directly altering the velocity of rotation the energy of rotation must be sustained by the energy of translation and we must have $p_1'^2 = p_1^2$. The second equation then becomes

$$M_1k_1^2p_1^2 = \frac{(1+e)\,A}{8}\left\{v_1^2+v_2^2+w_1^2+w_2^2+\frac{8}{5}(a_1^2p_1^2+a_2^2p_2^2)\right.$$

$$\left. +\frac{1}{5}(a_1^2q_1^2+a_2^2q_2^2+a_1^2r_1^2+a_2^2r_2^2)\right\}.$$

Since the right hand side of this expression would remain the same if the suffixes were exchanged, we must have

$$M_1k_1^2p_1^2 = M_2k_2^2p_2^2 = P \quad \text{suppose}$$

or the energy of rotation about the axis of x is the same for large & small particles.

Let
$$\frac{a_1^2}{M_1k_1^2}+\frac{a_2^2}{M_2k_2^2} = 2C$$

then if we write $Mk^2q^2 = Q$ and $Mk^2r^2 = R$ we find

$$P = \frac{(1+e)\,A}{8}\left\{v_1^2+v_2^2+w_1^2+w_2^2+\frac{2}{5}C(8P+Q+R)\right\}.$$

If we write down the two similar equations and add, we shall have

$$P+Q+R = \frac{(1+e)\,A}{4}\{u_1^2+v_1^2+w_1^2+u_2^2+v_2^2+w_2^2+2C(P+Q+R)\}$$

(24) Maxwell's values are discussed and confirmed by Fuller, 'James Clerk Maxwell's Cambridge manuscripts': 652–4.

writing V_1^2 & V_2^2 for the squares of the resultant velocities

$$2(P+Q+R)\,(2-(1+e)\,AC) = (1+e)\,A(V_1^2+V_2^2).$$

When $e=1$ this expression becomes

$$= k_1^2(p_1^2+q_1^2+r_1^2)+k_2^2(p_2^2+q_2^2+r_2^2) = V_1^2+V_2^2.$$

When $e=0$ it becomes

$$=\frac{2}{A}(P+Q+R) = V_1^2+V_2^2.^{(25)}$$

Subtracting the equation for Q from that for P we get

$$(P-Q)\left(\frac{2}{A}-\frac{7}{10}(1+e)\,C\right)=\frac{1+e}{4}(v_1^2+v_2^2-(u_1^2+u_2^2)).$$

Returning to the equation for $u_1'^2$ and adding the two similar equations

$$V_1'^2 = V_1^2-\frac{(1+e)\,A}{M_1}\left(1+\frac{1}{3}B\right)V_1^2+\frac{(1+e)^2A^2}{4M_1^2}\left(1+\frac{2}{3}B+\frac{1}{3}B^2\right)(V_1^2+V_2^2)$$
$$+\frac{(1+e)^3A^3C}{6M_1^2(2-(1+e)\,AC)}(V_1^2+V_2^2)$$

which gives the value of $V_1'^2$ in terms of V_1^2 and V_2^2. As we do not know the relation between V_1^2 & V_2^2 we shall begin with the case of equal masses and $V_1^2=V_2^2$. Then we have $A=\dfrac{Mk^2}{a^2+k^2}$ $B=\dfrac{a^2}{k^2}$ $C=\dfrac{a^2}{Mk^2}$ and if we make $V'^2 = p'V^2$ we find

$$p' = 1-\frac{(1+e)\,(a^2+3k^2)}{3(a^2+k^2)}+\frac{(1+e)^2(a^4+2a^2k^2+3k^4)}{6(a^2+k^2)^2}$$
$$+\frac{(1+e)^3a^2k^4}{3(a^2+k^2)^2((1-e)\,a^2+2k^2)}$$

p' is the ratio of the whole energy after collision to the whole energy before collision on an average of all possible cases.[26]

In the same way we get by subtracting v'^2 from u'^2 and putting

$$u'^2-v'^2 = q'(u^2-v^2)$$

$$q' = 1-\frac{(1+e)\,(a^2+3k^2)}{3(a^2+k^2)}+\frac{(1+e)^2(\frac{2}{5}a^4+2a^2k^2+3k^4)}{6(a^2+k^2)^2}$$
$$+\frac{(1+e)^3a^2k^4}{6(a^2+k^2)^2(4(a^2+k^2)-\frac{7}{5}a^2(1+e))}.$$

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(25) Read: $\dfrac{2}{A}(2-AC)\,(P+Q+R) = V_1^2+V_2^2$.

(26) p' and q' here denote p and q as used in §2 *supra*.

When $e = 1$ we find $p' = 1$ or the energy is the same before & after impact. When $e = 0$ and $k^2 = \frac{2}{5}a^2$ as in a solid sphere,

$$p' = \frac{1803}{2646} = \frac{601}{882} \quad \frac{1}{p'} = 1.467$$

$$q' = \frac{1837}{3087} \qquad \frac{1}{q'} = 1.681.^{(27)}$$

[4] [ON COLLISION PROBABILITIES]

To find an expression for the number of particles which are struck in unit of time and for the proportion of these which describe an angle θ round the central body before being struck again.

Let x be the number of particles which describe an angle θ without being struck – then while they are describing the additional angle $d\theta$, a number of these will be struck depending on x on the distribution of other particles and on $d\theta$ which may be expressed by

$$-dx = \frac{x}{\lambda}d\theta$$

whence

$$x = Ce^{-\frac{\theta}{\lambda}}.$$

Let N = the whole number of particles then the number struck in unit of time will be $\frac{N}{\lambda}\frac{d\theta}{dt}$

and the number of these which reach a distance θ without being struck will be $x = \frac{N}{\lambda}\frac{d\theta}{dt}e^{-\frac{\theta}{\lambda}}$

and the number of these which will be struck between θ and $\theta + d\theta$ will be

$$-dx = \frac{N}{\lambda^2}\frac{d\theta}{dt}e^{-\frac{\theta}{\lambda}} \quad ^{(28)}$$

(27) There are slips in Maxwell's arithmetic:

$$p' = \frac{1813}{2646} = \frac{37}{54}, \quad \frac{1}{p'} = 1.459$$

$$q' = \frac{1921}{3087}, \quad \frac{1}{q'} = 1.607.$$

(28) Read: $-dx = \frac{N}{\lambda^2}\frac{d\theta}{dt}e^{-\frac{\theta}{\lambda}}d\theta.$

λ is the mean value of the angle described by a particle between successive collisions.

[5] [ON THE ENERGIES OF THE PARTICLES]

To find expressions for the integrals of $\frac{1}{2}Mu^2$ $\frac{1}{2}Mv^2$ $\frac{1}{2}Mw^2$ $\frac{1}{2}Mz^2$ and Muv in terms of the energies of agitation along the principal axes and the inclination of these axes to the radius vector.

Let a be the sum and b the difference of the energies of agitation in the principal axes in the plane of the orbit and let α be the angle between the radius vector and the greater axis measured in the direction of rotation. Let c be the energy in the normal direction. Then by the ordinary investigation of moments of inertia, internal pressures &c &c,

$$\frac{1}{2}\sum Mu^2 = \frac{1}{2}a+\frac{1}{2}b\cos 2\alpha$$

$$\frac{1}{2}\sum Mv^2 = \frac{1}{2}a-\frac{1}{2}b\cos 2\alpha$$

$$\frac{1}{2}\sum Mw^2 = c$$

$$\sum Muv = \frac{1}{2}b\sin 2\alpha.$$

[6] [ON PARTICLE COLLISIONS]

To find the relation between the nature of the agitation of the particles just after being struck and just before the next collision.

The values of u^2 v^2 w^2 & uv are connected by the following equations

$$u^2 = u_0^2\cos^2\theta+4v_0^2\sin^2\theta+4u_0v_0\sin\theta\cos\theta \tag{1}$$

$$v^2 = v_0^2\cos^2\theta+\frac{1}{4}u_0^2\sin^2\theta-u_0v_0\sin\theta\cos\theta \tag{2}$$

$$w^2 = w_0^2\cos^2\mu\theta+n^2\mu^2z_0^2\sin^2\mu\theta-2n\mu z_0w_0\sin\mu\theta\cos\mu\theta \tag{3}$$

$$z^2 = z_0^2\cos^2\mu\theta+\frac{1}{n^2\mu^2}w_0^2\sin^2\mu\theta+\frac{2}{n\mu}z_0w_0\sin\mu\theta\cos\mu\theta \tag{4}$$

$$uv = \left(2v_0-\frac{1}{2}u_0\right)^{(29)}\sin\theta\cos\theta+u_0v_0(\cos^2\theta-\sin^2\theta) \tag{5}$$

$$wz = \left(\frac{w_0^2}{n\mu}-z_0^2n\mu\right)\sin\mu\theta\cos\mu\theta+z_0w_0(\cos^2\mu\theta-\sin^2\mu\theta). \tag{6}$$

(29) Read: $(2v_0^2-\frac{1}{2}u_0^2)$.

From these we obtain

$$u^2+4v^2 = u_0^2+4v_0^2$$
$$w^2 = n^2\mu^2z^2 = w_0^2+n^2\mu^2z_0^2$$

which are independent of θ.

In order to get the energy of the agitation of the particles projected in unit of time we must multiply the left side of each equation by $\frac{\frac{1}{2}NM}{\lambda}\frac{d\theta}{dt}$ and the right hand side by $\frac{\frac{1}{2}NM}{\lambda^2}\frac{d\theta}{dt}e^{-\frac{\theta}{\lambda}}d\theta$ and integrate from $\theta = 0$ to $\theta = \infty$. It will be sufficient to multiply the right side by $\frac{1}{\lambda}e^{-\frac{\theta}{\lambda}}d\theta$ and integrate remembering that

$$\int_0^\infty \sin m\theta\, e^{-\frac{\theta}{\lambda}}d\theta = \frac{\lambda^2 m}{1+\lambda^2m^2} \quad\text{and}\quad \int_0^\infty \cos m\theta\, e^{-\frac{\theta}{\lambda}}d\theta = \frac{\lambda}{1+\lambda^2m^2}.$$

We thus get from the equations (1), (2) &c the following

1 $(4\lambda^2+1)\,u^2 = (2\lambda^2+1)\,u_0^2+8\lambda^2v_0^2+4\lambda u_0\,v_0$

2 $(4\lambda^2+1)\,v^2 = (2\lambda^2+1)\,v_0^2+\frac{1}{2}\lambda^2u_0^2-\lambda u_0\,v_0$

3 $(4\mu^2\lambda^2+1)\,w^2 = (2\mu^2\lambda^2+1)\,w_0^2+2\mu^4\lambda^2n^2z_0^2-2\mu^2\lambda nw_0\,z_0$

4 $(4\mu^2\lambda^2+1)\,z^2 = (2\mu^2\lambda^2+1)\,z_0^2+2\frac{\lambda^2}{n^2}w_0^2+2\frac{\lambda}{n}w_0\,z_0$

5 $(4\lambda^2+1)\,uv = \left(2v_0^2-\frac{1}{2}u_0^2\right)\lambda+u_0\,v_0$

6 $(4\mu^2\lambda^2+1)\,wz = \left(\frac{w_0^2}{n\mu}-z_0^2\,n\mu\right)\mu\lambda+w_0\,z_0.$

From (4) and (6) we find that if $z^2 = z_0^2$, $wz = 0$ and since the equations must be true independent of the value of λ, $w_0\,z_0 = 0$ whence we find by (4) that $w^2 = w_0^2 = n^2\mu^2z^2$.

[7]

To express these relations in terms of $a\ b\ c\ \&\ \alpha$.(30)

Let these quantities be written $a_0\ b_0\ c_0\ \&\ \alpha_0$ when they refer to the particles when first projected. We have two equations independent of λ

$$5a-3b\cos 2\alpha = 5a_0-3b_0\cos 2\alpha_0$$
$$c = c_0 = n^2\mu^2z^2.$$

(30) The quantities a, b, c here denote half mean square values.

(1) − (2) gives $(4\lambda^2+1)\,2b\cos 2\alpha = \frac{15}{2}\lambda^2 a_0 + \left(2-\frac{9}{2}\lambda^2\right) b_0 \cos 2\alpha_0$

$\qquad + 5\lambda b_0 \sin 2\alpha_0$

(5) $\qquad (4\lambda^2+1)\, b\sin 2\alpha = \frac{3}{2}\lambda a_0 - \frac{5}{2}\lambda b_0\cos 2\alpha_0 + b_0 \sin 2\alpha_0$

whence $\qquad (2\cos 2\alpha - 5\lambda \sin 2\alpha)\, b = 2b_0 \cos 2\alpha_0.$

[8] [STEADY STATE CONDITIONS]

To find the values of $a_0\ b_0\ c_0\ \alpha_0$ and λ when the motion of agitation is exactly sustained.

Let us suppose that the result of the collision, on an average of all possible cases is to reduce the total energy of agitation in the ratio of p to 1 and to reduce the *difference* of energy in any two principal axes in the ratio of q to 1 while the direction of these principal axes remains unchanged. We shall calculate the numerical values of p and q by a separate investigation. We then have

$$a+c = p(a_0+c_0)$$
$$b = qb_0$$
$$\frac{1}{2}a - c = q\left(\frac{1}{2}a_0 - c_0\right)$$

whence
$$a = \frac{1}{3}\{2p(a_0+c_0)+q(a_0-2c_0)\}$$
$$b = qb_0$$
$$c = \frac{1}{3}\{p(a_0+c_0)-q(a_0-2c_0)\}.$$

Substituting these values of $a\ b\ c$ and omitting the suffixes, remembering that $a\ b\ c$ now refer to the velocities at projection only(31)

$$5\{(2p+q-3)\,a+2(p-q)\,c\} = 9(q-1)\,b\cos 2\alpha$$
$$(p+2q)\,c = (3+q-p)\,a$$
$$\{(4\lambda^2+1)\,q-1\}\,b\sin 2\alpha + \frac{5}{2}\lambda b\cos 2\alpha = \frac{3}{2}\lambda a$$
$$2(q-1)\cos 2\alpha = 5\lambda \sin 2\alpha.$$

(31) Errors here enter the algebra; see Fuller, 'Maxwell's Cambridge manuscripts': 645–7.

Whence we find

$$\begin{aligned} a &= 3\lambda(q-1)(p+2q)h \\ c &= 3\lambda(q-1)(3+q-p)h \\ b\sin 2\alpha &= 2(q-1)\{p(3q+1)-4q\}h \\ b\cos 2\alpha &= 5\lambda\{p(3q+1)-4q\}h \end{aligned}$$

where h is a quantity not yet determined. We also find the following equations in λ^2 p & q

$$\{\lambda^2(16q^2-16q+25)+4(q-1)^2\}(p(3q+1)-4q) = 9\lambda^2(q-1)(p+2q).$$

$$\lambda^2 = 4(q-1)^2$$

$$\times\frac{(3q+1)p-4q}{(64q^2+18q)(q-1)+100q-\{(3q+1)16q(q-1)+25(3q+1)-9(q-1)\}p}$$

$$= 2(q-1)^2\frac{(3q+1)p-4q}{32q^3-23q^2+41q-\{24q^3-16q^2+25q+17\}p}.$$

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LETTER TO HERMANN HELMHOLTZ[1]

12 APRIL 1864

From the original in the Akademie-Archiv, Berlin[2]

Dear Sir

I have been a long time getting my instrument for mixing colours[3] put right but it is now ready.

Can you come and take lunch with us on Saturday[4] about half past one o Clock and then we can have light to analyze.

Yours truly
J. CLERK MAXWELL

8 Palace Gardens Terrace, W.
1864 April 12

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(1) On 14 April 1864 Helmholtz read his Croonian Lecture to the Royal Society: see Number 279 note (5).

(2) Nachlass Helmholtz 305 Briefe Maxwell, Akademie-Archiv, Berlin.

(3) See Number 202 esp. note (3).

(4) 16 April 1864.

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LETTER TO JOHN TYNDALL

20 APRIL 1864

From the original in the Smithsonian Institution Libraries(1)

8 Palace Gardens Terrace
W
1864 Ap 20

Dear Tyndall

It would give me much pleasure to belong to the Philosophical Club.(2) I got your letter at King's College today.

Yours truly
J. Clerk Maxwell

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(1) Dibner Library, Smithsonian Institution Libraries, Washington, DC.

(2) Founded in 1847, a group of leading members of the Royal Society: 'The purpose of the Club is to promote as much as possible the scientific objects of the Royal Society, to facilitate intercourse between those Fellows who are actively engaged in cultivating the various branches of Natural Science & who have contributed to its progress; to increase the attendance at the Evening Meetings & to encourage the contribution & the discussion of Papers.'; See 'Objects & Rules of the Philosophical Club' in 'Minute Book, Volume I' (Royal Society, London); and also T. G. Bonney, *Annals of the Philosophical Club of the Royal Society* (London, 1919). Maxwell was elected a member on 25 April 1864; see 'Minute Book, Volume I': 309–10; and Bonney, *Annals of the Philosophical Club*: 57.

PAPER ON THE MOTION OF THE EARTH THROUGH THE ETHER[1]

circa 24 APRIL 1864[2]

From the original in the University Library, Cambridge[3]

ON AN EXPERIMENT TO DETERMINE WHETHER THE MOTION OF THE EARTH INFLUENCES THE REFRACTION OF LIGHT

by J. Clerk Maxwell, F.R.S.

According to an experiment of M Fizeau*[4] the propagation of light in a tube carrying a stream of water takes place with greater velocity in the direction in which the water moves than in the opposite direction. This phenomenon of acceleration and retardation was not observed when air was substituted for water in the tube, and the amount of the effect in the case of water was much smaller than it would have been if the whole motion of the water had been compounded with that of the light.

The explanation given by M. Fizeau is founded on the hypothesis of Fresnel with respect to the constitution of the luminiferous medium within dense bodies, according to which the motion of the body only partially affects the motion of the ether within it so as to make its average velocity $= v(\mu^2-1)$, where v is the velocity of the body and μ its index of refraction.[5]

(1) The paper was submitted to the Royal Society for publication, but was withdrawn: see note (6) and Maxwell's letter to Stokes of 6 May 1864 (Number 228).

(2) The paper is endorsed 'Rec'd. 26 April 1864'; in the paper itself Maxwell refers to observations recorded on 23 April 1864.

(3) ULC Add. MSS 7655, V, b/15.

(4) Hippolyte Fizeau, 'Sur les hypothèses relatives a l'éther lumineux. Et sur une expérience qui parait démontrer que le mouvement des corps change la vitesse avec laquelle la lumière se propage dans leur intérieur', *Ann. Chim. Phys.*, ser. 3, **57** (1859): 385–404; (trans.) 'On the effect of the motion of a body upon the velocity with which it is traversed by light', *Phil. Mag.*, ser. 4, **19** (1860): 245–58. The paper was presented to the Académie des Sciences on 29 September 1851, and reported in the *Comptes Rendus*, **33** (1851): 349–55. Maxwell had inquired about this report of Fizeau's experiment in a letter to Stokes of 8 May 1857 (Volume I: 503).

(5) [A. J. Fresnel,] 'Lettre de M. Fresnel à M. Arago, sur l'influence du mouvement terrestre dans quelques phénomènes d'optique', *Ann. Chim. Phys.*, **9** (1818): 57–66. In 1810 Arago had carried out an experiment to detect the deviation of stellar light, finding no effect of the earth's motion on the refrangibility of light. See François Arago, 'Mémoire sur la vitesse de la lumière, lu à la première Classe de l'Institut, le 10 décembre 1810', *Comptes Rendus*, **36** (1853): 38–49. Fresnel proposed his hypothesis of partial ether drag in response to Arago's conclusion.

In fact if the density of the ether within the body is μ^2 times that outside it, and if we suppose the ether outside to be undisturbed by the motion of the body, and the motion of the ether inside to be independent of the shape of the body, then this law is a consequence of the law of continuity of the ether. But other modes of motion which would also satisfy the law of continuity would have been the result if our suppositions had been different, as for instance if we had supposed that ether cannot pass the boundary of the body, or that there is a resistance to the motion of the body through it.

Taking the hypothesis in its most simple shape as adopted by M Fizeau, if the velocity of the ether be reckoned with respect to the body itself then if the velocity of the ether outside the body is v that within will be $\frac{1}{\mu^2}v$ and in the same direction.

If the direction of propagation of a ray of light makes an angle θ with the direction of motion before incidence, and ϕ within the medium then the index of refraction will become

$$\mu\frac{1+\dfrac{v}{V}\cos\theta}{1+\dfrac{v}{V}\dfrac{1}{\mu}\cos\phi}$$

where μ is the index of refraction for the body at rest, and V is the velocity of Light.

Hence if P is the angle of a prism, D the minimum deviation of light due to the prism when at rest, and ϕ the direction of the ray within the prism when in the position of minimum deviation and δ the displacement due to the motion in circular measure

$$\delta = \frac{v}{V}\cos\phi\frac{\sin(\frac{1}{2}P+D)-\sin\frac{1}{2}P}{\cos\frac{1}{2}(P+D)}.$$

The deviation of the ray is increased by this quantity when the ether moves relatively to the prism in the same direction as the ray.[6]

(6) Maxwell ignores the compensating change in the density of the medium (which varies as μ^2; see 'Lettre de M. Fresnel à M. Arago': 62). According to Fresnel's theory the ether and the transparent medium satisfy a continuity equation at their boundary; this has the consequence that the retardation due to the refractive medium is not affected by the motion of the earth. Maxwell acknowledges his error, to which Stokes drew his attention, in his letter to Stokes of 6 May 1864 (Number 228). Stokes had himself established this consequence of Fresnel's theory in 1846 (see Number 228 note (4)). Maxwell gives a revised account of the problem in a letter to William Huggins of 10 June 1867 (Number 271).

If v is the velocity of the earth in its orbit then $\frac{v}{V}$ is the coefficient of aberration in circular measure or about $20''\frac{1}{2}$. For a prism whose angle is $60°$ and minimum deviation $49°\frac{1}{2}$

we find $$\delta = 17''.17 \cos\phi$$

where ϕ is the angle between the direction of motion and the ray within the prism.

The greatest displacement for one prism is therefore $17''.17$. For two similar prisms it is $31''.28$, and for three $39''.20$. A greater number of prisms would be of no use, as the displacement would diminish when the ray moves oppositely to the ether.

Hence a good spectroscope constructed so firmly that it might be turned round without altering the relative position of its parts and capable of reading to half a minute, might measure the displacement directly.

In the Annales de Chimie et de Physique for Feb. 1860 M Fizeau has described an experiment in which a ray of polarized light was passed through a series of inclined glass plates so as to rotate the plane of polarization.(7) The amount of this rotation depends on the index of refraction of the glass, and therefore may be made to indicate any change in that index. The methods by which disturbing influences were checked, and the effects multiplied are described in the memoir of M Fizeau. His result was that a displacement of the plane of polarization in the direction expected was obtained when the direction of the ray was coincident with or opposed to the Earth's motion and to an amount not discordant with the theory.(8)

It appears to me that it is as easy to measure the deviation of a marked line in the spectrum to one minute as to ascertain the plane of polarization of a ray

(7) H. Fizeau, 'Sur une méthode propre a rechercher si l'azimut de polarisation du rayon réfracté est influencé par le mouvement du corps réfringent – essai de cette méthode', *Ann. Chim. Phys.*, ser. 3, **58** (1860): 129–63. The paper was reported in the *Comptes Rendus*, **49** (1859): 717–23; an English summary of this report was appended to the translation of Fizeau's paper on the propagation of light in a tube carrying a stream of water, *Phil. Mag.*, ser. 4, **19** (1860): 258–60.

(8) Fizeau, 'Sur une méthode propre a rechercher si l'azimut de polarisation du rayon réfracté est influencé par le mouvement du corps réfringent': 162; 'Les rotations du plan de polarisation, produites par des piles de glaces inclinées, sont constamment plus grandes lorsque l'appareil est dirigé vers l'ouest, que lorsqu'il est dirigé vers l'est, l'observation étant faite vers le milieu du jour'. Fizeau concluded that the azimuth of the plane of polarisation of a refracted ray is influenced by the motion of the refracting medium, and that the motion of the earth has an effect upon the rotation of the plane of polarisation produced by a series of inclined glass plates. In his letter to Huggins of 10 June 1867 Maxwell questions the validity of Fizeau's observations and conclusions; see note (6).

to several minutes, so that I should prefer the direct method with prisms if it were possible.

But as we may have reason to suspect some mechanical displacement of the telescopes or prisms, or some alteration of temperature of the prisms between the times of observation, I have adopted the following arrangement which appears to me to be free from such sources of error, to afford a more distinct object to observe, and to double the displacement.

If we could send every ray back to the prism in the exact direction in which it emerged, then if there were no motion, it would return on its own path, but if the effect due to motion takes place, the refraction will be diminished on the return ray as much as it is increased in the direct one, so that the ray will no longer return to its starting point but will be displaced to an extent double of its original displacement.

Now by receiving the ray on the object glass of a telescope having a plane mirror at its principal focus to throw the light back, we can cause every ray to return in a direction exactly parallel to that in which it fell on the telescope. This is the arrangement for reflecting light used by M. Fizeau in his experiment on the velocity of light.*[(9)]

The apparatus I have made use of consists of a spectroscope constructed by Mr Becker.[(10)] Light is admitted through a tube at right angles to the axis of the first telescope and is reflected towards the object glass by a transparent plate of parallel glass at an angle of 45°. In the tube slides a screen with a vertical slit, in the middle of which is a vertical spider line. This spider line is placed so that its virtual image in the first surface of the glass plate coincides with the crossing of the spider lines of the telescope, and with the principal focus of the object glass.

Hence rays from the vertical spider line emerge from the telescope in parallel pencils. They then fall on the plane faces of the prisms, and after refraction the rays of each colour emerge parallel and fall on the second telescope, which has a mirror at its principal focus, and therefore returns the rays still parallel and in an exactly reverse direction. After returning through the prisms they enter the first telescope, and form an image of the vertical spider line at the cross lines of the first telescope. On looking through the eyepiece the image of the vertical slit and spider line is seen in contact with the crossing of the spider lines in the axis of the telescope.

* Comptes Rendus vol XXIX (1849) p 90[(9)]

(9) H. Fizeau, 'Sur une expérience relative à la vitesse de propagation de la lumière', *Comptes Rendus*, **29** (1849): 90–2. Maxwell had inquired about this paper in his letter to Stokes of 8 May 1857.

(10) See Number 214 note (5).

The coincidence of the images depends only on the relative position of the vertical slit, the glass plate and the cross lines, and on the accurate focussing of the telescopes. If the telescopes are moved relatively to each other different coloured rays fall on the mirror and form the image of the slit, but the position of the image as seen through the eye piece remains the same. The colour of the image is the resultant of the mixture of all that portion of the spectrum which falls on the second telescope with its mirror and therefore varies with the position of the prisms and telescopes, but the coincidence of the images of the lines is unaltered by such movement and the dispersion is so exactly compensated by returning the rays through the prisms that the image of the spider line is seen perfectly distinct.

The image of the slit subtends 14′ that of the spider line about 10 [′ so that] a displacement of the spider line could be easily observed. Now on April 23rd
a m
the motion of the earth at $11^{\mathrm{h}}\ 10^{\mathrm{m}}$ before noon was nearly horizontal and towards a point about 26° South of West. Hence if three prisms are used and placed so that the direction of the ray within the middle prism coincides with that of the earth motion, we shall have an increase of deviation $= 39''.2$ when the ray moves from West to East and an equal diminution of deviation when it returns. If the ray enters from the west and returns the total displacement will be $78''.4$ and by turning the instrument round so as to make the ray enter from the East this displacement will be reversed so that if our hypothesis be correct we should have a difference of position $= 156''.8'$ or two minutes and a half.

The experiment was tried in full sunlight at the proper time of day in the open air but no displacement could be observed in whatever direction the instrument was turned, provided the telescopes were correctly focussed.

It was also tried at night with candle light. The image was very distinct but no displacement could be detected.

Hence the result of the experiment is decidedly negative to the hypothesis about the motion of the ether in the form stated here.

I hope to repeat the experiment at a different time of year and in more exposed situations.[(a)] [(b)]

(a) {Maxwell} Note. The distinctness of definition can be shown by candlelight if desired. J.C.M.

(b) {Maxwell} Klinkerfues – Nachrichten v.d. Kön Ges. der Wiss. in Gottingen. Jan 31, 1866.[(11)]

(11) Wilhelm Klinkerfues, 'Fernere Mittheilungen über den Einfluss der Bewegung der Lichtquelle auf die Brechbarkeit eines Strahls', *Nachrichten von der Königl. Gesellschaft der Wissenschaften und der Georg–August–Universität zu Göttingen* (1866): 33–60. Klinkerfues had maintained 'dass die Bewegung eines Sterns, zerlegt nach der Richtung des Visions-Radius die Brechung des Strahls beeinflusst'; see his paper 'Ueber ein Einfluss der Bewegung der

Lichtquelle und eines brechenden Mediums auf die Richtung des gebrochenen Strahls', *ibid.* (1865): 157–60, 210. Maxwell's appended reference to Klinkerfues' 'Fernere Mittheilungen' probably dates from 1867–8. Compare his critical comment in his letter to William Huggins of 10 June 1867 (Number 271) on the use of an achromatic prism (as in Klinkerfues' experiments) to detect the motion of stars. It is possible that Huggins drew Maxwell's attention to Klinkerfues' paper. See Number 271 note (2) on the circumstances of Maxwell's letter to Huggins. Huggins included Maxwell's letter in his paper 'Further observations on the spectra of some of the stars and nebulae, with an attempt to determine therefrom whether these bodies are moving towards or from the earth...', *Phil. Trans.*, **158** (1868): 529–64, on 532–5; and commented critically, in terms similar to Maxwell's remarks on experiments with an achromatic prism, on Klinkerfues' 'Fernere Mittheilungen': 'as Klinkerfues employs an achromatic prism, it does not seem possible, by his method of observing, to obtain any information of the motion of the stars... [as] the difference of period... would be as far as possible, annulled' (on 531).

LETTER TO GEORGE GABRIEL STOKES

6 MAY 1864

From the original in the University Library, Cambridge(1)

8 Palace Gardens Terrace
London W.
1864 May 6

Dear Stokes

I have your letter and my paper.(2) Fizeau(3) does not lean with confidence on any theory of the dependence of the rotation of the plane of polarization on the velocity-ratio of propagation in the media.

The rotation of course depends on the ratios of *intensity* of rays polarized in different planes and passing through the inclined plate.

You have proved(4) that on Fresnels theory the *direction* of the refracted ray is not affected by the motion of the aether.(5)

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(1) ULC Add. MSS 7656, M 422. First published in Larmor, *Correspondence*, **2**: 23–5.

(2) Number 227. Stokes had clearly criticised Maxwell's assumption, in his paper 'On an experiment to determine whether the Motion of the Earth influences the Refraction of Light', that such an effect was to be anticipated. See Number 227 note (6) and Maxwell's revised discussion of the issue in his letter to William Huggins of 10 June 1867 (Number 271), where he acknowledges that no such effect is to be expected; and see note (5).

(3) H. Fizeau, 'Sur une méthode propre a rechercher si l'azimut de polarisation du rayon refracté est influencé par le mouvement du corps réfringent – essai de cette méthode', *Ann. Chim. Phy.*, ser. 3, **58** (1860): 129–63. See Number 227 note (7).

(4) G. G. Stokes, 'On Fresnel's theory of the aberration of light', *Phil. Mag.*, ser. 3, **28** (1846): 76–81, esp. 81 (= *Papers*, **1**: 141–7). Having considered refraction from a vacuum into a refracting medium, Stokes concluded that 'the laws of reflexion and refraction at the surface of a refracting medium will not be affected by the motion of the ether.' He went on to point out that 'the result is the same in the general case of refraction out of one medium to another, and reflexion at the common surface.'

(5) [A. J. Fresnel,] 'Lettre de M. Fresnel à M. Arago, sur l'influence du mouvement terrestre dans quelques phénomènes d'optique', *Ann. Chim. Phys.*, **9** (1818): 57–66. See Number 227 note (5). In 1865 Maxwell drafted a question for the 1866 Cambridge Mathematical Tripos on Stokes' interpretation of Fresnel's ether drag hypothesis: 'If the velocity of light in the luminiferous medium is inversely proportional to the index of refraction of the body through which the light passes, and if the luminiferous medium itself is not fixed in the body, show that the retardation due to a plate of glass interposed between two given points will be independent of the motion of the medium provided that that motion is inversely proportional to the square of the index of refraction and that the square of the ratio of the velocity of the medium to that of light may be neglected. / Hence show that on Fresnels hypothesis about the luminiferous medium in bodies all phenomena of reflexion & refraction will be the same whether the medium is carried along with the earth or does not partake of its motion.' (King's College London Archives, Maxwell Notebook (2), question (45)). See the question as set in *The Cambridge*

Have you[(6)] or Jamin[(7)] or anyone else done anything towards the determination of the intensity.

Fizeau rests his calculation on experiments with plates of different indices of refraction.

I am not inclined and I do not think I am able to do the dynamical theory of reflexion and refraction on different hypotheses & unless I see some good in getting it up, I would rather gather the result from men who have gone into the subject.[(8)]

If Fizeau has really found a phenomenon related to the earth's motion in space or rather in the luminiferous medium a great deal may be founded upon it independently of a good optical theory.

I think M. Faye corroborates Fizeaus experiment.[(9)] If the experiment is good it would be worth the while of an eminent astronomical observer to have the instrument properly mounted and so to work for a year determining frequently the direction and (proportional) velocity of the rush of æther through his observatory and so to have a log book of the earth's motion.

I have been reading Fraunhofer on the spectrum[(10)] and making estimates of the intensity of the pure colours by bringing each to an equality with my

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University Calendar for the Year 1866 (Cambridge, 1866): 475 (question (8)): '... Fresnel supposes that when bodies move in the luminiferous medium, the relative motion of the medium with respect to the body is in the same direction within and without the body, but in the proportion of 1 to μ^2: shew that the retardation due to a plate of glass interposed between two given points will be independent of the motion of the earth, if we neglect the square of the ratio of the earth's velocity to that of light.'

(6) Stokes had discussed the rotation of the plane of polarisation of plane-polarised light which has undergone reflection or refraction at the surface of a transparent uncrystallised medium, in his paper 'On the dynamical theory of diffraction', *Trans. Camb. Phil. Soc.*, **9** (1849): 1–62, esp. 47–61 (= *Papers*, **2**: 243–327). He had recently published a paper 'On the intensity of the light reflected from or transmitted through a pile of plates', *Proc. Roy. Soc.*, **11** (1862): 545–57 (= *Papers*, **4**: 145–56).

(7) Jules Jamin had recently published a paper entitled 'Note sur la théorie de la réfléxion et de la réfraction', *Ann. Chim. Phys.*, ser. 3, **59** (1860): 413–26. For Maxwell's discussion of this paper see his draft on the reflection and refraction of light (Number 236) and his letter to Stokes of 15 October 1864 (Number 237).

(8) See Numbers 236 and 237.

(9) H. A. Faye, 'Sur les expériences de M. Fizeau considerées au point de vue du mouvement de translation du système solaire', *Comptes Rendus*, **49** (1859): 870–5. Faye had presented his paper to the Académie des Sciences on 5 December 1859, following Fizeau's paper presented on 14 November 1859; see *Comptes Rendus*, **49** (1859): 717–23.

(10) Joseph Fraunhofer, 'Bestimmung des Brechungs- und Farben-Zerstreuungs-Vermögens verschiedener Glasarten, in bezug auf die Vervollkommnung achromatischer Fernröhre', *Annalen der Physik*, **56** (1817): 264–313. See Maxwell's comment (Volume I: 569) that 'Fraunhofers lines are the land marks of the spectrum'.

standard white light.[11] I used to try this by making an adjustment and then observing, then making another adjustment and so on, and I got very inconsistent results. Now the adjustment is made while I observe and I find the results not very inconsistent not worse I think than Fraunhofer's. Has any one else repeated such measurements. I can do them quickly and mean to get a good many to compare.

Dove believes in the comparison of intensity of light of different colours by his photometer with a photographic print.[12]

I mean to try it with two half-square prisms  joined with Canada Balsam[13] laid on in narrow streaks so that certain portions are transparent and others reflecting.

Yours truly
J. Clerk Maxwell

(11) See Volume I: 617, 619–20, 635–6.

(12) H. W. Dove, 'Beschreibung eines Photometers', *Ann. Phys.*, **114** (1861): 145–63; (trans.) 'On a new photometer', *Phil. Mag.*, ser. 4, **25** (1863): 14–27, esp. 24–5.

(13) A transparent gum used for cementing in optical apparatus.

LETTER TO ROBERT DUNDAS CAY[1]

12 JULY 1864

From the original in the Library of Peterhouse, Cambridge[2]

Craiglachie
July 12 1864

Dear Uncle

I enclose the receipt of £8..8..4.[3] I called on Willy[4] at his office in London and found him surrounded by Indian bridges.[5] After 16th inst address to Glenlair. I am very busy reducing observations of the electrical experiments at Kings College[6] so that the standard resistance coils may be made up before the British Asses meet.[7]

Your affᵗ nephew
J. Clerk Maxwell

(1) See Number 216.
(2) Maxwell MSS (25), Peterhouse.
(3) See Volume I: 682n.
(4) William Dyce Cay, Robert Cay's son.
(5) See Number 221.
(6) See Number 222 esp. note (3).
(7) See Number 232 note (5).

DRAFT ON THE DETERMINATION OF COEFFICIENTS OF SELF-INDUCTION[1]

circa SUMMER 1864[2]

From the original in the University Library, Cambridge[3]

WHEATSTONES BRIDGE[4] APPLIED TO THE DETERMINATION OF THE INDUCTION OF A CURRENT UPON ITSELF[5]

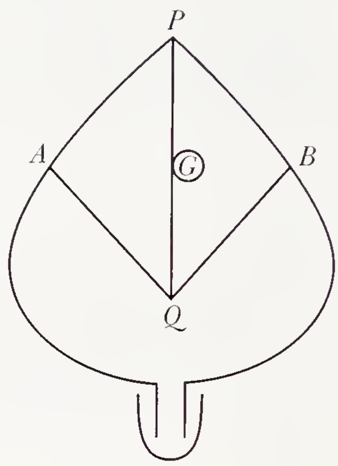

Figure 230,1

Let A and B be two points kept at uniform tension by an electrometer and let them be connected by the four conductors AP, PB, AQ, QB and let P and Q be connected by a conductor part of which acts as a galvanometer.

If the resistances in the four conductors form the terms of a proportion then after the currents have come to equilibrium there will be no current in PQ but at the first instant of the passage of the current if there be induction of some of the conductors on themselves there may be transient currents in PQ which may produce their effect in giving an impulse to the galvanometer. Let us investigate the conditions of this impulse.

(1) A draft of the section 'On the determination of coefficients of induction by the electric balance' in 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 475–7 (= *Scientific Papers*, **1**: 547–50).

(2) 'A dynamical theory of the electromagnetic field' was completed in October 1864: see Numbers 237, 238 and 239.

(3) ULC Add. MSS 7655, V, f/4.

(4) The term 'Wheatstone's bridge' is used by William Thomson, 'On the measurement of electric resistance', *Proc. Roy. Soc.*, **11** (1861): 313–28, esp. 313n (= *Math. & Phys. Papers*, **5**: 369n). Thomson preferred the term 'Wheatstone's balance' for Wheatstone's 'differential resistance measurer'; see Charles Wheatstone, 'An account of several new instruments and processes for determining the constants of a voltaic current', *Phil. Trans.*, **133** (1843): 303–27, esp. 323–5 and Figs. 5 and 6. See also Maxwell's historical note in the *Treatise*, **2**: 439n.

(5) Compare Maxwell's account of the use of Wheatstone's bridge in the *Treatise*, **1**: 398–408 (§§347–52).

Let the five conductors be	AP	PB	AQ	QB	PQ
The resistance in each	$r+s$	$r-s$	$r'-s'$	$r'+s'$	R
The coefficient of induction in each	$p+q$	$p-q$	$p'-q'$	$p'+q'$	L
The current at any instant	$x+\frac{1}{2}z$	$x-\frac{1}{2}z$	$y-\frac{1}{2}z$	$y+\frac{1}{2}z$	Z.

Let E be the potential at A P at P Q at Q and let that at B be zero.

Then the electromotive force arising from induction is equal to the decrement of current multiplied by the proper coefficient so that we have these five equations

(1) $$(r+s)\left(x+\frac{1}{2}z\right)+(p+q)\left(\frac{dx}{dt}+\frac{1}{2}\frac{dz}{dt}\right)=E-P$$

(2) $$(r-s)\left(x-\frac{1}{2}z\right)+(p-q)\left(\frac{dx}{dt}-\frac{1}{2}\frac{dz}{dt}\right)=P$$

(3) $$(r'-s')\left(y-\frac{1}{2}z\right)+(p'-q')\left(\frac{dy}{dt}-\frac{1}{2}\frac{dz}{dt}\right)=E-Q$$

(4) $$(r'+s')\left(y+\frac{1}{2}z\right)+(p'+q')\left(\frac{dy}{dt}+\frac{1}{2}\frac{dz}{dt}\right)=Q$$

(5) $$Rz+L\frac{dz}{dt}=P-Q.$$

By adding (1)+(2) and (3)+(4) and by taking away (4)−(2)+(5) we get the following

$$2rx+2P\frac{dx}{dt}+sz+q\frac{dz}{dt}=E \tag{6}$$

$$2r'y+2p'\frac{dy}{dt}+s'z+q'\frac{dz}{dt}=E \tag{7}$$

$$2sx+2q\frac{dx}{dt}+2s'y+2q'\frac{dy}{dt}+(r+r'+2R)z+(p+p'+2L)\frac{dz}{dt}=0. \tag{8}$$

These being linear simultaneous equations the solution will be a number of terms of the form Ce^{nt} in the value of x y & z with constant terms the values of which are found by simple equations.

FRAGMENT OF A DRAFT OF 'A DYNAMICAL THEORY OF THE ELECTROMAGNETIC FIELD'[1]

circa SUMMER 1864[2]

From the original in the University Library, Cambridge[3]

[GENERAL EQUATIONS OF THE ELECTROMAGNETIC FIELD]

[1] Specific resistance ρ[4]

Let ρ be the coefficient of resistance to current electricity of a portion of a substance unity of length and unity of section, the electromotive force P required to maintain a current p is $P = \rho p$ and we shall have the equations of resistance in an isotropic medium

$$P = \rho p \quad Q = \rho q \quad R = \rho v. \tag{11}$$

Electric elasticity k

Let the electromotive force P required to produce an electrical displacement f be kf then k is the coefficient of electric elasticity or resistance to displacement for the given medium. The equations of elasticity in an isotropic medium are therefore

$$P = kf \quad Q = kg \quad R = kh. \tag{12}$$

Electric potential ψ

The electric potential is a kind of pressure arising from the resultant action of all the electromotive forces.

(1) *Phil. Trans.*, **155** (1865): 495–512 (= *Scientific Papers*, **1**: 526–97).

(2) See Number 230 note (2).

(3) ULC Add. MSS 7655, V, c/8, f/4. The three folios are consecutive, and are numbered '22', '23' and '24'.

(4) Compare 'A dynamical theory of the electromagnetic field': 484–5 (= *Scientific Papers*, **1**: 559–61).

Equations of Electromotive Force

$$\left.\begin{aligned} P &= \mu\left(\gamma\frac{dy}{dt}-\beta\frac{dz}{dt}\right)-\frac{dF}{dt}-\frac{d\psi}{dx} \\ Q &= \mu\left(\alpha\frac{dz}{dt}-\gamma\frac{dx}{dt}\right)-\frac{dG}{dt}-\frac{d\psi}{dy} \\ R &= \mu\left(\beta\frac{dx}{dt}-\alpha\frac{dy}{dt}\right)-\frac{dH}{dt}-\frac{d\psi}{dz}. \end{aligned}\right\} \quad (13)$$

The first term on the right hand side of each equation is the electromotive force arising from the motion of the conductor in the magnetic field, where μ is the magnetic coefficient of the medium $\alpha\,\beta\,\gamma$ the components of magnetic force, and $\frac{dx}{dt}\,\frac{dy}{dt}\,\frac{dz}{dt}$ the components of the velocity of the conductor.

The second term shows the effect of changes in the position or strength of magnets or currents in the field.

The third term shows the effect of the electric potential which arises from the presence of free electricity.

[2] Statical Electricity[5]

Let us now consider the case of a field in which there is no motion or change and no force except that which arises from electric displacement. We have then[6]

$$P = -\frac{d\psi}{dx} = -kf$$

$$Q = -\frac{d\psi}{dy} = -kg$$

$$R = -\frac{d\psi}{dz} = -kh$$

$$e = -\left(\frac{df}{dx}+\frac{dg}{dy}+\frac{dh}{dz}\right) = -\frac{1}{k}\left(\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}+\frac{d^2\psi}{dz^2}\right) = -\frac{1}{k}\nabla^2\psi.^{(7)}$$

(5) Compare 'A dynamical theory of the electromagnetic field': 490–2 (= *Scientific Papers*, **1**: 568–9).

(6) Compare the positive sign in equations (12) *supra*. The MS shows Maxwell's hesitancy in here inserting minus signs in the equations for P, Q, R. In 'A dynamical theory of the electromagnetic field': 485 (= *Scientific Papers*, **1**: 560) he deletes the minus sign. For discussion see Daniel M. Siegel, *Innovation in Maxwell's Electromagnetic Theory* (Cambridge, 1991): 180–1.

(7) On the use of ∇^2 see Number 239 note (15).

The work done by the electromotive forces in producing the displacement is $\sum \frac{1}{2}(Pf+Qg+Rh)$

$$= \sum \frac{1}{2k}\left(\overline{\frac{d\psi}{dx}}\Big|^2 + \overline{\frac{d\psi}{dy}}\Big|^2 + \overline{\frac{d\psi}{dz}}\Big|^2\right).$$

Now let us suppose two small electrified bodies e_1 and e_2 at a distance $= a$ then the equation $e = -\frac{1}{k}\nabla^2\psi$ can be satisfied by one and only one value of ψ in the space outside the electrified bodies namely

$$\psi = \frac{1}{4\pi}k\left(\frac{e_1}{r_1}+\frac{e_2}{r_2}\right)$$

where r_1 is the distance from e_1 and r_2 the distance from e_2.

The whole work stored up in the dielectric in consequence of the first displacement of the electricity within it is found by integrating by parts and remembering that ψ vanishes at an infinite distance to be $\sum \frac{1}{2k}\psi\nabla^2\psi$ or $\sum \frac{1}{2}\psi e$.

[3] In this case ψ consists of two parts, one depending on e_1 and the other on e_2. Let us call them ψ_1 and ψ_2, then the expression becomes

$$\sum \frac{1}{2}(\psi_1 e_1 + \psi_2 e_2 + \psi_1 e_2 + \psi_2 e_1).$$

Since the values of ψ_1 depend on the distance from e_1 it is manifest that $\psi_1 e_1$ will not alter when the distance between e_1 & e_2 is changed and the same may be said of $\psi_2 e_2$. But

$$\psi_1 e_2 = \frac{1}{4\pi}k\frac{e_1 e_2}{a} \quad \text{and} \quad \psi_2 e_1 = \frac{1}{4\pi}k\frac{e_2 e_1}{a}$$

so that the part of the work stored up in the dielectric which may be altered by varying a is

$$\frac{1}{4\pi}k\frac{e_1 e_2}{a}.$$

If a is increased, this quantity is diminished, that is to say, work is done by the dielectric upon the electrified bodies and this can only be done by urging the bodies asunder.

It appears, therefore that two bodies electrified with quantities of electricity e_1 e_2 in electromagnetic measure and at a distance a, repel each other with a force

$$\frac{1}{4\pi}k\frac{e_1 e_2}{a^2}.$$

Now let η_1 η_2 be the quantities e_1 e_2 reduced to electrostatic measure and let

E be the number of electrostatic units of electricity in one electromagnetic unit, then this force will be

$$\frac{k}{4\pi E^2}\frac{\eta_1\eta_2}{a^2}$$

but by definition of electrical quantity in electrostatic measure the repulsion of η_1 & η_2 is $\frac{\eta_1\eta_2}{a}$ therefore we have

$$\frac{1}{4\pi}k = E^2.^{(8)}$$

(8) Compare Number 239 §4.

232

FROM A LETTER TO CHARLES HOCKIN[1]

7 SEPTEMBER 1864

From Campbell and Garnett, *Life of Maxwell*[2]

Glenlair
Dalbeattie
September 7 1864

I have been doing several electrical problems. I have got a theory of 'electric absorption', *i.e.* residual charge, etc.,[3] and I very much want determinations of the specific induction, electric resistance, and absorption of good dielectrics, such as glass, shell-lac, gutta-percha, ebonite, sulphur, etc.

I have also cleared the electromagnetic theory of light from all unwarrantable assumption, so that we may safely determine the velocity of light by measuring the attraction between bodies kept at a given difference of potential, the value of which is known in electromagnetic measure.[4]

I hope there will be resistance coils at the British Association.[5]

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(1) St John's 1859, third wrangler 1863, Fellow 1864–73 (Venn).

(2) *Life of Maxwell*: 340.

(3) See Maxwell's discussion in 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 494–7 (= *Scientific Papers*, **1**: 573–6).

(4) Maxwell, 'A dynamical theory of the electromagnetic field': 497–505 (= *Scientific Papers*, **1**: 577–88); and Number 235.

(5) On the display of standard resistance coils see the 'Report of the committee on standards of electrical resistance', *Report of the Thirty-fourth Meeting of the British Association; held at Bath in September 1864* (London, 1865): 345–67, esp. 348.

MANUSCRIPT ON THE DETERMINATION OF THE NUMBER OF ELECTROSTATIC UNITS IN ONE ELECTROMAGNETIC UNIT OF ELECTRICITY

circa SEPTEMBER 1864[(1)]

From the original in the University Library, Cambridge[(2)]

EXPERIMENT TO DETERMINE THE NUMBER v OF ELECTROSTATIC UNITS IN ONE ELECTROMAGNETIC UNIT OF ELECTRICITY

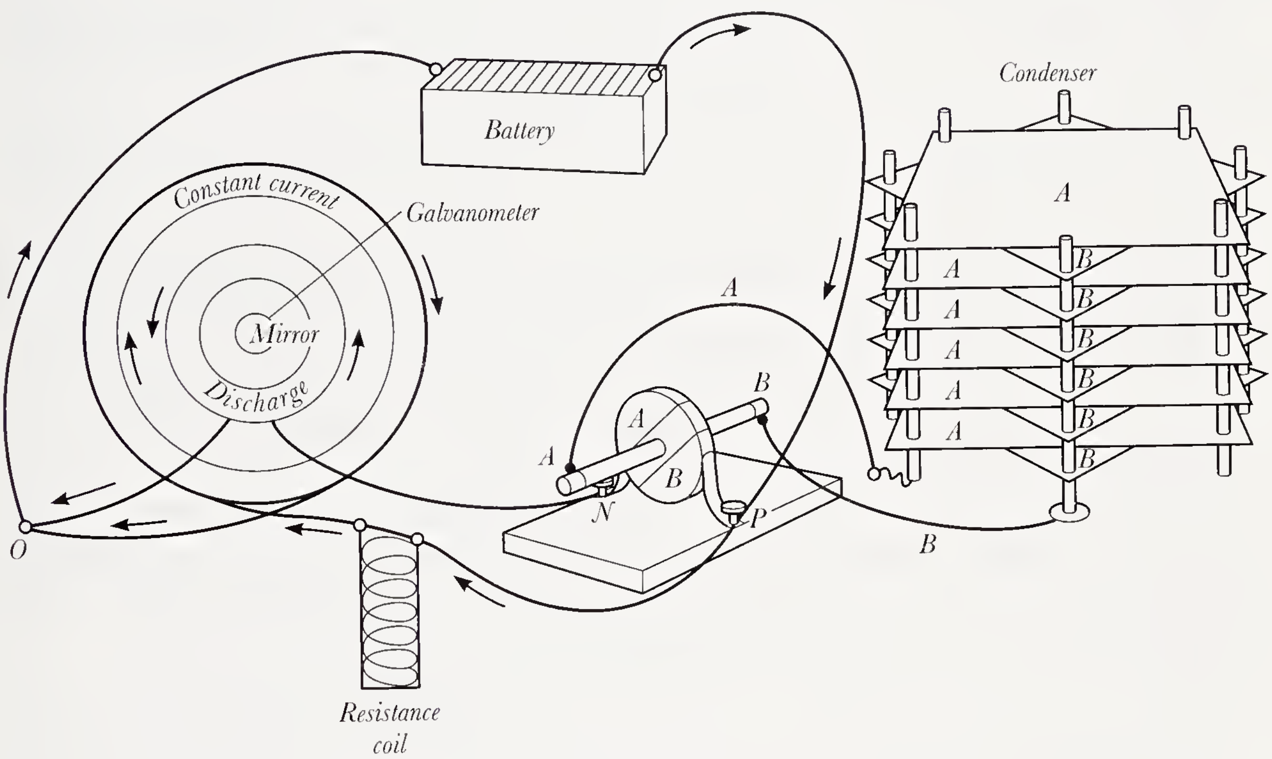

Figure 233,1

(1) Compare Maxwell's letter to Thomson of 27 September 1864 (Number 234), where the same experimental arrangement is described. There is no evidence that the experiment was performed.

(2) ULC Add. MSS 7655, V, c/14(ii). Endorsed: 'Experimental Determination of $v = \dfrac{\text{electrostatic}}{\text{electromagnetic}}$ measure of electricity'. The manuscript is described by I. B. Hopley, 'Maxwell's determination of the number of electrostatic units in one electromagnetic unit of electricity', *Annals of Science*, **15** (1959): 91–108, esp. 99–105.

The method proposed is founded on the comparison of the current produced by the repeated discharges of a condenser of known capacity with that kept up against a known resistance, the electromotive force employed in charging the condenser and in keeping up the current being the same.

The condenser is formed of alternate plates of metal nearly square the set A being nowhere in contact with the set B and each set being fixed on four metal pillars independent of the other set. In this way a powerful condenser may be formed having air as its dielectric and therefore free from 'electric absorption'.(3)

The capacity of this condenser is to be ascertained by dividing its charge with Thomsons Standard Condenser(4) a considerable number of times and finding the ratio of its potential before and after the process.

The capacity of the Standard Condenser being known in Electrostatic measure from its known form and dimensions, that of the large condenser may be determined. Let it be $= c$ meters (little c).

The commutator consists of a revolving disc & axle driven at constant speed $= n$ revolutions per second. The circumference of the disc consists of two nearly semicircular pieces of metal well insulated from each other. Of these one A is connected with the metal ring A and the other B, with the ring B. The rings A & B are kept connected with the plates A & B of the condenser.

The springs P & N press on exactly opposite extremities of the disc. P is always connected with the positive pole of the battery and N with the inner coil of the Galvanometer.

The Galvanometer consists of two separate coils, the inner one of a good many revolutions, the outer one of a smaller number. The magnet and mirror suspended in the centre of the coils should be of considerable mass, and the time of a single swing should be at least 10^{s}.

When the same current passes through both coils the effect of the inner coil on the magnet is ρ times that of the outer coil.

To determine ρ let a current be divided and sent in opposite directions through the two coils and let resistances be introduced into one of them till there is no effect on the magnet. Then

$$\rho = \frac{\text{resistance of inner coil \& appendages}}{\text{resistance of outer coil}}.$$

In the determination of v, the positive pole of the battery is connected with the spring P and the negative pole with O. P is kept constantly connected with

(3) See Faraday, *Electricity*, **1**: 364–7 (paras. 1169 to 1178); and Numbers 232 and 486.

(4) See Number 234 esp. note (2).

O by a resistance coil of great resistance and the outer coil of the galvanometer. Let the resistance from P to O by this course be R.

P is also connected with one set B of the plates of the condenser while the other set A is connected with the spring N and N is connected with O through the inner coil of the galvanometer.

When the commutator is in this position B is charged + & A − with the quantity of electricity corresponding to the difference of potentials between P & O. When the commutator alters the connexions this charge is sent through the galvanometer and the current goes on till an equal and opposite charge is produced so that a double charge goes through the galvanometer at each change of connexions. The speed &c are so arranged that the magnet is not affected. To determine v from the results

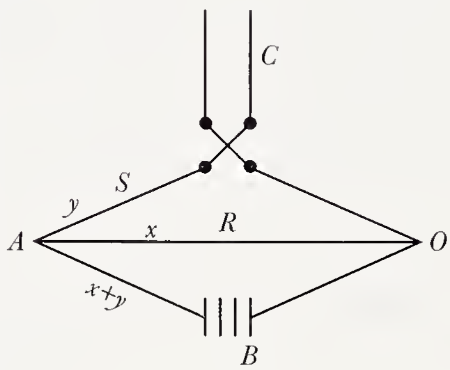

Figure 233,2

Let x = current through resistance coil &c R whose resistance = R
y = current through commutator &c S ———— S
$x+y$ = ———— battery, resistance ———— B
C = capacity of condenser in electromagnetic measure
F = electromotive force of battery
η = difference of potentials of condenser
Y = charge of condenser
K = force on magnet due to unit current in outer coil
H = Earths hor. intensity
T = time of single vibration of magnet
M' = magnetic moment[5]
A = moment of inertia
$L\ M\ N$ coeffts of induction for x & y[6]
A & O potentials at A & O.

(5) Maxwell writes M, but to avoid confusion the magnetic moment of the galvanometer needle is here denoted M'.

(6) L and N are the coefficients of induction of the outer and inner galvanometer coils, M the coefficient of mutual induction between them.

Equations of Currents

$$Rx+L\frac{dx}{dt}+M\frac{dy}{dt}=A-O$$

$$Sy+N\frac{dy}{dt}+M\frac{dx}{dt}=A-O+\eta$$

$$B\,(x+y)=F+O-A$$

$$Y=C\eta$$

$$y=-\frac{dY}{dt}$$

Eliminating A & O

$$(R+B)\,x+By+L\frac{dx}{dt}+M\frac{dy}{dt}=F$$

$$+Rx-Sy+(L-M)\frac{dx}{dt}+(M-N)\frac{dy}{dt}=-\eta=-\frac{Y}{C}.$$

Let us suppose that the time of contact is sufficient for complete discharge then at first and also at last

$$x=\frac{F}{R+B}\qquad y=O.$$

Since the currents are the same at first and at last the inductive currents due to the coefficients $L\ M\ N$ will each be zero when integrated during the whole time. We may therefore make our calculation, as if quantities were zero.

We find then
$$x=\frac{FS-\frac{Y}{C}B}{(R+B)\,S+BR}$$

$$\frac{dY}{dt}=\frac{-FR-\frac{Y}{C}(R+B)}{(R+B)\,S+BR}.$$

Whence
$$Y=-FC\frac{R}{R+B}(1+Y_0\,e^{-\alpha t})$$

where
$$\alpha=\frac{R+B}{C(RS+BS+BR)}.$$

When $t=0$ the charge on the condenser is $FC\dfrac{R}{R+B}$ so that we get

$$Y=FC\frac{R}{R+B}(2e^{-\alpha t}-1)\quad\text{whence}\quad y=2\alpha FC\frac{R}{R+B}e^{-\alpha t}$$

and $$x = \frac{F}{(R+B)\,S+BR}\left(S+\frac{RB}{R+B}(1-2e^{-\alpha t})\right).$$

The equation of motion of the magnet is

$$A\frac{d^2\theta}{dt^2} = -M'H\sin\theta + M'K(x-\rho y)\cos\theta.^{(7)}$$

The oscillations of the magnet are partly free, performed in the time $T = \pi\sqrt{\dfrac{A}{M'H}}$ and of indeterminate extent and partly forced, performed in the time $\dfrac{1}{4n}$, and their extent is

$$\theta = \frac{1}{32n}\frac{M'K}{A}\frac{F}{R+B} = \frac{\pi^2}{32nT^2}\tan\beta$$

where β is the deflexion due to constant current.

The mean position of the magnet is determined by the equation $\tan\theta = \dfrac{K}{H}(x-\rho y)$

where x and y have their mean values. To find these.
Integrate x & y from $t = 0$ to $t = T$ where T is the time of contact.

$$\int x\,dt = \frac{Ft}{R+B} + \frac{2CFRB}{(R+B)^2}(e^{-\alpha t}-1)$$

$$\int y\,dt = 2CF\frac{R}{R+B}(1-e^{-\alpha t})$$

$$\tan\theta = \frac{K}{H}\frac{\int x\,dt - \rho\int y\,dt}{t}$$

$$t = \frac{1}{2n}$$

$$\tan\theta = \frac{K}{H}\frac{F}{R+B}\left\{1-4CRn\rho\left(1-e^{-\alpha T}\right)\left(1-\frac{1}{\rho}\frac{B}{R+B}\right)\right\}$$

an equation to determine the value of C in electromagnetic measure.

R or n should be varied till $\tan\theta$ is nearly zero then

$$C = \frac{1}{4Rn\rho} \quad \text{roughly}$$

the corrections depending on $e^{-\alpha t}$ and $\dfrac{1}{\rho}\dfrac{B}{R+B}$ being small.

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(7) θ is the mean deflection of the galvanometer needle.

In order to read the galvanometer distinctly the forced oscillations ought to be small compared with the deflexion due to the current in x alone. If $T = 10$ seconds and $n = 50$ $\frac{\pi^2}{32nT^2} = \frac{1}{16000}$ nearly which would be sufficiently accurate as these oscillations consist of a series of kicks the apparent mean position will be one third from the extreme end of the oscillation.

To find the correction $e^{-\alpha T}$. Let the ratio of the metallic to the whole circumference of the disc be r, r is not far from 1 and $T = \frac{r}{2n}$ also $n = \frac{1}{4CR\rho}$ nearly

$$\alpha T = \frac{2(R+B)\,R\rho r}{RS+SB+BR}.$$

If R be great compared with B or S and if ρ be a large number αT will be a large number and the correction $e^{-\alpha T}$ will be negligible.

The most important correction is that depending on $\frac{1}{\rho}\frac{B}{R+B}$ since it is difficult to ascertain B, the resistance of the battery with much accuracy.

If however $\rho = 100$ and $R = 100B$ the correction is less than $\frac{1}{10{,}000}$ and its approximate value may still be ascertained.

If c is 100 in electrostatic measure, it is $\frac{100}{v^2}$ in electromagnetic measure and

$$v^2 = \frac{c}{C} = 4cRn\rho \ (1 + \text{corrections}).$$

From Webers experiments $v =$ about (300,000,000) metres per second.(8)

Hence if $c = 100$ metres $\rho = 100$ $n = 25$ revolutions per second R should be about 90,000,000,000 metres per second or about 900 times the standard resistance coil of 1863.(9)

(8) Weber's experiments gave a value of 310,740,000 meters per second; see Number 218 esp. notes (4) and (7).

(9) The value of the resistance of the coils was recorded in the 1863 'Report of the Committee...on standards of electrical resistance'; see Number 214, esp. note (18). No standards based on the 1863 determination were officially issued, but some coils were made; see the 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-fourth Meeting of the British Association* (London, 1865): 345–67, esp. 345.

then $$v^2 = 4cRn\rho \frac{\left(1 - \frac{1}{\rho}\frac{B}{R+B}\right)\left(1 - e^{-\alpha T}\right)}{1 - \frac{\tan\theta}{\tan\beta}}$$

c = electrostatic capacity, θ = mean galv$^{\mathrm{r}}$ reading
β = reading due to current through R.

234

LETTER TO WILLIAM THOMSON

27 SEPTEMBER 1864

From the original in the University Library, Glasgow[1]

Glenlair
Dalbeattie
Sept 27 1864

Dear Thomson

Is your Standard Condenser in actual existence? that is to say a plate of air which will have a known capacity of so many feet or meters capable of measurement to about $\frac{1}{10}$ per cent.[2]

Figure 234,1

If so I should like to get hold of it to measure the capacity of an air battery I propose to make.[3]

Two sets of square metal plates are fixed each to four vertical pillars at the corners and arranged so that the one set are interleaved with the other set but nowhere in contact. The whole to be firmly fixed on an insulating plate so that the distances of the plates cannot change, and enclosed in a dry atmosphere.

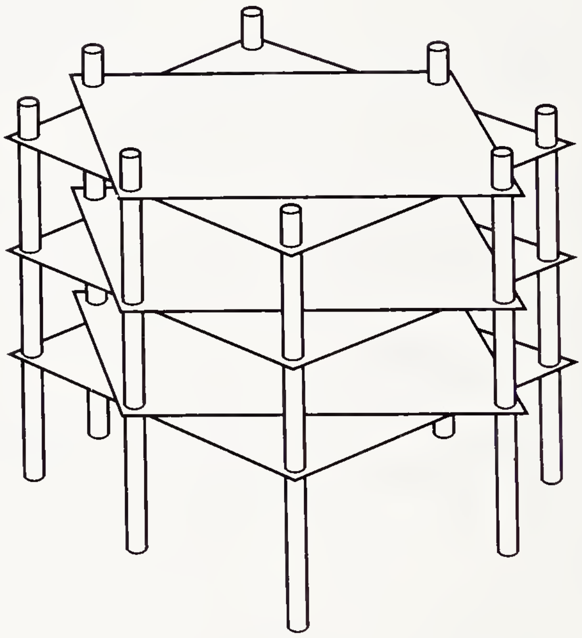


Figure 234,2

Thus we may with plates 9 or 10 inches square easily and safely get a capacity of 2 meters per plate,

$$\frac{\frac{1}{25}}{4\pi \times \frac{1}{1000}} = \frac{1000}{100\pi}.$$

Let the capacity of say 50 plates be 100 meters and let it be well measured by comparison with your standard condenser, by charging the condenser say

(1) Glasgow University Library, Kelvin Papers, M 16.

(2) See Maxwell, *Treatise*, **1**: 283–5 (§228) and Fig. 20 (corresponding to Figure 234, 1), an 'arrangement, due to Sir W. Thomson, which we may call the Guard-ring arrangement, by means of which the quantity of electricity on an insulated disk may be exactly determined in terms of its potential'.

(3) See Number 233.

100 times and always discharging it every time and finding the loss of potential by your electrometer.

Now let *A*, *B* be two conducting parts of the rim of a revolving disc insulated from each other and let *A* be connected permanently with one set of plates & *B* with the other. Also let *P* & *N* be connected with the electrodes of a battery (voltaic). Then *B* will be charged + & *A*−.

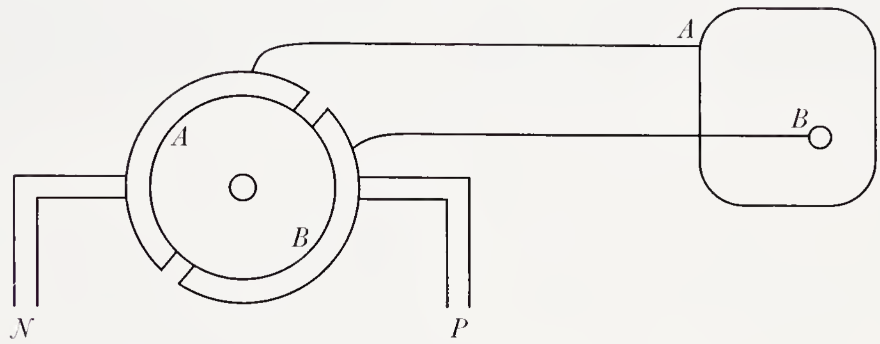


Figure 234,3

But when *A* & *B* are reversed this charge will be let off and a new and equal reverse charge established so that at every reversal a double charge is sent through *P N* and by reversing often a continuous effect on the galvan[r] may be produced.

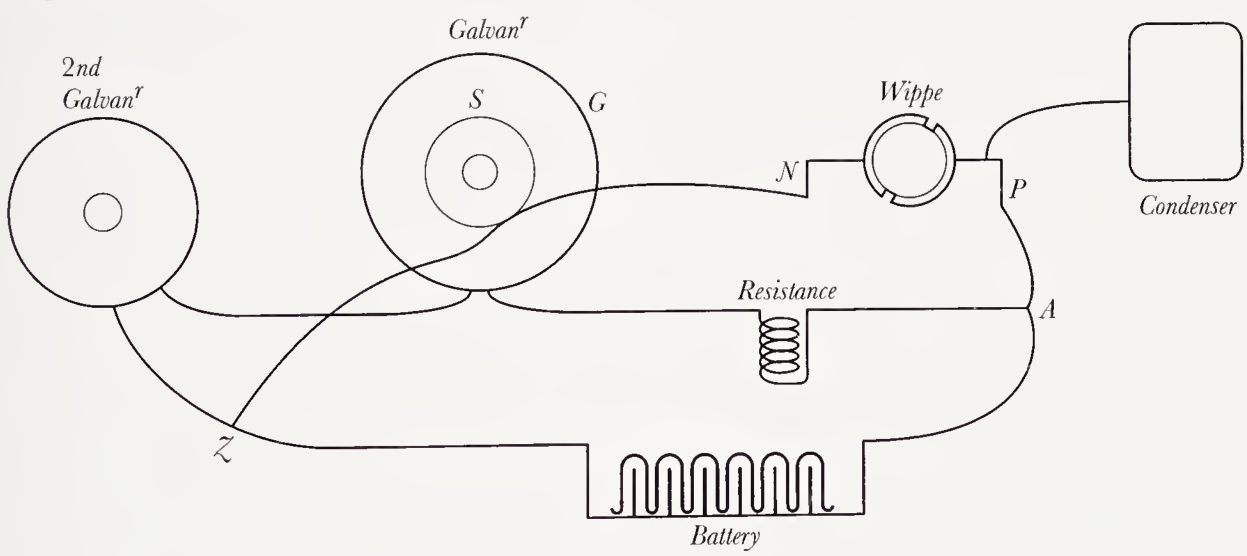


Figure 234,4

Now let the battery current be divided at *A* and part sent by way of *P* to condenser thence to *N* thence to a sensitive galvan[r] *S* and to *Z*.

The other part through a resistance coil an outer coil of the galvan[r] *G* and a second galv[r] still less sensitive.

S and *G* run in opposite directions and act on the same needle. *S* is ρ times more sensitive than G the 2nd galvr is σ times less sensitive than G that is if a current 1 passed through S & a current ρ through G the needle would be at rest, and if 1 passed through G and σ thro the 2nd galvr the effects wd be equal.

Now let the Wippe(4) go round n times per second and let the resistance be so arranged that the 1st galvr is very little affected, and its mean deflexion is θ while that of the 2nd is β

we get the value of v or $\dfrac{\text{electrostatic}}{\text{electromagnetic}}$ measure of electricity by the formula

$$v^2 = 4c\,Rn\,\rho\frac{\left(1-\dfrac{1}{\rho}\dfrac{B}{R+B}\right)\left(1-e^{-\alpha T}\right)}{1-\dfrac{1}{\sigma}\dfrac{\tan\theta}{\tan\beta}}.^{(5)\,(a)}$$

Here

c = capacity of condenser electrostatic measure
R = resistance from A to Z through resistance coil &c
n = no of revolutions per second of commutator
ρ = ratio of currents giving equal effects in the two coils of the 1st galvanr
σ = ratio of the 2nd coil of do to the 2nd galvanr

(a) {Thomson}
V, R resistce electrost
r ——— [electro] m[agnetic]
$Q = \mu =$ flowing, electrost.
$\dfrac{V}{R} = Q; \quad \dfrac{r}{R} = r\dfrac{Q}{V}$

resistce of wippe $= \dfrac{1}{nc}$ electrostatic
——— coils $= R$ ——— magnc
$v = 1000 \times 10^6$ feet 192000 miles

$v^2 = \rho nc\,R$ Let $R = 10^{12}\ \frac{\text{feet}}{\text{metres}}/\text{sec}$

$v^2 \div R = \dfrac{v}{1000} = 10^6$ feet

$n \times c \times e =$ fory in electrost meas.
$\dfrac{Vv}{R} =$ ——— mag. 〃
$= v^{-1} q$
$n \times c \times R = v^2$

(4) Commutator (see the *Treatise*, 2: 375 (§775)).

(5) See Number 233.

θ = mean deflexion of 1st galvanr
β = ———— 2nd ————
B = resistance of battery from Z to A

$$\alpha T = \frac{2\,(R+B)\,R\rho r}{RS+SB+BR}\text{[6]}$$

where S is resistance from A to Z through condenser &c & r = ratio of metallic part of the disc to the whole. αT is very large. If $c = 100$ & $n = 25$ & $\rho = 100$ R should be about 900 of the unit coils of 1863 which would do very well.[7] I think we might get this done this year by diligence.

Yours truly
J. Clerk Maxwell

(6) T is the time of an oscillation of the galvanometer needle.
(7) See Number 233 (note (9)).

235

LETTER TO WILLIAM THOMSON

15 OCTOBER 1864

From the original in the University Library, Glasgow(1)

8 Palace Gardens Terrace W
1864 Oct 15

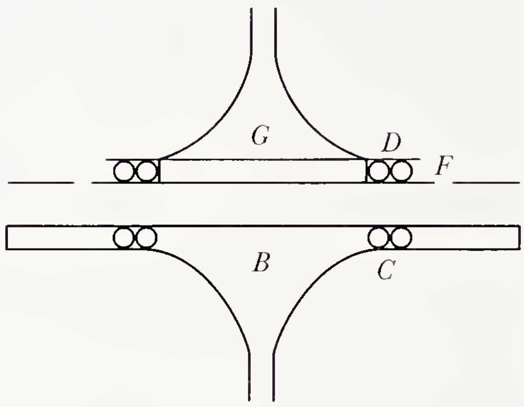


Figure 235,1

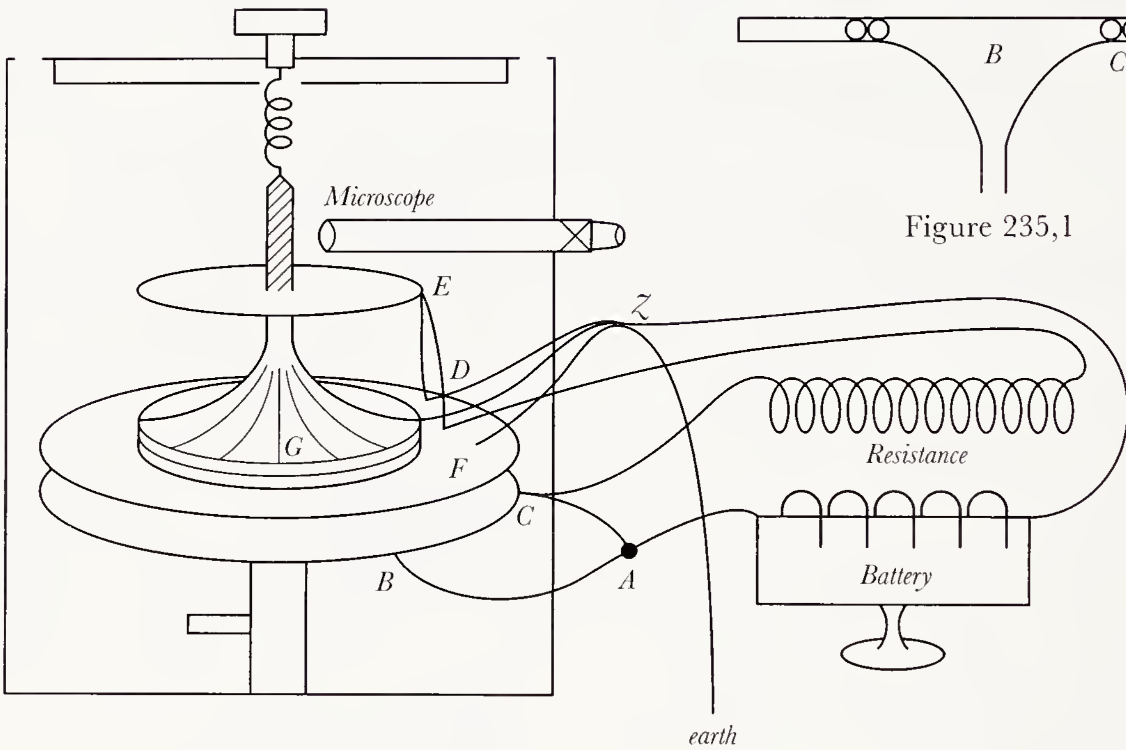


Figure 235,2

Dear Thomson

Here is a plan to weigh an electrostatic attraction against an electromagnetic repulsion directly,(2) so as to require no standard except a

(1) Glasgow University Library, Kelvin Papers, M 17.

(2) The experimental arrangement which Maxwell devised at this time established the principle of the method employed in his paper 'On a method of making a direct comparison of electrostatic with electromagnetic force; with a note on the electromagnetic theory of light', *Phil. Trans.*, **158** (1868): 643–57, esp. 643–52 (= *Scientific Papers*, **2**: 125–36). See Number 289.

resistance coil(3) and some proportional measures of dimensions in any scale you please.

The electrostatic attraction of two discs kept at a constant potential is balanced(4) against the electromagnetic repulsion of two coils of wire through which a current is urged by the same electromotive force which keeps up the difference of potential.

G Fig. [235,]1 is an aluminium disc strengthened by ribs behind the surface plane &c potential always zero.

F is a ring surrounding it the lower surface being in the same plane with *G*.

D is a coil of insulated wire, from 5 to ten turns as close as possible to the upper surface of *G*.

E is an equal coil a good bit above *D* wound in the opposite direction to avoid the effects of terrestrial magnetism.(5)

B is a large disc as big as the ring with a coil *C* just under its surface like *D* and opposite to *D*.(6)

B and *C* move vertically by a micrometer screw.

G D and *E* are hung up by a spring and adjusted to be in the plane of *F* and certain marks on the stem are observed with a microscope.

The electrode *A* of a battery is connected

1st with the disc *B*

2nd with the following series Coil *C*, Resistance coil, Coil *E* Coil *D* to electrode *Z* of battery.

Z is connected with the hanging disc *G* the ring *F* the box of the instrument and the Earth.

Turn on the current and observe the stem with the microscope. If it is pulled down screw the disc *B* down & vice versa till turning on the current produces no effect.

Then immediately measure the resistance between *A* and *Z* by comparison with a standard coil, and at your leisure measure the distance from *B* to *G*.

---

(3) 'The ratio of the electromagnetic unit to the electrostatic unit is...that of a certain distance to a certain time...this ratio is a *velocity*...The electromagnetic value of the resistance of a conductor is also a quantity of the nature of a velocity [see Number 211 note (7)], and therefore we may express the ratio of the two electrical units in terms of the resistance of a known standard coil; and this expression will be independent of the magnitude of our standards of length, time, and mass'; Maxwell, 'On a method of making a direct comparison of electrostatic with electromagnetic force': 643–4 (= *Scientific Papers*, 2: 126). On the ratio of electrical units see the Introduction and the *Treatise*, 2: 241–4 (§§625–8).

(4) In the 1868 experiments the comparison was made using a torsion balance. For a description of the torsion balance see Number 243.

(5) See Maxwell, 'On a method of making a direct comparison of electrostatic with electromagnetic force': 646 (= *Scientific Papers*, 2: 130).

(6) The current passes through the coils in opposite directions so as to produce a repulsion.

Do this with various resistances in circuit.

The electrostatic attraction is $\frac{1}{2}\frac{F^2}{4\pi v^2}\frac{\pi a^2}{d^2}$ (7)

where $F =$ electromotive force from A to Z
$a =$ radius of disc
$d =$ distance of discs.

The electromagnetic repulsion is

$$4\pi\frac{F^2}{R^2}\frac{a'}{d'}n^2 \text{ (8)} + \text{corrections which I can make (9)}$$

where $R =$ resistance from A to Z
$a' =$ effective radius of coils C and D less than a
$n =$ number of windings in each
$d' =$ distance between them $= d + \text{const.}$

$$v = \frac{1}{\sqrt{32\pi}}\frac{a}{d}\sqrt{\frac{d'}{a'}}\frac{1}{n}R \quad \text{for 1}^{\text{st}} \text{ approx}^{\text{n}}\text{ (10)}$$

If $\frac{a}{d} = 10$, 100 Daniels(11) would give ⟨400 grain W⟩ ⟨1250 grammes weight⟩.

---

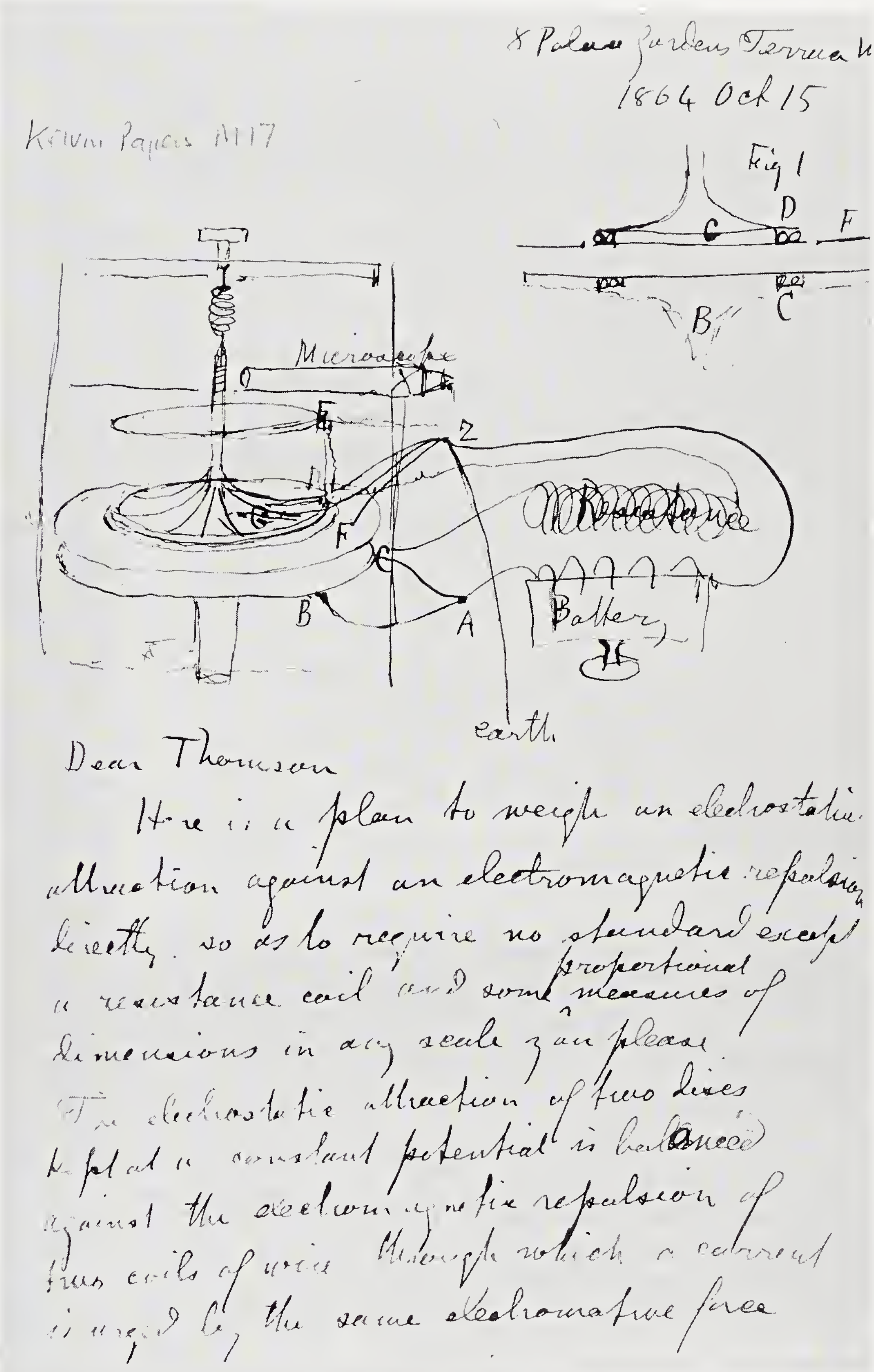
(7) F is the difference of potential between the two discs in electromagnetic measure, v is the ratio of electrical units; see Maxwell, 'On a method of making a direct comparison of electrostatic with electromagnetic force': 645 ($=$ *Scientific Papers*, **2**: 128). On electrostatic attraction between plane surfaces see William Thomson, 'On the mathematical theory of electricity in equilibrium. I. On the elementary laws of statical electricity', *Camb. & Dubl. Math. J.*, **1** (1845): 75–95, esp. 78 ($=$ *Electrostatics and Magnetism*: 15–37).

(8) The repulsive force depends on the ratio of the diameter of the coils to their distance. Maxwell writes this relation in slightly different form in 'On a method of making a direct comparison of electrostatic with electromagnetic force': 645 ($=$ *Scientific Papers*, **2**: 128).

(9) In his paper 'On a method of making a direct comparison of electrostatic with electromagnetic force': 646 ($=$ *Scientific Papers*, **2**: 128–9) Maxwell explained that the correction factor would 'take into account the fact that the section of each coil is of sensible area.' Making the depth of the coil equal to the breadth of the section, from the differential equation of the potential of two coils given in his paper 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, on 508 ($=$ *Scientific Papers*, **1**: 591), he obtains a correction factor of the form $(1-\alpha'^2/12a'^2)$, where α' is the depth of the coil.

(10) Compare the expression in 'On a method of making a direct comparison of electrostatic with electromagnetic force': 649 ($=$ *Scientific Papers*, **2**: 133). The only quantities to be determined in absolute measure are the resistances (see note (3)).

(11) The electric battery (with zinc and copper electrodes) invented by John Frederic Daniell; see his papers 'On voltaic combinations', *Phil. Trans.*, **126** (1836): 107–24; 'Additional observations on voltaic combinations', *ibid.*: 125–9; and 'Further observations on voltaic combinations', *ibid.*, **127** (1837): 141–60.



8 Palace Gardens Terrace W
1864 Oct 15

Kelvin Papers M17

Dear Thomson

Here is a plan to weigh an electrostatic attraction against an electromagnetic repulsion directly so as to require no standard except a resistance coil and some proportional measures of dimensions in any scale you please.

The electrostatic attraction of two discs kept at a constant potential is balanced against the electromagnetic repulsion of two coils of wire through which a current is urged by the same electromotive force

Plate II. A suggested experiment (1864) to establish the ratio of electrostatic and electromagnetic units of electricity, from a letter to William Thomson (Number 235).

The absolute attraction is about

$$\frac{1}{80}\mathcal{N}^2\frac{a^2}{d^2} \text{ in grammes or } \frac{1}{800}\mathcal{N}^2\frac{a^2}{d^2} \text{ gr wt}^{(12)}$$

where $\mathcal{N}$ is the number of Daniels employed so it could be easily got up to a good value.

Can you tell me about the absolute dimensions for the discs and rings for the electrostatic department after your experience of such things.(13) I can get the repulsion of the coils of wire as correct as I please to any no. of terms.

I have just got your letter of the 14th and mean to work out your calculations when I have digested my dinner. Meanwhile your little magnet and mirror might do as a test of immobility of the suspended disc if it were not for electromagnetic action of the coils.

Do you think of doing the Cavendish expt.(14) I have been some time devising a plan for doing it in a vacuum tube like a T upside down  in a cellar in the country.

The Joulian Expt(15)

Is not ☿(16) a good thing to find $\mathcal{J}$.(17)

Get an iron or steel tube 12 to 20 feet long and ½ or ¾ inch internal diamr.
Put a cistern for mercury at the top and screw on at the bottom a plug of cane through which the mercury is squeezed. Just above the cane have a ring shaped cistern of mercury to test the temperature there (or stick a thermometer into the tube if you can). Let the mercury come out of the cane into another cistern and try the temperature there too. The

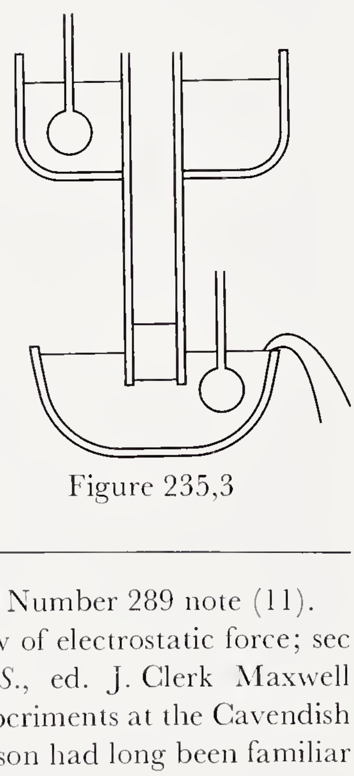


Figure 235,3

(12) Grain weight (1/7000 lb.). (13) See Number 289 note (11).

(14) Possibly Cavendish's experimental demonstration of the law of electrostatic force; see *The Electrical Researches of The Honourable Henry Cavendish, F.R.S.*, ed. J. Clerk Maxwell (Cambridge, 1879): 104–12. Maxwell subsequently repeated the experiments at the Cavendish Laboratory 'in a somewhat different manner', *ibid.*: 417–18. Thomson had long been familiar with Cavendish's unpublished MSS (see Number 435).

(15) To determine the mechanical equivalent of heat, as determined by James Prescott Joule: see Number 207 note (43). Compare Maxwell's account of this proposed experiment in his letter to P. G. Tait of 23 December 1867 (Number 278). (16) Mercury.

(17) The mechanical equivalent of heat: see Number 207 note (43).

difference will be that due to the column $h = \frac{h}{772} \cdot 32 \times 13.5$[18] $= .56$ degree Fah per foot.

Cover the whole tube well with wool &c measure with the same thermometer the temperature at the top and bottom and at the outflow through the porous cane and keep it going for a long time till all is steady. Having got J for your mercury in feet get for water by finding sp. heat of your mercury and its sp gravity by the best methods.

This only requires plenty ☿ and not any great height and no row of wheels noise or shaking.

I shall go through your wippe plan[19] when I am free from post considerations.[20]

The tendency in my rotatory theory of magnetism[21] was towards the to me inconceivable and ∴ no doubt to the misty[22] though why you put a *c* after the *y* I cannot see why. Perhaps the eminent London scavengers Mess^rs Cleavers and Mist might find a weapon to combat the tendency.

I can find the velocity of transmission of electromagnetic disturbances indep^t of any hypothesis now & and it is $= v$ and the disturbances must be transverse to the direction of propagation or there is no propagation thereof.[23]

One result is that if the resistance of gold is exactly the same for small electromotive force as for great gold leaf $\frac{1}{282000}$ inch thick ought to transmit 10^{-300} of the incident light. I find it very difficult to estimate the amount of light transmitted on account of the holes in the gold leaf, but I think it is between $\frac{1}{400}$ and $\frac{1}{900}$ when light from green glass is used.[24]

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(18) $J = 772$ foot pounds per British thermal unit (see Number 207 note (43)); specific gravity of mercury = 13.5 (see Number 278 note (19)); acceleration due to gravity = 32 ft/sec^2.

(19) See Number 234 note (4); and see Number 241 and Maxwell's letter to Thomson of 25 February 1865 (Number 242).

(20) An early indication of Maxwell's intention to resign his post of Professor of Natural Philosophy at King's College, London. According to the minutes of King's College Council Maxwell's resignation was recorded on 10 February 1865: 'The Principal laid before the Council a letter of J. C. Maxwell Esq. resigning his office of Professor of Natural Philosophy, but expressing his readiness to continue his work until the appointment of his successor.' (King's College London Archives, King's College Council Vol. I, minute 410).

(21) The theory of molecular vortices advanced by Maxwell in 'On physical lines of force', *Phil. Mag.*, ser. 4, **21** (1861): 161–75, 281–91, 338–48; *ibid.*, **23** (1862): 12–24, 85–95 (= *Scientific Papers*, **1**: 451–513).

(22) Compare Maxwell's comment in his letter to P. G. Tait of 23 December 1867 (Number 278).

(23) Maxwell, 'A dynamical theory of the electromagnetic field': 497–505 (= *Scientific Papers*, **1**: 577–88).

(24) Compare Number 239 § 7.

Any way nobody could see 10^{-300} times the light of a candle.

Hockin is going to measure the resistance of a bit of gold leaf to compare.[(25)]

I hope to spread some leaf smooth by means of a wrinkle of Faraday's[(26)] and get the proportion of light better.

So the connexion between transparency and resistance is not complete at least for gold leaf.[(27)]

Yours truly
J. CLERK MAXWELL

---

(25) See Number 239 note (17).

(26) Faraday had described a technique for stretching leaves of gold film – by laying the leaves on dampened glass before stretching them – in his 1857 Bakerian Lecture. See Michael Faraday, 'Experimental relations of gold (and other metals) to light', *Phil. Trans.*, **147** (1857): 145–82, esp. 147 (= Faraday, *Experimental Researches in Chemistry and Physics* (London, 1859): 391–443). Maxwell acknowledged receipt of this paper in a letter to Faraday of 9 November 1857 (Volume **I**: 548).

(27) See Maxwell's discussion of the 'relation between electric resistance and transparency' in 'A dynamical theory of the electromagnetic field': 504–5 (= *Scientific Papers*, **1**: 586–7). He found that the opacity of a body is greater, the greater its conductivity, but that 'gold, silver, and platinum are good conductors, and yet when reduced to sufficiently thin plates they allow light to pass through them.... This result cannot be reconciled with the electromagnetic theory of light'. Compare the *Treatise*, **2**: 394–5 (§§ 798–800). See Number 239 § 7.

NOTES ON THE EXPLANATION OF THE REFLECTION AND REFRACTION OF LIGHT BY THE ELECTROMAGNETIC THEORY OF LIGHT

OCTOBER 1864(1)

From the original in the University Library, Cambridge(2)

[ON THE REFLECTION AND REFRACTION OF LIGHT](3)

[Three waves of amplitude 1, A, B: incident, reflected and refracted waves]

Equation of [conservation of] energy(4)

$$(1-A^2)k\sin i\cos i = k' B^2 \sin r\cos r^{(5)}$$

[where i and r are the angles of incidence and refraction]

---

(1) See Maxwell's letter to Stokes of 15 October 1864 (Number 237).

(2) ULC Add. MSS 7655, V, b/12.

(3) Maxwell's argument is based on the treatment of the reflection and refraction of light by Jules Jamin, 'Note sur la théorie de la réflexion et de la réfraction', *Ann. Chim. Phys.*, ser. 3, **59** (1860): 413–26; as he explains in his letter to Stokes of 15 October 1864. In this paper Jamin begins by discussing the solutions which had been obtained by Fresnel, MacCullagh and Neumann. See A. J. Fresnel, 'Mémoire sur la loi des modifications que la réflexion imprime à la lumière polarisée', *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, **11** (1832): 393–433; James MacCullagh, 'On the laws of crystalline reflexion', *Phil. Mag.*, ser. 3, **10** (1837): 42–5; MacCullagh, 'On the laws of crystalline reflexion and refraction', *Transactions of the Royal Irish Academy*, **18** (1837): 31–74; and Franz Neumann, 'Theoretische Untersuchung der Gesetze, nach welchen das Licht an der Grenze zweier vollkommen durchsichtigen Medien reflectirt und gebrochen wird', *Mathematische Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin*, Aus dem Jahre 1835 (Berlin, 1837): 1–160. These papers propose hypothetical boundary conditions which determine the oscillation of the ether at the interface between two media. For the alternative boundary conditions proposed by Fresnel, and by MacCullagh and Neumann, see note (9) and Number 237 notes (4) and (7). These boundary conditions impose constraints on the continuity of the media at their interface: for Maxwell's comments on their validity see Number 237.

(4) The law of the conservation of *vis viva* is assumed by Fresnel, MacCullagh and Neumann. See Fresnel, 'Mémoire sur la loi...': 400, and MacCullagh, 'On the laws of crystalline reflexion': 43.

(5) Compare Jamin, 'Note sur la théorie de la réflexion et de la réfraction': 421, equation (α): $(1-a^2) = b^2\dfrac{\sin r\cos r}{\sin i\cos i}\dfrac{d'}{d}$, where d' and d are the densities of the ether in the two media. Jamin obtains this equation on the supposition (with MacCullagh and Neumann) that $d = d'$; see his 'Note...': 415, and Number 237 note (5). Maxwell interpolates a relation between the density of the ether and his coefficient of 'electric elasticity' k; see note (6).

[where amplitudes] 1 A B [are] displacements
[and k and k' are coefficients of electric elasticity in the two media]

[so] k kA $k'B$ [are electric] forces.(6)

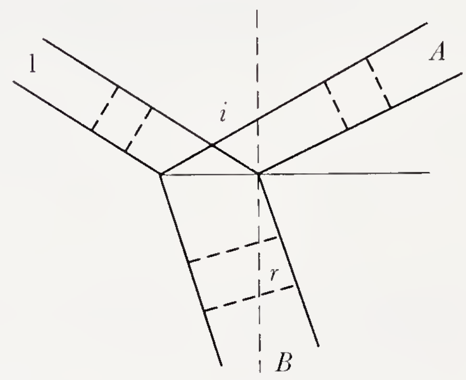

Figure 236,1

[The equation for the propagation of an electromagnetic wave is]

$$v^2 = \frac{k}{4\pi\mu}^{(7)}$$

[where μ is the coefficient of magnetic induction](8)

$$\begin{aligned} &\text{displacement} &&= 1 \\ &\text{mag force} &&= 4\pi v \\ &\text{e m f} &&= 4\pi\mu v^2 = k \\ &\text{mag induction} &&= 4\pi\mu v = \frac{k}{v} \end{aligned}$$

[Boundary conditions](9)

(6) To establish an electromagnetic theory of optical reflection and refraction Maxwell seeks to integrate results drawn from his electromagnetic theory of light into Jamin's expressions for the oscillation of the ether at the interface between two media. He equates the displacements in the ether with the 'electric displacement' in the electromagnetic medium. He introduces equations (E), the 'Equations of electric elasticity', from his paper 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 485 (= *Scientific Papers*, **1**: 560). For isotropic substances k is 'the ratio of the electromotive force to the electric displacement'. See Number 231, equations (12).

(7) From equation (71) of Maxwell's 'A dynamical theory of the electromagnetic field': 498 (= *Scientific Papers*, **1**: 579); v is the velocity of propagation of magnetic disturbances transmitted through the electromagnetic field.

(8) See Maxwell, 'A dynamical theory of the electromagnetic field': 480 (= *Scientific Papers*, **1**: 556); 'μ is a quantity depending on the nature of the medium, its temperature, the amount of magnetization already produced' (for an isotropic medium).

(9) Jamin considers two boundary conditions, as assumed by Fresnel and by MacCullagh and Neumann. Fresnel assumed that the components of the oscillation parallel to the interface are continuous across the interface; and then considered two cases, for oscillations parallel to the plane of incidence and oscillations parallel to the interface. See Fresnel, 'Mémoire sur la loi des modifications que la réflexion imprime...': 394–402; and Jamin, 'Note sur la théorie de la réflexion et de la réfraction': 415, where he observes that '[Fresnel] à considérer deux cas: 1° celui où les vibrations sont normales au plan d'incidence et parallèles à la surface; 2° celui où elles sont dans le plan d'incidence.' These two cases are considered by Maxwell. MacCullagh and Neumann however supposed that 'the vibrations are equivalent at the common surface of two

I Displacements in plane of incidence

[displacements]	1	a	b
e m f	k	ka	$k'b$
mag ind[uction]	$\frac{k}{v}$	$\frac{k}{v}a$	$\frac{k'}{v'}b$
mag force	$4\pi v$	$4\pi va$	$4\pi v'b$

[where k, k' and v, v' are the coefficients of electric elasticity and the velocities of wave propagation in the two media]

[Equations for]

normal displacement $\quad (1-a)\sin i = b\sin r$(10)

tangential mag force? $\quad (1+a)\,v = bv'$.(11)

II Displacements perp to plane of incidence

$1 \quad a \quad b$

[Equations for]

normal mag ind[uction] $\quad (1-a)\,\frac{k}{v}\sin i = b\frac{k'}{v'}\sin r$

or $\quad (1-a)\,\mu = b\,\mu'$

tangential e m f $\quad (1+a)\,k = b\,k'$

[To obtain a and b]

If $\mu = \mu'$ [then] $k \propto v^2$

and [writing conservation of energy equation as]

$$(1-A^2)\sin r\cos i = B^2\sin i\cos r^{(12)}$$

normal displacement $\quad (1-a)\sin i = b\sin r$

[assume Snell's law as] $v\sin i = v'\sin r$(13)

$$\left[\text{and substitute } \frac{k}{k'} = \left(\frac{v}{v'}\right)^2 = \left(\frac{\sin r}{\sin i}\right)^2\right]$$

media'; MacCullagh, 'On the laws of crystalline reflexion': 43. This is the boundary condition favoured by Jamin, but questioned by Maxwell: see Number 237 esp. note (4).

(10) The expression obtained by Jamin, 'Note sur la théorie de la réflexion et de la réfraction': 420, for an oscillation normal to the interface.

(11) Compare Jamin, 'Note sur la théorie de la réflexion et de la réfraction': 415; and Fresnel, 'Mémoire sur la loi...': 401, for an oscillation parallel to the interface.

(12) The equation for the conservation of *vis viva* obtained by Fresnel, 'Mémoire sur la loi...': 400, equation (A), on the supposition that the densities of the ether in the two media are different, the ratio of densities d'/d being equal to $\sin^2 i/\sin^2 r$. See Fresnel, 'Mémoire sur la loi...': 398, and Jamin, 'Note sur la théorie de la réflexion et de la réfraction': 415, esp. equation (1); and see also note (5).

(13) Read: $v/v' = \sin i/\sin r$; see Jamin, 'Note sur la théorie de la réflexion et de la réfraction': 414. The expressions obtained below are therefore incorrect.

$$\text{[tangential magnetic force]}\ \frac{(1+a)\sin r\cos i}{\sin i} = b\frac{\sin i\cos r}{\sin r}$$

$$(1-a)\frac{\sin i}{\sin r} = \frac{(1+a)\sin^2 r\cos i}{\sin^2 i\cos r}.$$

If $k = k'$ [then] $\mu \propto \dfrac{1}{v^2}$

and [conservation of energy equation]

$$(1-A'^2)\sin i\cos i = B'^2\sin r\cos r^{(14)}$$

[for displacements] $\parallel$ $1-a'$.

(14) The equation for the conservation of *vis viva* obtained by Jamin on the supposition that the density of the ether is the same in both media; see note (5).

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LETTER TO GEORGE GABRIEL STOKES

15 OCTOBER 1864

From the original in the University Library, Cambridge[1]

8 Palace Gardens Terrace
1864 Oct 15

Dear Stokes

I have been reading Jamins Note on the Theory of Reflexion & Refraction Ann de Ch 1860 pt. 1 p 413.[2]

I am not yet able to satisfy myself about the conditions to be fulfilled at the surface except of course the condition of conservation of energy.[3]

Jamin insists on the equality of the motion both horizontal & vertical in the two media.[4] I do not see the necessity for equality of motion but I think action and reaction must be equal between the media provided the media pure and simple vibrate and nothing along with them.

If the gross matter in each medium does not vibrate or has a different phase and amplitude from the ether then there will be 6 relations between the 4 quantities – Two portions of ether & two kinds of gross matter.

Have you written anything about the rival theories of reflexion? or can you tell me of any thing you agree with or eminently differ from on that subject. I think you once told me that the subject was a stiff one to the best skilled in undulations.

Jamin deduces (p 422) from his conditions of equality of motion in the two

(1) ULC Add. MSS 7656, M 423. First published in Larmor, *Correspondence*, **2**: 25–6.

(2) Jules Jamin, 'Note sur la théorie de la réflexion et de la réfraction', *Ann. Chim. Phys.*, ser. 3, **59** (1860): 413–26.

(3) See Maxwell's draft on an electromagnetic theory of optical reflection and refraction (Number 236).

(4) On Jamin's boundary condition see his 'Note sur la théorie de la réflexion et de la réfraction': 419–21; 'je vais faire en admettant comme Neumann et MacCullagh que les composantes horizontales et verticales sont égales au-dessus et au-dessous de la surface de séparation des deux milieux.' Jamin is alluding to papers by James MacCullagh, 'On the laws of crystalline reflexion and refraction', *Transactions of the Royal Irish Academy*, **18** (1837): 31–74, and by Franz Neumann, 'Theoretische Untersuchung der Gesetze, nach welchen das Licht an der Grenze zweier vollkommen durchsichtigen Medien reflectirt und gebrochen wird', *Mathematische Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin*, Aus dem Jahre 1835 (Berlin, 1837): 1–160. MacCullagh proposed four fundamental hypotheses, including the boundary condition: 'The vibrations in two contiguous media are equivalent; that is, the resultant of the incident and reflected vibrations is the same, both in length and direction, as the resultant of the refracted vibrations' (see MacCullagh, 'On the laws of crystalline reflexion and refraction': 34).

media for vibrations in the plane of incidence that the density of the medium is the same in all substances.(5)

That is to say he gets this by pure mathematics without any experiment.

Or according to him no such vibrations could exist in the media unless they were of equal density.

This I think simply disproves his original assumption of the equality of the displacements in the two media.

In fact the equality of displacements combined with the equality of energy involves the equality of density.

Therefore the general theory, which ought to be able to explain the case of media of unequal density(6) (even if there were none such) must not assume equality of displacements, of contiguous particles on each side of the surface.(7)

I suppose if two media of different density were glued hard together and large vibrations sent through them they would be separated at their common surface.

But there is nothing in the surface of separation of two media analogous to this gluing together that I can detect.

I have now got materials for calculating the velocity of transmission of a magnetic disturbance through air founded on experimental evidence without

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(5) Jamin, 'Note sur la théorie de la réflexion et de la réfraction': 422. He obtains the equation $(1-a^2) = b^2\dfrac{\sin r\cos r}{\sin i\cos i}$ (where 1, a, b are the amplitudes of incident, reflected, and refracted rays, and i and r the angles of incidence and refraction), and concludes: 'en comparant cette dernière équation à celle des forces vives, il faut admettre que $d' = d$, c'est-à-dire que la densité de l'éther est la même dans tous les corps.' This equation is Jamin's equation (α) (see his 'Note...': 415, 417, 421), 'l'équation des forces vives' obtained 'on supposait, comme MacCullagh et Neumann, que $d = d'$'; and see Number 236 note (5). MacCullagh and Neumann had supposed that the density of the ether is the same in all media; see MacCullagh, 'On the laws of crystalline reflexion and refraction': 34, and Neumann, 'Theoretische Untersuchung der Gesetze...': 8.

(6) As supposed by A. J. Fresnel, 'Mémoire sur la loi des modifications que la réflexion imprime à la lumière polarisée', *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, **11** (1832): 393–433, esp. 395–400; and see Number 236 note (12).

(7) Jamin had concluded that Fresnel's solution (based on the boundary condition that the components of the oscillation parallel to the interface are continuous across the interface, and the hypothesis that the densities of the two media are unequal) 'entraîne implicitement entre les composantes verticales des vibrations une relation qu'il est difficile d'admettre, tandis qu'en supposant avec Neumann et MacCullagh que la densité de l'éther est constante dans tous les corps' (Jamin, 'Note sur la théorie de la réflexion et de la réfraction': 420). For this reason Jamin had adopted the boundary condition and the assumption of equal density in the two media as proposed by MacCullagh and Neumann; see notes (4) and (5), and Number 236 note (9).

any hypothesis about the structure of the medium or any mechanical explanation of electricity or magnetism.(8)

The result is that only transverse disturbances can be propagated and that the velocity is that found by Weber and Kohlrausch(9) which is nearly that of light.(10) This is the velocity with which such slow disturbances as we can make would be propagated. If the same law holds for rapid ones then there is no difference between polarized light and rapid electromagnetic disturbances in one plane.(11)

I have written out so much of the theory as does not involve the conditions at bounding surfaces and will send it to the R.S. in a week.(12)

I am trying to understand the conditions at a surface for reflexion and refraction but they may not be the same for the period of vibration of light and for experiments made at leisure.(13)

We are devising methods to determine this velocity $= \dfrac{\text{electromagnetic}}{\text{electrostatic}}$ unit of electricity.(14) Thomson is going to weigh an electromotive force.(15) Jenkin & I are going to measure the capacity of a conductor both ways(16) and I have a plan of direct equilibrium between an electromagnetic repulsion and electrostatic attraction.(17)

Yours truly
J. C. Maxwell

(8) Hence differing from the physical model employed in Maxwell's paper 'On physical lines of force. Part III. The theory of molecular vortices applied to statical electricity', *Phil. Mag.*, ser. 4, **23** (1862): 12–24 (= *Scientific Papers*, **1**: 489–502). See Maxwell's letters to Faraday and William Thomson of 19 October and 10 December 1861 (Volume I: 683–9, 692–8).

(9) Rudolf Kohlrausch and Wilhelm Weber, 'Elektrodynamische Maassbestimmungen insbesondere Zurückführung der Stromintensitätsmessungen auf mechanisches Maass', *Abhandlungen der Königlichen Sächsischen Gesellschaft der Wissenschaften, math.-phys. Klasse*, **3** (1857): 219–92, esp. 260. See Number 238.

(10) See Maxwell, 'On physical lines of force. Part III': 22 (= *Scientific Papers*, **1**: 500); and 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 499 (= *Scientific Papers*, **1**: 580).

(11) See Maxwell, 'A dynamical theory of the electromagnetic field': 501–3 (= *Scientific Papers*, **1**: 583–6); and compare Maxwell, *Treatise*, **2**: 392–4 (§§ 794–7).

(12) See Numbers 238 and 239.

(13) See Number 236.

(14) See Number 235 note (3).

(15) See the 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-ninth Meeting of the British Association* (London, 1870): 434–8, on 434–6.

(16) See Numbers 233 and 234.

(17) See Numbers 235 and 289.

ABSTRACT OF PAPER 'A DYNAMICAL THEORY OF THE ELECTROMAGNETIC FIELD'

[27 OCTOBER 1864][(1)]

From the *Proceedings of the Royal Society*[(2)]

A DYNAMICAL THEORY OF THE ELECTROMAGNETIC FIELD[(3)]

By Professor J. Clerk Maxwell, F.R.S.

Received October 27, 1864

(Abstract)

The proposed Theory seeks for the origin of electromagnetic effects in the medium surrounding the electric or magnetic bodies, and assumes that they act on each other not immediately at a distance, but through the intervention of this medium.

The existence of the medium is assumed as probable, since the investigations of Optics have led philosophers to believe that in such a medium the propagation of light takes place.

The properties attributed to the medium in order to explain the propagation of light are –

1st. That the motion of one part communicates motion to the parts in its neighbourhood.

2nd. That this communication is not instantaneous but progressive, and depends on the elasticity of the medium as compared with its density.

The kind of motion attributed to the medium when transmitting light is that called transverse vibration.

An elastic medium capable of such motions must be also capable of a vast variety of other motions, and its elasticity may be called into play in other ways, some of which may be discoverable by their effects.

One phenomenon which seems to indicate the existence of other motions than those of light in the medium, is that discovered by Faraday, in which the plane of polarization of a ray of light is caused to rotate by the action of

(1) The date the paper was received by the Royal Society. The paper was read on 8 December 1864: see note (2).

(2) *Proc. Roy. Soc.*, **13** (1864): 531–6.

(3) Published in *Phil. Trans.*, **155** (1865): 459–512 (= *Scientific Papers*, **1**: 526–97). Reporting on the paper in a letter to Stokes of 15 March 1865 (Royal Society, *Referees' Reports*, **5**: 137), William Thomson declared that the paper was 'most decidedly suitable for publication in the Transactions'.

magnetic force.(4) Professor W. Thomson*(5) has shown that this phenomenon cannot be explained without admitting that there is motion of the luminiferous medium in the neighbourhood of magnets and currents.(6)

The phenomena of electromotive force seem also to indicate the elasticity or tenacity of the medium. When the state of the field is being altered by the introduction or motion of currents or magnets, every part of the field experiences a force, which, if the medium in that part of the field is a conductor, produces a current. If the medium is an electrolyte, and the electromotive force is strong enough, the components of the electrolyte are separated in spite of their chemical affinity, and carried in opposite directions. If the medium is a dielectric, all its parts are put into a state of electric polarization, a state in which the opposite sides of every such part are oppositely electrified, and this to an extent proportioned to the intensity of the electromotive force which causes the polarization. If the intensity of this polarization is increased beyond a certain limit, the electric tenacity of the medium gives way, and there is a spark or 'disruptive discharge'.

Thus the action of electromotive force on a dielectric produces an electric displacement within it, and in this way stores up energy which will reappear when the dielectric is relieved from this state of constraint.

A dynamical theory of the Electromagnetic Field must therefore assume that, wherever magnetic effects occur, there is matter in motion, and that, wherever electromotive force is exerted, there is a medium in a state of constraint; so that the medium must be regarded as the recipient of two kinds of energy – the actual energy of the magnetic motion, and the potential energy of the electric displacement.(7) According to this theory we look for the explanation of electric and magnetic phenomena to the mutual actions between the medium and the electrified or magnetic bodies, and not to any direct action between those bodies themselves.

* Proceedings of the Royal Society June 1856 and June 1861.

(4) Michael Faraday, 'Experimental researches in electricity. Nineteenth series. On the magnetization of light and the illumination of magnetic lines of force', *Phil. Trans.*, **136** (1846): 1–20 (= *Electricity*, **3**: 1–26).

(5) William Thomson, 'Dynamical illustrations of the magnetic and the heliocoidal rotatory effects of transparent bodies on polarized light', *Proc. Roy. Soc.*, **8** (1856): 150–8; and Thomson, 'On the measurement of electric resistance', *ibid.*, **11** (1861): 313–28, esp. 327n (= *Math. & Phys. Papers*, **5**: 383n).

(6) On Maxwell's theory of the rotation of molecular vortices see Volume I: 692–5.

(7) The terms 'actual energy' and 'potential energy' were introduced by W. J. M. Rankine, 'On the general law of the transformation of energy', *Phil. Mag.*, ser. 4, **5** (1853): 106–17, esp. 106.

In the case of an electric current flowing in a circuit *A*, we know that the magnetic action at every point of the field depends on its position relative to *A*, and is proportional to the strength of the current. If there is another circuit *B* in the field, the magnetic effects due to *B* are simply added to those due to *A*, according to the well-known law of composition of forces, velocities, &c. According to our theory, the motion of every part of the medium depends partly on the strength of the current in *A*, and partly on that in *B*, and when these are given the whole is determined. The mechanical conditions therefore are those of a system of bodies connected with two driving-points *A* and *B*, in which we may determine the relation between the motions of *A* and *B*, and the forces acting on them, by purely dynamical principles. It is shown that in this case we may find two quantities, namely, the 'reduced momentum' of the system referred to *A* and to *B*, each of which is a linear function of the velocities of *A* and *B*. The effect of the force on *A* is to increase the momentum of the system referred to *A*, and the effect of the force on *B* is to increase the momentum referred to *B*. The simplest mechanical example is that of a rod acted on by two forces perpendicular to its direction at *A* and at *B*.[(8)] Then any change of velocity of *A* will produce a force at *B*, unless *A* and *B* are mutually centres of suspension and oscillation.

Assuming that the motion of every part of the electromagnetic field is determined by the values of the currents in *A* and *B*, it is shown –

1st. That any variation in the strength of *A* will produce an electromotive force in *B*.

2nd. That any alteration in the relative position of *A* and *B* will produce an electromotive force in *B*.

3rd. That if currents are maintained in *A* and *B*, there will be a mechanical force tending to alter their position relative to each other.

4th. That these electromotive and mechanical forces depend on the value of a single function *M*, which may be deduced from the form and relative position of *A* and *B*, and is of one dimension in space; that is to say, it is a certain number of feet or metres.

The existence of electromotive forces between the circuits *A* and *B* was first deduced from the fact of electromagnetic attraction, by Professor Helmholtz*[(9)] and Professor W. Thomson†[(10)] by the principle of the Con-

* Conservation of Force. Berlin, 1847: translated in Taylor's Scientific Memoirs, Feb. 1853, p. 114.[(9)]

† Reports of British Association, 1848. Phil. Mag. Dec. 1851.[(10)]

(8) See Number 239 §1.

(9) Hermann Helmholtz, *Über die Erhaltung der Kraft, eine physikalische Abhandlung* (Berlin, 1847); (trans.) 'On the conservation of force', in *Scientific Memoirs, Natural Philosophy*, ed. J. Tyndall and W. Francis (London, 1853): 114–62, esp. 156–8.

(10) William Thomson, 'On the theory of electro-magnetic induction', *Report of the Eighteenth*

servation of Energy.[11] Here the electromagnetic attractions, as well as the forces of induction, are deduced from the fact that every current when established in a circuit has a certain persistency or momentum – that is, it requires the continued action of an unresisted electromotive force in order to alter its value, and that this 'momentum' depends, as in various mechanical problems, on the value of other currents as well as itself. This momentum is what Faraday has called the Electrotonic State of the circuit.[12]

It may be shown from these results, that at every point in the field there is a certain direction possessing the following properties –[13]

A conductor moved in that direction experiences no electromotive force.

A conductor carrying a current experiences a force in a direction perpendicular to this line and to itself.

A circuit of small area carrying a current tends to place itself with its plane perpendicular to this direction.

A system of lines drawn so as everywhere to coincide with the direction having these properties is a system of lines of magnetic force; and if the lines in any one part of their course are so distributed that the number of lines enclosed by any closed curve is proportional to the 'electric momentum' of the field referred to that curve, then the electromagnetic phenomena may be thus stated –

The electric momentum of any closed curve whatever is measured by the number of lines of force which pass through it.

If this number is altered, either by motion of the curve, or motion of the inducing current, or variation in its strength, an electromotive force acts round the curve and is measured by the decrease of the number of lines passing through it in unit of time.

If the curve itself carries a current, then mechanical forces act on it tending to increase the number of lines passing through it, and the work done by these forces is measured by the increase of the number of lines multiplied by the strength of the current.

Meeting of the British Association for the Advancement of Science; held at Swansea in August 1848 (London, 1849), part 2: 9–10 (= *Math. & Phys. Papers*, **1**: 91–2); and Thomson, 'Applications of the principle of mechanical effect to the measurement of electro-motive forces, and of galvanic resistances, in absolute units', *Phil. Mag.*, ser. 4, **2** (1851): 551–62 (= *Math. & Phys. Papers*, **1**: 490–502).

(11) See Volume I: 259.

(12) See Faraday, *Electricity*, **1**: 16 (§60), **3**: 420 (§3269). See Number 214 note (9).

(13) For the discussion of lines of force in the electromagnetic field, which follows, compare 'Part II. On Faraday's electro-tonic state' of Maxwell's 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83 (= *Scientific Papers*, **1**: 155–229). See especially the 'summary of the theory of the electro-tonic state', in 'On Faraday's lines of force': 65–7 (= *Scientific Papers*, **1**: 205–9); and the abstract of the paper (Volume I: 371–5).

A method is then given by which the coefficient of self-induction of any circuit can be determined by means of Wheatstone's electric balance.(14)

The next part of the paper is devoted to the mathematical expression of the electromagnetic quantities referred to each point in the field, and to the establishment of the general equations of the electromagnetic field, which express the relations among these quantities.

The quantities which enter into these equations are – Electric currents by conduction, electric displacements, and Total Currents; Magnetic forces, Electromotive forces, and Electromagnetic Momenta. Each of these quantities being a directed quantity, has three components; and besides these we have two others, the Free Electricity and the Electric Potential, making twenty quantities in all.

There are twenty equations between these quantities, namely Equations of Total Currents, of Magnetic Force, of Electric Currents, of Electromotive Force, of Electric Elasticity, and of Electric Resistance, making six sets of three equations, together with one equation of Free Electricity, and another of Electric Continuity.

These equations are founded on the facts of the induction of currents as investigated by Faraday,(15) Felici,(16) &c., on the action of currents on a magnet as discovered by Oersted,(17) and on the polarization of dielectrics by electromotive force as discovered by Faraday(18) and mathematically developed by Mossotti.(19)

An expression is then found for the intrinsic energy of any part of the field, depending partly on its magnetic, and partly on its electric polarization.

From this the laws of the forces acting between magnetic poles and between electrified bodies are deduced, and it is shown that the state of constraint due

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(14) See Number 230, esp. note (4).

(15) Michael Faraday, 'Experimental researches in electricity. First series. On the induction of electric currents...', *Phil. Trans.*, **122** (1832): 125–62 (= *Electricity*, **1**: 1–41); Faraday, 'Experimental researches in electricity. Ninth series. On the influence, by induction of an electric current on itself: and on the inductive action of electric currents generally', *Phil. Trans.*, **125** (1835): 41–56 (= *Electricity*, **1**: 322–43).

(16) Riccardo Felici, 'Mémoire sur l'induction électrodynamique', *Ann. Chim. Phys.*, ser. 3, **34** (1852): 64–77.

(17) H. C. Oersted, *Experimenta circum Effectum Conflictus Electrici in Acum Magneticam* (Copenhagen, 1820).

(18) Michael Faraday, 'Experimental researches in electricity. Eleventh series. On induction', *Phil. Trans.*, **128** (1838): 1–40, 79–81 (= *Electricity*, **1**: 360–416).

(19) O. F. Mossotti, 'Discussione analitica sull' influenza che l'azione di un mezzo dielettrico ha sulla distribuzione dell' elettricità alla superfizie di più corpi elettrici disemminati in esso', *Memorie di Matematica e di Fisica della Società Italiana delle Scienze* (Modena), **24** (1850): 49–74.

to the polarization of the field is such as to act on the bodies according to the well-known experimental laws.

It is also shown in a note that, if we look for the explanation of the force of gravitation in the action of a surrounding medium, the constitution of the medium must be such that, when far from the presence of gross matter, it has immense intrinsic energy, part of which is removed from it wherever we find the signs of gravitating force. This result does not encourage us to look in this direction for the explanation of the force of gravity.(20)

The relation which subsists between the electromagnetic and the electrostatic system of units is then investigated, and shown to depend upon what we have called the Electric Elasticity of the medium in which the experiments are made (i.e. common air). Other media, as glass, shellac, and sulphur have different powers as dielectrics; and some of them exhibit the phenomena of electric absorption and residual discharge.

It is then shown how a compound condenser of different materials may be constructed which shall exhibit these phenomena, and it is proved that the result will be the same though the different substances were so intimately intermingled that the want of uniformity could not be detected.

The general equations are then applied to the foundation of the Electromagnetic Theory of Light.

Faraday, in his 'Thoughts on Ray Vibrations'*,(21) has described the effect of the sudden movement of a magnetic or electric body, and the propagation of the disturbance through the field, and has stated his opinion that such a disturbance must be entirely transverse to the direction of propagation. In 1846 there were no data to calculate the mathematical laws of such propagation, or to determine the velocity.

The equations of this paper, however, show that transverse disturbances, and transverse disturbances only, will be propagated through the field, and that the number which expresses the velocity of propagation must be the same as that which expresses the number of electrostatic units of electricity in one electromagnetic unit, the standards of space and time being the same.

The first of these results agrees, as is well known, with the undulatory

* Phil. Mag. 1846. Experimental Researches, vol. iii. p. 447.(21)

(20) Compare Maxwell's discussion of gravity in his letters to Faraday of 9 November 1857 (Volume I: 550), to George Phillips Bond of 25 August 1863 (Number 217), and to William Huggins of 13 October 1868 (Number 309).

(21) Michael Faraday, 'Thoughts on ray-vibrations', *Phil. Mag.*, ser. 3, **28** (1846): 345–50 (= *Electricity*, **3**: 447–52). On Faraday's statement, in his letter to Maxwell of 25 March 1857 (*Life of Maxwell*: 519–20), of his intention to 'make some experiments on the time of magnetic action' see Volume I: 686n.

theory of light as deduced from optical experiments. The second may be judged of by a comparison of the electromagnetical experiments of Weber and Kohlrausch(22) with the velocity of light as determined by astronomers in the heavenly spaces, and by M. Foucault in the air of his laboratory.

Electrostatic units in an electromagnetic unit	310,740,000 metres per second.(23)
Velocity of light as found by M. Fizeau	314,858,000.(24)
Velocity of light by M. Foucault	298,000,000.(25)
Velocity of light deduced from aberration	308,000,000.(26)

At the outset of the paper, the dynamical theory of the electromagnetic field borrowed from the undulatory theory of light the use of its luminiferous medium. It now restores the medium, after having tested its powers of transmitting undulations, and the character of those undulations, and certifies that the vibrations are transverse, and that the velocity is that of light. With regard to normal vibrations, the electromagnetic theory does not allow of their transmission.

What, then, is light according to the electromagnetic theory? It consists of alternate and opposite rapidly recurring transverse magnetic disturbances, accompanied with electric displacements, the direction of the electric displacement being at right angles to the magnetic disturbance, and both at right angles to the direction of the ray.

The theory does not attempt to give a mechanical explanation of the nature of magnetic disturbance or of electric displacement, it only asserts the identity of these phenomena, as observed at our leisure in magnetic and electric experiments, with what occurs in the rapid vibrations of light, in a portion of time inconceivably minute.

This paper is already too long to follow out the application of the electromagnetic theory to the different phenomena already explained by the

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(22) Rudolf Kohlrausch and W. Weber, 'Elektrodynamische Maassbestimmungen insbesondere Zurückführung der Stromintensitätsmessungen auf mechanisches Maass', *Abhandlungen der Königlichen Sächsischen Gesellschaft der Wissenschaften, math.-phys. Klasse*, **3** (1857): 219–92 (= *Wilhelm Weber's Werke*, 6 vols. (Berlin, 1892–4), **3**: 609–76).

(23) Kohlrausch and Weber, 'Elektrodynamische Maassbestimmungen': 260. See Volume I: 685, 695; and Number 218 note (7).

(24) Hippolyte Fizeau, 'Sur une expérience relative à la vitesse de propagation de la lumière', *Comptes Rendus*, **29** (1849): 90–2.

(25) Léon Foucault, 'Détermination expérimentale de la vitesse de la lumière; parallaxe du soleil', *Comptes Rendus*, **55** (1862): 501–3, esp. 502.

(26) Foucault, 'Détermination expérimentale de la vitesse de la lumière': 502.

undulatory theory. It discloses a relation between the inductive capacity of a dielectric and its index of refraction. The theory of double refraction in crystals is expressed very simply in terms of the electromagnetic theory. The non-existence of normal vibrations and the ordinary refraction of rays polarized in a principal plane are shown to be capable of explanation; but the verification of the theory is difficult at present, for want of accurate data concerning the dielectric capacity of crystals in different directions.

The propagation of vibrations in a conducting medium is then considered, and it is shown that the light is absorbed at a rate depending on the conducting-power of the medium. This result is so far confirmed by the opacity of all good conductors, but the transparency of electrolytes shows that in certain cases vibrations of short period and amplitude are not absorbed as those of long period would be.

The transparency of thin leaves of gold, silver, and platinum cannot be explained without some such hypothesis.(27)

The actual value of the maximum electromotive force which is called into play during the vibrations of strong sunlight is calculated from Pouillet's data,(28) and found to be about 60,000,000, or about 600 Daniell's cells per metre.(29)

The maximum magnetic force during such vibrations is .193, or about $\frac{1}{10}$ of the horizontal magnetic force at London.

Methods are then given for applying the general equations to the calculation of the coefficient of mutual induction of two circuits, and in particular of two circles the distance of whose circumferences is small compared with the radius of either.

The coefficient of self-induction of a coil of rectangular section is found and applied to the case of the coil used by the Committee of the British Association on Electrical Standards. The results of calculation are compared with the value deduced from a comparison of experiments in which this coefficient enters as a correction, and also with the results of direct experiments with the electric balance.

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(27) See Number 239 §7.

(28) C. S. M. Pouillet, 'Mémoire sur la chaleur solaire, sur les pouvoirs rayonnants et absorbants de l'air atmosphérique, et sur la température de l'espace', *Comptes Rendus*, **7** (1838): 24–65; (trans.) 'Memoir on the solar heat, on the radiating and absorbing powers of the atmospheric air, and on the temperature of space', in *Scientific Memoirs*, ed. R. Taylor, **4** (London, 1846): 44–90, esp. 79. See William Thomson, 'On the mechanical energies of the solar system', *Trans. Roy. Soc. Edinb.*, **21** (1854): 63–80, esp. 66 (= *Maths. & Phys. Papers*, **2**: 6), where Thomson uses Pouillet's data to calculate the energy of sunlight at the earth's surface.

(29) On Daniell's cell see Number 235 note (11).

CANCELLED PASSAGES IN THE MANUSCRIPT AND PROOFS OF 'A DYNAMICAL THEORY OF THE ELECTROMAGNETIC FIELD'[1]

OCTOBER 1864[2]

From the originals in the Libraries of the Royal Society, London and The Johns Hopkins University[3]

[1. Passage deleted in the manuscript from §23 on the 'Mutual action of two currents':][4]

As a dynamical illustration suppose two horses harnessed to a carriage by the intervention of a lever so that each horse pulls at its own arm of the lever while the lever is attached to the carriage by its fulcrum. Then if one horse increases its speed the immediate effect will be to produce a tension in the traces of the other horse tending to pull him back.

[2. Passage deleted in the manuscript from §34 on the 'Mechanical action between conductors', concerning the explication of the relations between electric currents 'by mechanical reasoning':][5]

[...] all such considerations and confine myself to the solution of problems on the mutual action of currents, the experimental determination of coefficients of induction, and the calculation of these coefficients from the known form of the circuit.

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(1) J. Clerk Maxwell, 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512 (= *Scientific Papers*, **1**: 526–97).

(2) The paper was received by the Royal Society on 27 October 1864: see Number 238. The proofs are dated 10 August 1865.

(3) The MS is in Royal Society, PT. 72.7. The proofs are in the Stokes Collection, Electromagnetism volume 1, QC 760 E3, Eisenhower Library, The Johns Hopkins University, Baltimore.

(4) See 'A dynamical theory of the electromagnetic field': 467 (= *Scientific Papers*, **1**: 537).

(5) See 'A dynamical theory of the electromagnetic field': 471 (= *Scientific Papers*, **1**: 542).

[3. Passage deleted in the manuscript from §55, on 'Electrical displacements (f, g, h)':][6]

Electrical quantity (e)

Let e represent the quantity of free electricity in unit of volume (either positive or negative) then the equation of continuity[7] is

$$e+\frac{df}{dx}+\frac{dg}{dy}+\frac{dh}{dz}=0.$$

[4. Passage deleted in the manuscript from §81, on the 'Measurement of electrostatic effects':][8]

The quantity E which denotes the number of electrostatic units of electricity which are contained in the electromagnetic unit in Webers system of electrical measurements is a velocity which as determined by Weber and Kohlrausch[9] is

310 740 kilometers per second
195 647 miles per second.[10]

This quantity determines according to our theory what we have called k the electric elasticity of the medium, by the equation

$$k = 4\pi E^2.$$ [11]

As the value of E is of great importance in electrical science the Committee of the British Association on Electric Standards are making arrangements for a new determination of it.[12]

(6) See 'A dynamical theory of the electromagnetic field': 480 (= *Scientific Papers*, **1**: 554). The passage is transferred (in revised form) to §68 of the paper; see 'A dynamical theory of the electromagnetic field': 485 (= *Scientific Papers*, **1**: 561).

(7) This equation is termed, in §68 of the paper, the 'equation of free electricity'; on the significance of the sign in the equation for e, see Daniel M. Siegel, *Innovation in Maxwell's Electromagnetic Theory* (Cambridge, 1991): 149–50.

(8) See 'A dynamical theory of the electromagnetic field': 492 (= *Scientific Papers*, **1**: 570).

(9) See Number 238 notes (22) and (23).

(10) In 'On physical lines of force. Part III. The theory of molecular vortices applied to statical electricity', *Phil. Mag.*, ser. 4, **23** (1862): 12–24, esp. 22 (= *Scientific Papers*, **1**: 499–500) Maxwell gives 193,088 miles per second as the equivalent of Kohlrausch and Weber's metric value, and calculates Fizeau's value of the velocity of light as 195,647 miles per second. See Volume I: 695.

(11) See 'A dynamical theory of the electromagnetic field': 491 (= *Scientific Papers*, **1**: 569); and Number 231.

(12) See Number 237 esp. notes (15) to (17).

[5. Final paragraph in §99 on the propagation of transverse vibrations, which was amended in proof:][13]

Hence J is either zero or it continually increases or diminishes with the time, if e remains constant, which no physical quantity can do. Hence J is zero and the only disturbance propagated is that indicated by F', G', H'[14] which is wholly transversal.

[An additional passage on the verso of the manuscript is cancelled in pencil:]

Differentiating these three equations with respect to x y & z respectively and adding we find that the first terms destroy each other leaving

$$\frac{d^2J}{dt^2}+\frac{d}{dt}\nabla^2\psi = 0.^{(15)}$$

[6. Passage deleted in the manuscript at end of §102 on the 'Propagation of electromagnetic disturbances in a crystallised medium':][16]

[...] and that (as in the case of an isotropic medium) the terms including ψ have nothing to do with the result.

(13) See 'A dynamical theory of the electromagnetic field': 500 (= *Scientific Papers*, **1**: 582) for Maxwell's revision: 'Since the medium is a perfect insulator, e, the free electricity is immoveable, and therefore dJ/dt is a function of x, y, z, and the value of J is either constant or zero, or uniformly increasing or diminishing with the time; so that no disturbance depending on J can be propagated as a wave.' Here $J = dF/dx+dG/dy+dH/dz$, where F, G, H are the components of the electro-tonic state (electromagnetic momentum); see 'A dynamical theory of the electromagnetic field': 497 (= *Scientific Papers*, **1**: 578). His concern here is to obtain a condition for the propagation of transverse electromagnetic waves. The question of transverse and longitudinal vibrations in dynamical theories of the optical ether had been recently discussed by G. G. Stokes, 'Report on double refraction', *Report of the Thirty-second Meeting of the British Association* (London, 1863): 253–82 (= *Papers*, **4**: 157–202). Compare the alternative form of the argument presented in the *Treatise*, **2**: 385 (§783).

(14) Maxwell wrote $F' = F-d\chi/dt, \ldots$ and $\nabla^2\chi = J$; see 'A dynamical theory of the electromagnetic field': 500 (= *Scientific Papers*, **1**: 581).

(15) Maxwell writes $\nabla^2 = d^2/dx^2+d^2/dy^2+d^2/dz^2$; see 'A dynamical theory of the electromagnetic field': 497 (= *Scientific Papers*, **1**: 578). For this usage see William Thomson, 'Dynamical problems regarding elastic spheroidal shells and spheroids of incompressible liquid', *Phil. Trans.*, **153** (1863): 583–616, esp. 583 (= *Math. & Phys. Papers*, **3**: 351–94). ψ is the electric potential.

(16) See 'A dynamical theory of the electromagnetic field': 501 (= *Scientific Papers*, **1**: 583).

[7. Part of the final paragraph of §107 on transparent solid bodies, which was amended in proof:][17]

[...] experiment, the amount of light which passes through a thickness of $\frac{1}{282000}$ inch would be only 10^{-300} of the incident light, a totally imperceptible quantity. I find that between $\frac{1}{500}$ and $\frac{1}{900}$ of green light gets through such gold leaf.

[8. Following §115 on the determination of the coefficient of self-induction of a coil of wire (L), a passage on the verso of the manuscript is cancelled in pencil:][18]

Hence the total counter current in each element

$$\frac{1}{\rho}(T_{\infty}-T_0)-\frac{\mu\pi}{\rho^2}Pr^2.^{(19)}$$

Integrating over the section of the wire from $r=0$ to $r=r$

$$\frac{1}{\rho}(T_{\infty}-T_0)\,\pi r^2+\frac{\mu\pi}{\rho^2}\frac{P\pi r^4}{2}$$

or since $\dfrac{P}{\rho}=p$ and if C be the total current $C=\pi r^2 p$ the second term becomes $\dfrac{1}{2}\dfrac{\mu\pi}{\rho}Cr^2$.

Now if p instead of being variable from the centre to the circumference of the wire had been uniform at every instant throughout the section the second term would have been

$$\frac{3}{4}\frac{\mu\pi}{\rho}Cr^2.$$

Now the counter current is $\dfrac{L}{R}C$.

(17) See 'A dynamical theory of the electromagnetic field': 504–5 (= *Scientific Papers*, **1**: 587) for the revision: '[...] make experiments, the amount of light which passes through a piece of gold-leaf, of which the resistance was determined by Mr. C. Hockin, would be only 10^{-50} of the incident light, a totally imperceptible quality. I find that between 1/500 and 1/1000 of green light gets through such gold-leaf'. See Number 235 esp. note (27).

(18) See 'A dynamical theory of the electromagnetic field': 510–11 (= *Scientific Papers*, **1**: 595–6).

(19) P is the electromotive force in a cylindrical wire of specific resistance ρ, μ is the coefficient of magnetic induction, T is a function of the time, and r is the distance from the axis of the cylinder.

Hence the correction to be applied on account of the current being variable is

$$-\frac{1}{4} \text{ per unit length.}$$ [20]

(20) Compare Maxwell's letter to Thomson of 11 September 1863 (Number 219, esp. note (4)).

FROM A LETTER TO CHARLES HOPE CAY[1]

5 JANUARY 1865

From Campbell and Garnett, *Life of Maxwell*[2]

Glenlair
5 January 1865

The Manse of Corsock is now finished;[3] it is near the river, not far from the deep pool where we used to bathe.

I set Prof. W. Thomson a prop. which I had been working with for a long time. He sent me 18 pages of letter of suggestions about it, none of which would work; but on Jan 3, in the railway from Largs, he got the way to it, which is all right; so we are jolly, having stormed the citadel, when we only hoped to sap it by approximations.[4]

The prop. was to draw a set of lines like this so that the ultimate reticulations shall all be squares.[5]

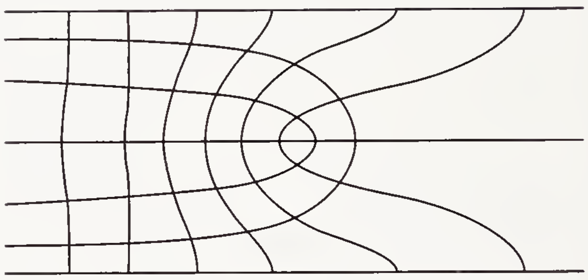

Figure 240,1

The solution is exact, but rather stiff. Now I have a disc *A* hung by a wire *D*, between two discs *B*, *C*, the interval being occupied by air, hydrogen, carbonic acid, etc., the friction of which gradually brings *A* to rest. In order to calculate the thickdom or viscosity of the gas, I require to solve the problem above mentioned, which is now done, and I have the apparatus now ready to begin.[6] We are also intent on electrical measurements, and are getting up apparatus, and have made sets of wires of alloy of platinum and silver, which

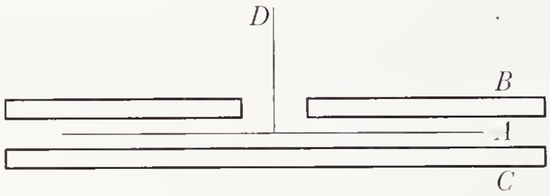

Figure 240,2

(1) Maxwell's cousin (son of Robert Cay), Caius 1860, 6th wrangler 1864, Fellow 1865–9 (Venn); see *Life of Maxwell*: 315, 324, 343–4.

(2) *Life of Maxwell*: 341–2; abridged.

(3) See Number 216 esp. note (3).

(4) See also Number 303. The problem arose in the mathematical theory of Maxwell's experiments on gas viscosity; see Maxwell, 'On the viscosity or internal friction of air and other gases', *Phil. Trans.* **156** (1866): 249–68, esp. 261–2 (= *Scientific Papers*, **2**: 16–17).

(5) Compare Fig. 9 of 'On the viscosity or internal friction of air and other gases': Plate XXI (= *Scientific Papers*, **2**: Plate IX).

(6) See Numbers 244, 245 and 246.

are to be sent all abroad as standards of resistance.(7) I have also a paper afloat, with an electromagnetic theory of light, which, till I am convinced to the contrary, I hold to be great guns.(8)

Spice(9) is becoming first-rate: she is the principal patient under the ophthalmoscope,(10) and turns her eyes at command, so as to show the tapetum, the optic nerve, or any required part. Dr. Bowman, the great oculist,(11) came to see the sight, and when we were out of town he came again and brought Donders of Utrecht(12) with him to visit Spice.

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(7) See the 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-fifth Meeting of the British Association... in September 1865* (London, 1866): 308–13.

(8) See Numbers 238 and 239.

(9) A dog.

(10) On Maxwell's construction of an ophthalmoscope, based on the instrument devised by Helmholtz, and his use of it in 1854–5 to study dogs' eyes, see Volume I: 250, 304, 308, 315.

(11) William Bowman, Surgeon to the Royal London Ophthalmic Hospital, 'among the first to become expert' in the use of the ophthalmoscope (*DNB*): see *Proc. Roy. Soc.*, **52** (1893): i–vii.

(12) Franciscus Cornelis Donders; see Bowman's obituary notice in *Proc. Roy. Soc.*, **49** (1891): vii–xxiv.

NOTES ON THE DETERMINATION OF THE NUMBER OF ELECTROSTATIC UNITS IN ONE ELECTROMAGNETIC UNIT OF ELECTRICITY

circa FEBRUARY 1865(1)

From the original in the University Library, Cambridge(2)

[1] DETERMINATION OF v BY WIPPE(3)

B = resistance of Battery
Electromotive force of d° = F.
P, Q R resistance coils,
G = resistance of galvanometer.
$KHLM$ potentials of these points
C = capacity of condenser.

Exterior of condenser always connected with H, Interior alternately with K & L.

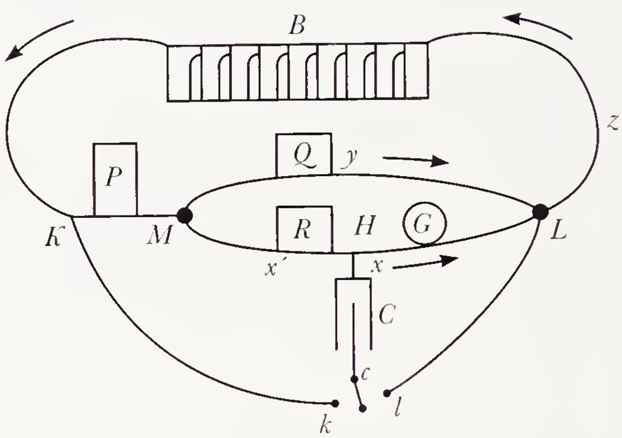

Figure 241,1

1st When C is in equilibrium let x be the current through G then

$$M-L = (R+G)\,x = Qy$$

$$K-M = P(x+y) = P\left(1+\frac{R+G}{Q}\right)x$$

$$\therefore K-L = \left(P+R+G+\frac{P}{Q}(R+G)\right)x.$$

2nd When c after touching k leaves it and goes to l it discharges through lL a quantity of electricity = $C(K-L)$ which returns to H in various ways. 1st direct through G 2nd through Q and R 3rd through B, P and R. Hence the discharge through G

$$= X_1 = -C(K-L)\left\{1-\frac{(B+P)\,G+GQ}{(B+P+R)\,Q+(B+P)\,(R+G)+GQ}\right\}.$$

(1) See Maxwell's letters to William Thomson of 15 October 1864 and 25 February 1865 (Numbers 235 and 242).

(2) ULC Add. MSS 7655, V, c/14 (iii).

(3) Commutator (see Number 234 note (4)). This MS is discussed and reproduced in facsimile by I. B. Hopley, 'Maxwell's determination of the number of electrostatic units in one electromagnetic unit of electricity', *Annals of Science*, **15** (1959): 91–108, esp. 105–8.

3rd When c after touching l leaves it and goes to k it discharges through kK a quantity of electricity $= -C(K-L)$ which returns to H.

1st by P and R, 2nd by $P\ Q\ \&\ G$ 3rd by B and G.

The discharge through G is

$$X_2 = C(K-L)\left\{\frac{R(P+B)+Q(P+R)}{(P+B)\,R+Q(P+R+B+G)+B(R+G)}\right\}.^{(4)}$$

The total discharge through G when c makes a double oscillation is X_1+X_2. If this takes place in time t and if the total current in $G = \xi$(5)

$$\xi = x\left\{1-\frac{C}{t}\left\{P+(R+G)\left(1+\frac{P}{Q}\right)\right\}\right\}\left\{1-\frac{(P+Q+B)\,G}{(P+B)\,(R+G)+Q(P+R+G+B)}\right.$$
$$\left.-\frac{R(P+B)+Q(P+R)}{(P+B)\,R+B(R+G)+Q(P+R+B+Q)}{}_{(6)}\right\}.$$

[2] THOMSONS PLAN. DOUBLE WIPPE.(7)

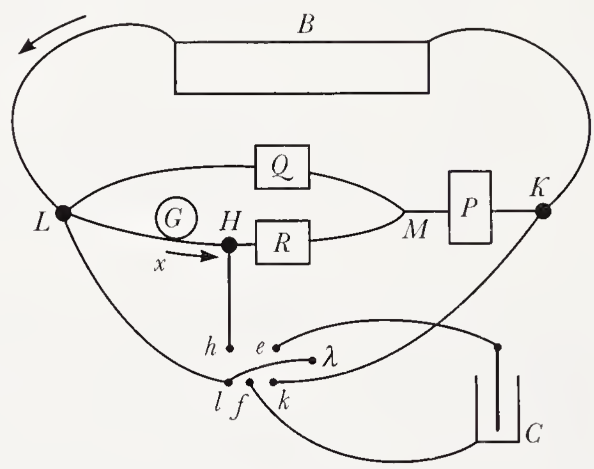

Figure 241,2

e and f go simultaneously to right and left when connected to h & l difference of potentials

$$H-L = -GX$$

λ & k

$$L-M = \left(\frac{(G+R)\,(P+Q)+P}{Q}\right)x$$

1st motion left to right. Quantity discharged $= C(H-L-\overline{L-K})$

$$= -C\left(2G+R+(G+R)\frac{P}{Q}+P\right)x.$$

The discharge is from L to K through three conductors $G+R+P$, $Q+P$ and B.

(4) The denominator should read: $Q(P+R+B+Q)+(B+P)\,(R+G)$.

(5) From the galvanometer reading C may be determined in electromagnetic units. From a determination of C in electrostatic units the value of v may be obtained.

(6) The denominator should read: $Q(P+R+B+Q)+(B+P)\,(R+G)$.

(7) See Number 242.

The proportion through G is $\dfrac{1}{1+\dfrac{G+R}{Q}+\dfrac{P}{B}\dfrac{Q+G+R}{Q}}$

$$=\frac{BQ}{(B+P)\,(Q+G+R)+Q(G+R)}.$$

2nd motion right to left. Discharge $= C(L-K-\overline{H-L})$

$$=C\left(2G+R+P+(G+R)\frac{P}{Q}\right)x.$$

This discharge is from H to L.

Proportion through $G = 1-\dfrac{G}{G+R+\dfrac{(P+B)Q}{P+B+Q}}$

Total discharge through G in a complete double vibration

$$-C\left(2G+R+P+(G+R)\frac{P}{Q}\right)x\left\{1-\frac{G(P+B+Q)}{(G+R)\,(P+B+Q)+(P+B)\,Q}\right.$$
$$\left.+\frac{BQ}{(B+P)\,(Q+G+R)+Q(G+P)}\right\}$$
$$=-Cx\left(2G+P+R+(G+R)\frac{P}{Q}\right)\left\{1+\frac{B(Q-G)-G(P+Q)}{B(Q+G+R)+(P+Q)\,(G+R)+PQ}\right\}.$$

The condition of B disappearing is $\dfrac{G-Q}{Q+G+R}=\dfrac{G(P+Q)}{(P+Q)\,(G+R)+PQ}$

whence $P(Q+G+R)+Q(2G+R)=0$.

This cannot be fulfilled but by making P much greater than Q R or G its effect is insignificant.

Let $G:Q:R:P::1:10:10:100$ the fraction becomes

$$1-\frac{110+9B}{110\times 11+1100+21B}.$$

LETTER TO WILLIAM THOMSON

25 FEBRUARY 1865

From the original in the University Library, Glasgow[1]

8 Palace Gardens Terrace
1865 Feb 25

Dear Thomson

I have been considering the comparison of capacities of condensers by means of an electro*scope* instead of an electro*meter*. (Of course a delicate electrometer is an electroscope too.)[2] I have devised an instrument for exchanging electricity in various forms, and I want to know if you have either a plan or an actual machine before I set it to be made.

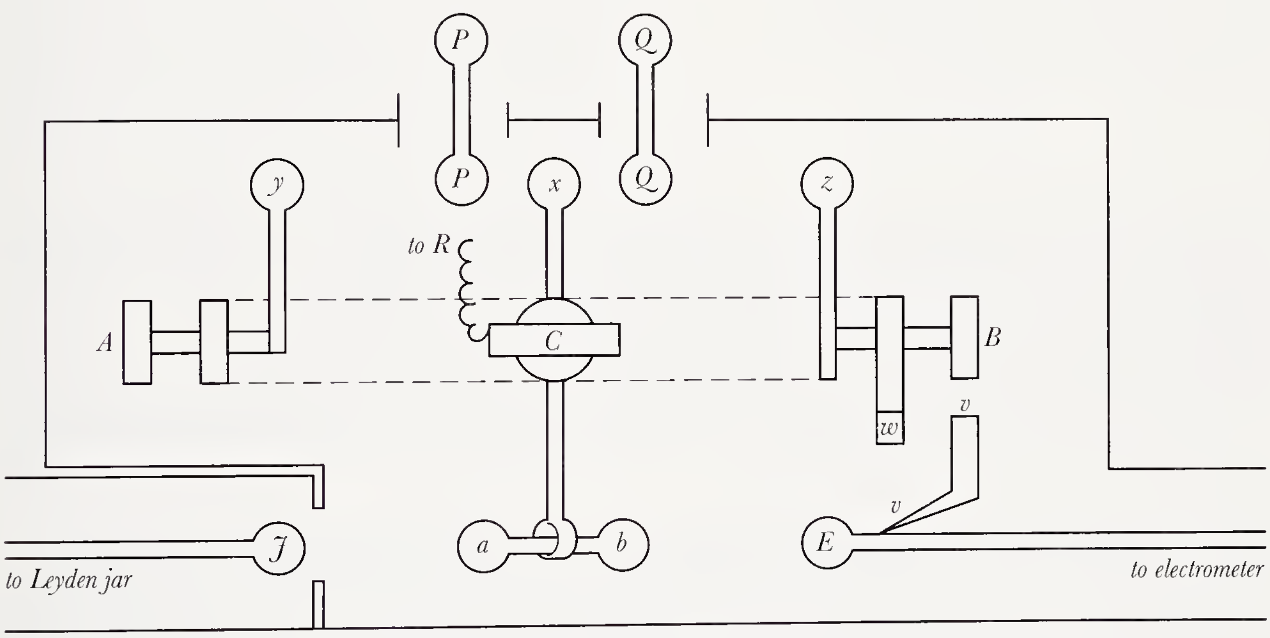

Figure 242,1. Handle is in front. *R* is below *P* connected with *C*. *S* is below *Q* a knob on the box.

J Leyden jar *E* electrometer Electrodes
PQRS the four conductors of 2 condensers all at the back of the box.
ABC a sector centred at the bottom & working between 2 stops about $\frac{5}{8}$ inch plug.

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(1) Glasgow University Library, Kelvin Papers, M 18.

(2) Compare Maxwell's comments in the *Treatise*, **1**: 262–3 (§214); and see William Thomson, 'Report on electrometers and electrostatic measurements', in 'Report of the committee on standards of electrical resistance', *Report of the Thirty-seventh Meeting of the British Association* (London, 1868): 489–512, esp. 489 (= *Electrostatics and Magnetism*: 260–86).

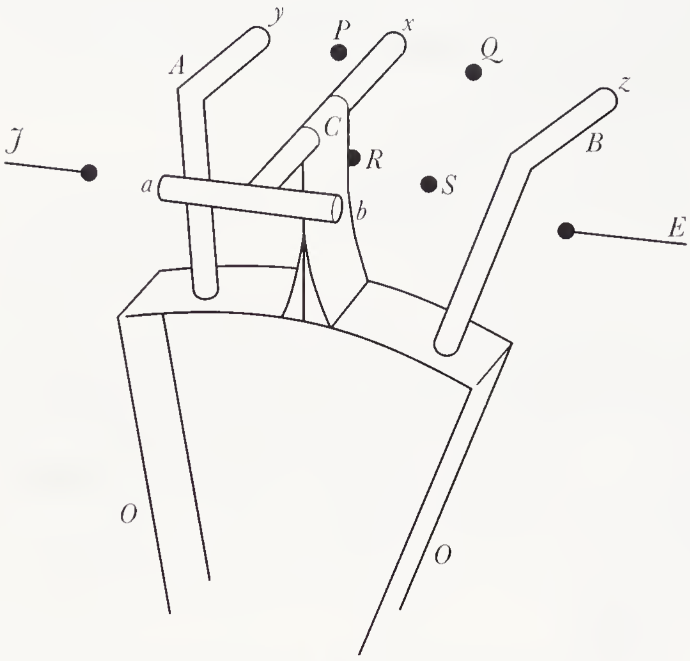

Figure 242,2

Figure 242,3. Process to compare 2 condensers.

It carries the T shaped metal $x\,a\,b$ on the vulcanite arm C which has a slight spring also Ay and Bz metal springs connected with the ground.
R is always connected to x and S to ground
v is a crank which touches E and keeps it to earth till wanted.
1st Put sector hard up to left. z touches Q and x touches P and a is twisted round till it touches J.
This makes $P = R = J$, $Q = S = 0$.
2nd Move to right slightly. a leaves J but x and z still touch P and Q.

$P = R$ insulated $\quad Q = S = 0$.

3 More to right x and z leave P and Q

$P\ Q$ and R all separately insulated.

4 More to right y touches P and

$$P = 0.$$

5 More to right x touches Q and connects R & Q $\quad W$ lifts v from x

$R = Q$, $\quad P = 0 \quad S = 0$, $\quad E$ insulated.

6 Hard up to right b touches E and discharges $R + Q$ to electrometer.

$R = Q = E \quad P = 0, \quad S = 0.$

If $E = 0$ then the capacities are equal of R+electrode x, a, b and Q+electrode Q.

So much for the comparison of equal condensers. I think you said you had a machine for it if not I will get one made.

I think your disc is about 4 inches diameter in the condenser with micrometer screw.(3)

The micrometer will act well in measuring distances from $\frac{1}{4}$ to $\frac{1}{2}$ inch or for capacities from 4 to 2 inches. If the diameter of each plate of the great condenser is 8 inches and distance about $\frac{1}{20}$ inch the capacity of each pair of surfaces will be 80 inches or for 2 pair 160 inches.

I would therefore make a set of condensers equal respectively to 2, 4, 8, 16, 32, 64, 128, inches, roughly estimated thus.

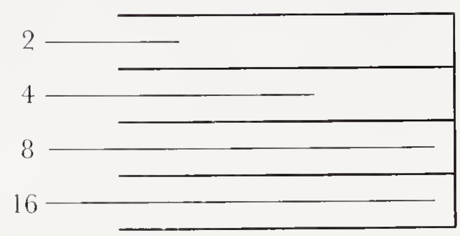

Figure 242,4

These are all to consist of brass plates in a brass vessel insulated by small discs or pillars of vulcanite or glass. The first two can be measured directly by the micrometrical condenser. The others by combining the micrometer condenser with the smaller condensers of the series.

The great condenser is to consist of a series each of which = 160 inches or thereby and can be measured separately in this way. 25 pairs would give about 100 metres capacity which is a very good quantity for the exp^{t}.

I wrote you a plan of the wippe exp^{t} about January which I now see to be all wrong because of the double discharge. I now see we must use a double wippe, if the discharge is to go through the same wire as the constant current. So we must either have double wippe or double galvanometer coil as I

(3) Thomson's guard-ring modification to the attracted disc electrometer (see Number 289 note (11)), which Maxwell employed in his measurement of the ratio of electromagnetic and electrostatic units: see his letter to Thomson of 15 October 1864 (Number 235); and his drawing of the torsion balance for the experiment, dated 1 March 1865 (Number 243).

proposed at first.[4] The latter is the simplest mechanically but the former I most approve.

Jenkin has done nothing towards the electro dynamometer for you & Joule as yet.[5] We have the experimental coil still in existence and accurately measured.[6] Now you want a more compact and sensitive instrument than our proposed great one so I think we should make the instrument on a smallish scale, say great coils 4 inches radius small coils about 1 inch radius and determine their true coefficients by galvanometric comparison with the King's College Coils – thus.

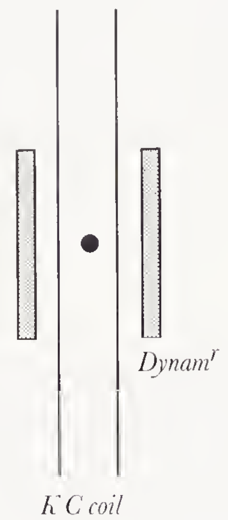

Figure 242,5

for great coil
Send currents opposite ways and determine ratio of resistances when the magnet is not affected.

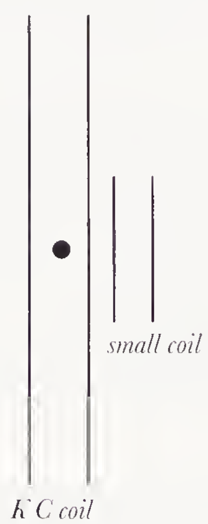

Figure 242,6

for small coil
Send the same current opposite ways and determine the distance of the small coil from the magnet when there is no deflexion.

(4) See Number 241, and the experimental arrangement described by Maxwell in his letter to Thomson of 27 September 1864 (Number 234).

(5) See the 'Report of the committee on standards of electrical resistance', *Report of the Thirty-seventh Meeting of the British Association*: 479; and J. P. Joule, 'Determination of the dynamical equivalent of heat from the thermal effects of electric currents', *ibid.*: 512. See also the *Treatise*, **2**: 332–3 (§726).

(6) The experimental coils used in the British Association measurements of electrical resistance (at King's College, London) in 1863 and 1864: see Numbers 210 note (2) and 222 note (3).

I am reading Webers expts with Dynam^r[7] and will have a little more experience presently. Gassiot[8] has a dynam^r of Webers construction wh: is at present with Robinson, Armagh.[9] Gassiot has also 2000 cells of sulphate of mercury[10] which are doing very well and Matthiessen is to make him a tellurium resistance and measure it.[11]

Gassiot says he will be delighted to let you measure his electromotive force by electrometer or dynam^r.

How do you get on with your electrometer balance?[12] If you can work it with the dynamometer well and good, otherwise I am game to construct a special electrometer and dynamometer combined in which the electric attraction of 2 discs is balanced by the electromagnetic repulsion of two coils.

$A\,A'$ suspended discs at potential 0
$B\,B'$ fixed large discs at potential F
$a\,a'$ rings surrounding $A\,A'$ at 0
coils in A and B' $+^{\text{ve}}$ A' and B $-^{\text{ve}}$
$\therefore$ no terrestrial directive action[13]

$$R = \text{resistance} \quad \text{current} = \frac{F}{R}$$

R to be altered till there is no deflexion for a certain distance AB or $A'B'$ or these distances are to be altered R remaining.

(7) Wilhelm Weber, 'Elektrodynamische Maassbestimmungen', *Ann. Phys.*, **73** (1848): 193–240, figs. facing 336; (trans.) 'On the measurement of electrodynamic forces', *Scientific Memoirs*, ed. R. Taylor, **5** (London, 1852): 489–529. Weber gave a full account of his experiments with the electrodynamometer in his 'Elektrodynamische Maassbestimmungen, über ein allgemeines Grundgesetz der elektrischen Wirkung', *Abhandlungen der Königlichen Sächsischen Gesellschaft der Wissenschaften* (1846): 211–378 (= *Wilhelm Weber's Werke*, 6 vols. (Berlin, 1892–4), **3**: 25–214). See Maxwell's account in the *Treatise*, **2**: 328–9 (§725).

(8) John Peter Gassiot, a researcher in electricity; see his 'A description of an extensive series of the water battery', *Phil. Trans.*, **134** (1844): 39–52.

(9) See Number 198 note (3).

(10) Compare the battery of Gassiot's used by Maxwell in his 1868 determination of the ratio of electrical units: see Number 289.

(11) Augustus Matthiessen: see Number 245. See A. Matthiessen and M. von Bose, 'On the influence of temperature on the electric conducting power of metals', *Phil. Trans.*, **152** (1862): 1–27, on 20–2.

(12) See Thomson's account of attracted disc electrometers in his 'Report on electrometers and electrostatic measurements', *Report of the Thirty-seventh Meeting of the British Association*: 497–509.

(13) Compare Number 243 note (2).

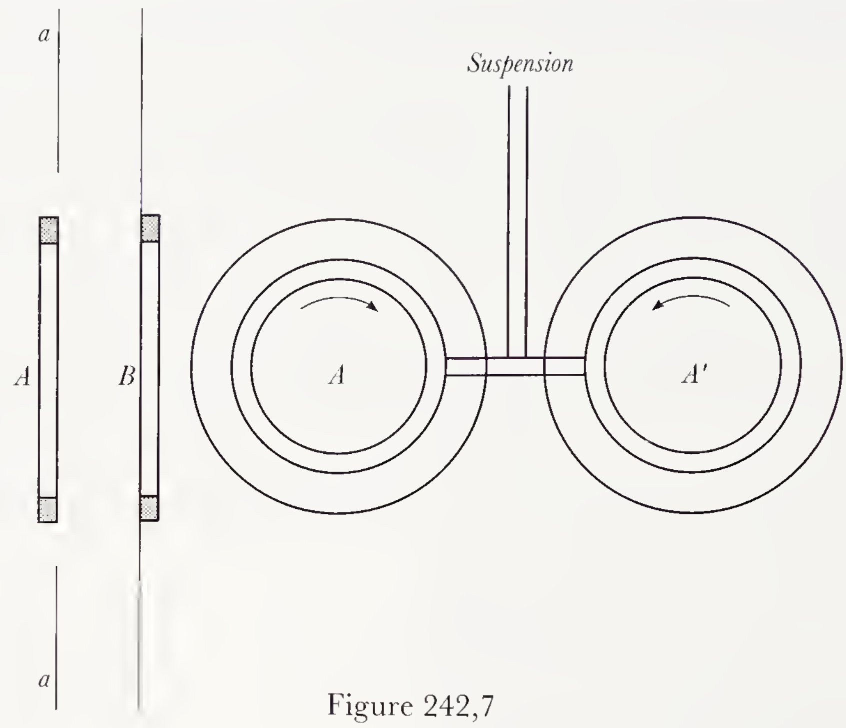

Figure 242,7

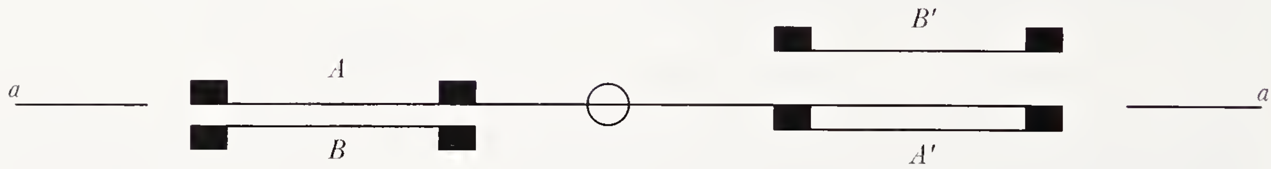

Figure 242,8. Plan

Optical devices to ascertain the position of *A* & *A'* at once during experiments.

I have had what Sharpey[14] calls an implosion of my gas apparatus due to external pressure.[15] It is now remade stronger and will be ready soon.

I hope the Peelers[16] will have more distinct views in future as to the nature of their duties, and the necessity of concocting evidence with a view to its being compared with that of persons not belonging to the force.

I remain
Yours truly
J. Clerk Maxwell

(14) See Number 197 note (1). (15) See also Number 244.

(16) Officers of the Metropolitan Police established by Sir Robert Peel, as Home Secretary, in 1829.

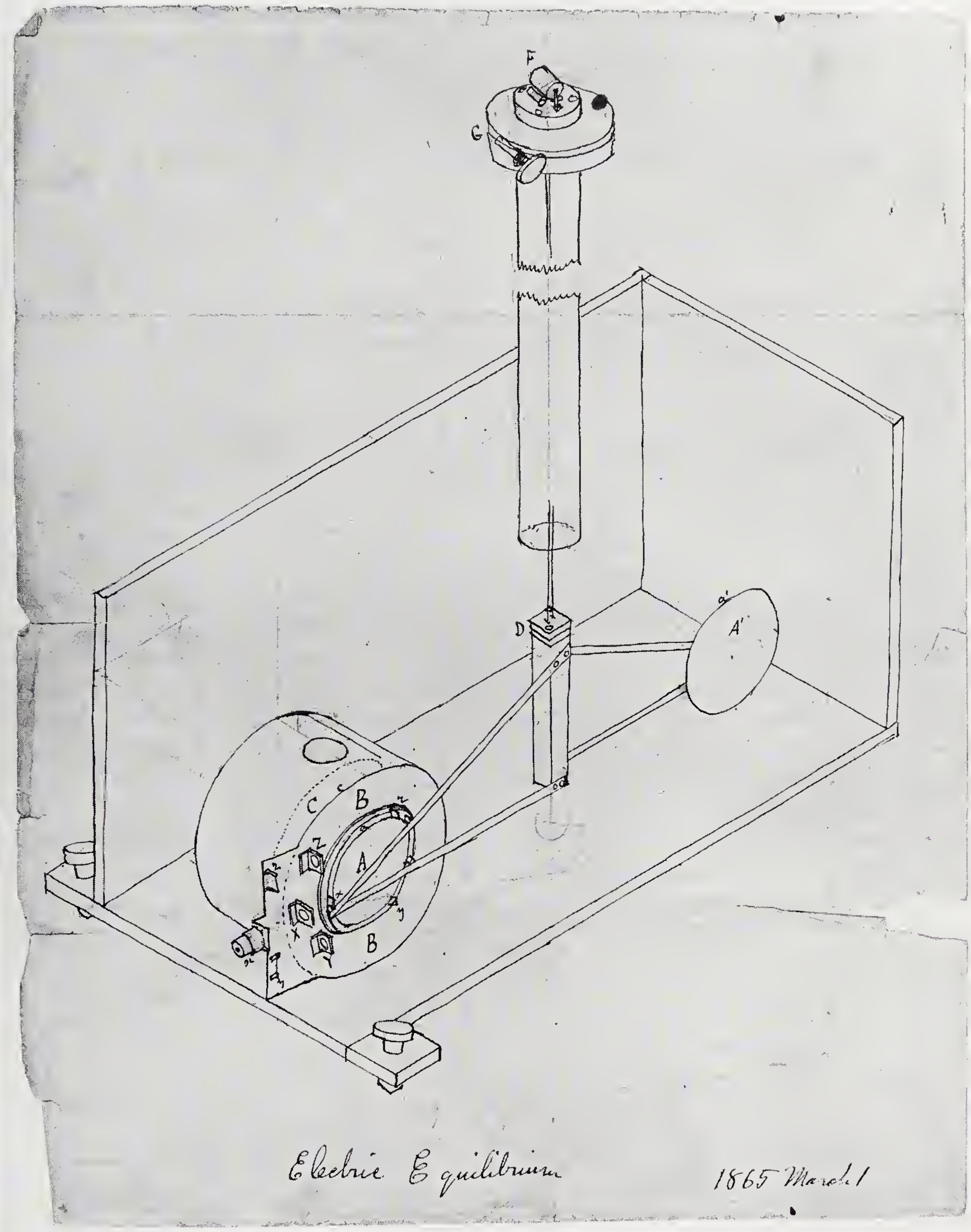

Plate III. Maxwell's drawing (1865) of the torsion balance for an experiment on the determination of the ratio of the electrostatic to the electromagnetic unit of electricity (Number 243).

TORSION BALANCE TO COMPARE AN ELECTROSTATIC ATTRACTION WITH AN ELECTROMAGNETIC REPULSION

I MARCH 1865

From the original in the University Library, Cambridge[1]

ELECTRIC EQUILIBRIUM[2] 1865 MARCH 1

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(1) ULC Add. MSS 7655, V, c/14 (i), Previously published by I. B. Hopley in *Annals of Science*, **15** (1959): plate facing 104 (see Number 241 note (3)).

(2) Plate III. The torsion balance for Maxwell's experiment on the determination of the ratio of the electrostatic and electromagnetic units of electricity, by observing the equilibrium between the electrostatic attraction of two discs and the electromagnetic repulsion of two coils. See his letter to William Thomson of 15 October 1864 (Number 235 esp. note (3)); and his paper 'On a method of making a direct comparison of electrostatic with electromagnetic force', *Phil. Trans.*, **158** (1868): 643–57, esp. 643–52 (= *Scientific Papers*, **2**: 125–36), and see Number 289. One of the discs, with one of the coils at its back (compare Figure 235, 1), is attached to one arm of the torsion balance, while the larger fixed disc, with its coil, could be moved to various distances from the suspended disc by a micrometer screw. A counterpoise disc and coil, which is traversed by the same current in the opposite direction, is attached to the other arm of the torsion balance, to eliminate the effect of terrestrial magnetism. The torsion balance consists of a light brass frame suspended by a copper wire from a torsion head which is supported by a hollow pillar. The balance was made by Carl Becker (see Number 289). The suspended disc was fitted with a guard-ring: see Number 289 esp. note (11). See Plate XI.

LETTER TO PETER GUTHRIE TAIT

7 MARCH 1865

From the original in the University Library, Cambridge[1]

8 Palace Gardens Terrace
London W.
1865 March 7

Dear Tait[2]

The true origin of Electrical Resistance as expressed in B.A. units is Fleeming Jenkin Esq[re] 6 Duke Street Adelphi W.C. Price £2..10 in a box.[3]

I was happy to see that the 1[st] instalment of B. Stewart & Tait was in the R S Proceedings.[4] I suppose the wooden disc is either at Edinburgh or at Kew at a given time and that you have not yet imparted to it the velocity so frequently referred to in books on Elementary Dynamics which at the end of time *t* would cause it to be in Edin[h] & Kew at once.

Does any one write quaternions but Sir W. Hamilton[5] & you?[6]

I heard him greatly slanged by a mathematical clergyman unknown to me, along with Plucker & Jacobi.[7] We were to see an end of all that school of mathematics very soon.

---

(1) ULC Add. MSS 7655, I, b/3.

(2) On 4 March 1865 Tait had written to Stokes (ULC Add. MSS 7656, T 56) inquiring: 'Can you tell me anything about Clerk Maxwell? He used to be a regular correspondent – but several late letters of mine are unanswered, and I see his Chair advertised as vacant.' See Number 235 note (20).

(3) See Fleeming Jenkin, 'Electric standard', *Phil. Mag.*, ser. 4, **29** (March 1865): 248, and also the 'Report of the committee on standards of electrical resistance', *Report of the Thirty-fifth Meeting of the British Association...held in September 1865* (London, 1866): 308–13, on the copies of standard coils made available for sale.

(4) Balfour Stewart and P. G. Tait's 'Preliminary note on the radiation from a revolving disc' was read at a meeting of the Royal Society on 23 February 1865; see *Proc. Roy. Soc.*, **14** (1865): 90.

(5) William Rowan Hamilton, *Lectures on Quaternions* (Dublin, 1853).

(6) See P. G. Tait, 'Quaternion investigations connected with electrodynamics and magnetism', *Quarterly Journal of Pure and Applied Mathematics*, **3** (1860): 331–42; 'Quaternion investigation of the potential of a closed circuit', *ibid.*, **4** (1861): 143–4; 'Formulae connected with small continuous displacements of the particles of a medium', *Proc. Roy. Soc. Edinb.*, **4** (1862): 617–23; and 'Note on a quaternion transformation', *Proc. Roy. Soc. Edinb.*, **5** (1863): 115–19.

(7) Julius Plücker and Carl Gustav Jacobi. The incident very likely occurred at a meeting of the Royal Society on 2 February 1865, when Plücker read his paper 'On a new geometry of space', *Proc. Roy. Soc.*, **14** (1865): 53–8.

Where can one obtain the most rapid approx[ns] to the areas of spherical ellipses whose major axes are nearly $= \pi$.[(8)]

I made an erroneous estimate by rule of thumb as to the strength of a glass plate $\frac{1}{2}$ inch thick in consequence of which when exposed to a pressure of $\frac{3}{4}$ atmosphere it succumbed with a stunning implosion and sent me a month back with regard to the friction of gases and exhibited beautiful specimens of radial cracks in this style.

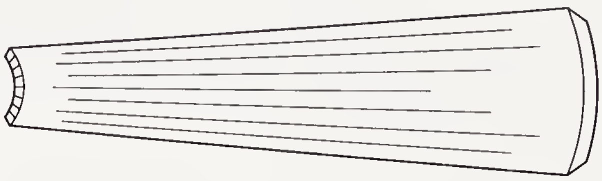

Figure 244,1

Thick ribbed brass now takes the place of glass and if the glass receiver goes it will only smash itself.

Yours truly
J. CLERK MAXWELL

Find the sum of

$$\frac{3}{4}(1-x)+\frac{3.5.7}{4.4.8}(1-x)^3+\frac{3.5.7.9.11}{4.4.8.8.12}(1-x)^5$$
$$+\frac{3.5.7.9.11.13.15}{4.4.8.8.12.12.16}(1-x)^7+\&c$$

in decent terms of x a small quantity. It ought to come out convergently because I have solved the identical problem otherwise where x is small but as it stands it is not agreeable to me to engage it.[(9)]

(8) For a calculation in Maxwell's paper 'On the viscosity or internal friction of air and other gases', *Phil. Trans.* **156** (1866): 249–68, on 261–2 (= *Scientific Papers*, **2**: 16–17).

(9) A preliminary attempt to calculate the decrement in amplitude of the vibrating discs, in the experiments on the viscosity of gases. Maxwell soon abandoned this method: see Number 245 esp. note (4).

245

LETTER TO PETER GUTHRIE TAIT

3 APRIL 1865

From the original in the University Library, Cambridge[1]

8 Palace Gardens Terrace W
1865 April 3

Dear Tait

My results about friction of gases are now in a fair way towards existence. I got into swinging order on the 21$^{\mathrm{st}}$ and have done something every day since but I have been occupied lately with the measurement of my dimensions and the determination of moments of inertia which are not yet reduced to numbers. I can promise you nothing about friction of ether but the machine goes like a clock or better and if there is anything appreciable say $\frac{1}{2}$ per cent I think it will find it. I have had nothing but undried air as yet but I have only to provide other innocent gases and my machine will do for them.

In 1860 I gave out that the coeff$^{\mathrm{t}}$ of friction in air is independent of density Phil Mag Jan$^{\mathrm{y}}$.[2] Behold the results of the first days experiments the exhaustion was not carried far that day because I had not proved my apparatus then.

21 March

3 discs oscillating between 4 fixed ones distance .186 inch between moving & fixed 45 °F. Time 71.5 seconds

Figure 245,1

Barometer[3]	Log decrement of arc[4]
29.9	.02780
16.51	.02746
12.21	.02783
29.7	.02769

(1) ULC Add. MSS 7655, I, b/3A.

(2) J. C. Maxwell, 'Illustrations of the dynamical theory of gases. Part I. On the motions and collisions of perfectly elastic bodies', *Phil. Mag.*, ser. 4, **19** (1860): 19–32, on 32 (= *Scientific Papers*, **1**: 391). See also Number 207 §10 and Volume I: 610.

(3) Barometric pressure in inches.

(4) On Maxwell's calculation of the mean logarithmic decrement in amplitude of the vibrating discs, see 'On the viscosity or internal friction of air and other gases', *Phil. Trans.*, **156** (1866): 249–68, on 252–3 (= *Scientific Papers*, **2**: 5–6).

In case you should say this resistance is independent of the air here is the log dec. when the three discs are in contact and 1 inch of air between them & the fixed.

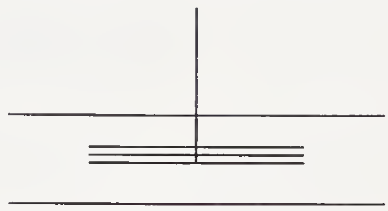

Figure 245,2

Barometer	Log dec
29.9	.00603
13.5	.00596
1.03	.00516

Here the difference partly depends on the influence of mass of air 1 inch thick & is greater when dense.

After Thursday I mean to go ahead with the exp[ts] as I now know my distances to .001 inch. Never mind my series[(5)] I have forgotten it and have got my results a different way.[(6)]

I have not tried the disc with currents but if you have a spheroid polar semiaxis b equatorial ditto a made of stuff of resistance ρ per unit of volume spinning with velocity ω in a field of mag. intensity I about an axis inclined α to the mag. force in an insulating medium the work done against electromagnetic forces and converted into heat is

$$I^2 \sin^2 \alpha\, \omega^2 . \frac{4\pi}{3} \frac{a^2 b}{\rho} . \frac{2}{5} \frac{a^2 b^2}{(a+b)^2} .$$ [(7)]

You may get ρ by exp[t] or from Jenkins table of Resistance Units, see Matthiessens copper $\frac{1}{16}$ inch wire 1 mile long, British Ass[n] 1862.[(8)]

N.B. This does not include the action of the induced currents in inducing other currents in the disc.

Yours truly
J. C. Maxwell

P. G. Tait Esq[re]

(5) See Number 244. (6) See note (4).

(7) In their paper 'On the heating of a disc by rapid rotation', *Proc. Roy. Soc.*, **14** (1865): 339–43, on 342 (read 15 June 1865), Balfour Stewart and Tait comment on the cause of heating of the disc: 'it is not due to revolution under the earth's magnetic force, for Professor Maxwell has kindly calculated the effect due to this cause under the conditions of the experiment, and he finds it infinitesimally small.'

(8) Read: 1864. See the value of ρ obtained by Augustus Matthiessen, recorded in the table of units of resistance, in the 'Report of the committee on standards of electrical resistance', *Report of the Thirty-fourth Meeting of the British Association... in September 1864* (London, 1865): 345–67, table facing 349 on 'Approximate relative values of various units of electrical resistance.'

LETTER TO WILLIAM THOMSON

17 AND 18 APRIL 1865

From the original in the University Library, Cambridge[1]

8 Palace Gardens Terrace
London W
1865 Ap 17

Dear Thomson

You told me some time ago you wanted to hear about the friction of gases. I have only got results very lately but the apparatus is in full swing now and I expect to get on as fast as I can prepare gases and heat mix & measure them.

The apparatus is 3 discs swinging about a vertical axis between 4 fixed ones all glass. Time of double vibration 71.6 seconds diameter of swinging discs 10.562 inches. Thickness of air = .425 at present. All distances measured to .001 inch by a gauge I have tested, except the distance from fixed to moving disc which is got optically so as to make the moving disc at equal distances from the fixed ones wh: makes the error of the 2nd order.[2]

The first result is that the coefficient of friction μ is independent of the density of the gas, temperature being the same.[3]

μ = dyn measure of tangential pressure on one square inch due to its moving at 1 inch per second at 1 inch from a fixed plane.

$$\text{The dimensions of } \mu \text{ are } \frac{\dfrac{\text{Force}}{\text{area}}}{\dfrac{\text{velocity}}{\text{distance}}} = \frac{\dfrac{\text{ML}}{\text{L}^2\text{T}^2}}{\dfrac{\text{L}}{\text{TL}}}$$

$$\text{or } \frac{\text{M}}{\text{LT}}.^{(4)}$$

Therefore if μ is in grain inch second measure multiply by 12 to bring it to feet and divide by 7000 to bring it to pounds.

Stokes worked in inches and made $\mu = .00417$.[5] I find at present for air $\mu = .00406$ but I shall be better when I have reduced more obsns.[6]

I suppose Carbonic Acid $\mu = .0033$
and Hydrogen $\mu = .0018$
(all at about 60° in grain–inch, second) both much less than air.

---

(1) ULC Add. MSS 7655, II, 22A.

(2) On Thomson's interest in the experiment see Number 252 note (3).

(3) See Number 245 esp. note (2).

(4) On Maxwell's dimensional notation see the Introduction: 8.

(5) See Number 252.

(6) Compare the values given in Numbers 249 and 252.

Hydrogen 14.88 inches + Air 14.14 inches $\mu = .0036$ so that air is more powerful than hydrogen in determining the μ of a mixture.

All the gases are dried with sulphuric acid but I do not find much difference for ordinary air & dry air. I think μ is greatest for dry air. Here is the comparison for pressures.

Barometer	Therm.	Log dec for 5 swings	
30.07	63°	.0810	dry air
9.7	68.5	.0810	dry air
1.6	69	.0814	dry air
30.1	62°	.08062	
12.76	61	.0810	
7.86	61	.08025	

I expected a result for hot air today but it was dark before I got everything ready and temperature constant.

I expect μ to vary as $\sqrt{\text{absolute temperature}}$.(7)

April 18. I find μ for air at 115° Fah$^{\text{r}}$ = .00465 the change is much greater than $\sqrt{T}$ gives but I mean to try steam next before the end of the week. The increase with temperature is quite plain.(8)

Measurement of v by equilibrium(9)

The advantage of equilibrating electrostatic attraction with electromagnetic repulsion derived from the same source, instead of balancing each separately by weights or springs is that the forces are applied simultaneously to a body already in equil$^{\text{m}}$ and you have no trouble about unstable eq$^{\text{m}}$ when you have eq$^{\text{m}}$ at all, and the instability may be just overcome by the elasticity of the suspension.

Hockin(10) is working at the galvanometer. I mean to make out the

(7) As predicted by Maxwell in 'Illustrations of the dynamical theory of gases. Part I. On the motions and collisions of perfectly elastic spheres', *Phil. Mag.*, ser. 4, **19** (1860): 19–32, esp. 30–2 (= *Scientific Papers*, **1**: 389–91); and see also Number 207.

(8) Maxwell ultimately concluded that the viscosity is nearly a linear function of the absolute temperature (see Number 252).

(9) See Maxwell's letter to Thomson of 15 October 1864 (Number 235).

(10) Charles Hockin: see Number 232 note (1).

corrections in summer, ie the series for the potentials, but at present I am busy with μ.

I suppose you will be here in May.

Yours truly
J. Clerk Maxwell

247

LETTER TO ROBERT DUNDAS CAY

28 APRIL 1865

From the original in the library of Peterhouse, Cambridge[1]

8 Palace Gardens Terrace W
1865 Ap 28

Dear Uncle

I enclose receipt of £47..9..4.[2] I met Willy[3] in a high state of preservation in Hyde Park. He told me of the flitting. At present I am measuring the stiffness of various gases, air is very stiff to work carbonic acid less so and Hydrogen very smooth indeed. Hot air is stiffer than cold. I have got it all in grains on the square foot now.

Your aff^t nephew
J. CLERK MAXWELL

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(1) Maxwell MSS (26), Peterhouse.
(2) See Volume I: 682n.
(3) William Dyce Cay; see Number 221 note (4).

LETTER TO THOMAS GRAHAM[1]

I MAY 1865

From the original in the University Library, Cambridge[2]

8 Palace Gardens Terrace
London W
1865 May 1

Dear Sir

I have now got a few results to compare with your experiments on the transpiration of gases[3] but I find that my method of observation is more exact than some of my data derived from measurements so that at present I have taken the apparatus down to measure everything and so get results worthy of the trouble.

I have tried air at pressures from 30 inches to 0.7 inches and from 42° to 158° Fah.[4]

The friction is the same for all densities, but increases with the temperature, apparently in the same proportion as the air expands.[5] I expected it would be as the square root of the absolute temperature but I think I am wrong.[6]

These results agree with yours.[7]

I have also tried hydrogen and carbonic acid and find the velocity for hydrogen about 2.16 of that for air a little more than yours but my results are not fully reduced yet.[8] Carbonic acid is also a good deal smoother than air.

Mixtures of air and hydrogen are more like air than like hydrogen a little air making it very rough. Damp air does not differ very much from dry.[9]

---

(1) Maxwell had been familiar with Graham's work on gases since 1859: see Volume I: 609, 615, 660, 706.

(2) ULC Add. MSS 7655, II, 23. Previously published in *Molecules and Gases*: 351–2.

(3) Thomas Graham's experiments on the passage of gases through a tube: 'On the motion of gases', *Phil. Trans.*, **136** (1846): 573–632; and 'On the motion of gases. Part II', *Phil. Trans.*, **139** (1849): 349–401.

(4) Compare J. C. Maxwell, 'On the viscosity or internal friction of air and other gases', *Phil. Trans.*, **156** (1866): 249–68, esp. 256–7 (= *Scientific Papers*, **2**: 10–11).

(5) Maxwell, 'On the viscosity or internal friction of air and other gases': 256 (= *Scientific Papers*, **2**: 10); and see Number 252.

(6) See Number 246.

(7) Graham concluded that the transpiration times for air at different pressures were 'highly uniform', and for other gases varied only slightly; and he observed a relation between transpiration times and the temperature of the gas. See Graham, 'On the motion of gases. Part II': 356–61.

(8) Compare the value which Maxwell gives in 'On the viscosity or internal friction of air and other gases' (Number 252).

(9) See Number 252.

Have you got any more results about the transpiration velocity of mixed gases, especially hydrogen & oxygen or Ether vapour & oxygen. I see you have determined the transpiration of equal volumes of H & O.[10] If you have also that of 2 volumes H & 1 volume O it would serve as a test for the theory.

I think the absolute value of the friction of a few gases may be best determined by my method but the comparison of gases and the effects of mixture can be best done by transpiration through tubes by your method.

Has any one but you made such experiments on gases, of course Poisueille[11] & others[12] have tried liquids.

Have you any remaining copy of your paper on Transpiration in the Phil Trans.[13] I have your papers on Molecular Mobility &c[14] & Liquid Diffusion[15] and on Gaseous Diffusion[16] too and I should like to put them all together if you have a copy of the Transpiration to spare.

I suppose the hydrogen particles must either be much bigger than the oxygen ones or else they must act on one another at a greater distance, though they are 16 times less in mass.

Yours truly
J. Clerk Maxwell

The Master of the Mint

(10) But see Graham's results reported in 'On the motion of gases': 627.

(11) J. L. M. Poiseuille, 'Récherches expérimentales sur le mouvement des liquides dans les tubes de très petits diamètres, *Mémoires Présentés par Divers Savants à l'Académie Royale de l'Institut de France*, **9** (1846): 433–544.

(12) The 'Rapport fait à l'Académic des Sciences, le 26 décembre 1842, au nom d'une Commission composée de MM. Arago, Babinet, Piobert, Regnault rapporteur, sur un Mémoire de M. le docteur Poiseuille, ayant pour titre: Recherches expérimentales sur le mouvement des liquides dans les tubes de très-petits diamètres', *Ann. Chim. Phys.*, ser. 3, **7** (1843): 50–74, esp. 50, cited by Graham in 'On the motion of gases. Part II': 350n, refers to earlier work on the flow of liquids in tubes of small diameter, notably by P. S. Girard, 'Mémoire sur le mouvement des fluides dans les tubes capillaires et l'influence de la température sur ce mouvement', *Mémoires de la Classe des Sciences Mathématiques et Physiques de l'Institut de France. Années 1813, 1814, 1815* (1818): 249–380.

(13) See note (3).

(14) Thomas Graham, 'On the molecular mobility of gases', *Phil. Trans.*, **153** (1863): 385–405.

(15) Thomas Graham, 'Liquid diffusion applied to analysis', *Phil. Trans.*, **151** (1861): 183–224. See also Graham, 'On the diffusion of liquids', *ibid.*, **140** (1850): 1–46, 805–36; *ibid.*, **141** (1851): 483–94.

(16) Thomas Graham, 'On the law of the diffusion of gases', *Trans. Roy. Soc. Edinb.*, **12** (1834): 222–58.

LETTER TO PETER GUTHRIE TAIT

17 JUNE 1865

From the original in the University Library, Cambridge[1]

Craiglachie
Errol
1865 June 17

Dear Tait

23 is founded on fact,[2] the problem like others about that date was anon.[3] but was legitimized in the Q J of M. March 1858 at the end of a theory of Opt Ins[ts].[4]

I am reducing friction of air to numerical values, but I get on slowly by reason of great heat and no logarithms at hand. However in foot, grain, second measure

$$\mu = 0.0925$$

for air at 60.6.[5]

μ is independent of pressure and proportional to absolute temperature not to square root thereof.[6] Hydrogen about $\frac{1}{2}$ air.

The value above given is nearly double what Stokes deduces from pendulum exp[ts] but agrees with Grahams exp[ts] on transpiration.[7]

---

(1) ULC Add. MSS 7655, I, b/4. Previously published in *Molecules and Gases*: 352–3.

(2) Tait had sent Maxwell proof sheets of §24 (§23 in the proof) of his paper 'On the application of Hamilton's characteristic function to special cases of constraint', *Trans. Roy. Soc. Edinb.*, **24** (1865): 147–66, esp. 163–4 (ULC Add. MSS 7655, I, a/1). Drawing an analogy between the brachistochrone (the path of swiftest descent) and the path of a light ray in a heterogeneous refracting medium, Tait refers to: 'The following due I believe to Maxwell. If the path of a medium be such a function of the distance from a given point that the path of any one ray is a circle, the path of every other ray is a circle; and all rays diverging from any one point converge accurately in another.' He refers to a paper in *Camb. & Dubl. Math. J.*, **9** (1854): 9, and appends on the proof: 'Report, O Maxwell! whether the underwritten accusation §23 be printed in fact – or no. Excuse speed. If it be printed say are you content with the form thereof. Y[rs] P.G.T.' See also Number 380.

(3) Proposed and solved by Maxwell (see Number 421). See *Camb. & Dubl. Math. J.*, **8** (1853): 188; and [J. C. Maxwell,] 'Solutions to problems', *ibid.*, **9** (1854): 7–11, esp. 9–11 (= *Scientific Papers*, **1**: 76–9). See Volume I: 232–5.

(4) J. C. Maxwell, 'On the general laws of optical instruments', *Quarterly Journal of Pure and Applied Mathematics*, **2** (1858): 232–46, esp. 246 (= *Scientific Papers*, **1**: 284–5).

(5) Compare the value given in Maxwell's letter to William Thomson of 17 April 1865 (Number 246), and that in 'On the viscosity or internal friction of air and other gases' (Number 252).

(6) See Numbers 246 and 252.

(7) See Number 252.

To reduce to French or other measure divide by $\frac{\text{Foot}}{\text{grain second}}$ and multiply by the corresponding foreign measures.

Yours truly
J. C. MAXWELL

FROM A LETTER TO HENRY RICHMOND DROOP[(1)]

19 JULY 1865

From Campbell and Garnett, *Life of Maxwell*[(2)]

Glenlair
Dalbeattie
19 July 1865

There are so many different forms in which Societies may be cast, that I should like very much to hear something of what those who have been thinking about it propose as the plan of it.

There is the association for publishing each other's productions; for delivering lectures for the good of the public and the support of the Society; for keeping a reading room or club, frequented by men of a particular turn; for dining together once a month, etc.

I suppose W—'s object is to increase the happiness of men in London who cultivate physical sciences, by their meeting together to read papers and discuss them, the publication of these papers being only one, and not the chief end of the Society, which fulfils its main purpose in the act of meeting and enjoying itself.

The Royal Society of Edinburgh used to be a very sociable body, but it had several advantages. Most of the fellows lived within a mile of the Society's rooms. They did not need to disturb their dinner arrangements in order to attend.

Many of them were good speakers as well as sensible men, whose mode of considering a subject was worth hearing, even if not correct.

The subjects were not limited to mathematics and physics, but included geology, physiology, and occasionally antiquities and even literary subjects. Biography of deceased fellows is still a subject of papers. Now those who cultivate the mathematical and physical sciences are sometimes unable to discuss a paper, because they would require to keep it some days by them to form an opinion on it, and physical men can get up a much better discussion about armour plates or the theory of glaciers than about the conduction of heat or capillary attraction.

The only man I know who can make everything the subject of discussion is Dr. Tyndall.[(3)] Secure his attendance and that of somebody to differ from him, and you are all right for a meeting.

If we can take the field with a plan in our head, I dare say we could find a good many men who would co-operate.

---

(1) See Volume I: 557, 703–6. (2) *Life of Maxwell*: 342–3. (3) See Number 226.

We ride every day, sometimes both morning and evening, and so we consume the roads. I have made 68 problems, all stiff ones, not counting riders.(4)

I am now getting the general equations for the motion of a gas considered as an assemblage of molecules flying about with great velocity. I find they must repel as inverse fifth power of distance.(5)

---

(4) For the Cambridge Mathematical Tripos in 1866: see Numbers 228 note (5) and 253: Appendix I.

(5) See Number 263.

LETTER TO LEWIS CAMPBELL

21 NOVEMBER 1865

From the original in the University Library, St Andrews[1]

8 Palace Gardens Terrace
London W
1865 Nov 21

Dear Lewis[2]

Trinity College Cambridge is said to favour duplicity and multiplicity in its elections to fellowships.[3]

No College however can be consistent in this for occasionally they must prefer a single man who will be of use to a man who is merely free from ignorance on all the curriculum.

If a man can do *good* Greek & Latin either prose or verse there is no doubt it will be a great lift to him at Trinity and it will be of advantage to himself too. But if he can only grind up bad verses, he will suffer more for this than for none at all. Bad mathematics do no man any harm (with examiners) but bad classics do.[4] Thompson (Greek Prof^r^)[5] said before I went in that if they elected me they never had let through such bad classics. I am confident however that if I had done any they would have kept me waiting a while.[6]

If young Forbes[7] likes Logic he can get it in good style where he is whatever heresies he may imbibe along with it concerning the necessity of being known.

This kind of thing gets on very well at Trinity, better than at any other College.

But I have not myself the faculty of the learned tongues so that I cannot judge of the deficiencies of a man who cannot make verses in them and no one can tell whether young Forbes verses (when made) are likely to be good

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(1) St Andrews University Library, Forbes MSS 1865/161.

(2) Lewis Campbell had been appointed Professor of Greek at St Andrews in 1863, where James David Forbes (on whose son's behalf this letter was written: see note (7)) was Principal of the United College of St Salvator and St Leonard.

(3) Candidates for fellowships at Trinity College, Cambridge were required to be examined in classics, mathematics and metaphysics. See Charles Astor Bristed, *Five Years in an English University*, 2 vols. (New York, 1852), **1**: 386–96.

(4) For a similar judgment see Bristed, *Five Years in an English University*, **1**: 388.

(5) William Hepworth Thompson, Trinity 1828, Regius Professor of Greek 1853, Master 1866 (Venn).

(6) On Maxwell's failure to gain a fellowship at Trinity in October 1854, and his success the following year, see Volume I: 12, 280n, 327.

(7) George Forbes, Christ's 1867, from St Andrews University (Venn).

without seeing him and knowing whether verse making is prevalent in the company which he moves in.

Your aft friend
J. Clerk Maxwell

ABSTRACT OF PAPER 'ON THE VISCOSITY OR INTERNAL FRICTION OF AIR AND OTHER GASES'

[23 NOVEMBER 1865][1]

From the *Proceedings of the Royal Society*[2]

FEBRUARY 8, 1866.

Lieut.-General Sabine, President, in the Chair.

The Bakerian Lecture was delivered by James Clerk Maxwell, M.A., F.R.S., 'On the Viscosity or Internal Friction of Air and other Gases.'[3] The following is an abstract.

All bodies which are capable of having their form indefinitely altered, and which resist the change of form with a force depending on the rate of deformation, may be called Viscous Bodies. Taking tar or treacle as an instance in which both the change of form and the resistance opposed to it are easily observed, we may pass in one direction through the series of soft solids up to the materials commonly supposed to be most unyielding, such as glass and steel, and in the other direction through the series of liquids of various degrees of mobility to the gases, of which oxygen is the most viscous, and hydrogen the least.

The viscosity of elastic solids has been investigated by M. F. Kohl-

(1) The date the paper was received by the Royal Society. The paper was read on 8 February 1866: see note (2).

(2) *Proc. Roy. Soc.*, **15** (1866): 14–17.

(3) Published in *Phil. Trans.*, **156** (1866): 249–68 (= *Scientific Papers*, **2**: 1–25). There is a referee's report by William Thomson: see his letter to Stokes of 11 April 1866 (Royal Society, *Referees' Reports*, **6**: 178; printed in Wilson, *Stokes–Kelvin Correspondence*, **1**: 324–5). Thomson declared that: 'The evidence it contains as to the accuracy of the results in absolute measure is very satisfactory.... The plan he adopts is quite the same as one I had long contemplated, and intended applying myself, for liquids (except that I intended to have only one vibrating disc).' Thomson had outlined an experiment on the viscosity of liquids, using a vibrating disc, in a letter to Stokes of 10 March 1862 (ULC Add. MSS 7656, K 133; see *Stokes–Kelvin Correspondence*, **1**: 288–9). At the time, Stokes had been investigating Graham's results on the transpiration of gases, concluding in favour of Maxwell's result that gas viscosity is independent of density; see Stokes to Thomson 22 February and 25 February 1862 (*Stokes–Kelvin Correspondence*, **1**: 283–7). Maxwell mentioned Stokes' conclusion in a letter to H. R. Droop of 28 January 1862 (Volume I: 706).

Plate IV. Maxwell's apparatus (1865) to determine the viscosity of gases as a function of pressure and temperature (Number 252).

rausch*[4] and Professor W. Thomson†[5] that of gases by Professor Stokes‡,[6] M. O. E. Meyer§,[7] and Mr. Graham‖.[8]

The author has investigated the laws of viscosity in air by causing three horizontal glass disks, 10.56 inches diameter, to perform rotatory oscillations about a vertical axis by means of the elasticity of a steel suspension wire about 4 feet long. The period of a complete oscillation was 72 seconds, and the maximum velocity of the edge of the disks was about $\frac{1}{12}$ inch per second.

The three disks were placed at known intervals on the vertical axis, and four larger fixed disks were so adjusted above and below them and in the intervals between them, that strata of air of known thickness were intercepted between the surfaces of the moving disks and the fixed disks. During the oscillations of the moveable disks, the viscosity of the air in these six strata caused a gradual diminution of the amplitude of oscillation, which was measured by means of the reflexion of a circular scale in a mirror attached to the axis.

The whole apparatus was enclosed in an air-tight case, so that the air might be exhausted or exchanged for another gas, or heated by a current of steam round the receiver. The observed diminution in the arc of oscillation is in part due to the viscosity of the suspending wire. To eliminate the effect of the wire from that of the air, the arrangement of the disks was altered, and the three disks, placed in contact, were made to oscillate midway between two fixed glass disks, at distances sometimes of 1 inch, and sometimes of .5 inch.

From these experiments on two strata of air, combined with three sets of experiments on six strata of thicknesses .683, .425, and .1847 inches respectively, the value of the coefficient of viscosity or internal friction was determined.

Let two infinite planes be separated by a stratum of air whose thickness is unity. Let one of these planes be fixed, while the other moves in its own plane

* Pogg. Ann. cxix. (1863).[4]

† Proceedings of the Royal Society, May 18, 1865.[5]

‡ Cambridge Philosophical Transactions, 1850.[6]

§ Pogg. Ann. cxiii. (1861).[7]

‖ Phil. Trans. 1846 & 1849.[8]

(4) F. W. G. Kohlrausch, 'Ueber die elastische Nachwirkung bei der Torsion', *Ann. Phys.*, **119** (1863): 337–68.

(5) William Thomson, 'On the elasticity and viscosity of metals', *Proc. Roy. Soc.*, **14** (1865): 289–97.

(6) G. G. Stokes, 'On the effect of the internal friction of fluids on the motion of pendulums', *Trans. Camb. Phil. Soc.*, **9**, part 2 (1851): [8]–[106] (= *Papers*, **3**: 1–136).

(7) O. E. Meyer, 'Ueber die Reibung der Flüssigkeiten', *Ann. Phys.*, **113** (1861): 55–86, 193–238, 383–425.

(8) Thomas Graham, 'On the motion of gases', *Phil. Trans.*, **136** (1846): 573–632; 'On the motion of gases. Part II', *ibid.*, **139** (1849): 349–401.

with a uniform velocity unity; then, if the air in immediate contact with either plane has the same velocity as the plane, every unit of surface of either plane will experience a tangential force μ, where μ is the coefficient of viscosity of the air between the planes.

The force μ is understood to be measured by the velocity which it would communicate in unit of time to unit of mass.

If L, M, T be the units of length, mass, and time, then the dimensions of μ are $L^{-1}\ M\ T^{-1}$.

In the actual experiment, the motion of the surfaces is rotatory instead of rectilinear, oscillatory instead of uniform, and the surfaces are bounded instead of infinite. These considerations introduce certain complications into the theory, which are separately considered.

The conclusions which are drawn from the experiments agree, as far as they go, with those of Mr. Graham on the Transpiration of Gases*.[(9)] They are as follows.

1. The coefficient of viscosity is independent of the density, the temperature being constant. No deviation from this law is observed between the atmospheric density and that corresponding to a pressure of half an inch of mercury.

This remarkable result was shown by the author in 1860†[(10)] to be a consequence of the Dynamical Theory of Gases. It agrees with the conclusions of Mr. Graham, deduced from experiments on the transpiration of gases through capillary tubes.[(11)] The considerable thickness of the strata of air in the present experiments shows that the property of air, to be equally viscous at all densities, is quite independent of any molecular action between its particles and those of solid surfaces, such as those of the capillary tubes employed by Graham.

2. The coefficient of viscosity increases with the temperature, and is proportional to $1+\alpha\theta$, where θ is the temperature and α is the coefficient of expansion per degree for air.

This result cannot be considered so well established as the former, owing to the difficulty of maintaining a high temperature constant in so large an

* Phil. Trans. 1846.[(9)]

† Phil. Mag. Jan. 1860.[(10)]

(9) Graham, 'On the motion of gases'. See Maxwell's letter to Graham of 1 May 1865 (Number 248).

(10) J. C. Maxwell, 'Illustrations of the dynamical theory of gases. Part I. On the motions and collisions of perfectly elastic spheres', *Phil. Mag.*, ser. 4, **19** (1860): 19–32, esp. 32 (= *Scientific Papers*, **1**: 391).

(11) Graham, 'On the motion of gases. Part II': 361. See Number 248 note (7).

apparatus, and measuring it without interfering with the motion. Experiments, in which the temperature ranged from 50° to 185° F, agreed with the theory to within 0.8 per cent, so that it is exceedingly probable that this is the true relation to the temperature.

The experiments of Graham led him to this conclusion also.(12)

3. The coefficient of viscosity of hydrogen is much less than that of air. I have never succeeded in filling my apparatus with perfectly pure hydrogen, for air leaks into the vacuum during the admission of so large a quantity of hydrogen as is required to fill it. The ratio of the viscosity of my hydrogen to that of air was .5156. That obtained by Graham was .4855.

4. The ratio for carbonic acid was found to be .859. Graham makes it .807.(13) It is probable that the comparative results of Graham are more exact than those of this paper, owing to the difficulty of introducing so large a volume of gas without letting in any air during the time of filling the receiver. I find also that a very small proportion of air causes a considerable increase in the viscosity of hydrogen. This result also agrees with those of Mr. Graham.(14)

5. Forty experiments on dry air were investigated to determine whether any slipping takes place between the glass and the air in immediate contact with it.

The result was, that if there were any slipping, it is of exceedingly small amount; and that the evidence in favour of the indicated amount being real is very precarious.

The results of the hypothesis, that there is no slipping, agree decidedly better with the experiments.

6. The actual value of the coefficient of viscosity of dry air was determined, from forty experiments of five different kinds, to be

$$\mu = .0000149\,(461^\circ + \theta),$$

where the inch, the grain, and the second are the units, and the temperature is on Fahrenheit's scale.

At 62° this gives $\mu = .007802$.

(12) Graham, 'On the motion of gases. Part II': 356. See Number 248 note (7).

(13) In 'On the motion of gases. Part II': 364, Graham gives the transpiration times of equal volumes of hydrogen, oxygen, air and carbon dioxide under the same pressure. Taking the transpiration time of oxygen as 1, he gives those for hydrogen as 0.4375, and carbon dioxide as 0.7272, while that for air is 0.9010. Maxwell reduces Graham's values to ratios of hydrogen and carbon dioxide to air.

(14) Graham, 'On the motion of gases': 622–3, 628 and Plate XXXV facing 634.

Professor Stokes, from the experiments of Baily on pendulums,[15] has found[16]

$$\sqrt{\frac{\mu}{\rho}} = .116,$$

which, with the average temperature and density of air, would give

$$\mu = .00417,$$

a much smaller value than that here found.

If the value of μ is expressed in feet instead of inches, so as to be uniform with the British measures of magnetic and electric phenomena, as recorded at the observatories,

$$\begin{aligned}\mu &= .000179\,(461+\theta)\\ &= .08826 \text{ at } 32°.\end{aligned}$$

In metre-gramme-second measure and Centigrade temperature,

$$\mu = .01878\,(1+.00366\,\theta).$$

M. O. E. Meyer (Pogg. Ann. cxiii. (1861) p. 383)[17] makes μ at 18 °C = .000360 in centimetres, cubic centimetres of water, and seconds as units, or in metrical units,

$$\mu = .0360.$$

According to the experiments here described, μ at 18 °C = .02.

M. Meyer's value is therefore nearly twice as great as that of this paper, while that of Professor Stokes is only half as great.

In M. Meyer's experiments, which were with one disk at a time in an open space of air, the influence of the air near the edge of the disk is very considerable; but M. Meyer (Crelle, 59;[18] Pogg. cxiii. 76)[19] seems to have arrived at the conclusion that the additional effect of the air at the edge is proportional to the thickness of the disk. If the additional force near the edge is underestimated, the resulting value of the viscosity will be in excess.

7. Each of the forty experiments on dry air was calculated from the concluded values of the viscosity of the air and of the wire, and the result compared with the observed result. In this way the error of mean square of

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(15) Francis Baily, 'On the correction of a pendulum for the reduction to a vacuum: together with remarks on some anomalies observed in pendulum experiments', *Phil. Trans.*, **122** (1832): 399–492.

(16) Stokes, 'On the effect of the internal friction of fluids on the motion of pendulums': 65. See Volume I: 597–8, 606–11.

(17) Meyer, 'Ueber die Reibung der Flüssigkeiten', *Ann. Phys.*, **113** (1861): 383.

(18) O. E. Meyer, 'Ueber die Reibung der Flüssigkeiten', *Journal für die reine und angewandte Mathematik*, **59** (1861): 229–303 (on the mathematical theory of viscosity).

(19) Meyer, 'Ueber die Reibung der Flüssigkeiten', *Ann. Phys.*, **113** (1861): 76.

each observation was determined, and from this the 'probable error' of μ was found to be .036 per cent of its value. These experiments, it must be remembered, were made with five different arrangements of the disks, at pressures ranging from 0.5 inch to 30 inches, and at temperatures from 51° to 74 °F; so that their agreement does not arise from a mere repetition of the same conditions, but from an agreement between the properties of air and the theory made use of in the calculations.

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NOTES ON JAMES THOMSON'S VORTEX TURBINE[1]

circa LATE 1865[2]

From the original in the University Library, Cambridge[3]

VORTEX WHEEL – FIRST APPROXIMATION[4]

radius of wheel at entrance $= a$ at exit $= b$
angle of waves $= \alpha$ $= \beta$
inner angle of guide blades γ
depth of wheel c angular velocity ω moment of twisting R
Volume of water per second V effective head H
density of water ρ gravity $= g$
radius at any point r
then

$$\text{radial velocity} = \frac{V}{2\pi c}\frac{1}{r} \quad \text{Let } \frac{V}{2\pi c} \text{ be put} = k \tag{1}$$

1st to find R. The angular velocity of the water as it leaves the guide blades is $\frac{k}{r^2}\tan\gamma$. Hence the angular momentum of the water which enters per second is $V\rho k\tan\gamma$.

(1) James Thomson, 'On the vortex water-wheel', *Report of the Twenty-second Meeting of the British Association for the Advancement of Science* (London, 1853): 317–22. For Maxwell's likely sources see also notes (4) and (14).

(2) This is conjectural; the manuscript may well have relation to Maxwell's question on the stability of vortex motion in the Mathematical Tripos 1866: see Appendix I *infra*.

(3) ULC Add. MSS 7655, V, e/19.

(4) In James Thomson's water turbine water is directed by exterior guide blades and injected at the circumference, impinging on vanes which curve from radial to nearly tangential, and is ejected from near the centre. Water is injected at the same speed as the wheel: if the wheel slows, the inflow is greater, increasing the torque. The wheel tends to maintain equilibrium in response to fluctuations of inflow. See W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 596–7, on 'James Thomson's vortex water-wheel'. I am indebted to a personal communication (September 1990) from A. T. Fuller.

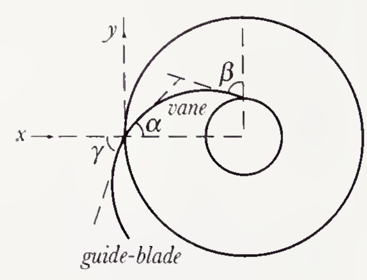

Figure 253,1

The angular velocity of the water which leaves the wheel is $\frac{k}{r^2}\tan\beta-\omega$(5) so that the angular momentum of the issuing water per second is $V\rho\ (k\tan\beta-b^2\omega)$.(6)

The difference of these angular momenta must be necessarily by pressure friction impact or otherwise communicated to the wheel.

Hence $R = V\rho\ (k(\tan\gamma-\tan\beta)+b^2\omega)$.(7) (2)

The useful work per second is

$$R\omega = V\rho(k\omega(\tan\gamma-\tan\beta)-b^2\omega^2) \tag{3}$$

The applied work per second is

$$W = V\rho g H \tag{4}$$

$\therefore$ the efficiency is

$$E = \frac{1}{gH}(k\omega(\tan\gamma-\tan\beta)+b^2\omega^2)^{(8)} \tag{5}$$

2nd To find the difference of pressures at entry & exit of the wheel p_1 & p_2.

Energy per cubic foot at entry =
$\frac{\rho}{2}\left\{k^2\frac{\sec^2\alpha}{a^2}+2k\omega\tan\alpha+\omega^2a^2\right\}+p_1$.

Work done by pressure in wheel (no impact) =
$\rho\{k\omega(\tan\alpha-\tan\beta)+(a^2-b^2)\,\omega^2\}$.

Energy at exit $=\frac{\rho}{2}\left\{k^2\frac{\sec^2\beta}{b^2}+2k\omega\tan\beta+\omega^2b^2\right\}+p_2$.

$$\text{Hence } p_1-p_2=\frac{\rho}{2}\left\{k^2\left(\frac{\sec^2\beta}{b^2}-\frac{\sec^2\alpha}{a^2}\right)+\omega^2(a^2-b^2)\right\}. \tag{6}$$

3rd to find the pressure p_1.

$$p_1=\rho gH-\frac{\rho}{2}k^2\frac{\sec^2\gamma}{a^2} \quad \& \ p_2=0$$

$$\text{Hence} \qquad 2gH=k^2\left\{\frac{\sec^2\gamma-\sec^2\alpha}{a^2}+\frac{\sec^2\beta}{b^2}\right\}+\omega^2(a^2-b^2). \tag{7}$$

(5) Read: $+\omega$.

(6) Read: $+b^2\omega$.

(7) Read: $-b^2\omega$.

(8) Read: $-b^2\omega^2$.

Diff$^{\text{t}}$ (2)(9)

$$dR = \left(2V\rho\frac{1}{2\pi c}(\tan\gamma - \tan\beta) + \rho b^2\omega\right)dV + V\rho b^2 d\omega.$$

Diff$^{\text{t}}$ (7)

$$0 = \frac{1}{4\pi^2c^2}VdV\left(\frac{\sec^2\gamma - \sec^2\alpha}{a^2} + \frac{\sec^2\beta}{b^2}\right) + \omega d\omega(a^2 - b^2).$$

Now let the standard values of the quantities be got thus

$$\frac{V}{2\pi c}\tan\beta = b^2\omega^{(10)}$$

$$\frac{V}{2\pi c}(\tan\gamma - \tan\alpha) = a^2\omega^{(11)} \;\&\text{ let } \alpha = 0^{(12)}$$

then

$$R = V\rho a^2\omega$$

$$2gH = 2a^2\omega^2 + \overline{\frac{V}{2\pi c}}\Big|^2\frac{1}{b^2}$$

$$E = \frac{a^2\omega^2}{a^2\omega^2 + \frac{1}{2}\overline{\frac{V}{2\pi c}}\Big|^2\frac{1}{b^2}}. \quad ^{(13)}$$

$$\frac{dV}{d\omega} = -\frac{V}{\omega}\frac{a^2 - b^2}{a^2 + b^2}$$

$$\frac{dR}{d\omega} = -2V\rho a^2\frac{a^2 - 2b^2}{a^2 + b^2}$$

$$\frac{dV}{dR} = -\frac{1}{2\rho a^2\omega}\frac{a^2 - b^2}{a^2 - 2b^2}.$$

$$\frac{d}{d\omega}(R\omega) = V\rho a^2\omega^2\frac{4b^2 - a^2}{a^2 + b^2}$$

$$\frac{d}{dV}(R\omega) = \rho a^2\omega^3\frac{4b^2 - a^2}{b^2 - a^2}.$$

(9) To calculate sensitivity of torque.

(10) Assuming the angular velocity of the water emerging from the wheel is zero.

(11) Assuming that the angular velocity of the water on passing from the guide blade into the wheel is constant.

(12) As assumed by Thomson.

(13) In obtaining the results *infra* Maxwell assumes that the efficiency E is approximately unity: hence $V/2\pi c \ll ab\omega$. There are algebraic errors in Maxwell's equations, and in the values given in his table.

Thomson	Fontaine parallel	Outward Fourneyron[14]
$a=2b$	$a=b$	$2a=b$
$\dfrac{dV}{d\omega}=-\dfrac{3}{5}\dfrac{V}{\omega}$	0	$+\dfrac{3}{5}\dfrac{V}{\omega}$
$\dfrac{dR}{d\omega}=-\dfrac{4}{5}V\rho a^2$	$+V\rho a^2$	$+\dfrac{14}{5}V\rho a^2$
$\dfrac{dV}{dR}=\dfrac{3}{4}\dfrac{1}{\rho a^2\omega}$	0	$+\dfrac{3}{14}\dfrac{1}{\rho a^2\omega}$

From this it appears that when $a=2b$ $\dfrac{dV}{d\omega}$ is $-^{\text{ve}}$ or the faster the speed the less the flow of water. When $2a=b$ the greater the speed the greater the flow. When the speed increases the resistance that can be overcome diminishes in Thomson and increases in the others. Hence Thomson is stable.

APPENDIX I: ON THE STABILITY OF VORTEX MOTION

circa LATE 1865[15]

From the original in the King's College London Archives[16]

[DRAFT QUESTION FOR THE MATHEMATICAL TRIPOS][17]

A mass M of fluid is running round a circular groove or channel of radius a with velocity u. An equal mass is running round another channel of radius b with velocity v.

(14) See Rankine's description of 'Thomson's turbine, or vortex wheel...[an] inward flow turbine', 'Fontaine's...parallel flow turbine', and 'Fourneyron's...outward flow turbine', in his *A Manual of the Steam Engine* (London/Glasgow, 1859): 201–10.

(15) See note (2) and Appendix II *infra*.

(16) King's College London Archives, Maxwell Notebook (2), question (101).

(17) Maxwell had been appointed a moderator for the Mathematical Tripos in 1866; see *The Cambridge University Calendar for the Year 1866* (Cambridge, 1866): 448. For the question as set in the Tripos see *Calendar for 1866*: 473. For a solution to the question see A. T. Fuller, 'Maxwell's Cambridge manuscripts: extracts relating to control and stability – VI', *International Journal of Control*, **43** (1986): 1135–68, esp. 1152–3.

The one channel is made to expand and the other to contract till their radii are exchanged show that the work expended in effecting the change is

$$-\frac{1}{2}\left(\frac{u^2}{b^2}-\frac{v^2}{a^2}\right)(a^2-b^2)\,M.$$

Hence show that the motion of a fluid in a circular whirlpool will be stable or unstable according as the areas described by particles in equal times increase or diminish from centre to circumference.

APPENDIX II: LETTER TO ROBERT DUNDAS CAY

8 DECEMBER 1865

From the original in the Library of Peterhouse, Cambridge(18)

8 Palace Gardens Terrace
W
1865 Dec 8

Dear Uncle

I enclose the receipt you sent me. We are glad to hear Uncle John is better and hope it will do Aunt Jane good. We should be much obliged to you for your carte de visite as we have not one of you.

The Cambridge questions are nearly all printed now but they have all to be cross questioned yet.

Your aff^{t} nephew
J. CLERK MAXWELL

(18) Maxwell MSS (27), Peterhouse.

MANUSCRIPT FRAGMENTS ON THE STABILITY OF FLUID MOTION

circa 1865[1]

From the originals in the University Library, Cambridge[2]

[1] ON THE CONDITION OF STABILITY OF THE STEADY MOTION OF AN INCOMPRESSIBLE FLUID[3]

The question as to the stability of any state implies the possibility of such a state. We must therefore treat of the necessary conditions of steady motion as well as of the method of distinguishing whether this motion if slightly disturbed will be restored to its original state or completely altered in character.

The ordinary hydrodynamical equations are of the form

$$\frac{dp}{dx} = -\rho\left(X - \frac{du}{dt} - u\frac{du}{dx} - v\frac{du}{dy} - w\frac{du}{dz}\right)^{(4)}$$

where p is the pressure, ρ the density X the force in x acting on the element due to external causes u v w the resolved velocities.

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(1) This manuscript may have been written in connection with a question Maxwell set for the Cambridge Mathematical Tripos in 1866. See *The Cambridge University Calendar for the Year 1866* (Cambridge, 1866): 478; 'If the motion of an incompressible homogeneous fluid under the action of such forces as occur in nature we put

$$\alpha = \frac{dv}{dz} - \frac{dw}{dy},\ \beta = \frac{dw}{dz} - \frac{du}{dz},\ \gamma = \frac{du}{dy} - \frac{dv}{dx}$$

shew that

$$\frac{d\alpha}{dt} + u\frac{d\alpha}{dx} + v\frac{d\alpha}{dy} + w\frac{d\alpha}{dz} = \alpha\frac{du}{dx} + \beta\frac{du}{dy} + \gamma\frac{du}{dz}.$$

P, Q are adjacent particles of the fluid, such that at a given instant the projections on the axes of x, y, z of the line joining them are proportional to α, β, γ respectively: shew that, during the subsequent motion of the fluid, the projections of the line joining P and Q will remain proportional to α, β, γ.' See Numbers 275 and 294 on setting the 'Helmholtz dogma' on fluid vortices; and see Hermann Helmholtz, 'Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen', *Journal für die reine und angewandte Mathematik*, **55** (1858): 25–55, esp. 31–5 for the source of the question.

(2) ULC Add. MSS 7655, V, c/5, 8.

(3) Compare the documents published in Volume I: 295–9, 507n.

(4) See S. D. Poisson, *Traité de Mécanique*, 2 vols. (Paris, 1833), **2**: 669; G. G. Stokes, 'On the theories of the internal friction of fluids in motion, and of the equilibrium and motion of elastic solids', *Trans. Camb. Phil. Soc.*, **8** (1845): 287–319, esp. 296–7 (= *Papers*, **1**: 75–129); Helmholtz, 'Über Integrale der hydrodynamischen Gleichungen': 28.

Let
$$\alpha = \frac{dw}{dy} - \frac{dv}{dz}$$
$$\beta = \frac{du}{dz} - \frac{dw}{dx}$$
$$\gamma = \frac{dv}{dx} - \frac{du}{dy}$$

be the velocities of rotation of an element of the fluid about the three directions of x y & z. If the motion of the fluid is regulated by a velocity potential(5) α β & γ will disappear.

If we have also $X = \frac{dV}{dx}$ $Y = \frac{dV}{dy}$ $Z = \frac{dV}{dz}$ and ρ constant

$$\frac{dp}{dx} = -\rho\left\{\frac{dV}{dx} - \frac{1}{2}\frac{d}{dx}(u^2+v^2+w^2) - \frac{du}{dt} + v\gamma - w\beta\right\}$$

or if we put $\frac{1}{2}\rho(u^2+v^2+w^2) - \rho V - p = G$(6) ⟨(the dynamic head)⟩(7)

$$\frac{dG}{dx} = \rho\left\{-\frac{du}{dt} + v\gamma - w\beta\right\}.$$

In a steady motion $\frac{du}{dt} = 0$,(8) therefore the condition of steady motion is given by the following equations

$$\frac{dG}{dx} = \rho(v\gamma - w\beta)$$
$$\frac{dG}{dy} = \rho(w\alpha - u\gamma) \qquad [(1)]$$
$$\frac{dG}{dz} = \rho(u\beta - v\alpha).$$

(5) Helmholtz's term 'Geschwindigkeitspotential'; see his 'Über Integrale der hydrodynamischen Gleichungen': 25.

(6) The Bernoulli equation. See G. G. Stokes, 'On the steady motion of incompressible fluids', *Trans. Camb. Phil. Soc.*, 7 (1842): 439–53, esp. 439 (= *Papers*, **1**: 1–16).

(7) See W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 568; 'The quotient p/ρ is what is called the *height, or head due to the pressure*... as the vertical ordinate z is measured *positively downwards* from a datum horizontal plane...$p-\rho z$ is the difference between the intensity of the actual pressure at... [a] particle and the pressure due to its depth below the datum horizontal plane; and $p/\rho - z = h$ is the *height* or *head due to that difference of intensity*, being what will be termed the *dynamic head*.'

(8) Compare Stokes, 'On the steady motion of incompressible fluids': 449.

[2] From these equations it appears that the system of surfaces $G =$ constant is such that the directions of motion of the fluid elements and their axes of rotation lie in these surfaces.

Let $F =$ const be the equation to another system of surfaces of fluid motion so arranged that by the intersection with the surfaces (G) they form tubes of fluid motion, through each of which unit of fluid passes in unit of time, then

$$u = \frac{dF}{dy}\frac{dG}{dz} - \frac{dF}{dz}\frac{dG}{dy}$$

$$v = \frac{dF}{dz}\frac{dG}{dx} - \frac{dF}{dx}\frac{dG}{dz}$$

$$w = \frac{dF}{dx}\frac{dG}{dy} - \frac{dF}{dy}\frac{dG}{dx}.$$

Let $H =$ const be the equation to a third system of surfaces so drawn that by their intersection with the surfaces (G) they form unit eddy tubes (Helmholtz's Wirbelfäden)(9) such that the direction of the tube at any point corresponds to the axis of rotation and its section multiplied by the velocity of rotation is unity.

Then
$$\alpha = \frac{dH}{dz}\frac{dG}{dy} - \frac{dH}{dy}\frac{dG}{dz}$$

$$\beta = \frac{dH}{dx}\frac{dG}{dz} - \frac{dH}{dz}\frac{dG}{dx}$$

$$\gamma = \frac{dH}{dy}\frac{dG}{dx} - \frac{dH}{dx}\frac{dG}{dy}$$

and equations [(1)] will become

$$\frac{dG}{dx} = \rho\frac{dG}{dx}\left\{\frac{dF}{dx}\cdot\frac{dG}{dy}\frac{dH}{dz} + \frac{dG}{dx}\cdot\frac{dH}{dy}\frac{dF}{dz}\right.$$
$$\left. + \frac{dH}{dx}\cdot\frac{dF}{dy}\frac{dG}{dz} - \frac{dF}{dz}\cdot\frac{dG}{dy}\frac{dH}{dx} - \frac{dG}{dz}\cdot\frac{dH}{dy}\frac{dF}{dx} - \frac{dH}{dz}\cdot\frac{dF}{dy}\frac{dG}{dx}\right\}.$$

(9) Helmholtz, 'Über Integrale der hydrodynamischen Gleichungen': 26. Rendered as 'vortex-filaments' by P. G. Tait in his translation of Helmholtz's 'On the integrals of the hydrodynamical equations, which express vortex motion', *Phil. Mag.*, ser. 4, **33** (1867): 485–512, on 486.

Let us put

$$K = \frac{dF}{dx}\frac{dG}{dy}\frac{dH}{dz} + \frac{dG}{dx}\frac{dH}{dy}\frac{dF}{dz} + \frac{dH}{dx}\frac{dF}{dy}\frac{dG}{dz}$$

$$- \frac{dF}{dz}\frac{dG}{dy}\frac{dH}{dx} - \frac{dG}{dz}\frac{dH}{dy}\frac{dF}{dx} - \frac{dH}{dz}\frac{dF}{dy}\frac{dG}{dx}$$

$$= u\frac{dH}{dx} + v\frac{dH}{dy} + w\frac{dH}{dz}$$

$$= \alpha\frac{dF}{dx} + \beta\frac{dF}{dy} + \gamma\frac{dF}{dz}.$$

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REPORT ON A PAPER BY JOHN TYNDALL ON CALORESCENCE

1 JANUARY 1866

From the original in the Library of the Royal Society, London[1]

REPORT ON PROF^r^ TYNDALLS PAPER ON CALORESCENCE[2]

This paper gives an account of experiments on invisible radiant heat and shows that rays which are not capable of exciting in us the sense of sight may, by heating a body, cause the body to emit luminous rays.[3]

The progress of knowledge of the distribution of heat in the spectrum is first

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(1) Royal Society, *Referees' Reports*, **6**: 292.

(2) John Tyndall, 'On calorescence', *Phil. Trans.*, **156** (1866): 1–24. The paper was received by the Royal Society on 20 October 1865, and read on 23 November 1865; see the abstract in *Proc. Roy. Soc.*, **14** (1865): 476.

(3) See note (8). Tyndall introduced the term 'calorescence' for this phenomenon of infra-red radiation, to 'express this transmutation of heat-rays into others of higher refrangibility... the invisible being rendered visible.' He observed that it 'harmonizes well with the term "fluorescence" introduced by Professor Stokes', and he noted the phrase 'transmutation of rays' introduced by James Challis; see 'On calorescence': 17. See G. G. Stokes, 'On the change of refrangibility of light', *Phil. Trans.*, **142** (1852): 463–562, on 479n (= *Papers*, **3**: 267–409); and James Challis, 'On the transmutation of rays', *Phil. Mag.*, ser. 4, **12** (1856): 521–6. Tyndall's investigation of the phenomenon arose in the course of his work on the absorption and radiation of heat in the 1860s (see Number 258). See especially his paper 'Contributions to molecular physics', *Phil. Trans.*, **154** (1864): 327–64, esp. 360–2, where he suggests the possibility of calorescence. The speculation, though not the discovery of the phenomenon, had already been made by C. K. Akin, leading to public confrontation between the two men, prior to Tyndall's presentation of his paper 'On calorescence' to the Royal Society. See John Tyndall, 'On luminous and obscure radiation', *Phil. Mag.*, ser. 4, **28** (1864): 329–41; C. K. Akin, 'Note on ray-transmutation', *ibid.*: 554–60; Akin, 'On calcescence', *Phil. Mag.*, **29** (1865): 28–43; Tyndall, 'On the history of negative fluorescence', *ibid.*: 44–55; Akin, 'Further statements concerning the history of calcescence', *ibid.*: 136–51; Tyndall, 'On the history of calorescence', *ibid.*: 218–31; Tyndall, 'On combustion by invisible rays', *ibid.*: 241–4. Tyndall conceded Akin's priority in speculation, but not of discovery and investigation. In his report on Tyndall's paper (Royal Society, *Referees' Reports*, **6**: 293), dated 18 January 1866, Stokes emphasised the experimental basis of Tyndall's work, at the commencement of his report: 'There can be no question, I think, as to the propriety of printing this paper in the Philosophical Transactions. It contains the experimental answer to the question Is it possible to render a body luminous simply by concentration upon it rays of solely invisible heat? The affirmative answer, besides solving the theoretical problem, renders it possible to make experiments on these invisible rays by a ready and direct method, and is connected collaterally with various questions of high interest.'

considered, and the experiments of Sir J. Herschel(4) and Profr Müller(5) on the heat of the Solar spectrum are compared with those of the author on the spectrum of the Electric Lamp.

If we had good methods of splitting up a ray into its spectrum and selecting any given portion corresponding to wave lengths between known limits, the determination of the heating effect of all such portions would furnish the most complete quantitative knowledge of the nature of the light. For if the ray is entirely consumed in heating a body, the quantity of heat produced in a second is a measure in known terms of the energy of the ray, and though the luminous properties of rays of different wave-lengths are in very different ratios to their heating powers, this ratio must be fixed for each determinate wave length.

Hence a determination of the distribution of heat in the spectrum of a given kind of light, combined with a previous knowledge of the relation of the heat to the other properties of each ray as a function of its wave-length, would lead to a complete knowledge of the properties of the given beam of light, whether chromatic or chemical.

Every step, therefore, which is made in the knowledge of the distribution of heat in a spectrum, is a step towards a complete knowledge, not of its heat alone, but of its other properties. In the paper, the heat radiated by the carbon points of the electric lamp is examined. As the temperature rises, the radiation, which was at first confined to the invisible part of the spectrum, extends into the visible part, but the invisible part of the radiation continues to increase in intensity, so that the temperature of the source the heating power of the invisible rays and the brightness of the luminous rays all increase together indefinitely as far as we can carry our observations.

The author in p. 3 expresses this by saying that 'the luminous and non luminous emissions augment together, the *maximum* of brightness of the visible rays coinciding with the maximum calorific power of the invisible ones'.(6) I

(4) In 'On calorescence': 7 Tyndall does refer to John Herschel's experiments on thermal effects of solar radiation; see J. F. W. Herschel, 'On the chemical action of the rays of the solar spectrum on preparations of silver and other substances, both metallic and non-metallic, and on some photographic processes', *Phil. Trans.*, **130** (1840): 1–59, esp. 52–9. But the work on the distribution of heat in the solar spectrum, which Maxwell here alludes to, was due to William Herschel, and it is this work which Tyndall reviews in the introduction to 'On calorescence': 1–2; see William Herschel, 'Experiments on the solar spectrum, and on the terrestrial rays that occasion heat', *Phil. Trans.*, **90** (1800): 293–326, 437–538, esp. 439–41 and Plate XX.

(5) J. Müller, 'Untersuchungen über die thermischen Wirkungen des Sonnenspectrums', *Ann. Phys.*, **105** (1858): 337–59; (trans.) 'Investigations on the thermal effects of the solar spectrum', *Phil. Mag.*, ser. 4, **17** (1859): 233–50, esp. 242.

(6) Tyndall, 'On calorescence: 2 (the italic is Maxwell's). In the published text there is a

do not think that the use of the word maximum here conduces to express the meaning. If the luminosity rises and falls (in time) the calorific power does the same and their maxima occur at the same time, but the maximum calorific effect in the spectrum, which has been spoken of before never coincides with the brightest point of the spectrum.

Experiments are then described on the distribution of heat in the spectrum of the electric light as formed by rock salt prisms and lenses. These show that the heat radiations extend far beyond the red end of the spectrum, that the maximum of heat is considerably beyond the red and that the total invisible heat is about eight times the visible. The length of the spectrum from the beginning of the green to the end of the red appears to have been about 8 millimeters. From the end of the red to the maximum about 2.75 m.m. and from the end of the red till the heat became insensible 19 m.m.

In repeating these experiments it would be of great advantage to test the purity of the spectrum by observing the absorption bands seen in it through small blue glass or still better by sodium vapour.(7)

If the focal lengths of the rock salt lenses were ascertained for any luminous ray and the index of refraction of a given invisible ray ascertained from the deviation produced by the prism of rock salt, the proper distance of the screen for receiving a pure spectrum could be calculated for any invisible part of the spectrum. It would however require several additional precautions to ascertain that the *extreme* portions of the observed heat spectrum are not partly due to scattering of the rays within the prism owing to its not being perfectly transparent with respect to such rays.

This observation does not apply to the observed maximum which is evidently a physical fact.

From the experiments in which a white heat was produced in platinum and other bodies by the concentration of invisible rays(8) it appears that a small part of the radiation from the carbons namely that which fell on the mirror

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footnote appended to this sentence referring to Tyndall's 'Rede Lecture for 1865'; see John Tyndall, *On Radiation* (London, 1865): 30, where there is discussion of the 'material jostling of the atoms' of the incandescent substance, so that 'the light-giving waves would follow as the necessary progeny of the heat-giving vibrations'.

(7) In his report on Tyndall's paper (see note (3)) Stokes commented: 'It is not easy to connect [Tyndall's readings]...with definite points in the spectrum. I think the value and interest of the table would be increased by supplementary measures'. See Tyndall, 'On calorescence': 4–5.

(8) Focusing the radiation of an electric arc on a foil of platinised platinum, the beam was sifted of luminous radiation by transmission through a solution of iodine in carbon bisulphide; the foil was raised to incandescence and emitted light. See Tyndall, 'On calorescence': 10–14.

or lens and afterwards escaped absorption by the iodine has sufficient energy of motion to bring solid bodies to a white heat. If by any means the more rapid vibrations of the heated carbons could be deadened in the carbon itself, as the luminous radiation is deadened by iodine in the intermediate medium, we should have a non-luminous body able to render a neighbouring body white hot, and therefore the original body would have a temperature higher than white heat without emitting luminous rays. But it is probable that the absorbing power of iodine depends on its temperature and that unless it were much colder than the source of heat it would itself supply any loss of luminous rays.

The experiment of putting the eye into the focus of heat is striking(9) but is quite unnecessary as a much better evidence of non luminosity is obtained by simply looking at the carbons through the iodine. For it is better to compare a small area than a large one with total blackness in order to test its non-luminosity, since the physiological test can be best applied by looking at the boundary between the given area and total blackness.

Of course when the eye is placed *at* the focus the variation of luminosity over the mirror depends on the defects of the mirror or focussing, and the eye should see a large uniform field of light or dimness. The carbons themselves were seen directly and not by reflexion at the mirror.

In conclusion, I consider this paper, as demonstrating the power of rays of long period to heat a body to such an extent as to make it emit copiously rays of shorter period to be worthy of a place in the Philosophical Transactions as a step in the history of science, while the employment of the electric lamp as a source of radiant heat and of iodine as an absorber of the luminous rays opens the way for a more complete investigation of the non luminous heat rays by affording us a source of great intensity and it may be hoped that the wave length of many of these rays their index of refraction in rock salt and their peculiar relations to various absorbing materials may be more fully studied in consequence of this paper.(10)

J. Clerk Maxwell
Cambridge, 1st Jan 1866(11)

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(9) Tyndall commented: 'I do not recommend the repetition of these experiments'; 'On calorescence': 15n.

(10) In his report (see note (3)) Stokes criticised Tyndall's 'illustration of calorescence derived from the production of small waves when a huge wave of the sea dashes against a rock'. He commented: 'Everything tends to show that in phosphorescence, fluorescence, calorescence the molecules of the body are thrown into a state of agitation, and that it is their agitation, communicated to the ether, which is the source of the altered period.' Compare Tyndall's discussion as published in 'On calorescence': 5–6n, on the oscillation of atoms; and see note (6).

(11) Maxwell was in Cambridge to examine for the Mathematical Tripos.

FROM A LETTER TO CHARLES BENJAMIN TAYLER[1]

2 FEBRUARY 1866

From Campbell and Garnett, *Life of Maxwell*[2]

8 Palace Gardens Terrace
W.
2 February 1866

You ask for my history since I wrote to you before my marriage. We remained in Aberdeen till 1860, when the union or fusion of the Colleges took place, and I went to King's Coll., London, where I taught till last Easter,[3] when I was succeeded by W. G. Adams,[4] brother of the astronomer.[5] I have now my time fully occupied with experiments and speculations of a physical kind, which I could not undertake as long as I had public duties.

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(1) See Volume I: 220–1.

(2) *Life of Maxwell*: 344–5; abridged.

(3) See Number 235 note (20).

(4) William Grylls Adams, St John's 1855 (Venn), who had been appointed to succeed G. R. Smalley (see Number 209 note (2)) in the lectureship in Natural Philosophy on 9 October 1863, was appointed Maxwell's successor as Professor of Natural Philosophy on 10 March 1865; see King's College London Archives, King's College Council Vol. I, minutes 276, 415.

(5) John Couch Adams, St John's 1839, Lowndean Professor of Astronomy and Geometry at Cambridge 1859 (Venn).

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LETTER TO HUGH ANDREW JOHNSTONE MUNRO(1)

7 FEBRUARY 1866

From the original in the Library of Trinity College, Cambridge(2)

8 Palace Gardens Terrace
London W.
1866 Feb 7

Dear Sir

I am writing about the Dynamical Theory of Gases and am making a short statement of those who have started or embraced similar theories before from Lucretius(3) down to D Bernoulli,(4) Le Sage of Geneva under the name of Lucrèce Newtonien(5) and Clausius(6) now professor at Zurich and myself.(7) The details of the mechanics are very different in these different writers on account of their different measure of acquaintance with the theory of collision &c. With respect to those who flourished since the revival of science I can make out pretty well what they really meant but I am afraid to say anything of Lucretius because his words sometimes seem so appropriate that it is with great regret that one is compelled to cut off a great many marks from him for showing that he did not mean what he has already said so well.

Here is what I have written. Will you tell me if you think it unjust to Lucretius either in excess or defect.

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(1) Trinity 1838, Tutor 1855–7, Junior Bursar 1862–6 (Venn); see *Titi Lucreti Cari De Rerum Natura Libri Sex*, ed. and trans. H. A. J. Munro, 2 vols. (Cambridge, 1864, $_2$1866). Garber, Brush and Everitt, *Molecules and Gases*: 83n suggest Munro as the addressee of Maxwell's letter.

(2) Trinity College, Cambridge, Add. MSS. c. 111^{10}. Previously published in *Molecules and Gases*: 82–3.

(3) Lucretius, *De Rerum Natura*: see note (1).

(4) Daniel Bernoulli, *Hydrodynamica, sive de Viribus et Motibus Fluidorum Commentarii* (Strasbourg, 1738): 200–2.

(5) G. L. Le Sage, 'Lucrèce Newtonien', *Nouveaux Mémoires de l'Académie des Sciences et Belles-Lettres de Berlin* (1782): 404–32; and in Pierre Prevost, *Notice de la Vie et des Écrits de George-Louis Lesage de Genève* (Geneva, 1805): 561–604. In his paper 'Ueber die Wärmeleitung gasförmiger Körper', *Ann. Phys.*, **115** (1862): 1–56, on 2n (see Number 207) Clausius cites Lesage's 'Physique mécanique' in *Deux Traités de Physique Mécanique* publiés par Pierre Prevost (Geneva/Paris, 1818): 1–186, a reference repeated by Maxwell in 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 42–88, on 50 (= *Scientific Papers*, **2**: 28).

(6) Rudolf Clausius, 'Ueber die Art der Bewegung welche wir Wärme nennen', *Ann. Phys.*, **100** (1857): 353–80; Clausius, 'Ueber die mittlere Länge der Wege...', *ibid.*, **105** (1858): 239–58.

(7) J. C. Maxwell, 'Illustrations of the dynamical theory of gases', *Phil. Mag.*, ser. 4, **19** (1860): 19–32; *ibid.*, **20** (1860): 21–37 (= *Scientific Papers*, **1**: 377–409).

'The notion of particles flying about in all directions, like the motes in a sunbeam, and causing by their impact the motion of larger bodies is to be found in the exposition of the theories of Democritus by Lucretius, but the nature of the impacts and the deviation produced in the path of the particles are described in language which we must interpret according to the physical conceptions of the age of the author that is we must get rid of every distinct physical idea which his words may have suggested to us.'(8)

Is this saying it too severely about a clever and intelligent ancient?

In particular have the Lucretian atoms an original motion all the same and in the same (downward) direction and equally accelerated (lib II 238, 239)(9) except insofar as they deviate, and so and only so come into collision (v 220 &c)(10) whereas (at v 90) spatium sine fine modoque est.(11)

The words are such a good illustration of the modern theory at v 100 lib II &c(12) that it would be a pity if they meant something quite different.

The *great intervals* between the collisions in air are in fact about $\frac{1}{400\,000}$ of an inch(13) but they are great compared to those in other media.

In my late capacity of Junior Moderator(14) I have to obtain a poet for Tripos day 7th April. Finding no impulse towards poesy existing at Trinity Hall I asked Gray(15) if the Trinity men were inclined that way. Can you tell

(8) Compare Maxwell, 'On the dynamical theory of gases': 50 (= *Scientific Papers*, **2**: 27–8).

(9) *De Rerum Natura*, ed. Munro, 2 vols. (Cambridge, ₂1866), **1**: 92, 'omnia quapropter debent per inane quietum/aeque ponderibus non aequis concita ferri'; ('and for this reason all things must be moved and borne along with equal velocity though of unequal weights through the unresisting void', *ibid.*, **2**: 33).

(10) *De Rerum Natura*, ed. Munro, **1**: 91, 'tantum quod momen mutatum dicere possis./quod nisi declinare solerent, omnia deorsum/imbris ubi guttae, caderent per inane profundum/nec foret offensus natus nec plaga creata/principiis; ita nil umquam natura creasset'; ('you just and only just can call it a change of inclination. If they were not used to swerve, they would all fall down like drops of rain, through the deep void, and no clashing would have been begotten nor blow produced among the first beginnings: thus nature never would have produced aught', *ibid.*, **2**: 33). See Numbers 377 and 439 on the swerve of Lucretian atoms.

(11) *De Rerum Natura*, ed. Munro, **1**: 86 (Book II, 92); 'space is without end and limit' (*ibid.*, **2**: 30).

(12) *De Rerum Natura*, **1**: 87 (Book II, 100–103), 'et quaecumque magis condenso conciliatu/exiguis intervallis convecta resultant,/indupedita suis perplexis ipsa figuris,/haec validas saxi radices et ferra ferri'; ('and all that form a denser aggregation when brought together, rebound leaving trifling spaces between, held fast by their own close-tangled shapes, these form enduring bases of stone and unyielding bodies of iron and such like', *ibid.*, **2**: 30).

(13) See Maxwell's paper presented to the British Association in 1860 (Volume I: 659–60).

(14) See *The Cambridge University Calendar for the Year 1866* (Cambridge, 1866): 448.

(15) Charles Gray, Trinity 1851, Junior Dean 1862–6 (Venn).

me how the appointment is made, and whether Pollock[16] or any other Trinity man would compose a hymn worthy of the day when the names of Colleges assume their Latin forms.

I remain
Yours truly
J. Clerk Maxwell

(16) Frederick Pollock, Trinity 1863, Browne Medal 1866 (Venn).

REPORT ON A PAPER BY JOHN TYNDALL ON RADIATION

22 FEBRUARY 1866

From the original in the Library of the Royal Society, London(1)

8 Palace Gardens Terrace
London W
1866 Feb 22

I have read Professor Tyndall's 'Sixth Memoir on Radiation and Absorption',(2) and consider it worthy of publication in the Philosophical Transactions.(3)

The author has shown by numerous instances that the behaviour of bodies with respect to radiations from dark sources is different from their behaviour with respect to visible radiations, and that the former is much more connected with simplicity of chemical constitution than the latter. He has also shown that rock salt is not equally transparent to all radiations from bodies at 100 °C but that it is less transparent to radiation from salt, and more transparent to radiation from platinum black, than to other radiations. Hence 1st All radiations from bodies at 100 °C are not alike, but differ in quality, and 2nd Rock salt has variations of absorptive power in that part of the spectrum which corresponds to rays of great wave-length such as are alone emitted from sources of low temperature. Hence if curves were drawn, the abscissae of which corresponded to wavelengths or to periods of vibration, and the ordinates to the absorptive power of a substance for radiations of such periods; the curve corresponding to rock salt which we know to have very small absorption for radiations invisible from their shortness of period as well as to visible radiations and invisible heat radiations from sources of high temperature would exhibit one or more maxima of absorption for certain rays of very long period, the numerical relation of which to the period of visible rays is not yet ascertained.

J. Clerk Maxwell

(1) Royal Society, *Referees' Reports*, **6**: 294.

(2) John Tyndall, 'Sixth memoir on radiation and absorption. – Influence of colour and mechanical condition on radiant heat', *Phil. Trans.*, **156** (1866): 83–96. The paper was received by the Royal Society on 21 December 1865, and read on 18 January 1866; see the abstract in *Proc. Roy. Soc.*, **15** (1866): 5.

(3) Tyndall's five previous papers 'On the absorption and radiation of heat' had been published in *Phil. Trans.*, **151** (1861): 1–36; *ibid.*, **152** (1862): 59–98; *ibid.*, **153** (1863): 1–12; *ibid.*, **154** (1864): 201–25; *ibid.*, **154** (1864): 327–68.

DRAFTS OF 'ON THE DYNAMICAL THEORY OF GASES'(1)

LATE 1865 – EARLY 1866(2)

From the originals in the University Library, Cambridge(3)

[ON THE DYNAMICAL THEORY OF GASES]

[1] [On the mutual action of two molecules](4)

Let us now consider the alteration in the path of a molecule in consequence of the action of another molecule which comes near it in its course.

Let us suppose the two molecules moving with equal momenta in opposite directions so that their centre of gravity is at rest.

Let their masses be M_1, M_2, their initial velocities V_1, V_2 and their distances from the centre of gravity r_1 r_2. We shall suppose them initially so distant that ⟨the⟩ force between them vanishes and they are moving sensibly in parallel straight lines at distances b_1 b_2 from the centre of gravity.

In consequence of the mutual action between the molecules each will describe a plane curve, the two curves being symmetrical with respect to the centre of gravity, and when the molecules are again out of reach of their mutual action they will be found moving from one another with velocities V_1 V_2 on straight lines distant b_1 b_2 from the centre of gravity but inclined to the directions of the original lines of motion by an angle 2θ where θ is the angle between the asymptotes and the apse of the orbit.

If the molecules are not mere centres of force(5) but bodies capable of internal motions their paths after the encounter will be different, and their velocities may not be exactly equal before and after but as we do not know the constitution of molecules we shall treat them as if they were mere centres of force.

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(1) J. Clerk Maxwell, 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88 (= *Scientific Papers*, **2**: 26–78).

(2) The paper was received by the Secretary of the Royal Society on 16 May 1866; see Number 263. On its composition, see Maxwell's letters to H. R. Droop of 19 July 1865, H. A. J. Munro of 7 February 1866, and William Thomson of 27 February 1866 (Numbers 250, 257 and 260).

(3) ULC Add. MSS 7655, V, f/6. Portions of this manuscript have been published in *Molecules and Gases*: 387–97.

(4) Compare 'On the dynamical theory of gases': 56–7 (= *Scientific Papers*, **2**: 35–6).

(5) See Number 266 note (6).

The angle θ may be calculated when we know V, b and the law of force where $V = V_1 + V_2$ & $b = b_1 + b_2$.

[2] [On the mutual action of two molecules](6)

Now let the components of the velocity of M_1 be $\xi_1\ \eta_1\ \zeta_1$ those of M_2 $\xi_2\ \eta_2\ \zeta_2$ and those of the centre of gravity of M_1 & M_2 $\bar{\xi}\ \bar{\eta}\ \bar{\zeta}$. Then we know by Dynamics that the velocity of the centre of gravity will be unchanged by the mutual action and that the motion of either molecule relatively to the centre of gravity will not be affected by the circumstance that the whole system is in motion.

The components of V_1 the velocity of M_1 relative to the centre of gravity are $\xi_1 - \bar{\xi}$, $\eta_1 - \bar{\eta}$, $\zeta_1 - \bar{\zeta}$. After M_1 and M_2 have met and deflected each other from their courses the value of V_1 will be the same but its direction will be turned through an angle 2θ in the plane containing the directions of V_1 and b. Let ϕ be the angle which this plane makes with the plane containing the direction of V and parallel to the axis of x and let $\xi_1 + \delta\xi_1$ be the component velocity of M_1 in the direction of x after the mutual action of the molecules.

$$\xi_1 + \delta\xi_1 = \frac{M_1\xi_1 + M_2\xi_2}{M_1 + M_2}$$

$$+\frac{M_2}{M_1+M_2}\{(\xi_1+\xi_2)\cos 2\theta + \sqrt{(\eta_1-\eta_2)^2+(\zeta_1-\zeta_2)^2}\sin 2\theta\cos\phi\}$$

or

$$\xi_1 + \delta\xi_1 = \xi_1 + \frac{M_2}{M_1+M_2}\{(\xi_2-\xi_1)\,2\sin^2\theta + \sqrt{(\eta_2-\eta_1)^2+(\zeta_2-\zeta_1)^2}\sin 2\theta\cos\phi\}.$$

There will be similar expressions for the components of the new velocity of M_1 in the other coordinate directions.

[3] [On the mutual action of molecules in motion](7)

To find the Equations of Motion of a Medium composed of Molecules in motion acting on one another with forces which are insensible at distances which are small compared with the average distance of the Molecules.

Let us begin by considering two particles whose masses are M_1 M_2 at a considerable distance from each other so that the force between them is insensible. Let their velocities resolved in the coordinate directions be

(6) Compare 'On the dynamical theory of gases': 57 (= *Scientific Papers*, 2: 36–7).
(7) Compare 'On the dynamical theory of gases': 60 (= *Scientific Papers*, 2: 40).

$\xi_1\,\eta_1\,\zeta_1$ and $\xi_2\,\eta_2\,\zeta_2$. Let V be the velocity of M_1 relative to M_2 and let G be the velocity of the centre of gravity of M_1 & M_2. Also let b be the minimum distance to which the particles would approach if there were no action between them.

In consequence of the mutual action between M_1 & M_2 when they come near each other, each will describe a curve about G their centre of gravity in the plane of V and b and when they have passed out of each others influence each will have a velocity relative to the centre of gravity equal in magnitude to its former value but in a direction inclined 2θ to its former direction in the plane of V & b, 2θ being the angle between the asymptotes of the orbit.

Let us assume that the moving force between the particles is as the n^{th} power of the distance inversely and that its value at distance unity is K and repulsive then the well known equation(8)

$$\frac{d^2u}{d\theta^2}+u+\frac{P}{h^2u^2}=0$$

becomes by integration and putting $u=\dfrac{x}{b}$ and $b=\alpha\left(\dfrac{K}{V^2}\dfrac{(M_1+M_2)}{M_2}\right)^{\frac{1}{n-1}}$

$$\theta=\int\frac{dx}{\sqrt{1-x^2-\dfrac{2}{n-1}\left.\dfrac{x}{\alpha}\right|^{n-1}}}.^{(9)}$$

Let θ be the value of this integral taken between the limits $x=0$ and x a root of the equation

$$1-x^2-\frac{2}{n-1}\left.\frac{x}{\alpha}\right|^{n-1}=0.$$

[4] **_Law of Volumes_**

To determine the variation in the quantity of ⟨heat⟩ energy, put

$$Q=\tfrac{1}{2}M_1(\xi_1^2+\eta_1^2+\zeta_1^2)$$

(8) The orbital equation for the path under a central force P. See J. H. Pratt, *The Mathematical Principles of Mechanical Philosophy* (Cambridge, $_2$1845): 223–4, where $u=\dfrac{1}{r}$, $h=\text{constant}=x\dfrac{dy}{dt}-y\dfrac{dx}{dt}$, as cited in Maxwell's undergraduate notebook 'Statics Dynamics' (ULC Add. MSS 7655, V, m/10, on f. 89).

(9) These equations are corrected in 'On the dynamical theory of gases': 60 (= *Scientific Papers*, 2: 40). On the result that $n=5$ see Number 263; and on the computation of the paths of the molecules see Number 262.

and let us first consider the value of $\frac{\delta Q}{\delta t}$ due to the action of the molecules of the second kind on those of the first in a mixed medium in equilibrium when $u\ v\ w$ are $= 0$(10)

then
$$\frac{\delta Q_1}{\delta t} = \frac{k\rho_2}{M_2}\Theta_1\left\{\frac{1}{2}M_2(\xi_2^2+\eta_2^2+\zeta_2^2)-\frac{1}{2}M_1(\xi_1^2+\eta_1^2+\zeta_1^2)\right.$$
$$\left.+\frac{1}{2}(M_1-M_2)\,(\xi_1\,\xi_2+\eta_1\,\eta_2+\zeta_1\,\zeta_2)\right\}.^{(11)}$$

Since ξ_1 is independent of ξ_2 and since the mean values of both are zero the product $\xi_1\,\xi_2$ has zero for its mean value so that the terms $\xi_1\,\xi_2$, $\eta_1\,\eta_2$, $\zeta_1\,\zeta_2$ disappear and we may write the result

$$\frac{\delta Q_1}{\delta t} = \frac{k\rho_2}{M_2}\Theta_1\{Q_2-Q_1\}.$$

Similarly
$$\frac{\delta Q_2}{\delta t} = \frac{k\rho_1}{M_1}\Theta_1\{Q_1-Q_2\}$$

whence
$$\frac{\delta}{\delta t}(Q_1-Q_2) = -k\Theta_1\left(\frac{\rho_1}{M_1}+\frac{\rho_2}{M_2}\right)(Q_1-Q_2)$$

or
$$Q_1 - Q_2 = Ce^{-\frac{t}{T}} \quad \text{where} \quad \frac{1}{T} = k\Theta_1\left(\frac{\rho_1}{M_1}+\frac{\rho_2}{M_2}\right).^{(12)}$$

(10) u, v, w are the components of the mean velocity of all the molecules which are at a given instant in a given element of volume, hence there is no motion of translation. ξ, η, ζ, are the components of the relative velocity of one of these molecules with respect to the mean velocity, the 'velocities of agitation of the molecules'. See 'On the dynamical theory of gases': 68 (= *Scientific Papers*, **2**: 51).

(11) ρ_1, ρ_2 are the densities of the two systems of molecules, Θ the absolute temperature.

(12) In 'On the dynamical theory of gases': 82 (= *Scientific Papers*, **2**: 69) Maxwell terms T the 'modulus of the time of relaxation'. In the introduction to 'On the dynamical theory of gases': 52–4 (= *Scientific Papers*, **2**: 30–2) he introduced the concept of 'time of relaxation' as a method of defining viscosity. He suggested that the rate of relaxation of the stress F of a viscous body is related to the strain S by the equation $\frac{dF}{dt} = E\frac{dS}{dt}-\frac{F}{T}$, where E is the coefficient of elasticity for that particular kind of strain. In his paper 'On double refraction in a viscous fluid in motion', *Proc. Roy. Soc.*, **22** (1873): 46–7 (= *Scientific Papers*, **2**: 379–80) he described an attempt to establish the relaxation time: 'In 1866 I made some attempts to ascertain whether the state of strain in a viscous fluid in motion could be detected by its action on polarized light' (compare Volume I: 125–7, 145–6, 148, 151–6 for his early investigation of induced double refraction in strained solids), recording that 'I observed an effect on polarized light when I compressed some Canada balsam', but that the effect was so transient that 'I have hitherto been unable to determine the rate of relaxation of that state of strain'.

Hence if Q_1 is originally different from Q_2 the values of Q_1 and Q_2 will rapidly approach to equality and will be sensibly equal in a few multiples of the very short time T, and in all movements of the media except the most violent Q_1 & Q_2 will remain equal. Now Q_1 is the actual energy of a single molecule of the first system due to the motion of agitation of its centre of gravity and Q_2 is the same for a single molecule of the second system. The energies of single molecules of mixed systems therefore tend to equality whatever the mass of each molecule may be.

Now when the gases are such that neither communicates energy to the other, their temperatures are the same and we have seen that $Q_1 = Q_2$ in this case.

But for either gas $NQ = \frac{3}{2}p$

therefore if both the temperature and the pressure be the same in two different gases, then N the number of molecules in unit of volume is also the same in the two gases. This is the law of Volumes of gases,[13] first discovered by Gay-Lussac from chemical considerations.[14] It is a necessary result of the Dynamical Theory of Gases.

In the case of a single gas in motion let Q be the total energy of a single molecule then

$$Q = \tfrac{1}{2}M((u+\xi)^2+(v+\eta)^2+(w+\zeta)^2+\beta(\xi^2+\eta^2+\zeta^2))^{(15)}$$

and $\dfrac{\delta Q}{\delta t} = M(uX+vY+wZ)$.

The general equation ( ) becomes

$$\frac{1}{2}\rho\frac{\partial}{\partial t}\left(u^2+v^2+w^2+(1+\beta)\,(\xi^2+\eta^2+\zeta^2)\right)+\frac{d}{dx}(u\rho\xi^2+v\rho\xi\eta+w\rho\xi\zeta)$$

$$+\frac{d}{dy}(u\rho\xi\eta+v\rho\eta^2+w\rho\eta\zeta)$$

$$+\frac{d}{dz}(u\rho\xi\zeta+v\rho\eta\zeta+w\rho\zeta^2)+\frac{1}{2}\frac{d}{dx}(1+\beta)\,\rho(\xi^3+\xi\eta^2+\xi\zeta^2)$$

(13) Compare Maxwell's discussion of 'Avogadro's hypothesis' in his letter to Stokes of 30 May 1859 (Volume I: 610) and see his 'Illustrations of the dynamical theory of gases. Part I. On the motions and collisions of perfectly elastic spheres', *Phil. Mag.*, ser. 4, **19** (1860): 19–32, esp. 30 (= *Scientific Papers*, **1**: 390); 'This result agrees with the chemical law, that equal volumes of gases are chemically equivalent'.

(14) See Number 263 where Maxwell repeats his attribution to Gay-Lussac; and 'On the dynamical theory of gases': 78 (= *Scientific Papers*, **2**: 64). See also Number 375 note (2).

(15) 'β is the ratio of the total energy [of a molecule] to the energy of translation'; see 'On the dynamical theory of gases': 55 (= *Scientific Papers*, **2**: 34), and Number 207 esp. note (20).

$$+\frac{1}{2}\frac{d}{dy}(1+\beta)\,\rho\,(\xi^2\eta+\eta^3+\eta\zeta^2)+\frac{1}{2}\frac{d}{dz}(1+\beta)\,\rho(\xi^2\zeta+\eta^2\zeta+\zeta^3)$$
$$=\rho(uX+vY+wZ).$$

Substituting the values of ρX ρY ρZ

$$\frac{1}{2}\rho\frac{\partial}{\partial t}(1+\beta)\,(\xi^2+\eta^2+\zeta^2)+\rho\xi^2\frac{du}{dx}+\rho\eta^2\frac{dv}{dy}+\rho\zeta^2\frac{dw}{dz}$$
$$+\rho\eta\zeta\left(\frac{dv}{dz}+\frac{dw}{dy}\right)+\rho\zeta\xi\left(\frac{dw}{dx}+\frac{du}{dz}\right)+\rho\xi\eta\left(\frac{du}{dy}+\frac{dv}{dx}\right)$$
$$+\frac{1}{2}(1+\beta)\left\{\frac{d}{dx}\rho(\xi^3+\xi\eta^2+\xi\zeta^2)+\frac{d}{dy}\rho(\xi^2\eta+\eta^3+\eta\zeta^2)\right.$$
$$\left.+\frac{d}{dz}\rho(\xi^2\zeta+\eta^2\zeta+\zeta^3)\right\}=0.$$

[5] **[Specific heat at constant volume]**(16)

The total energy of agitation of unit of volume of the medium is $\frac{3}{2}(1+\beta)\,p$ hence the total energy of agitation of unit of mass is

$$E=\frac{3}{2}(1+\beta)\frac{p}{\rho}.$$

If now additional energy be communicated to it in the form of heat without altering its density

$$\partial E=\frac{3}{2}(1+\beta)\frac{\partial p}{\rho}=\frac{3}{2}(1+\beta)\frac{p}{\rho}\frac{\partial\theta}{\theta}.$$

Hence the specific heat of unit of mass at constant volume is in dynamical measure

$$\frac{\partial E}{\partial\theta}=\frac{3}{2}(1+\beta)\frac{p}{\rho\theta}.$$

[Specific heat at constant pressure](17)

If the gas be now allowed to expand without receiving more heat from without till the pressure sinks to p the temperature will sink by a quantity $\partial\theta'$ such that

$$\frac{\partial\theta'}{\theta'}=\frac{2}{5+3\beta}\frac{\partial p}{p}=\frac{2}{5+3\beta}\frac{\partial\theta}{\theta}.$$

(16) Compare 'On the dynamical theory of gases': 79 (= *Scientific Papers*, 2: 65).
(17) Compare 'On the dynamical theory of gases': 79–80 (= *Scientific Papers*, 2: 66).

The total change of temperature is therefore $\partial\theta - \partial\theta' = \dfrac{3+3\beta}{5+3\beta}\partial\theta$ and the specific heat of unit of mass at constant pressure is

$$\frac{\partial E}{\partial\theta'} = \frac{5+3\beta}{2}\frac{p}{\rho\theta}.$$

The ratio of the specific heat at constant pressure to that at constant volume is $\dfrac{5+3\beta}{3+3\beta}$ a quantity which is generally denoted by the symbol γ.[18]

We have then $\beta = \dfrac{5-3\gamma}{3\gamma-3}$

and $\quad \dfrac{dE}{d\theta} = \dfrac{1}{\gamma-1}\dfrac{p}{\rho\theta}$ the specific heat at constant volume

$\dfrac{dE}{d\theta'} = \dfrac{\gamma}{\gamma-1}\dfrac{p}{\rho\theta}$ the specific heat at constant pressure

expressions from which the specific heat of air has been calculated by Professor Rankine and found to agree with the values determined experimentally by M. Regnault.[19]

[6] **[Determination of the inequality of pressure in a medium]**[20]

Let us next determine the variation of the pressure in the direction of x in a simple medium and make $Q = M(u+\xi)^2$ then by equation ( ) we find

$$\frac{\delta Q}{\delta t} = 2k\rho_2\,\Theta_2(\eta^2+\zeta^2+\xi^2)+2M\xi X$$

whence

$$\rho\frac{\partial\overline{\xi^2}}{\partial t}+2\left(\xi^2\rho\frac{du}{dx}+\xi\eta\rho\frac{du}{dy}+\xi\zeta\rho\frac{du}{dz}+\frac{d}{dx}\xi^3\rho\right.$$
$$\left.+\frac{d}{dy}\xi^2\eta\rho+\frac{d}{dz}\xi^2\zeta\rho\right) = \frac{6k\rho}{M}\Theta_2(p-\xi^2\rho).^{(21)}$$

Omitting for the present the terms involving three dimensions in $\xi\ \eta\ \zeta$

(18) The symbol was introduced by Poisson; see S. D. Poisson, *Traité de Mécanique*, 2 vols. (Paris, $_2$1833), **2**: 714–15; and Volume I: 608n.

(19) W. J. M. Rankine, 'On the mechanical action of heat', *Trans. Roy. Soc. Edinb.*, **20** (1853): 565–89, esp. 588–9, citing values determined by H. V. Regnault, 'Recherches sur les chaleurs spécifiques des fluides élastiques (3)', *Comptes Rendus*, **36** (1853): 676–87, esp. 685–6.

(20) Compare 'On the dynamical theory of gases': 80–3 (= *Scientific Papers*, **2**: 68–71).

(21) Maxwell terms k the 'coefficient of mutual interference' of the molecules; see 'On the dynamical theory of gases': 84 (= *Scientific Papers*, **2**: 72–3).

which refer to conduction of heat and taking the value of $\frac{\partial p}{\partial t}$ from equation ( )

$$\frac{\partial}{\partial t}(\rho\xi^2 - p) + 2p\frac{du}{dx} - \frac{2}{3}p\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right) = \frac{6k\rho\Theta_2}{M}(p - \xi^2\rho)$$

putting p for $\rho\xi^2$ and omitting $\xi\eta\rho$ and $\xi\zeta\rho$ in terms not involving the large coefficient $6k\rho\Theta_2$. If the motion is not very violent we may also neglect $\frac{\partial}{\partial t}(\rho\xi^2 - p)$ and then we have

$$\xi^2\rho = p - \frac{M}{9k\rho\Theta_2}p\left(2\frac{du}{dx} - \frac{dv}{dy} - \frac{dw}{dz}\right)$$

with similar expressions for $\eta^2\rho$ and $\zeta^2\rho$. By transformation of coordinates we can easily obtain the expressions for $\xi\eta\rho$ $\eta\zeta\rho$ and $\zeta\xi\rho$.

They are of the form

$$\eta\zeta\rho = -\frac{M}{6k\rho\Theta_2}p\left(\frac{dv}{dz} + \frac{dw}{dy}\right).$$

Having thus obtained the values of the pressures in different directions we may substitute them in the equation of motion.

$$\rho\frac{\partial u}{\partial t} + \frac{d}{dx}(\rho\xi^2) + \frac{d}{dy}(\rho\xi\eta) + \frac{d}{dz}(\rho\xi\zeta) = X\rho$$

which becomes

$$\rho\frac{\partial u}{\partial t} + \frac{dp}{dx} - \frac{pM}{6k\rho\Theta_2}\left\{\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2} + \frac{1}{3}\frac{d}{dx}\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right)\right\} = X\rho.^{(22)}$$

[Coefficient of Viscosity]

This is the equation of motion in the direction of x. The other equations may be written down by symmetry. The form of the equations is identical with that deduced by Poisson*[(23)] from the theory of elasticity by supposing the strain to be constantly relaxed at a given rate and the ratio of the

* Journal de l'Ecole Polytechnique 1829 tom XIII cah. 20 p. 139.[(23)]

(22) The equation of fluid motion: see notes (23) and (25), and Number 260.

(23) Siméon Denis Poisson, 'Mémoire sur les équations générales de l'équilibre et du mouvement des corps solides élastiques et des fluides', *Journal de l'École Polytechnique*, **13** cahier 20 (1831): 1–174, see esp. 152, equation (9).

coefficients of $\nabla^2 u$ and $\frac{d}{dx}\frac{1}{\rho}\frac{\partial\rho}{\partial t}$[24] agrees with that given by Professor Stokes.†[25]

The quantity $\frac{pM}{6k\rho\Theta_2}$ is the coefficient of viscosity or of internal friction and is denoted by μ in the writings of Professor Stokes and in my paper on the Viscosity of Air and other Gases.[26]

In this expression Θ_2 is a numerical quantity k is a quantity depending on the intensity of the action between two molecules at unit of distance.[27] The ratio of p to ρ is proportional to the temperature from absolute zero and is independent of the density. Hence in a given gas, μ is independent of the pressure[28] and proportional to the temperature,[29] as is found by experiment.‡[30]

Putting $$k = \left(\frac{K}{8M}\right)^{\frac{1}{2}} \quad p = MN\xi^2 \quad \rho = MN$$

σ = specific gravity compared with air

$$\mu = \frac{2^{\frac{3}{2}}}{6\Theta_2}\frac{M^{\frac{1}{2}}}{K^{\frac{1}{2}}}M\xi^2.$$

Now $M\xi^2$ is the same for all gases at the same temperature, therefore μ varies as $\left(\frac{M}{K}\right)^{\frac{1}{2}}$ for different gases at the same temperature.

† Cambridge Phil Trans. vol VIII (1845).[25]

‡

(24) The fourth term (from the equation of continuity).

(25) George Gabriel Stokes, 'On the theories of the internal friction of fluids in motion, and of the equilibrium and motion of elastic solids', *Trans. Camb. Phil. Soc.*, **8** (1845): 287–319, esp. 297, equation (12) (= *Papers*, **1**: 75–129).

(26) See note (30).

(27) In 'On the dynamical theory of gases': 84–5 (= *Scientific Papers*, **2**: 73), Maxwell refers to Thomas Graham's experiments in determining values of k, the coefficient of mutual interference of the molecules of the gases. See Number 263.

(28) On the independence of μ and the density of a gas compare 'Illustrations of the dynamical theory of gases. Part I': 32 (= *Scientific Papers*, **1**: 391); his letter to Stokes of 30 May 1859 (Volume I: 610); and Numbers 207 and 252.

(29) See Numbers 248, 249 and 252.

(30) J. C. Maxwell, 'On the viscosity or internal friction of air and other gases', *Phil. Trans.*, **156** (1866): 249–68, and see Number 252. O. E. Meyer, 'Ueber die innere Reibung der Gase', *Ann. Phys.*, **125** (1865): 177–209, 401–20, 564–99 gave experimental support for Maxwell's result, which had so surprised him in 1859 (see note (28)), that the viscosity is independent of the density of a gas.

[7] [Conduction of heat][31]

We have next to determine the rate of variation of the quantity $\xi^3+\xi\eta^2+\xi\zeta^2$ in a simple medium. Putting

$$Q = M(u+\xi)\,(u^2+v^2+w^2+2u\xi+2v\eta+2w\zeta+(1+\beta)\,(\xi^2+\eta^2+\zeta^2))$$

and making u, v & w zero after the differentiations and neglecting terms of the form $\xi\eta$ in comparison with those of the form ξ^2 and remembering that terms of the forms ξ^3 and $\xi\eta^2$ are also very small we get

$$N\frac{\delta Q}{\delta t} = -\frac{\sqrt{2}}{M}\sqrt{\frac{K}{M}}(1+\beta)\,\Theta_2\,\rho^2(\xi^3+\xi\eta^2+\xi\zeta^2)+2\rho\xi^2X$$
$$+2\rho\xi\eta Y+2\rho\xi\zeta Z+(1+\beta)\,\rho(\xi^2+\eta^2+\zeta^2)X.$$

Hence equation (   ) becomes

$$\rho(1+\beta)\,(\xi^2+\eta^2+\zeta^2)\frac{\partial u}{\partial t}+2\rho\xi^2\frac{\partial u}{\partial t}+2\rho\xi\eta\frac{\partial v}{\partial t}+2\rho\xi\zeta\frac{\partial w}{\partial t}$$
$$+\rho\frac{\partial}{\partial t}(1+\beta)\,(\xi^3+\xi\eta^2+\xi\zeta^2)+(1+\beta)\frac{d}{dx}\rho\xi^2\,(\xi^2+\eta^2+\zeta^2)$$
$$= -\frac{\sqrt{2}}{M}\sqrt{\frac{K}{M}}(1+\beta)\,\Theta_2\,\rho^2(\xi^3+\xi\eta^2+\xi\zeta^2)+2\rho\xi^2X$$
$$+2\rho\xi\eta Y+2\rho\xi\zeta Z+(1+\beta)\,\rho\,(\xi^2+\eta^2+\zeta^2)X.$$

Putting $\rho\dfrac{\partial u}{\partial t}+\dfrac{dp}{dx}$ for ρX and omitting terms in $\xi\eta$ &c

$$(1+\beta)\,\rho\frac{\partial}{\partial t}(\xi^3+\xi\eta^2+\xi\zeta^2)+(1+\beta)\frac{d}{dx}\rho(\xi^4+\xi^2\eta^2+\xi^2\zeta^2)$$
$$-(1+\beta)\,(\xi^2+\eta^2+\zeta^2)\frac{dp}{dx}-2\xi^2\frac{dp}{dx}$$
$$= \frac{\sqrt{2}}{M}\sqrt{\frac{K}{M}}\Theta_2(1+\beta)\,\rho^2(\xi^3+\xi\eta^2+\xi\zeta^2).$$

(31) Errors arose in establishing these equations; compare 'On the dynamical theory of gases': 85–6 (= *Scientific Papers*, 2: 74–5).

When the motion is steady or when it is not very violent the first term will disappear and the equation may be written

$$3(1+\beta)\frac{d}{dx}\cdot\frac{p^2}{\rho}-(5+3\beta)\frac{p}{\rho}\frac{dp}{dx} = -\sqrt{2}\sqrt{\frac{K}{M^3}}\Theta_2(1+\beta)\,\rho^2(\xi^3+\xi\eta^2+\xi\zeta^2)$$

or
$$(1+\beta)\,\rho(\xi^3+\xi\eta^2+\xi\zeta^2) = \sqrt{\frac{M^3}{2K}}\frac{1}{\Theta_2}\frac{p^2}{\rho^2}\left(2\frac{1}{p}\frac{dp}{dx}-3(1+\beta)\frac{d\theta}{dx}\right)$$
$$= \frac{3}{2}\mu\frac{p}{\rho}\left(2\frac{1}{p}\frac{dp}{dx}-3(1+\beta)\frac{1}{\theta}\frac{d\theta}{dx}\right)$$

where μ is the coefficient of viscosity. This is the quantity of heat, measured as mechanical energy which is carried over unit of area in unit of time when the pressure and temperature vary from point to point.

[8] **[Condition of equilibrium of a gas]**

A quantity of gas is in equilibrium in a vessel under the action of gravity, to determine the pressure and temperature at any point when both the pressure and temperature have assumed their final state.

The condition of mechanical equilibrium is

$$\frac{dp}{dx} = \rho g$$

where x is measured in the direction in which g acts. We have also

$$p = \tfrac{1}{3}\rho(\xi^2+\eta^2+\zeta^2)$$

and in the state of equilibrium $\xi^2 = \eta^2 = \zeta^2$.

The whole energy of a molecule M is

$$\tfrac{1}{2}M(1+\beta)\,(\xi^2+\eta^2+\zeta^2)$$

where ξ η ζ and β have values peculiar to that molecule.

Now let the molecule move so as to increase x by dx then gravity will do work upon it $= Mgdx$ and its energy will now be increased by this amount. If its energy is now greater than the mean of that of the molecules among which it has arrived it will increase the mean energy of the molecules in that part of the field, and if it is less it will diminish the mean energy but if the mean energy of these molecules is greater by $Mgdx$ than the original energy of the molecule considered, its arrival will not alter the mean energy of the surrounding molecules.

That this must be the case generally we must have

$$\frac{1+\beta}{2}\frac{d}{dx}(\xi^2+\eta^2+\zeta^2) = g$$

and if this condition be fulfilled, any molecule passing from one position to another will have the excess of its energy above the mean energy of the surrounding molecules the same throughout its path.

[9] [Effect of gravity on the temperature of a column of gas]

Since μ is independent of the density and proportional to the pressure the coefficient of conductivity for heat which is $\frac{9}{2}\frac{p}{\rho\theta}\mu(1+\beta)$(32) will also be independent of the density and will vary directly as the temperature.

The condition of there being no conduction of heat is

$$\frac{d\theta}{dp} = \frac{2}{3(1+\beta)}\frac{\theta}{p}.$$

Now when the pressure of a gas is gradually changed no heat being allowed to enter or escape

$$\frac{d\theta}{dp} = \frac{2}{5+3\beta}\frac{\theta}{p}.$$

If therefore a mass of gas under the action of gravity were to be left to itself till conduction of heat ceased, the temperature would diminish more rapidly with the height than that of a portion of gas carried up bodily. If the portion of gas were to ascend it would be warmer than the surrounding gas and would therefore tend to ascend still, and if it were to descend below its original position it would be colder than the surrounding gas and would tend to descend further. Hence the condition of final equilibrium of heat in a gas acted on by gravity is one of mechanical instability so that such a mass of gas left to itself will perpetually be converting part of its heat into visible motion or currents and the energy thus developed will be reconverted into heat by friction.(33)

If however the motion were properly regulated the energy thus developed

(32) See the last equation in §7.

(33) In an 'Addition made December 17, 1866' to 'On the dynamical theory of gases': 86–7 (= *Scientific Papers*, **2**: 75–6) Maxwell recollected that: 'When I first attempted this investigation [of the final equilibrium of temperature of a column of gas] I overlooked the fact that $\overline{\xi^4}$ is not the same as $\overline{\xi^2}.\overline{\xi^2}$, and so obtained as a result that the temperature diminishes as the height increases at a greater rate than it does by expansion when air is carried up in mass. This leads at once to a condition of instability, which is inconsistent with the second law of thermodynamics. I wrote to Professor Sir W. Thomson about this result, and the difficulty I had met with [Number 260], but presently discovered *one* of my mistakes, and arrived at the conclusion that the temperature would increase with the height'. This was the conclusion Maxwell stated in the paper as submitted to the Royal Society in May 1866: see Number 263, esp. note (23).

could be transferred to machinery so as to convert the invisible agitation of the gas into any other form of energy and thus form a perpetual motion. For instance if a portion of the gas were carried upwards and made to expand so as always to be at the same temperature with the surrounding gas it would be rarer than the surrounding gas and the resultant of pressure and gravity on the portion of gas would act upwards and so do work. If when at the highest point it is allowed to acquire the temperature and pressure of the surrounding gas and is then lowered and compressed so as to be always at the temperature of the surrounding gas, it will be denser than the surrounding gas and the resultant of gravity and pressure will act downwards and still do work. Thus from a mass of gas acted on by gravity energy may be abstracted to any amount, and the gas cooled to a corresponding extent.

This result is directly opposed to the second law of Thermodynamics which affirms that 'it is impossible by means of inanimate material agency to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest part of the surrounding objects.'*(34)

I think it necessary to confirm a result so much opposed to so important a doctrine by a more elementary investigation in which we do not require to consider the precise nature of the action between the molecules or to determine the coefficient of conductivity.(35)

* W. Thomson. On the Dynamical Theory of Heat Trans. Edin. 1851 p 265(34)

(34) William Thomson, 'On the dynamical theory of heat', *Trans. Roy. Soc. Edinb.*, **20** (1851): 261–88, on 265 (= *Math. & Phys. Papers*, **1**: 179). Maxwell modified Thomson's wording slightly: compare Number 277 note (9).

(35) See Numbers 260 and 263.

LETTER TO WILLIAM THOMSON

27 FEBRUARY 1866

From the original in the University Library, Glasgow(1)

8 Palace Gardens Terrace
London W.
1866 Feb 27

Dear Thomson

In working at the Dynamical Theory of Gases I have come on the following paradox, which I intend to think about, but I should be obliged to you for the benefit of your views.(2)

1st Suppose $\overline{\xi^2}$ to represent the mean square of the velocity of a molecule in direction of x it is easy to show that

$$p = \rho\overline{\xi^2}.$$

2nd If $\overline{\eta^2}$ & $\overline{\zeta^2}$ be the mean squares of the velocities in the other two directions and if β be the ratio of the energy of rotation or other internal motion to that of translation then the mean total energy of a molecule will be

$$\frac{1}{2}M(\overline{\xi^2}+\overline{\eta^2}+\overline{\zeta^2})\,(1+\beta)$$

and that of unit of volume

$$\frac{3}{2}(1+\beta)\,\rho\xi^2 = \frac{3}{2}(1+\beta)\,p$$

and that of unit of mass

$$\frac{3}{2}(1+\beta)\frac{p}{\rho}.$$

3rd Now let unit of mass be enclosed in volume V and let V become $V+dV$ then work $= pdV$ is done and we must have

$$d\left(\frac{3}{2}(1+\beta)\,pV\right)+pdV = 0$$

or since $\qquad pV = \xi^2 \quad \text{and} \quad V = \dfrac{1}{\rho}$

$$\frac{3}{2}(1+\beta)\frac{d.\xi^2}{\xi^2} = \frac{d.\rho}{\rho}.$$

(1) Glasgow University Library, Kelvin Papers, M 19.

(2) See James Clerk Maxwell, 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88, esp. 86–7 (= *Scientific Papers*, **1**: 75–6). Compare Number 259 §8 and §9.

All this is the ordinary theory putting

$$\frac{3}{2}(1+\beta) = 1-\gamma \quad \text{and} \quad \xi^2 = CT(\text{emperature}).$$

Now comes the difficulty.

4th To determine the conditions of equilibrium of temperature in a heavy gas.

The mean energy of a molecule is

$$\frac{1}{2}M(\xi^2+\eta^2+\zeta^2)\,(1+\beta).$$

If it ascends a distance dx against a force g this is diminished by $gMdx$.

If its mean energy thus diminished is greater than that of the molecules in the stratum into which it has come it will increase the mean energy there if less it will diminish it therefore for eq^m of heat

$$gMdx+d\left(\frac{1}{2}M(\xi^2+\eta^2+\zeta^2)\,(1+\beta)\right) = 0$$

or
$$gdx+\frac{3}{2}(1+\beta)\,d\bar{\xi}^2 = 0.$$

Now
$$gdx = -\frac{dp}{\rho} =$$

$$\therefore \frac{3}{2}(1+\beta)\frac{d.\bar{\xi}^2}{\bar{\xi}^2} = \frac{dp}{p}.$$

This is the condition of no conduction of heat up or down in a heavy gas.

Now since $p = \rho\,\bar{\xi}^2$, $\dfrac{dp}{p} = \dfrac{d\rho}{\rho}+\dfrac{d\xi^2}{\xi^2}$.

If then a mass of gas under gravity is left to itself the law of temperature will be

$$\frac{dT}{T} = \frac{2}{3(1+\beta)}\frac{dp}{p}$$

whereas if the pressure of the gas is changed dp

$$\frac{dT}{T} = \frac{2}{5+3\beta}\frac{dp}{p}$$

so that the temperature in a vertical column decreases faster with the pressure than that of a portion of gas carried up the column bodily.

Now for the paradox

5th Take a large mass of gas and let it come into thermic equilibrium. Take a small portion in a cylinder & piston without weight or counterbalanced. Raise it and let it expand so as to have the same pressure with the surrounding gas, then as it is hotter it will be lighter and will be buoyant, so that, as it goes up it will do work. When it comes to the top let it remain till it cools to the surrounding temperature then lower it keeping the pressure equal to the surrounding pressure then it will be colder and therefore denser than the surrounding gas and will sink with a force which may be made to do work.

Thus by means of material agency mechanical effect is derived from the gas under gravity by cooling it below the temperature of the coldest of the surrounding objects. See Thomson Dyn Θ of H 2nd Law.(3)

Whether the dyn. Θ. of Gases is good or not 1 2 & 3 are good mechanics and true of gases. 4 is the only difficulty and the only way out of it seems to be that in the case supposed a molecule moving upwards has not the same mean energy as one moving horizontally or downwards, at the same height. This would involve different pressures in different directions and is otherwise objectionable. So there remains as far as I can see a collision between Dynamics & thermodynamics.

I have just heard Mr Everetts paper at the R S about elasticity of glass.(4) The method is superior to Kirchhoff's(5) in as much as the flexure is produced by couples so as to be uniform over the length of the rod as the torsion is, whereas Kirchhoffs makes the moment greatest in the middle so that the flexure and torsion have different forms for integration whereas here they are the same, and therefore σ(6) is more to be trusted. But Kirchhoffs apparatus was more symmetrical and better adapted to measurement.

Have four equal weights and put them

1st	at A B, A' B'	for torsion+
2	B C B' C'	flexure+
3	C D C' D'	torsion−
	D A D' A'	flexure−.

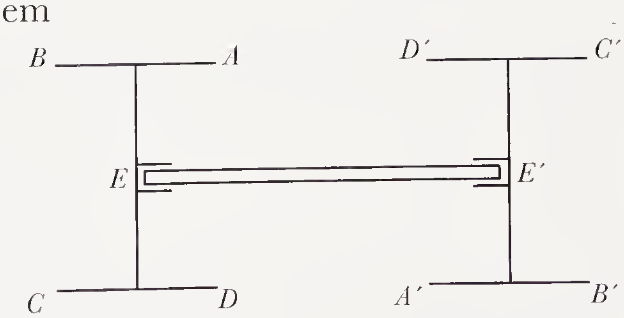

Figure 260,1

(3) William Thomson, 'On the dynamical theory of heat', *Trans. Roy. Soc. Edinb.*, **20** (1851): 261–88, on 265 (= *Math. & Phys. Papers*, **1**: 179). (4) See Number 261 esp. notes (3) and (4).

(5) Gustav Kirchhoff, 'Ueber das Verhältniss der Quercontraction zur Längendilatation bei Stäben von federhartem Stahl', *Ann. Phys.*, **108** (1859): 369–92.

(6) See Number 261 esp. note (6).

The results seem so good that it would be worth while to correct for the deviation of the arm of the couple from the horizontal.

I made a new set of expts on viscosity of air and came within $\frac{1}{150}$ of what I got in summer (corrected for temperature of course). If you want the results for your book or anything else here they are.

$$\text{Coeff}^{\text{t}} \text{ of friction } \mu \text{ dimension } \frac{\text{M}}{\text{LT}}$$

$$\text{at } 62\ {}^{\circ}\text{F } \mu = .09362 \frac{\text{grains}}{\text{feet seconds}}^{(7)}$$

$$\mu = .01878\ (1+\alpha\theta) \frac{\text{grammes}}{\text{metres seconds}} \text{ centigrade scale for } \theta.$$

μ is quite independent of the pressure and proportional to the absolute temperature from 50° to 185 °F.

This value is about double Stokes[8] and about half Meyer's.[9]

Hydrogen is .516 of air by my expts. Graham by transpiration gets .485 but he required less hydrogen and less time for his expts and I think got purer gas. Carbonic acid .859 by me .807 by Graham.[10] All these gases are dry. Damp air is a very little smoother than dry about $\frac{1}{60}$ part for pressure 4 in temp. 70 °F. Hydrogen and air, equal vols about $\frac{15}{16}$ of air.

Results of dynamical theory not yet tested.

1 Coefficient of conductivity for heat (measured as energy) $= \frac{9(1+\beta)}{2}\frac{p}{\rho\theta}\mu$ where μ is the coefft of friction or viscosity.[11]

2 Equations of motion are of the form

$$\rho\frac{\partial u}{\partial t}+\frac{dp}{dx}-\mu\left(\frac{d^2u}{dx^2}+\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}\right)-\frac{\mu}{3}\frac{d}{dx}\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)$$

the same as Stokes.[12] For different gases

$$\frac{1}{\mu} \propto \sqrt{\frac{M}{K}} \quad \text{where } M = \text{mass of molecule} \quad K \text{ force at unit distance.}$$

(7) The value cited in 'On the dynamical theory of gases': 83 (= *Scientific Papers*, **2**: 71). Compare Number 252.

(8) G. G. Stokes, 'On the effect of the internal friction of fluids on the motion of pendulums', *Trans. Camb. Phil. Soc.*, **9**, part 2 (1851): [8]–[106], esp. 65 (= *Papers*, **3**: 1–136).

(9) O. E. Meyer, 'Ueber die Reibung der Flüssigkeiten', *Ann. Phys.*, **113** (1861): 55–86, 193–238, 383–425, esp. 383.

(10) Thomas Graham, 'On the motion of gases. Part II', *Phil. Trans.*, **139** (1849): 349–401, esp. 364; and see Number 252 note (13).

(11) See Number 259 §9.

(12) See Number 259 §6, esp. note (25).

3 Mixed gases diffuse till the law of density for each is the same as if the others were away. The equation of diffusion is

$$u_1 p_1 = \frac{D}{p_1 + p_2}\left(X\rho_1 - \frac{dp_1}{dx}\right)$$

where u_1 is vel^y and p_1 the pressure of one gas
p_2 of the other

D is a coefft depending on the two gases mutual action.(13) It is independent of density and varies with square of temperature therefore since it is divided by $p_1 + p_2$ the actual rate of diffusion will be as the temperature and inversely as the density.

B Stewart(14) is busy with his 'Chimæra bombylans in vacuo'.(15)

I am going to see the new electrical machine at the R I(16) which I take to be a foreign development of C F Varleys multiplier.(17) There is no doubt that it is the right thing in electrical machines to have no friction but to work up the electricity by induction.

Yours truly
J. Clerk Maxwell

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(13) Compare 'On the dynamical theory of gases': 75 (= *Scientific Papers*, **2**: 60); 'D is the volume of gas reduced to unit of pressure which passes in unit of time through unit of area when the total pressure is uniform and equal to p, and the pressure of either gas increases or diminishes by unity in unit of distance. D may be called the coefficient of diffusion.'

(14) The experiments by Balfour Stewart and P. G. Tait, 'On the heating of a disc by rapid rotation *in vacuo*'; see Numbers 244 note (4), 245 note (7) and 262 note (12).

(15) A misquotation from Rabelais, *Pantagruel*, Book II, chap. 7; 'Quaestio subtilissima, utrum Chimæra in vacuo bombinans possit comedere secundas intentiones.' See Volume I: 493.

(16) The Holtz electrical machine: see W. Holtz, 'Ueber eine neue Elektrisirmaschine', *Ann. Phys.*, **126** (1865): 157–71; (trans.) 'On a new electrical machine', *Phil. Mag.*, ser. 4, **30** (1865): 425–33. See the *Treatise*, **1**: 260 (§212).

(17) C. F. Varley's instrument, a patent of 1860, is mentioned by Maxwell in the *Treatise*, **1**: 257n (§210): 'Specification of Patent, Jan. 27, 1860, No. 206'. See Number 302 esp. note (2).

REPORT ON A PAPER BY JOSEPH DAVID EVERETT[1] ON THE RIGIDITY OF GLASS

1 MARCH 1866

From the original in the Library of the Royal Society, London[2]

REPORT ON D^r^ EVERETTS PAPER ACCOUNT OF EXPERIMENTS ON THE FLEXURAL AND TORSIONAL RIGIDITY OF A GLASS ROD; LEADING TO THE DETERMINATION OF THE RIGIDITY OF GLASS[3]

The experiments here described[4] are in some respects similar to those made by M. Kirchhoff on steel rods Pogg. cviii p369 or Phil Mag 1862 Jan^y^ p28[5] in which the flexure and the torsion of the same rod is determined and the relation of the Lateral Contraction to the Longitudinal Extension is estimated.[6] This relation is of great importance in the theory of Elastic Solids, since if we assume elasticity to arise from the direct action of molecules on one another by attractive or repulsive forces acting along the line of centres, this ratio can be shown to be $\frac{1}{4}$,[7] provided the displacement of each

(1) Assistant to Hugh Blackburn, Professor of Mathematics at Glasgow, 1864; Professor of Natural Philosophy, Queen's College, Belfast 1867 (*DNB*).

(2) Royal Society, *Referees' Reports*, **6**: 117.

(3) J. D. Everett, 'Account of experiments on the flexural and torsional rigidity of a glass rod, leading to the determination of the rigidity of glass', *Phil. Trans.*, **156** (1866): 185–91. The paper was received by the Royal Society on 1 February 1866, and read on 22 February 1866; see the abstract in *Proc. Roy. Soc.*, **15** (1866): 19–20.

(4) Everett's experiments were carried out in William Thomson's laboratory at Glasgow University in the summer of 1865, following a plan devised by Thomson, who communicated the paper to the Royal Society; see Everett, 'Account of experiments on the flexural and torsional rigidity of a glass rod': 185. Thomson had himself recently reported work on the subject to the Royal Society; see William Thomson, 'On the elasticity and viscosity of metals', *Proc. Roy. Soc.*, **14** (1865): 289–97.

(5) Gustav Kirchhoff, 'Ueber das Verhältniss der Quercontraction zur Längendilatation bei Stäben von federhartem Stahl', *Ann. Phys.*, **108** (1859): 369–92; (trans.) 'On the relation of the lateral contraction to the longitudinal expansion in rods of spring steel', *Phil. Mag.*, ser. 4, **23** (1862): 28–47.

(6) Everett denotes this relation by the symbol σ, and terms it 'Poisson's ratio'; see 'Account of experiments on the flexural and torsional rigidity of a glass rod': 189. See notes (7) and (9).

(7) As stated by Siméon Denis Poisson, 'Mémoire sur l'équilibre et le mouvement des corps élastiques', *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, **8** (1829): 357–570, 623–7, on 451. This value is a consequence of Poisson's molecular force law and assumption of an invariable ratio between the linear rigidity and the compressibility of the volume of elastic solids: see note (9).

molecule is a function of its position in the body.[(8)] If however the displacement of each molecule is a function of a different form from that of the others, the ratio may be different, and if the molecules are in a state of motion or if the elasticity is in part due to the pressure of molecular atmospheres, the ratio will be greater than $\frac{1}{4}$.[(9)]

The torsional elasticity of any portion of a rod of any form depends on the moment of inertia of the section about its centre of gravity in its own plane.

The flexural elasticity of the same portion of the rod as regards bending in a given plane depends on the moment of inertia of the section about a diameter perpendicular to that plane. Now if two diameters be taken at right angles to each other the sum of the moments of inertia is equal to the moment of inertia about an axis perpendicular to the plane. Hence by comparing the torsion of a portion of a rod with the flexure produced first in one plane and then in a plane at right angles the ratio of the rigidity to the longitudinal elasticity can be determined without knowing the form of the section.

In experiments on torsion, the moment of twisting is the same at every section of the rod. It is desirable therefore that in the experiments on flexure the moment of bending should be uniform for every section. This condition is fulfilled in D^r^ Everetts experiments by applying two equal and opposite couples at the ends of the rod. In M Kirchhoffs experiments the rod was supported at the middle and loaded at the end so that the moment of bending decreased from the middle to the ends so that the result will depend more on the stiffness of the middle of the rod than on that of the ends. In this respect D^r^ Everetts method is to be preferred and I think this should be stated. The form of the apparatus for applying the force might I think be improved by an arrangement like this.

(8) In his paper 'On the equilibrium of elastic solids', *Trans. Roy. Soc. Edinb.*, **20** (1850): 87–120, esp. 87 (= *Scientific Papers*, **1**: 31), Maxwell had termed this 'the assumption of Navier'. For his discussion of theories of elasticity see Volume I: 133–5.

(9) Rejecting Poisson's molecular force law and assumption of an invariable ratio between linear rigidity and compressibility of volume, Stokes introduced two arbitrary independent coefficients of elasticity; see G. G. Stokes, 'On the theories of the internal friction of fluids in motion, and of the equilibrium and motion of elastic solids', *Trans. Camb. Phil. Soc.*, **8** (1845): 287–319 (= *Papers*, **1**: 75–129). In 'On the equilibrium of elastic solids' Maxwell followed Stokes' method; see Volume I: 133–5, 138–40, 142–4. In experiments on glass, Guillaume Wertheim had obtained an experimental value of $\frac{1}{3}$ for 'Poisson's ratio', at variance with Poisson's theoretical value; see Guillaume Wertheim, 'Mémoire sur l'équilibre des corps solides homogènes', *Ann. Chim. Phys.*, ser. 3, **23** (1848): 52–95, esp. 55, 57. See also G. Wertheim, 'On the cubical compressibility of certain solid homogeneous bodies', *Phil. Mag.*, ser. 4, **21** (1861): 447–51.

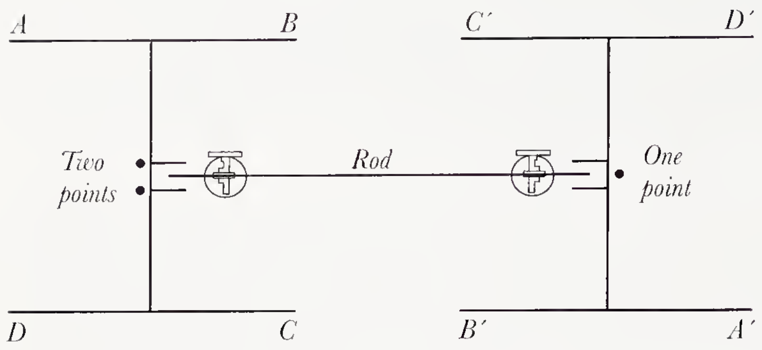

Figure 261,1

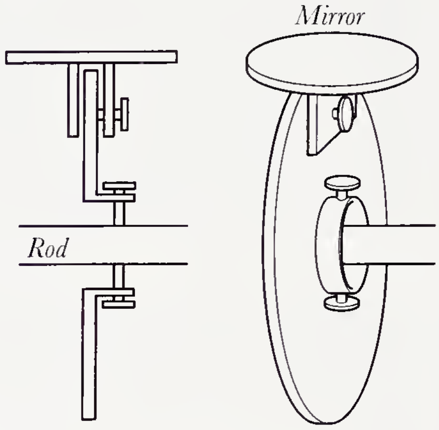

Figure 261,2. Small disc kept constantly clamped to one place.

The Weights to be placed on $ABA'B'$ then on $BCB'C'$ and so on. The flexibility of the supports of the mirrors should be carefully tested.(10)

I also think that since the points marked C, E in the figure in the paper are not in the same horizontal plane, a small alteration in the direction of the arm will make an appreciable alteration on the value of the moment.(11) These points therefore should either be placed in the same horizontal line or the angle of the line joining them should be measured. I think that the accuracy of the results obtained warrants this refinement.(12)

The value of σ(13) found by Dr Everett for glass is		.258	
That found by Wertheim(14) by a different method is about		.33	crystal
Kirchhoff(15) for	steel	.296	
	brass	.387	

These are all greater than .25 as they ought to be(16) but Dr Everetts is much

(10) Everett had measured the bending produced in a cylindrical rod by means of 'two mirrors, rigidly attached to the rod ... which form by reflexion upon a screen two images of a fine wire placed in front of a lamp-flame'; Everett, 'Account of experiments on the flexural and torsional rigidity of a glass rod': 185.

(11) The arm CE provides a twisting couple to a rod secured at C; see Plate XVI Fig. 3 in *Phil. Trans.*, **156** (1866).

(12) In a subsequent series of experiments Everett modified his apparatus on the lines Maxwell suggests: see Number 269.

(13) See note (6).

(14) See note (9).

(15) See note (5).

(16) See notes (7) and (9).

the smallest. By experiments in 1850[17] I made $\sigma = .267$ for iron and .332 for glass.[18]

I consider D^r Everetts paper worthy of publication, the reference to Kirchhoffs paper being made a little fuller as the methods are similar.

JAMES CLERK MAXWELL
1866 March 1

(17) Early in 1850, as an intended 'Appendix' to his paper 'On the equilibrium of elastic solids', Maxwell recorded the results of 'some rough experiments on the twisting and bending of cylindric rods', iron and brass wires and glass rods; see Volume I: 179–83. These experiments were continued in July 1850; see Volume I: 193–4.

(18) In his personal copy of 'On the equilibrium of elastic solids' reprinted from *Trans. Roy. Soc. Edinb.*, **20** (1850): 87–120 (Cavendish Laboratory, Cambridge), Maxwell recorded 'Results of Experiments July 1850' on the bending of iron wires. He subsequently appended the values for σ mentioned here to this record of his July 1850 experiments. These values were cited by Everett ('Professor J. Clerk Maxwell, by experiments in 1850, glass .332, iron .267') in his 'Account of experiments on the flexural and torsional rigidity of a glass rod': 191.

LETTER TO PETER GUTHRIE TAIT

4 APRIL 1866

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
1866 April 4

Dear Tait

I have not access to Legendre[2] here. Could you get for me or ask one of your students to get for me the following values of log F_c or

$$\int_0^{\frac{\pi}{2}} \frac{d\psi}{\sqrt{1-\sin^2\phi\sin^2\psi}}^{(3)} \text{ for these values of } \phi.$$

	Log F_c
$\phi = 30°$	0.226793
32°	
34°	
36°	
38°	
40°	0.252068
42°	

I have put down the values for 30° & 40° that there may be no mistake as to the table of F_c. It is the 1st or 2nd table of Complete Functions.[4] I have got all the values required except those which occur in a place I want more accuracy.[5]

---

(1) ULC Add. MSS 7655, I, b/5. Previously published in *Molecules and Gases*: 413–14.

(2) A. M. Legendre, *Traité des Fonctions Elliptiques et des Intégrales Eulériennes, Avec des Tables pour en faciliter le calcul numérique*, 2 vols. (Paris, 1825–6).

(3) In Legendre's classification of elliptic integrals, this is an integral of the first kind (the simplest of the elliptic transcendentals), of the form $F_c = \int(1/\Delta)\,d\psi$, where $\Delta = \sqrt{1-c^2\sin^2\psi}$ and $0 < c < 1$; see Legendre, *Fonctions Elliptiques*, **1**: 14–18. The modulus of the elliptic integral $c = \sin\phi$.

(4) This is the first table 'contenant les logarithmes des fonctions complètes'; see Legendre, *Fonctions Elliptiques*, **2**: 227–30. In his reply (see note (15)) Tait copied out the requested values.

(5) Maxwell required the values of the elliptic integrals to calculate the paths described by molecules about a centre of force repelling inversely as the fifth power of the distance. See J. C. Maxwell, 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88, on 60–1 (= *Scientific Papers*, **2**: 41–2). Assuming that the molecules originally moved with equal velocities

I hope to hear soon of you and Thomson coming out.(6) If you do not come out soon I shall not be able to tickle the Questionists next Jan^{y}(7) with the Scotch School in a lawful manner. I suppose you know that Laplace's Coeff^{ts}(8) and Fig ⊕(9) are lawful. If you were out I could set things that I can only set now with ten lines of explanation or problem upon problem, that is if Thomson quotes correct in his papers.(10)

The dynamical theory of Viscosity of gases, conduction of Heat in d^{o} and interdiffusion of d^{o} with absolute measures of most things will soon be out. That is what F_c is for.(11)

Stewart is buzzing away with the Chimæra(12) at Kew.(13) He should have a sulphuric acid vacuum gauge such as you and Andrews used(14) to observe

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in parallel paths, he computes the way in which their deflections depend on the distance of the path from the centre of force. See Number 259 §3.

(6) W. Thomson and P. G. Tait, *Treatise on Natural Philosophy* (Oxford, 1867). But see Tait's reply (note (15)).

(7) Maxwell had been appointed Examiner for the Cambridge Mathematical Tripos. See *The Cambridge University Calendar for the Year 1867* (Cambridge, 1867): 459. The term 'questionists' was the 'appellation of the students during the last six weeks of preparation' for the examination; see Volume I: 330.

(8) Spherical harmonics; see note (10). For a standard contemporary account see J. H. Pratt, *The Mechanical Principles of Mechanical Philosophy, and their Application to the Theory of Universal Gravitation* (Cambridge, $_2$1845): 159–75.

(9) Figure of the earth; see Thomson and Tait, *Natural Philosophy*: 399, and note (10).

(10) William Thomson made repeated reference to 'Thomson and Tait's *Natural Philosophy*, chap. i, Appendix B' (the treatment of spherical harmonic analysis in *Natural Philosophy*: 140–60) in his paper 'Dynamical problems regarding elastic spheroidal shells and spheroids of incompressible liquid', *Phil. Trans.*, **153** (1863): 583–616, esp. 585–6 (= *Math. & Phys. Papers*, **3**: 351–94); 'we shall call any homogeneous function of (x, y, z) which satisfies the equation $\nabla^2 V = 0$ a "spherical harmonic function", or more shortly, a "spherical harmonic"... spherical surface-harmonics of integral order, have been generally called "Laplace's coefficients" by English writers.' On the application of these methods to the problem of the rigidity of the earth see 'Dynamical problems regarding elastic spheroidal shells': 606–10; and Thomson, 'On the rigidity of the earth', *Phil. Trans.*, **153** (1863): 573–82. See Number 277 note (5).

(11) See note (5).

(12) Balfour Stewart and P. G. Tait, 'On the heating of a disc by rapid rotation *in vacuo*', *Proc. Roy. Soc.*, **15** (1866): 290–9. On the term 'chimæra' see Number 260 note (15). On Stewart and Tait's experiments see Numbers 244 note (4) and 245 note (7).

(13) Balfour Stewart was Director of the Kew Observatory (*DNB*).

(14) Thomas Andrews and P. G. Tait, 'On the volumetric relations of ozone, and the action of the electric discharge on oxygen and other gases', *Phil. Trans.*, **150** (1860): 113–31, esp. 114–15 and Plate III, where they describe a vacuum gauge to determine minute changes in the volume of a gas.

changes of pressure in the rarefied air (indicating changes of temp. due to friction of air) which will be proportionally greater as the air is rarer.

Yours truly,
J. CLERK MAXWELL(15)

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(15) In his reply of 6 April 1866 (ULC Add. MSS 7655, I, a/2) Tait listed the requested values from Legendre's first table of elliptic integrals.

'Coll. Library Edin^{h} 6/4/66

					For 0°.1					
ϕ	Log F'				Diff I			II		III
30°	0.226	793	259	758	211	349	731	796	387	1060
32	0.231	172	806	867	227	486	050	818	789	1188
34	0.235	879	485	458	244	095	463	843	883	1332
36	0.240	923	287	876	261	235	001	872	015	1492
38	0.246	315	415	669	278	969	140	903	590	1677
40	0.252	068	441	749	297	371	255	939	099	1886
42	0.258	196	504	876	316	525	426	979	129	2133

⌊Splendid exercise in interpolation. When are we (if ever) to see you in Edinburgh.⌋ / Dear Maxwell, / There they are, as large as life. Legendre gives them for every tenth of a degree so you may get any amount more if you want them. Thomson & I will be out certainly in May – & you will find ample materials to justify you in giving P_i & Fig ⊕. But perhaps you would like a few sheets now – If so, say so. / I'll take Stewart to task about the Chimæra next week – He is here, but too busy at present. / Yours truly / P. G. Tait.' The letter is annotated by Maxwell on the *verso* with some preliminary calculations relating to his computation of the paths of particles; see note (5).

ABSTRACT OF PAPER 'ON THE DYNAMICAL THEORY OF GASES'

[16 MAY 1866][1]

From the *Proceedings of the Royal Society*[2]

ON THE DYNAMICAL THEORY OF GASES[3]

By J. Clerk Maxwell, F.R.S. L. & E.

Received May 16, 1866

(Abstract)

Gases in this theory are supposed to consist of molecules in motion, acting on one another with forces which are insensible, except at distances which are small in comparison with the average distance of the molecules. The path of each molecule is therefore sensibly rectilinear, except when two molecules come within a certain distance of each other, in which case the direction of motion is rapidly changed, and the path becomes again sensibly rectilinear as soon as the molecules have separated beyond the distance of mutual action.

Each molecule is supposed to be a small body consisting in general of parts capable of being set into various kinds of motion relative to each other, such as rotation, oscillation, or vibration, the amount of energy existing in this form bearing a certain relation to that which exists in the form of the agitation of the molecules among each other.

The mass of a molecule is different in different gases, but in the same gas all the molecules are equal.

The pressure of the gas is on this theory due to the impact of the molecules on the sides of the vessel, and the temperature of the gas depends on the velocity of the molecules.

The theory as thus stated is that which has been conceived, with various degrees of clearness, by D. Bernoulli,[4] Le Sage and Prevost,[5]

(1) The date the paper was received by the Royal Society. The paper was read on 31 May 1866: see note (2).

(2) *Proc. Roy. Soc.*, **15** (1866): 167–71.

(3) Published in *Phil. Trans.*, **157** (1867): 49–88 (= *Scientific Papers*, **2**: 26–78). There is a referee's report by William Thomson: see his letter to Stokes of 13 October 1866 (Royal Society, *Referees' Reports*, **6**: 179; printed in Wilson, *Stokes–Kelvin Correspondence*, **1**: 327–30; and see Number 266 notes (4), (6) and (8)).

(4) Daniel Bernoulli, *Hydrodynamica* (1738); see Number 257 note (4).

(5) G. L. Le Sage, 'Lucrèce Newtonien' (1782), and *Deux Traités de Physique Mécanique* (1818) publiés par Pierre Prevost; see Number 257 note (5).

Herapath,[6] Joule,[7] and Krönig,[8] and which owes its principal developments to Professor Clausius.[9] The action of the molecules on each other has been generally assimilated to that of hard elastic bodies, and I have given some application of this form of the theory to the phenomena of viscosity, diffusion, and conduction of heat in the Philosophical Magazine for 1860.[10] M. Clausius has since pointed out several errors in the part relating to conduction of heat,[11] and the part relating to diffusion also contains errors.[12] The dynamical theory of viscosity in this form has been re-investigated by M. O. E. Meyer, whose experimental researches on the viscosity of fluids have been very extensive.[13]

In the present paper the action between the molecules is supposed to be that of bodies repelling each other at a distance, rather than of hard elastic bodies acting by impact; and the law of force is deduced from experiments on the viscosity of gases to be that of the inverse fifth power of the distance, any other law of force being at variance with the observed fact that the viscosity is proportional to the absolute temperature. In the mathematical application of the theory, it appears that the assumption of this law of force leads to a great simplification of the results, so that the whole subject can be treated in a more general way than has hitherto been done.

I have therefore begun by considering, first, the mutual action of two molecules; next that of two systems of molecules, the motion of all the molecules in each system being originally the same. In this way I have

(6) John Herapath, *Mathematical Physics; or the Mathematical Principles of Natural Philosophy: with a Development of the Causes of Heat, Gaseous Elasticity, Gravitation, and other Great Phenomena of Nature*, 2 vols. (London, 1847). Maxwell had been familiar with Herapath's book in 1859; see Volume I: 610–11. Garber, Brush and Everitt, *Molecules and Gases*: 80–1, publish a letter of Herapath's dated 23 February 1864, which they believe to be addressed to Maxwell.

(7) James Prescott Joule, 'Some remarks on heat, and the constitution of elastic fluids', *Memoirs of the Literary and Philosophical Society of Manchester*, **9** (1851): 107–14 (read 3 October 1848); and in *Phil. Mag.*, ser. 4, **14** (1857): 211–16.

(8) A. Krönig, 'Grundzuge einer Theorie der Gase', *Ann. Phys.*, **99** (1856): 315–22.

(9) R. Clausius, 'Ueber die Art der Bewegung welche wir Wärme nennen', *Ann. Phys.*, **100** (1857): 353–80; Clausius, 'Ueber die mittlere Länge der Wege...', *ibid.*, **105** (1858): 239–58.

(10) J. C. Maxwell, 'Illustrations of the dynamical theory of gases', *Phil. Mag.*, ser. 4, **19** (1860): 19–32; *ibid.*, **20** (1860): 21–37 (= *Scientific Papers*, **1**: 377–409).

(11) R. Clausius, 'Ueber die Wärmeleitung gasförmiger Körper', *Ann. Phys.*, **115** (1862): 1–56; see Number 207, esp. notes (9) and (39).

(12) See Maxwell's discussion of diffusion in his letter to William Thomson of 27 February 1866 (Number 260).

(13) O. E. Meyer, 'Ueber die innere Reibung der Gase', *Ann. Phys.*, **125** (1865): 177–209, 401–20, 564–99.

determined the rate of variation of the mean values of the following functions of the velocity of molecules of the first system –

α, the resolved part of the velocity in a given direction.
β, the square of this resolved velocity.
γ, the resolved velocity multiplied by the square of the whole velocity.

It is afterwards shown that the velocity of translation of the gas depends on α, the pressure on β, and the conduction of heat on γ.

The final distribution of velocities among the molecules is then considered, and it is shown that they are distributed according to the same law as the errors are distributed among the observations in the theory of 'Least Squares', and that if several systems of molecules act on one another, the average *vis viva* of each molecule is the same, whatever be the mass of the molecule. The demonstration is of a more strict kind than that which I formerly gave, and this is the more necessary, as the 'Law of Equivalent Volumes', so important in the chemistry of gases, is deduced from it.

The rate of variation of the quantities α, β, γ in an element of the gas is then considered, and the following conclusions are arrived at.

(α) 1st. In a mixture of gases left to itself for a sufficient time under the action of gravity, the density of each gas at any point will be the same as if the other gases had not been present.

2nd. When this condition is not fulfilled, the gases will pass through each other by diffusion. When the composition of the mixed gases varies slowly from one point to another, the velocity of each gas will be so small that the effects due to inertia may be neglected. In the quiet diffusion of two gases, the volume of either gas diffused through unit of area in unit of time is equal to the rate of diminution of pressure of that gas as we pass in the direction of the normal to the plane, multiplied by a certain coefficient, called the coefficient of interdiffusion of these two gases. This coefficient must be determined experimentally for each pair of gases. It varies directly as the square of the absolute temperature, and inversely as the total pressure of the mixture. Its value for carbonic acid and air, as deduced from experiments given by Mr. Graham in his paper on the Mobility of Gases,*(14) is

$$D = 0.0235,^{(15)}$$

the inch, the grain, and the second being units. Since, however, air is itself a

* Philosophical Transactions. 1863.(14)

(14) Thomas Graham, 'On the molecular mobility of gases', *Phil. Trans.*, **153** (1863): 385–405, esp. 404–5 for experiments on the interdiffusion of carbon dioxide and air.

(15) See Number 260 note (13).

mixture, this result cannot be considered as final, and we have no experiments from which the coefficient of interdiffusion of two pure gases can be found.

3rd. When two gases are separated by a thin plate containing a small hole, the rate at which the composition of the mixture varies in and near the hole will depend on the thickness of the plate and the size of the hole. As the thickness of the plate and the diameter of the hole are diminished, the rate of variation will increase, and the effect of the mutual action of the molecules of the gases in impeding each other's motion will diminish relatively to the moving force due to the variation of pressure. In the limit, when the dimensions of the hole are indefinitely small, the velocity of either gas will be the same as if the other gas were absent. Hence the volumes diffused under equal pressures will be inversely as the square roots of the specific gravities of the gases, as was first established by Graham†;[16] and the quantity of a gas which passes through a thin plug into another gas will be nearly the same as that which passes into a vacuum in the same time.

(β) By considering the variation of the total energy of motion of the molecules, it is shown that,

1st. In a mixture of two gases the mean energy of translation will become the same for a molecule of either gas. From this follows the law of Equivalent Volumes, discovered by Gay-Lussac from chemical considerations; namely, that equal volumes of two gases at equal pressures and temperatures contain equal numbers of molecules.[17]

2nd. The law of cooling by expansion is determined.

3rd. The specific heats at constant volume and at constant pressure are determined and compared. This is done merely to determine the value of a constant in the dynamical theory for the agreement between theory and experiment with respect to the values of the two specific heats, and their ratio is a consequence of the general theory of thermodynamics, and does not depend on the mechanical theory which we adopt.[18]

4th. In quiet diffusion the heat produced by the interpenetration of the gases is exactly neutralized by the cooling of each gas as it passes from a dense to a rare state in its progress through the mixture.

5th. By considering the variation of the difference of pressures in different directions, the coefficient of viscosity or internal friction is determined, and the equations of motion of the gas are formed. These are of the same form as

† 'On the Law of the Diffusion of Gases', Transactions of the Royal Society of Edinburgh, vol. xii. (1831).[16]

(16) Thomas Graham, 'On the law of the diffusion of gases', *Trans. Roy. Soc. Edinb.*, **12** (1832): 222–58.

(17) On Gay-Lussac and 'Avogadro's hypothesis' see Number 259 §4 and note (13).

(18) See Number 259 §5.

those obtained by Poisson(19) by conceiving an elastic solid the strain on which is continually relaxed at a rate proportional to the strain itself.

As an illustration of this view of the theory, it is shown that any strain existing in air at rest would diminish according to the values of an exponential term the modulus of which is $\frac{1}{5,100,000,000}$ second, an excessively small time, so that the equations are applicable, even to the case of the most acute audible sounds, without any modification on account of the rapid change of motion.

This relaxation is due to the mutual deflection of the molecules from their paths. It is then shown that if the displacements are instantaneous, so that no time is allowed for the relaxation, the gas would have an elasticity of form, or 'rigidity', whose coefficient is equal to the pressure.

It is also shown that if the molecules were mere points, not having any mutual action, there would be no such relaxation, and that the equations of motion would be those of an elastic solid, in which the coefficient of cubical and linear elasticity have the same ratio as that deduced by Poisson from the theory of molecules at rest acting by central forces on one another.(20) This coincidence of the results of two theories so opposite in their assumptions is remarkable.

6th. The coefficient of viscosity of a mixture of two gases is then deduced from the viscosity of the pure gases, and the coefficient of inter-diffusion of the two gases. The latter quantity has not as yet been ascertained for any pair of pure gases, but it is shown that sufficiently probable values may be assumed(21) which being inserted in the formula agree very well with some of the most remarkable of Mr. Graham's experiments on the Transpiration of Mixed Gases.*(22) The remarkable experimental result that the viscosity is independent of the pressure and proportional to the absolute temperature is a necessary consequence of the theory.

(γ) The rate of conduction of heat is next determined, and it is shown

1st. That the final state of a quantity of gas in a vessel will be such that the temperature will increase according to a certain law from the bottom to the top.(23) The atmosphere, as we know, is colder above. This state would be

* Philosophical Transactions, 1846.(22)

(19) See Number 259 §6, esp. note (23).

(20) See 'On the dynamical theory of gases': 81–2 (= *Scientific Papers*, **2**: 69–70); and G. G. Stokes, 'On the theories of the internal friction of fluids in motion, and of the equilibrium and motion of elastic solids', *Trans. Camb. Phil. Soc.*, **8** (1845): 287–319, esp. 311, equation (29) (= *Papers*, **1**: 75–129). See Number 261 notes (7), (8) and (9).

(21) See note (14).

(22) Thomas Graham, 'On the motion of gases', *Phil. Trans.*, **136** (1846): 573–632.

(23) On Maxwell's derivation of this result see Number 259 note (33); and for its subsequent correction see Number 266 esp. note (8). For his original conclusion that the temperature decreases as the height increases see Numbers 259 §9 and 260.

produced by winds alone, and is no doubt greatly increased by the effects of radiation. A perfectly calm and sunless atmosphere would be coldest below.

2nd. The conductivity of a gas for heat is then deduced from its viscosity, and found to be

$$\frac{5}{3}\frac{1}{\gamma-1}\frac{p_0}{\rho_0\,\theta_0}\frac{\mu}{S}\text{[24]};$$

where γ is the ratio of the two specific heats, p_0 the pressure, and ρ_0 the density of the standard gas at absolute temperature θ_0. S the specific gravity of the gas in question, and μ its viscosity. The conductivity is, like the viscosity, independent of the pressure and proportional to the absolute temperature. Its value for air is about 3500 times less than that of wrought iron, as determined by Principal Forbes.[25] Specific gravity is .0069.

For oxygen, nitrogen, and carbonic oxide, the theory gives the conductivity equal to that of air. Hydrogen according to the theory should have a conductivity seven times that of air, and carbonic acid about $\frac{7}{9}$ of air.

(24) Boltzmann subsequently discovered an error in Maxwell's calculation: the factor $\frac{5}{3}$ should be corrected to $\frac{5}{2}$. See Ludwig Boltzmann, 'Weitere Studien über das Wärmegleichgewicht unter Gasmolekülen', *Wiener Berichte*, **66**, Abtheilung II (1872): 275–370, esp. 332. See Number 425, esp. note (5).

(25) J. D. Forbes, 'Experimental inquiry into the laws of the conduction of heat in bars, and into the conducting power of wrought iron', *Trans. Roy. Soc. Edinb.*, **23** (1862): 133–46, esp. 145.

REPORT ON A PAPER BY THOMAS GRAHAM ON THE ABSORPTION AND SEPARATION OF GASES

17 JULY 1866

From the original in the Library of the Royal Society, London[1]

REPORT TO THE COMMITTEE OF PAPERS ON M^r GRAHAM'S PAPER ON THE ABSORPTION AND DIALYTIC SEPARATION OF GASES BY COLLOID SEPTA. PARTS I & II[2]

M^r Graham has on former occasions investigated the resistance opposed to the motion of gases by porous septa[3] and by capillary tubes.[4] He has also investigated the passage of liquids through the substance of that class of bodies to which he has applied the epithet colloid.[5] In the present paper he considers the apparent passage of gases through septa which are not known to be porous, such as Caoutchouc[6] Platinum & Iron.[7]

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(1) Royal Society, *Referees' Reports*, **6**: 138.

(2) Thomas Graham, 'On the absorption and dialytic separation of gases by colloid septa', *Phil. Trans.*, **156** (1866): 399–439. The paper was received by the Royal Society on 20 June 1866, and read on 21 June 1866; see the abstract in *Proc. Roy. Soc.*, **15** (1866): 223–4. In his paper on 'Liquid diffusion applied to analysis', *Phil. Trans.*, **151** (1861): 183–224, esp. 183, 186, Graham introduced the terms 'dialysis' and 'colloid'. Distinguishing 'colloids' from 'crystalloids', he noted that the 'softness of the gelatinous colloid partakes of fluidity', and that a 'colloid' was 'a medium for liquid diffusion'. He defined 'dialysis' as 'the method of separating by diffusion through a septum of gelatinous matter'. Thus, as he explained in 'On the absorption and dialytic separation of gases by colloid septa': 399, 'dialysis involves the passage of a substance through a septum composed of soft colloid matter, such as must be wholly destitute of open channels, and therefore be impermeable to gas as such'.

(3) Thomas Graham, 'On the molecular mobility of gases', *Phil. Trans.*, **153** (1863): 385–405.

(4) Thomas Graham, 'On the motion of gases', *Phil. Trans.*, **136** (1846): 573–632; *ibid.*, **139** (1849): 349–401.

(5) Graham, 'Liquid diffusion applied to analysis'; see note (2).

(6) See 'Part I – Action of a septum of caoutchouc' of Graham's 'On the absorption and dialytic separation of gases by colloid septa': 399–415, esp. 404, where Graham noted that 'a film of rubber appears to have no porosity, and to resemble a film of liquid in its relation to gases', and that 'liquids and colloids have an unbroken texture'.

(7) See 'Part II – Action of metallic septa at red heat' of Graham's 'On the absorption and dialytic separation of gases by colloid septa': 415–39, esp. 415, where he discussed the recent 'surprising passage of gases through the homogeneous substance of a plate of fused platinum or of iron at a red heat', discovered by H. Sainte-Claire Deville and L. Troost, 'Sur la perméabilité du fer à haute température', *Comptes Rendus*, **57** (1863): 965–7.

Mr Graham in this paper considers the passage of the gas from the one side of the septum to the other to be the result of a process more allied to that of liquid diffusion than to that of gaseous diffusion, he shows that the substances of which the septa are composed have the power of absorbing the gases which are observed to pass through them, and of giving out these gases under a reduced pressure or a higher temperature, and he considers that the physical state of the gas when absorbed is that of a liquid.

The connexion between the 'penetrativeness' of a gas[(8)] and its capacity of being absorbed by the substance through which it penetrates, together with its loss of the gaseous state while within the substance of the septum, are the theoretical principles brought out in the investigation, in addition to several new methods of procedure, many new facts, and a number of practical suggestions.

The relative velocity of passage of gases through porous septa has been shown by Mr Graham to depend entirely on the specific gravity of the gases and not on their chemical relation to the substance of the septum.[(9)] The flow seems to be regulated by strictly mechanical principles as much as that of gases through small holes. In the cases now discussed the relative velocity of the gases is not that of their specific gravities or of their viscosities but has reference to the relation of each gas to the particular septum employed.

If we assume that a molecule is perfectly free when in the gaseous state, then if we find it so far connected with another substance as to be affected by the chemical nature of that substance, we may conclude that the molecule is not in the gaseous state.

In the case of a gas absorbed by a liquid the whole is manifestly liquid, but when the absorbing substance is solid we may suppose either that the gas has entered into its pores and has been there condensed, or that the gas has been taken into the substance of the body. If the body is such that a portion highly charged with the absorbed substance parts with it to portions having a smaller charge, then the absorption will take place not only at the surface but throughout the body, and if at the opposite boundary of the body the conditions are favourable to the escape of the absorbed substance it will pass through the body.

Now a colloid body, according to Mr Graham, is one which can take up a little more or less of certain substances into itself independently of any true mechanical or geometrical porosity.

The explanation given by M. Deville of the passage of gases through iron

(8) Graham's term; see 'On the absorption and dialytic separation of gases by colloid septa': 402.

(9) Graham, 'On the molecular mobility of gases'.

and platinum[10] is apparently inconsistent with that given by Mr Graham. M. Devilles explanation is mechanical. The metals are expanded by heat till those gases which have the smallest molecules can penetrate the intermolecular spaces. If the order of penetration of the gases is different in different metals it must be due to the relation between the shape of the intermolecular pores and that of the molecules of each particular gas.

Mr Grahams explanation if I understand it right leads him to look for the phenomenon not among 'crystallised masses'[11] of metals having planes of cleavage &c but among amorphous masses whose molecules may interchange place without any breach of continuity.[12] When a portion of such a mass is charged with a substance capable of being absorbed by it, then there is such an interchange of molecules of that substance as to diffuse it through the mass. This diffusion however may not be possible except at a high temperature, which by increasing the excursions of and the velocity of the molecules, enables them to pass from one part of the mass to another.

This view of the matter leads to enquiries as to the relation between colloids and the absorbed gases within them, but tells us nothing about the absolute distances of the molecules of solids.

I consider Mr Graham's paper an important contribution to our knowledge of the physical states of matter, as well as to that of the properties of particular gases and solids, and that it ought to be printed in the Philosophical Transactions.

The knowledge of the penetration of iron by carbonic oxide may be useful to those who wish to improve the manufacture of steel[13] and the complete separation of hydrogen from other gases by a septum of heated platinum may afford the means of making experiments on the mechanical equivalent of chemical affinity, if we can overcome the affinity of hydrogen for other substances by inequality of pressure at a higher temperature.

James Clerk Maxwell
17 July 1866

(10) H. Sainte-Claire Deville, 'Note sur le passage des gaz au travers des corps solides homogènes', *Comptes Rendus*, **59** (1864): 102–7.

(11) Sainte-Claire Deville's term 'masses cristallisées' ('Note sur le passage des gaz': 102), as cited by Graham, 'On the absorption and dialytic separation of gases by colloid septa': 416.

(12) See Graham's distinction between 'colloids' and 'crystalloids' (note (2)).

(13) A point also made by William Allen Miller in his brief report on Graham's paper dated 4 July 1866 (Royal Society, *Referees' Reports*, **6**: 137): 'iron absorbs not only hydrogen but takes up a notable proportion of carbonic oxide which may have an important influence in the process of the conversion of iron into steel during cementation.'

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REPORT ON A PAPER BY EDWARD WYNDHAM TARN[1] ON THE STABILITY OF DOMES

18 DECEMBER 1866

From the original in the Library of the Royal Society, London[2]

REPORT ON A PAPER ON THE STABILITY OF DOMES BY E. W. TARN PART II[3]

In reporting on the second part of this paper I am obliged to make a few remarks on the paper as a whole,[4] and on the subject of Domes.

Mr Tarn has considered the equilibrium of arches, differing from common arches in being bounded laterally by two planes, passing through the crown of the arch, and inclined at an angle of 2°.[5] If a number of these were placed symmetrically about an axis, but not in contact they would form a crown, such as surmounts certain towers and steeples. A dome differs from a crown in that the component segments are in contact, and therefore are capable of exerting a lateral pressure which ought to be considered in treating of domes. I have not been able to find in any part of Mr Tarns paper any mention of this lateral pressure. He rightly says that 'domes of various sizes and forms have been erected for centuries past'[6] so that the *form* of the dome is in some measure indeterminate, but he does not give the reason why the form of the dome is not so determinate as that of the arch. Arches of various forms have been erected, but their stability has depended on the fact that their ribs had a finite depth, and that they were supported by masonry outside, but a dome, indefinitely thin, will be stable whatever be its form provided certain conditions be fulfilled. This arises from the lateral support given by the pressure of the sectors on each other in the direction of horizontal tangents to the dome, and the condition of stability of a dome of masonry is that this

(1) University College London 1843, Associate of the Royal Institute of British Architects 1855 (Boase).

(2) Royal Society, *Referees' Reports*, **6**: 276.

(3) E. Wyndham Tarn, 'On the stability of domes. – Part II' (Royal Society, AP. 48.8). The paper was received by the Royal Society on 23 October 1866, and read on 22 November 1866; see the abstract in *Proc. Roy. Soc.*, **15** (1866): 266–8.

(4) The first part of Tarn's paper 'On the stability of domes' had been published in *Proc. Roy. Soc.*, **15** (1866): 182–8; the paper was received on 5 May 1866, and read on 31 May 1866. While the first part of the paper is concerned with spherical domes, the second part considered domes of other forms than the spherical.

(5) This angle is denoted ϕ by Tarn.

(6) Tarn, 'On the stability of domes': 182.

pressure shall not be negative (that is a tension) which would throw the stress on the cement or the bond of the masonry. This condition with its consequence is correctly stated in Prof. Rankines Applied Mechanics Art 234 p 265[(7)] and may be thus stated.

Let W be the weight of a part of the dome above the circular joint at which the inclination of the surface of the dome to the horizon is θ and let r be the mean radius of the joint, P the pressure in the meridian and Q the lateral pressure N the length of the normal and R the radius of curvature of the meridian section.

Then
$$2\pi Pr\sin\theta = W$$
$$\frac{P}{R}+\frac{Q}{N}=\frac{dW}{d\theta}\frac{\cos\theta}{2\pi Rr}$$
whence
$$Q=\frac{1}{2\pi R}\frac{d}{d\theta}\cdot(W\cot\theta).$$

If $W\cot\theta$ always increases with θ, Q will be positive and the dome will stand if Q be negative the dome will split unless held together by a belt by cement or the friction of overlapping stones.

This is the theory of a dome indefinitely thin, a thick shell is more stable.

I think the author should have said something about Q, the lateral thrust since it is the feature which distinguishes domes from arches, and is most important in the mechanical theory. We do not see, from the paper, that it is possible to build a dome with a round hole at the top.

In the second part, Equation A, page 4, is not correct. It should be $P=\delta\iiint r(r\sin\overline{\alpha+\theta}-OZ)\,dr\,d\theta\,d\phi$.[(8)] The error introduced is not large. He also states that the ordinary rules for finding maxima will not apply to the case of the quantity N'.[(9)] I think the rules apply well enough, though the operation might be found tedious.

The mathematics, on the whole, is superior to the mechanics, especially in the paraboloidal dome. In the Ogival dome, p. 29,[(10)] the upper part would

(7) W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 265–6.

(8) P is the weight of the portion of rib above a given joint; δ the weight of a cubic foot of the material of the dome; r and R the internal and external radii of the dome; the angles α and θ corresponding to various shapes of 'Gothic' dome; OZ the distance between the centre of the arc of the rib and its vertical axis; and ϕ as stated above (note (5)). Tarn's expression reads: $P=\delta\iiint r^2(\sin(\alpha+\theta)-\sin\alpha)\,dr\,d\theta\,d\phi$; and see also 'On the stability of domes': 183 (for spherical domes).

(9) Maxwell here is referring to Tarn's discussion in 'On the stability of domes': 184.

(10) Tarn, 'On the stability of domes. – Part II': f. 29; 'There is another form of Dome commonly used in Eastern countries...sometimes called the "Ogival" dome, the contour being a curve which has a point of "contrary flexure"'.

not stand at all on the authors principle of neglecting the lateral thrust. If such an arch were built of this form, it would fall at once. It is only on account of the lateral thrust that the upper part of this dome can stand.

On the whole I think that a good deal of mathematical power has been expended on this paper with small mechanical effect, as the author has never alluded to the true reason why domes stand when built, though it is stated in various books. I think the Society should not be responsible for the printing of the paper at large in the Proceedings,(11) unless the author takes the true theory into account, (which, from the style of this paper, I have no doubt he could do,) or else gives reasons why he should do without it.

James Clerk Maxwell
1866 Dec 18

(11) Part II of Tarn's paper was only printed in abstract: see note (3).

LETTER TO GEORGE GABRIEL STOKES

18 DECEMBER 1866

From the original in the University Library, Cambridge[1]

8 Palace Gardens Terrace
W
1866 Dec 18

Dear Stokes

I enclose a report on M^r Tarn's paper[2] and Thomson's report on mine.[3] Thomsons theory of matter being continuous but much more dense at one place than another is a molecular theory, for the dense portions constitute a finite number of molecules and I do not assert nor does any one since Lucretius that the space between the molecules is absolutely empty.[4]

I take Statics to be the theory of systems of forces without considering what they act on

Kinetics to be the theory of the motions of systems without regard to the forces in action

Dynamics to be the theory of the motion of bodies as the result of given forces

and Energetics to be the theory of the communication of energy from one body to another.

I therefore call the theory a dynamical theory because it considers the motions of bodies as produced by certain forces.[5]

I think I have stated plainly enough the difference between my molecules and pure centres of force.[6] I do not profess to have a dynamical theory of the molecule itself.

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(1) ULC Add. MSS 7656, M 424. First published in Larmor, *Correspondence*, **2**: 27–8.

(2) Number 265.

(3) William Thomson's letter to Stokes of 13 October 1866 (see Number 263 note (3)) is a report on Maxwell's paper 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88 (= *Scientific Papers*, **2**: 26–78).

(4) In his report on Maxwell's paper (see note (3)) Thomson had questioned the word 'homogeneous' in Maxwell's initial assertion that 'Theories of the constitution of bodies suppose them either to be continuous and homogeneous, or to be composed of a finite number of distinct particles or molecules'; see 'On the dynamical theory of gases': 49 (= *Scientific Papers*, **2**: 26). Thomson suggested 'all space to be full but the properties of known bodies to be due to...vast variations of density from point to point' (Wilson, *Stokes–Kelvin Correspondence*, **1**: 330).

(5) On Maxwell's use of the terms 'kinetics' and 'dynamics' compare Volume I: 665–6 and Number 377 esp. note (4).

(6) See Maxwell's statement in 'On the dynamical theory of gases': 54–5 (= *Scientific Papers*,

I have considered the equilibrium of heat in a vertical column and find I made a mistake in eq^n 143.(7) I now make the temperature the same throughout, and have inserted (on two pages) an addition to that effect which I mean to be instead of p. 60.(8)

I have also amended equation (54) in which the effect of external forces was omitted by mistake in writing out and copying from eq^n 47.(9)

The result is a corroboration of the theory of the distribution of velocities as the errors are in the theory of 'Least Squares' for we require to have

$$\overline{\xi^4} = 3\,\overline{\xi^2}\,.\,\overline{\xi^2},$$

which agrees with that theory.(10)

I think something might be done to the statical theory of elasticity with centres of force whose displacement is a function of the index of the molecule

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2: 33); 'The molecules of a gas in this theory are those portions of it which move about as a single body. These molecules may be mere points, or pure centres of force endowed with inertia, or the capacity of performing work while losing velocity. They may be systems of several such centres of force, bound together by their mutual actions'. Thomson had commented: 'It should...be explained...that the molecules are regarded *not* as centres of force but as really (according with their name) little heaps of matter' (Wilson, *Stokes–Kelvin Correspondence*, **1**: 328).

(7) Maxwell, 'On the dynamical theory of gases': 85–6 (= *Scientific Papers*, **2** : 74–5).

(8) See the 'Addition made December 17, 1866' on the 'Final equilibrium of temperature' appended to 'On the dynamical theory of gases': 86–7 (= *Scientific Papers*, **2**: 75–6). On Maxwell's original conclusion that the temperature of a column of gas diminishes as the height increases, see Numbers 259 §9 and 260; and for his revision, concluding that the temperature would increase with the height (as stated in the paper as submitted to the Royal Society in May 1866), see Numbers 259 note (33) and 263. Thomson discussed the problem in his report on Maxwell's paper, acknowledging that 'What the flaw may be in Maxwell's investigation if any I have not been able to see.' (Wilson, *Stokes–Kelvin Correspondence*, **1**: 329). In the 'Addition' to his paper Maxwell concluded that: 'if the temperature of any substance, when in thermic equilibrium is a function of the height, that of any other substance must be the same function of the height. For if not, let equal columns of the two substances be enclosed in cylinders impermeable to heat, and put in thermal communication at the bottom. If, when in thermal equilibrium, the tops of the two columns are at different temperatures, an engine might be worked by taking heat from the hotter and giving it to the cooler, and the refuse heat would circulate round the system till it was all converted into mechanical energy, which is in contradiction to the second law of thermodynamics.'

(9) Maxwell, 'On the dynamical theory of gases': 67 (= *Scientific Papers*, **2**: 49–50).

(10) Maxwell, 'On the dynamical theory of gases': 87 (= *Scientific Papers*, **2**: 76); and see Number 259 notes (10) and (33). For Maxwell's subsequent discussion of his conclusion 'that the temperature in gases, when in thermal equilibrium, is independent of height', see Numbers 457, 472, 473 and 481. This conclusion was later challenged by Joseph Loschmidt, 'Über den Zustand des Wärmegleichgewichtes eines Systems von Körpern mit Rücksicht auf die Schwerkraft. I', *Wiener Berichte*, **73**, Abtheilung II (1876): 128–42, esp. 135–9.

as well as of its initial coordinates, the number of kinds of indices being small and the groups of n particles being originally of the same form, but it would require an investigation to itself.(11)

I remain
Yours truly
J. Clerk Maxwell

Professor Stokes

I shall be at Cambridge from 26 Dec to 26 Jan at Trinity.(12)

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(11) In his report on Maxwell's paper Thomson had alluded to molecular explanations of elasticity: 'a solid violating Poisson's condition [$\sigma = 1/4$] could be built up of small parts (molecules) each fulfilling it' (Wilson, *Stokes–Kelvin Correspondence*, **1**:330).

(12) Maxwell had been appointed Examiner for the Mathematical Tripos in 1867; see *The Cambridge University Calendar for the Year 1867* (Cambridge, 1867): 459.

LETTER TO JAMES JOSEPH SYLVESTER(1)

21 DECEMBER 1866

From the original in Columbia University Library(2)

8 Palace Gardens Terrace
London W
1866 Dec 21

Dear Sir

Some days ago my attention was called to the Cartesian Ovals by a statement of yours in the Philosophical Magazine(3) that the R. S. Laureat (Chasles,(4) not Tennyson)(5) had discovered that they had 3 foci,(6) which as you say is a remarkable bit of mathematics.(7) I tried to establish a third focus when I began mathematics about the [year] 1846 and several times afterwards.(8) My reason was that I had drawn all the kinds of them with pins and threads on paper and was struck with the visible resemblance of two kinds in one of which the

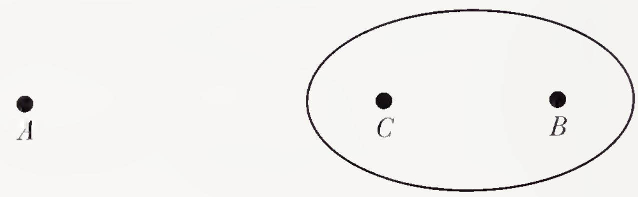

Figure 267,1

(1) It seems certain that the letter was addressed to Sylvester: see note (7).

(2) D. E. Smith Special MS Collection, Rare Book and Manuscript Library, Columbia University, New York.

(3) In the January 1866 number: see note (7).

(4) Michel Chasles had been awarded the Copley Medal of the Royal Society in 1865 'for his Historical and Original Researches in Pure Geometry'; see *Proc. Roy. Soc.*, **14** (1865): 493–6.

(5) The Poet Laureate.

(6) In 'Note XXI. Sur les ovales de Descartes, ou lignes aplanétiques' of his *Aperçu Historique sur l'Origine et le Développement des Méthodes en Géométrie* (Brussels, 1837), in *Mémoires couronnés par l'Académie Royale des Sciences et Belles-Lettres de Bruxelles*, **11** (1837): 350–3, esp. 352, Chasles had observed: 'qu'*au lieu de deux foyers seulement, elles en ont toujours trois*: c'est-à-dire, qu'outre les deux foyers qui servent à leur description, elles en ont un troisième qui joue le même rôle, avec l'un des deux premiers, que ces deux-ci-ensemble.'

(7) Sylvester had remarked on 'the knowledge of the existence of a third focus to the Cartesian ovals, that remarkable discovery of our illustrious Royal Society *Laureate* of the year...', adding that 'I am not aware that M. Chasles has ever disclosed that *aperçu* which led him to this unlooked for discovery', in his 'Astronomical prolusions: commencing with an instantaneous proof of Lambert's and Euler's theorems, and modulating through a construction of the orbit of a heavenly body from two heliocentric distances, the subtended chord, and the periodic time, and the focal theory of Cartesian ovals, into a discussion of motion in a circle and its relation to planetary motion', *Phil. Mag.*, ser. 4, **31** (1866): 52–76, esp. 61–2.

(8) But see Maxwell's manuscript paper 'Oval' (Volume I: 47–54), written in 1847, where he seems unaware that a Cartesian oval has three foci.

foci are A and B in the other A & C.[9] My other reason for which I renewed the search was to find out what Descartes means by what he says in his Geometria Lib. II p 61 where he has a figure like this[10] in which FE is a rule centred at F and the string goes from E round the pencil at C to K back to C and then is fastened at G so that

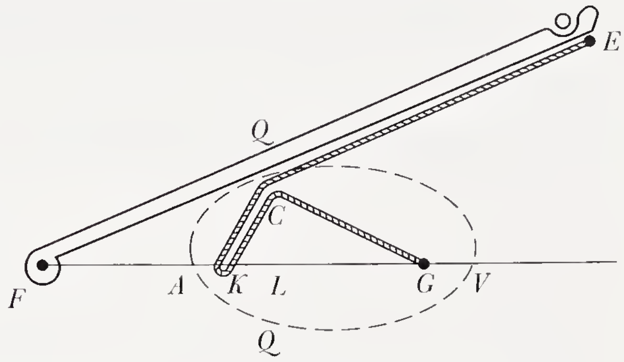

Figure 267,2

$$EC + 2CK + CG = \text{const.}$$

He makes $FA = AG$

$$FL = \mu LG^{(11)}$$

$$AK = KL$$

$$EC + 2CK + CG = GA + AL + FE - AF$$

then $FK = \dfrac{3\mu + 1}{4(\mu + 1)} FC$

and the oval is either

$$FC + \mu GC = \frac{\mu + 1}{2} FG$$

$$\text{or} \quad + FC - \frac{2\mu}{\mu + 1} KC = \frac{3\mu + 1}{2(\mu + 1)^2} FG$$

$$\text{or} \quad GC + \frac{2}{\mu + 1} KC = \frac{\mu}{2} \frac{\mu + 3}{(\mu + 1)^2} FG.$$

So that Descartes has not only found out a case of the 3rd focus but has overcome the difficulty of describing ovals with strings when the value of μ is incommensurable.[12] He has (as far as I know) lost the credit of it

(9) See Maxwell's manuscript paper 'Observations on circumscribed figures having a plurality of foci, and radii of various proportions' (Volume I: 35–42), written in February 1846.

(10) *Geometria, à Renato Des Cartes. Anno 1637 Gallicè edita; nunc autem Cum Notis Florimondi De Beaune,... In linguam Latinam versa, & commentariis illustrata, Operâ atque studio Francisci à Schooten...* (Leiden, 1649): 61.

(11) See George Salmon, *A Treatise on the Higher Plane Curves* (Dublin, 1852): 119, where 'a Cartesian oval is defined as the locus of a point whose distances from two given foci [ρ, ρ'] are connected by the relation $m\rho + n\rho' = c$.' Here $\mu = m/n$.

(12) In Maxwell's method of description of ovals in his 'Observations on circumscribed figures having a plurality of foci', a tracing pin describes an oval by moving so that m times its distance from one focus (ρ) together with n times its distance from another focus (ρ') is equal to

1st by being too short (though not obscure)
2nd . by using the words etiamsi hæ Ovales ejusdem fermé naturæ videntur, p 62(13)
3rd by erroneous statements about reflexion(14) which made men set this paragraph down as a mistake.

I suppose you know that rays from G in a medium whose index is μ falling on the part of QVQ which is beyond the tangent from F, will after refraction proceed in directions which touch the caustic produced by rays from F *reflected* at QVQ supposing the refractive medium abolished and QVQ a reflecting surface.(15)

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a constant quantity; m and n are determined by the number of times the thread is wrapped round the pins placed at the two given foci. J. D. Forbes remarked in his published 'On the description of oval curves and those having a plurality of foci. By Mr Clerk Maxwell junior; with remarks by Professor Forbes', *Proc. Roy. Soc. Edinb.*, **2** (1846): 89–91 (= *Scientific Papers*, **1**: 1–3), that 'it probably has not been suspected that so easy and elegant a method exists of describing these curves by the use of a thread and pins whenever the powers of the foci [m, n] are commensurable'. Maxwell thus recognises that the restriction that $\mu(m/n)$ is rational does not limit Descartes' description of ovals.

(13) 'Ad hæc, etiamsi hæ Ovales ejusdem fermè naturæ videntur, ipsæ nihilominus quatuor diversorum sunt generum, quorum unumquodque sub se infinita ali genera continet, & unumquodque rursus tot diversas species, quot facit Ellipsum aut Hyperbolarum genus.' ('Although these ovals seem to be of nearly the same nature, they nevertheless belong to four different classes, each containing an infinity of other classes, and each of which again so many different kinds, as does the class of ellipses or of hyperbolas.'); Descartes, *Geometria*: 62.

(14) First noticed by Maxwell in a letter to his father of April 1847 (Volume I: 62). In discussing the reflective properties of ovals Descartes supposes an oval as the concave surface of a mirror 'ex tali materiâ constaret, ut vim horum radiorum...diminuat' ('composed of such matter that the force of the rays is diminished' in a given proportion). He supposes an incident ray to be reflected at the angle with which it would be refracted were the oval supposed to be the surface of a lens: 'ut etiam reflexionum anguli non secus ac refractionum inæquales existant'; Descartes, *Geometria*: 63.

(15) In his note 'Sur les ovales de Descartes' Chasles had pointed out that 'M. Quetelet, dans sa belle théorie des *caustiques secondaires*... a trouvé que les caustiques secondaires produites par la réflexion et la réfraction dans un cercle éclairé par un point lumineux, sont les *ovales de Descartes*, ou lignes aplanétiques'; Chasles, *Aperçu Historique*: 350. See Adolphe Quetelet, 'Mémoire sur une nouvelle manière de considérer les caustiques, produites soit par réflexion soit par réfraction', *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles*, **3** (1826): 89–140; Quetelet, 'Démonstration et développemens des principes fondamentaux de la théorie des caustiques sécondaires', *ibid.*, **5** (1829); and in his 'Supplément au Traité de la lumière de Sir J. F. W. Herschel' appended to Herschel's *Traité de la Lumière*, (trans.) P. F. Verhulst and A. Quetelet, 2 vols. (Paris, 1829–33), **2**: 380–407. See Salmon, *Higher Plane Curves*: 118–19; and Arthur Cayley, 'A memoir upon caustics', *Phil. Trans.*, **147** (1857): 273–312.

Descartes Geometry is a book studied by Newton and all the mathematicians since so I daresay this proposition has been well known though I never understood it till now. If you do not know of Descartes discovery I shall think it worth republishing for the honour of old Renatus.(16)

I remain
Yours truly
J. Clerk Maxwell

(16) Following Sylvester's discussion of Cartesian ovals, on 19 March 1866 Morgan William Crofton read a paper to the newly instituted London Mathematical Society 'On certain properties of the Cartesian ovals, treated by the method of vectorial co-ordinates', *Proceedings of the London Mathematical Society*, **1** (1866): 5–18, esp. 5–7, giving a proof of the third focus, and declaring that: 'The Cartesian ovals seem at all times to have attracted considerable attention, though no great success has attended the attempts made to investigate their properties. Of these the most remarkable which has been discovered is that of the third focus, given by Chasles without proof'. For discussion of the third focus of Cartesian ovals see F. Gomes Teixeira, *Traité des Courbes Spéciales Remarquables*, 2 vols. (Coimbra, 1908–9), **1**: 220–1.

LETTER TO GEORGE GABRIEL STOKES

27 FEBRUARY 1867

From the original in the University Library, Cambridge(1)

8 Palace Gardens Terrace
W.
1867 Feb. 27.

Dear Sir

I find that I have made two mistakes in a paper I have sent to the R.S.(2) about Siemens & Wheatstones machines.(3)

It is all right about the *first* kind of commutator.(4)

In finding the effect of the second kind of commutator which breaks one of the circuits I have omitted to consider the effect of the stoppage on the other coil by induction. I send you the result taking the inductive effect into account.(5)

The second mistake was in the case of two currents always disconnected. Such a system cannot maintain currents by its motions.

The result I came to arose from neglecting the inductive effect of stopping the current.

In fact a closed circuit cannot have a current always in one direction produced by any inductive action.

The nearest approach to it is to take a long flexible magnet and wind it on the coil like as a silk is wound to cover a wire. During the winding a current

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(1) ULC Add. MSS 7656, M 425. First published in Larmor, *Correspondence*, **2**: 28.

(2) J. Clerk Maxwell, 'On the theory of the maintenance of electric currents by mechanical work without the use of permanent magnets', *Proc. Roy. Soc.*, **15** (1867): 397–402 (= *Scientific Papers*, **3**: 79–85). The paper was received on 28 February 1867, and read on 14 March 1867.

(3) Concerned with the self-excitation of a dynamo. See Charles William Siemens, 'On the conversion of dynamical into electrical force without the aid of permanent magnetism', *Proc. Roy. Soc.*, **15** (1867): 367–9; and Charles Wheatstone, 'On the augmentation of the power of a magnet by the reaction thereon of currents induced by the magnet itself', *Proc. Roy. Soc.*, **15** (1867): 369–72. The papers were read on 14 February 1867. These self-exciting magneto-electrical machines consisted of a fixed and a moveable electromagnet connected by a commutator.

(4) Maxwell, 'On the theory of the maintenance of electric currents': 399–401 (= *Scientific Papers*, **2**: 81–3). He considers four different arrangements of the contacts of the commutator with the two coils of the electromagnets. In the first kind of commutator the circuits of both coils are uninterrupted.

(5) In the second kind of commutator the circuits are closed, so the currents in them are independent: 'The second circuit is then broken, and the current in it is thus stopped. This produces an effect on the first circuit by induction'; see Maxwell, 'On the theory of the maintenance of electric currents': 401 (= *Scientific Papers*, **2**: 83).

will go in one direction and during the unwinding it will go in the other direction.

The part relating to two independent coils is therefore all wrong.

If you can allow that part to be cut out, and the enclosed to be substituted for what relates to commutators of the 2^{nd} kind it will prevent a false statement from going beyond you and me.

Yours truly
J. Clerk Maxwell.

REPORT ON A PAPER BY JOSEPH DAVID EVERETT ON THE DETERMINATION OF RIGIDITY

4 MARCH 1867

From the original in the Library of the Royal Society, London[1]

REPORT ON D^r EVERETT'S ACCOUNT OF EXPERIMENTS ON TORSION AND FLEXURE FOR THE DETERMINATION OF RIGIDITIES[2]

These experiments are a continuation of a former series[3] with better apparatus and a greater number of substances.[4] In my report on the former paper[5] I stated my opinion of the advantages of M. Kirchhoff's method of experimenting on the torsion and flexure of the same rod[6] in order to determine Poissons ratio,[7] and of the improvement introduced by D^r Everett in making the moment of flexure uniform throughout the length of the rod and in having no external force applied to the part of the rod whose bending is observed.

I have therefore only to report on the changes and additions which D^r Everett has since made.

He now clamps one end of the rod and applies a couple to the other by means of a cross piece weighted at the end of one of its limbs and supported in the centre by a balance. This is an improvement on the jointed apparatus formerly used[8] but a correction is required on account of the middle point of support not being in the plane of the points of suspension of the weights.[9] There seems no mechanical reason why the five points should not be placed

(1) Royal Society, *Referees' Reports*, **6**: 118.

(2) J. D. Everett, 'Account of experiments on torsion and flexure for the determination of rigidities', *Phil. Trans.*, **157** (1867): 139–53. The paper was received by the Royal Society on 25 January 1867, and read on 7 February 1867; see the abstract in *Proc. Roy. Soc.*, **15** (1867): 356.

(3) J. D. Everett, 'Account of experiments on the flexural and torsional rigidity of a glass rod, leading to the determination of the rigidity of glass', *Phil. Trans.*, **156** (1866): 185–91.

(4) In the paper under review Everett carried out experiments on rods of glass, brass and steel.

(5) Number 261.

(6) Gustav Kirchhoff, 'Ueber das Verhältniss der Quercontraction zur Längendilatation bei Stäben von federhartem Stahl', *Ann. Phys.*, **108** (1859): 369–92.

(7) The ratio of lateral contraction to longitudinal extension; see Number 261 notes (6), (7) and (9).

(8) Compare Maxwell's suggestions for improving the apparatus in Number 261.

(9) See Everett, 'Account of experiments on torsion and flexure': 143.

in one plane so as to get rid of the principal part of this correction which is uncertain, as no direct measure of the rotation of the cross &c were made.

The cone of support n might be screwed into a mortice hole in the stem of the apparatus and supported by a semistirrup hanging from the balance arm.(10)

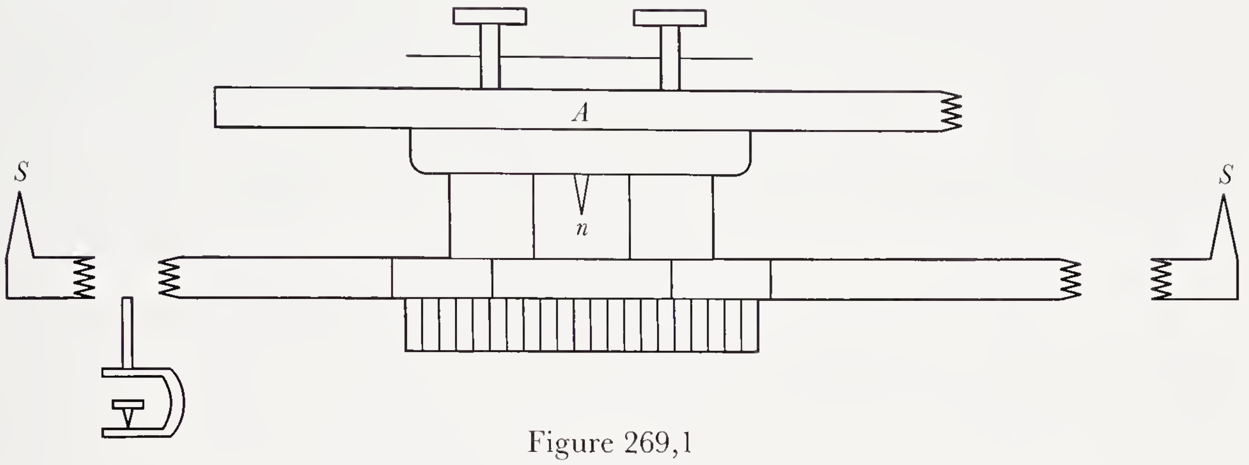

Figure 269,1

The counterpoise should be equal to the weight of this apparatus together with half the weight of the rod experimented on and the centre of gravity of this apparatus together with half the rod at A should coincide nearly with n.

No statement is made as to the horizontality of the cross piece. Its deviation from horizontality when the weights are on should be ascertained (roughly at least).

The method of observing with telescopes and a scale is better than that with a lamp and its image. The method of correction for the obliquity of the ray is not stated as fully as other parts of the paper are developed.

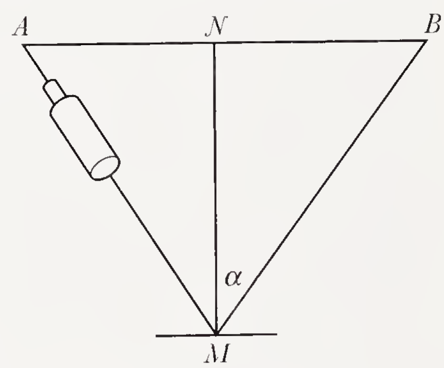

Figure 269,2

If AB is the plane of the scale AM the direction of the axis of the telescope B the point on the scale seen at the cross wires, MN the normal then by geometry $AN:NB::AM:MB$ and ANB is a straight line.

If $NMB = \alpha$ and if in the mean position MN is perpendicular to the scale then for a small displacement BB' where $BC = x$ and $CB' = y$ we have a corresponding displacement of the normal NN' where $NL = \xi$ and $LN' = \eta$

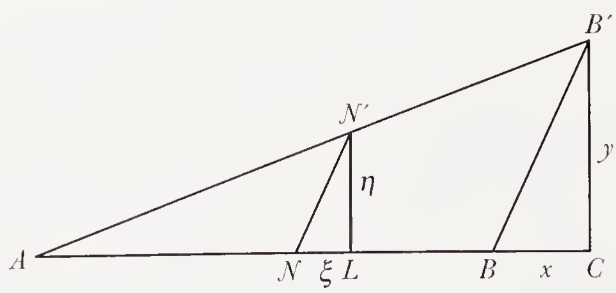

Figure 269,3

(10) Compare Everett's apparatus in *Phil. Trans.*, **157** (1867): Plate IX, Fig. 1.

and $$\xi = \frac{x}{2\sec^2\alpha} \quad \eta = \frac{y}{2}.$$

The mirrors should be adjusted to be horizontal which may be done by observing the reflexion of a plumb line in them.

The telescopes should then be adjusted(11) each in a plane through its own mirror perpendicular to the rod and at equal angles with the vertical such that the ray from the scale to the mirror always clears the top of the observers forehead in all the conditions of torsion.

If this angle be α then we have for torsion if c be the height of scale above the mirror and θ the angle of torsion x & y the scale readings

$$\tan\theta = \frac{2c\tan\alpha + x}{c + \sqrt{c^2 + cx\sin 2\alpha + (x^2+y^2)\cos^2\alpha}} - \tan\alpha$$

and if ϕ is the angle of flexure

$$\tan\phi = \frac{y}{c + \sqrt{c^2 + cx\sin 2\alpha + (x^2+y^2)\cos^2\alpha}}.$$

The value of Poissons ratio obtained by D^r Everett for glass is decidedly less than .25(12) being .229.(13) Well annealed glass is probably as isotropic as any substance we can find. A piece of the rod from $\frac{1}{2}$ inch to 1 inch long may be examined by passing polarized light through it longitudinally and if the light is unaffected the glass may be considered not only isotropic but free from strain. Most glass rods are in a state of constraint but this does not imply any want of isotropy in the substance but only the existence of forces.

Whether by pressure and heat a piece of glass can be made to have through its whole extent a doubly refractive power of the same kind and direction not depending on the actual existence of unequal pressures, I cannot say. It can be done with dried gelatine.(14)

The value of σ(15) for brass(16) comes very near its absolute limit $\frac{1}{2}$ and ought to be carefully tested.

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(11) 'The effect produced was observed, in Kirchhoff's experiments, by means of two telescopes looking down into two mirrors which reflected a scale of lines crossing each other at right angles placed horizontally overhead.'; Everett, 'Account of experiments on torsion and flexure': 139.

(12) See Number 261, esp. note (7).

(13) Everett, 'Account of experiments on torsion and flexure': 145. Compare the value of 0.258 found in his former paper (see Number 261).

(14) On Maxwell's interest in photoelasticity see Volume I: 117, 125–7, 151–6, 488–9.

(15) Poisson's ratio: see Number 261 note (6).

(16) Everett, 'Account of experiments on torsion and flexure': 148, gives a value of 0.469 for brass.

Lead is said to be very uniform in its properties when pressed into wires. If it is not too plastic it would be desirable to try its elasticity.

In conclusion I think that only one set of experiments should have all their numbers given in full. The others should be reduced and corrected and the results given, stating in each case the amount of discordance of the observations in the form of 'probable error' of single observations or of the result.(17) In other respects I think that both for method and results this paper is worthy to be printed in the Transactions.

J. Clerk Maxwell

1867 March 4

(17) This recommendation was not followed.

REPORTED COMMENTS ON BRODIE'S CHEMICAL CALCULUS[1]

6 JUNE 1867[2]

From a report on a meeting of the Chemical Society in *Chemical News*[3]

Professor Clerk Maxwell said he confessed that when he came into the room his feelings received a wholesome shock from two of the statements in the diagrams – first, that space was a chemical substance, and second, that hydrogen and mercury were operations. He now, however, understood what was meant. The present [calculus] seemed to be an endeavour to cause the symbols of chemical substances to act in the formulae according to their own laws. The formulae at present used were made to express many valuable properties of chemical substances, just as a great many formulae were employed to represent the syllogism in logic, which required a logical mind to form them, to understand them, and to reason upon them. The only successful attempt to introduce a new system in the logical representation was that of Mr. Boole, who accomplished it by the metaphysical and mathematical conception that x^2 was equal to x.[4] In Sir Benjamin Brodie's system α did not mean exactly 'hydrogen', but 'make hydrogen'; that is, take the cubic centimetre of space, and put hydrogen into it of the proper pressure and temperature. But if they were to compress into that space another volume of hydrogen, that would not be α^2, because it would increase the pressure to double what it was before. If it were possible to get α^2, they would require to combine two volumes together by a process unknown to chemists, keeping the pressure and temperature as before.[5] There was, in this respect, no doubt, an idea which differed from the mere collocation of symbols. The unit of ponderable matter described in the system was one

(1) Benjamin Collins Brodie, 'The calculus of chemical operations; being a method for the investigation, by means of symbols, of the laws of the distribution of weight in chemical change. Part I. On the construction of chemical symbols', *Phil. Trans.*, **156** (1866): 781–859.

(2) See note (3).

(3) A meeting of the Chemical Society on 6 June 1867; B. C. Brodie, 'On the mode of representation afforded by the chemical calculus, as contrasted with the atomic theory', *Chemical News*, **15** (1867): 295–305, on 303.

(4) See George Boole, *An Investigation of the Laws of Thought, on which are founded the Mathematical Theories of Logic and Probabilities* (London, 1854): 31. Brodie made no reference to Boole in his Chemical Society lecture; but compare Brodie, 'The calculus of chemical operations': 794–803, esp. 801n, where he refers to the logical relation $x^2 = x$ in Boole's system of logic.

(5) On Maxwell's comments see D. M. Dallas, 'The chemical calculus of Sir Benjamin Brodie', in *The Atomic Debates*, ed. W. H. Brock (Leicester, 1967): 49–50.

which had been derived by chemists from chemical considerations alone. It had also been advocated by physicists from considerations derived from the theory of heat. In order to decide with certainty on the truth or falsehood of the atomic theory, it would be necessary to consider it from a dynamical point of view. He meant that kind of dynamics treated of in books on mechanics. It was worth while to direct the attention of chemists to the fact that a belief in atoms conducted necessarily to exactly the same definition as was given there – namely,[6] that for every kind of substance the number of atoms, or molecules, in the gaseous state, occupying the space of a litre, at a temperature of 0 degrees, and of a pressure of 760 millimetres,[7] must necessarily be the same.[8] That was a consequence which could be deduced from purely dynamical considerations on the supposition advocated by Professor Clausius and others, that gases consists of molecules floating about in all directions, and producing pressure by their impact.[9] That theory was now under probation among chemists, physicists, and others. The next step was one which might be far off – the finding of the number of these molecules. That number was a fixed one; and when it could be arrived at, we should have another unit of ponderable matter – that of a fixed molecule.[10]

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(6) Brodie's definition of a 'unit of matter'; see Brodie, 'On the mode of representation afforded by the chemical calculus': 298.

(7) Of mercury.

(8) 'Avogadro's hypothesis': see Number 259 §4, esp. note (13).

(9) See Number 263, esp. note (9).

(10) See Number 470.

LETTER TO WILLIAM HUGGINS(1)

10 JUNE 1867

From the *Philosophical Transactions of the Royal Society*(2)

ON THE INFLUENCE OF THE MOTIONS OF THE HEAVENLY BODIES ON THE INDEX OF REFRACTION OF LIGHT

Let a source of light be such that it produces n disturbances or vibrations per second, and let it be at such a distance from the earth that the light requires a time T to reach the earth. Let the distance of the source of light from the earth be altered, either by the motion of the source of light, or by that of the earth, so that the light which emanates from the source t seconds afterwards reaches the earth in a time T'.

During the t seconds nt vibrations of the source of light took place, and these reached the earth between the time T and the time $t+T'$, that is, during $t+T'-T$ seconds. The number of vibrations which reached the earth per second was therefore no longer n, but $n\dfrac{t}{t+T'-T}$.

If v is the velocity of separation of the source of light from the earth, and V the velocity of light between the bodies relative to the earth, then $vt = V(T'-T)$, and the number of vibrations per second at the earth will be $n\dfrac{V}{V+v}$.

If V_0 is the velocity of propagation of light in the luminiferous medium, and if v_0 is the velocity of the earth,

$$V = V_0 - v_0,$$

(1) William Huggins, astronomer and astrophysicist, FRS 1865, Royal Medal 1866, Hon. Sec. Royal Astronomical Society 1867–70 (*DNB*).

(2) Published by William Huggins in his paper 'Further observations on the spectra of some of the stars and nebulæ, with an attempt to determine therefrom whether these bodies are moving towards or from the earth, also observations of the spectra of the sun and of Comet II., 1868', *Phil. Trans.*, **158** (1868): 529–64, on 532–5. In an introduction to his paper, Huggins explained the circumstances of Maxwell's letter: 'The subject of the motions of the heavenly bodies on the index of refraction of light had already...in 1864, occupied the attention of Mr. J. C. Maxwell, F.R.S., who had made some experiments in an analogous direction. In the spring of last year, at my request, Mr. Maxwell sent to me a statement of his views and of the experiments which he had made.' (on 530). For Maxwell's experiments see Number 227.

and the wave-length will be increased by a fraction of itself equal to

$$\frac{v}{V_0 - v_0}.$$

Since v_0 only introduces a correction which is small compared even with the alteration of wave-length, it cannot be determined by spectroscopic observations with our present instruments, and it need not be considered in the discussion of our observations.

If, therefore, the light of the star is due to the combustion of sodium, or any other element which gives rise to vibrations of definite period, or if the light of the star is absorbed by sodium vapour, so as to be deficient in vibrations of a definite period, then the light, when it reaches the earth, will have an excess or defect of rays whose period of vibration is to that of the sodium period as $V + v$ is to V.

As an example, let us suppose the star to be fixed and the earth to be moving directly away from the star with the velocity due to its motion round the sun. The coefficient of aberration indicates that the velocity of light is about 10,000 times that of the earth in its orbit,(3) and it appears from the observations of Ångström(4) and Ditscheiner(5) that the wave-length of the less refrangible of the lines forming D exceeds that of the other by about one-thousandth part of itself. Hence, if the lines corresponding to D in the light of the star are due to sodium in the star, these lines in the starlight will be less refrangible than the corresponding lines in a terrestrial sodium-flame by about a tenth part of the difference between D_1 and D_2.

When the earth is moving towards the star, the lines will be more refrangible than the corresponding terrestrial lines by about the same quantity.

The effect of the proper motion of stars would of course have to be compounded with the effect of the earth's own motion, in order to determine the velocity of approach or separation.

To observe these differences of the light from stars, a *spectroscope* is necessary, that is, an instrument for separating the rays of different periods; and it is immaterial in what direction the refraction of the light through the

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(3) See the value cited in Number 238, esp. note (26).

(4) A. J. Ångström, 'Neue Bestimmung der Länge der Lichtwellen, nebst einer Methode, auf optische Wege die fortschreitende Bewegung des Sonnensystems zu bestimmen', *Ann. Phys.*, **123** (1864): 489–505; (trans.) 'On a new determination of the lengths of waves of light, and of a method of determining, by optics, the translatory motion of the solar system', *Phil. Mag.*, ser. 4, **29** (1865): 489–501, esp. 491–2.

(5) Leander Ditscheiner, 'Bestimmung der Wellenlängen der Fraunhofer'schen Linien des Sonnenspectrums', *Wiener Berichte*, **50**, Abtheilung II (1865): 296–341, esp. 340.

prisms takes place, because the *period* of the light is the thing to be observed by comparison with that of a terrestrial flame.

If, instead of a spectroscope, an achromatic prism were used, which produces an equal deviation on rays of different periods, no difference between the light of different stars could be detected, as the only difference which could exist is that of their period.(6)

If the motion of a luminiferous medium in the place where the experiment is made is different from that of the earth, a difference in the deviation might be expected according to the *direction* of the ray within the prisms, and this difference would be nearly the same whatever the source of the light.

There are therefore two different and independent subjects of experiment. The one is the alteration in the period of vibration of light due to the relative motion of the stars and the earth.(7) The fact of such an alteration is independent of the form under which we accept the theory of undulations, and the possibility of establishing its existence depends on the discovery of lines in the stellar spectra, indicating by their arrangement that their origin is due to the existence of substances in the star having the same properties as substances found on the earth. Any method of observing small differences in the period of vibration of rays, if sufficiently exact, will enable us to verify the theory, and to determine the actual rate of approach or separation between the earth and any star.

The other subject of experiment is the relation between the index of refraction of a ray and the direction in which it traverses the prism. The essentials of this experiment are entirely terrestrial, and independent of the source of light, and depend only on the relative motion of the prism and the luminiferous medium, and on the direction in which the ray passes through the prism.

The theory of this experiment, however, depends on the form in which we accept the theory of undulations. In every form of the theory, the index of refraction depends on the retardation which a ray experiences on account of having to traverse a dense medium instead of a vacuum. Let us calculate this retardation.

Let there be a transparent medium whose thickness is a, and let it be supposed fixed. Let the luminiferous ether be supposed to move with velocity v in air, and with velocity v' within the medium. Let light be propagated

(6) See Number 227 note (11).

(7) The experiment addressed by Huggins. He found some evidence of the motion of the stars relative to the earth from observations of the spectral lines of Sirius as compared with the hydrogen spectrum; see his 'Further observations on the spectra of some of the stars and nebulæ': 546–50.

through the ether with velocity V in air and with velocity V' within the medium. Then the absolute velocity of the light will be $v+V$ in air and $v'+V'$ within the medium, and the retardation, or difference of *time* in traversing a thickness a of the medium and an equal thickness of air, will be

$$a\left(\frac{1}{v'+V'}-\frac{1}{v+V}\right);$$

and the retardation in *distance* reckoned as at the velocity, V will be

$$a\left\{\frac{V}{V'}-1-\frac{v'}{V}\left(\frac{V^2}{V'^2}-\frac{v}{v'}\right)+\frac{v'^2}{V^2}\left(\frac{V^3}{V'^3}-\frac{v^2}{v'^2}\right)-\&\text{c.}\right\}.$$

Now, according to every form of the theory, $\frac{V}{V'}=\mu$, the index of refraction, and according to Fresnel's form of the theory, in which the density of the medium varies as μ^2,(8) the equation of continuity requires that $\frac{v}{v'}=\mu^2$. In this case the second term disappears and the retardation is $a(\mu-1)+$ terms in $\frac{v'^2}{V^2}$, which may be neglected, as V is more than 10,000 times v.

Hence, on Fresnel's theory, the retardation due to the prism is not sensibly affected by the motion of the earth. The same would be true on the hypothesis that the luminiferous ether near the earth's surface moves along with the earth, whatever the form of the theory of the medium.

Since the deviation of light by the prism depends entirely on the retardation of the rays within the glass, no effect of the earth's motion on the refrangibility of light is to be expected.(9) Professor Stokes (Phil. Mag. 1846, p. 63)(10) has also given a direct proof of this statement, and the experiment of Arago(11) confirms it to a certain degree of exactness.

In order to test the equality of the index of refraction for light moving in

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(8) [A. J. Fresnel,] 'Lettre de M. Fresnel à M. Arago, sur l'influence du mouvement terrestre dans quelques phénomènes d'optique', *Ann. Chim. Phys.*, **9** (1818): 57–66, esp. 62. Fresnel assumes that isotropic media differ optically only in density; his argument is discussed by G. B. Airy, *Mathematical Tracts on the Lunar and Planetary Theories...and the Undulatory Theory of Optics* (Cambridge, $_3$1842): 340. See also Number 227 esp. note (6).

(9) Compare Maxwell's conclusion in Number 227; and see his letter to Stokes of 6 May 1864 (Number 228, esp. note (5)).

(10) Read: G. G. Stokes, 'On Fresnel's theory of the aberration of light', *Phil. Mag.*, ser. 3, **28** (1846): 76–81 (= *Papers*, **1**: 141–7).

(11) François Arago, 'Mémoire sur la vitesse de la lumière, lu à la prémière Classe de l'Institut, le 10 décembre 1810', *Comptes Rendus*, **36** (1853): 38–49.

opposite directions through a prism, I employed in 1864 the following arrangement.(12)

I made use of a spectroscope constructed by Mr. Becker,(13) and provided with a tube at right angles to the axis of the observing-telescope, carrying a transparent plate of parallel glass placed between the object-glass and its focus, so as to reflect the light which enters the tube along the axis of the telescope towards the object-glass. In this tube is placed a screen with a vertical slit, in the middle of which is a vertical spider-line so arranged that its virtual image formed by the first surface of the glass plate coincides with the crossing of the spider-lines of the telescope at the principal focus of the object-glass. This coincidence is tested by observing the cross lines through the other telescope, with the two telescopes facing each other. The eyepiece of the second telescope is then removed, and a plane mirror is placed at the focus of the object-glass, perpendicular to the axis, and the telescopes are so adjusted that light entering by the side tube is reflected down the axis of the first telescope, traverses the prisms in succession, enters the second telescope, is reflected by the mirror at its focus, and emerges from the telescope parallel to its direction at incidence; it then traverses the prisms in the reverse order, and is brought to a focus at the cross lines of the first telescope.

If the deviation of the rays in passing through the prisms from east to west differs from that produced during their passage from west to east, the image of the vertical spider-line formed by the rays which have traversed the prisms twice will not coincide with the intersection of the spider-lines as before.

I have found, however, that when the instrument is properly adjusted, the coincidence is so perfect with respect to rays of all refrangibilities, that the image of the vertical spider-line is seen with perfect distinctness, though the rays which form it have passed twice through three prisms of 60°.

If we observe the coincidence of this image with the intersection of the spider-lines at the focus when the rays pass through the prisms first in the direction of the earth's motion and return in the opposite direction, we may then reverse the whole instrument, so that the rays pursue an opposite path with respect to the earth's motion. I have tried this experiment at various times of the year since the year 1864, and have never detected the slightest effect due to the earth's motion. If the image of the spider-line is hid by the intersection of the cross lines in one position, it remains hid in precisely the same way in the other position, though a deviation corresponding to one-twentieth of the distance of the components of the line *D* could be easily detected.

(12) Number 227.

(13) See Number 214 note (5).

On the other hand, M. Fizeau*(14) has observed a difference in the rotation of the plane of polarization according as the ray travels in the direction of the earth's motion or in the contrary direction, and M. Ångström has observed a similar difference in phenomena of diffraction.(15) I am not aware that either of these very difficult observations has been confirmed by repetition.

In another experiment of M. Fizeau, which seems entitled to greater confidence, he has observed that the propagation of light in a stream of water takes place with greater velocity in the direction in which the water moves than in the opposite direction, but that the acceleration is less than that which would be due to the actual velocity of the water, and that the phenomenon does not occur when air is substituted for water.(16) This experiment seems rather to verify Fresnel's theory of the ether; but the whole question of the state of luminiferous medium near the earth, and of its connexion with gross matter, is very far as yet from being settled by experiment.(17)

JAMES CLERK MAXWELL

June 10, 1867.

* Ann. de Chimie et de Physique, Feb. 1860.(14)

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(14) Hippolyte Fizeau, 'Sur une méthode propre a rechercher si l'azimut de polarisation du rayon réfracté est influencé par le mouvement du corps réfringent – essai de cette méthode', *Ann. Chim. Phys.*, ser. 3, **58** (1860): 129–63.

(15) Ångström, 'On a new determination of the lengths of waves of light': 497–501.

(16) H. Fizeau, 'Sur les hypothèses relatives a l'ether lumineux...', *Ann. Chim. Phys.*, ser. 3, **57** (1859): 385–404. See Number 227 note (4).

(17) Maxwell summarized the argument of his letter to Huggins on discussing the problem of the relative motion of the ether in his article on 'Ether', in *Encyclopaedia Britannica* (9th edn), **8** (Edinburgh, 1879): 568–72, esp. 571 (= *Scientific Papers*, **2**: 769–70).

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BRITISH ASSOCIATION PAPER ON A STEREOSCOPE

[SEPTEMBER 1867]

From the *Report of the British Association for 1867*[1]

ON A REAL IMAGE STEREOSCOPE[2]

In all stereoscopes there is an optical arrangement, by which the right eye sees an image of one picture and the left eye that of another.[3] These images ought to be apparently in the same place, and at the distance of most distinct vision. In ordinary stereoscopes these images are virtual, and the observer has to place his two eyes near two apertures, and he sees the united images, as it were, behind the optical apparatus. In the stereoscope made for the author by Messrs. Elliott Brothers the observer stands at a short distance from the apparatus, and looks with both eyes at a large lens, and the image appears as a real object close to the lens. The stereoscope consists of a board about 2 feet long, on which is placed, first, a vertical frame to hold the pair of pictures, which may be an ordinary stereoscopic slide, turned upside down; secondly, a sliding piece near the middle of the board containing two lenses of 6 inches focal length, placed side by side, with their centres about $1\frac{1}{4}$ inch apart; and thirdly, a frame containing a large lens of about 8 inches focal length and 3 inches diameter. The observer stands with his eyes about 2 feet from the large lens. With his right eye he sees the real image of the left-hand picture formed by the left-hand lens in the air, close to the large lens, and with the left eye he sees the real image of the other picture formed by the other lens in the same place. The united images look like a real object in the air, close to the large lens. This image may be magnified or diminished at pleasure by sliding the piece containing the two lenses nearer to, or further from, the pictures.

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(1) *Report of the Thirty-seventh Meeting of the British Association for the Advancement of Science; held at Dundee in September 1867* (London, 1868), part 2: 11.

(2) Maxwell gave an account of the principles of his real image stereoscope in his paper 'On the cyclide', *Quarterly Journal of Pure and Applied Mathematics*, **11** (1867): 111–26, esp. 115n (= *Scientific Papers*, **2**: 148n). For the use of the stereoscope in drawing figures of surfaces see Numbers 274, 275, 277 and 279.

(3) Maxwell had first expressed interest in Charles Wheatstone's mirror or reflecting stereoscope and David Brewster's lenticular or refracting telescope in October 1849: see Volume I: 119. He devised a reflecting stereoscope in 1856; see Volume I: 391.

Plate V. Maxwell's real image stereoscope (1867), showing a stereogram of Steiner's surface (Number 272).

ON RECIPROCAL FIGURES AND DIAGRAMS OF FORCES

circa SEPTEMBER 1867

From *The Engineer* (1867)[1]

ON THE APPLICATION OF THE THEORY OF RECIPROCAL POLAR FIGURES[2] TO THE CONSTRUCTION OF DIAGRAMS OF FORCES*

Professor Rankine, in the *Philosophical Magazine* for February, 1864, has described a pair of reciprocal figures in three dimensions.[3] The reciprocity consists in the fact that they may be placed so that every straight line in the one figure is perpendicular to a plane face of the other, and every point of concourse of straight lines in the one figure corresponds in the other figure to a closed polyhedron. If these conditions be fulfilled, and if forces act between each connected pair of points in the first figure proportional to the areas of the plane faces of the second figure, which are perpendicular to the lines joining the pair of points, then this system of forces will keep all the points in equilibrium. In the *Philosophical Magazine* for April 1864, I have stated the conditions under which the construction of such reciprocal figures is possible.[4] I now propose to explain the connection between Professor Rankine's figure and the diagrams of forces for plane frames by means of the theory of reciprocal polars with respect to the sphere.

* British Association, Section G[5]

(1) *The Engineer*, **24** (8 November 1867): 402.

(2) The reference is to the method of reciprocal polars in projective geometry, introduced by J. V. Poncelet in the 1820s, and widely familiar. See especially Michel Chasles, *Aperçu Historique sur l'Origine et le Développement des Méthodes en Géométrie* (Brussels, 1837) and his supplementary 'Mémoire de géométrie sur deux principes généraux de la science: la dualité et l'homographie' in *Mémoires couronnés par l'Académie Royale des Sciences et Belles-Lettres de Bruxelles*, **11** (1837): 575–848; and Chasles, *Traité de Géométrie Supérieure* (Paris, 1852). Maxwell refers to Chasles' projective geometry in Number 373. The work of Chasles and Poncelet is presented by John Mulcahy, *Principles of Modern Geometry, with numerous applications to Plane and Spherical Figures* (Dublin, 1852, $_2$1862); Maxwell referred to Mulcahy's text in 1853 and 1856 (see Volume I: 230n, 485). See also Numbers 334, 472 §1, and 480.

(3) W. J. M. Rankine, 'Principle of the equilibrium of polyhedral frames', *Phil. Mag.*, ser. 4, **27** (1864): 92.

(4) J. Clerk Maxwell, 'On reciprocal figures and diagrams of forces', *Phil. Mag.*, ser. 4, **27** (1864): 250–61 (= *Scientific Papers*, **1**: 514–25).

(5) The abstract published in the British Association *Report* is reproduced as the Appendix *infra*.

Let any closed polyhedron with plane faces be taken, and let a system of triangles be described whose bases are the edges of the polyhedron and whose vertices are at the point P. Next let the polar reciprocal of the polyhedron be constructed with respect to a sphere whose centre is at P, and let triangles be described with their bases on the edges of this polyhedron and their vertices at P. Then every face of one polyhedron will be polar to a summit of the other, and every edge of the one to a corresponding edge of the other, by the theory of reciprocal polars.

The polyhedra with the accompanying triangles will also satisfy Professor Rankine's condition, for every face of either polyhedron will be perpendicular to the line joining P with the corresponding summit of the other, and every triangle whose vertex is at P will be perpendicular to the lines in the other figure which is polar to its base. Every summit of the one polyhedron corresponds to the pyramid in the other whose vertex is P, and whose base is the polar of the summit, and the polyhedron itself in the one figure corresponds to the point P in the other. Either figure may be taken to represent a jointed frame whose angles are acted on by forces whose direction passes through P, and the other figure will then represent the magnitude of each force by the area of the face which is perpendicular to the line of action of the force.

In order to pass from this case to that of plane frames, let any point Q be taken within one of the polyhedra, and let a plane be drawn through Q perpendicular to PQ. Next let the distance of every point of the polyhedron from this plane be diminished in the ratio of n to 1, and let the distance of P from the plane be increased in the ratio of 1 to n, and let the figure which is reciprocal to this altered figure be described. Then, when n is indefinitely increased, both the polyhedra will tend to become plane figures of finite dimensions, which are reciprocal figures in the sense that every line of the one is perpendicular to the corresponding line in the other, and that lines which meet in a point correspond to lines forming a closed polygon. I have proved in my former paper that the condition of a reciprocal figure being possible is the same as that of the figure being the projection of a polyhedron with plane sides,(6) and by the construction which I have now described, when this condition is fulfilled, the reciprocal figure may be drawn.

In calculating the equilibrium of frames, such as those of roofs and bridges, after resolving the weight of each piece into two parts applied at its extremities, we have a frame without weight acted on by external forces applied at its joints. If a diagram of forces without redundant lines is possible,

(6) Maxwell, 'On reciprocal figures and diagrams of forces': 252–8 (= *Scientific Papers*, **1**: 516–22).

every line of the frame must form a side of two, and only two, closed polygons answering to the two, and only two extremities of the corresponding line in the diagram of forces. Such a diagram can only be drawn when the external forces are applied at points on the boundary of the frame. For any line in the frame to the extremities of which external forces are applied forms part of a polygon of which the directions of the external forces are sides. If it is part of the boundary of the frame, it forms a side of one other polygon only and the reciprocal diagram is possible, but if it is in the interior of the frame it is a side of more than one other polygon, and it cannot be represented in the reciprocal diagram by a single line. In the complete diagram the line would be repeated till all the polygons of which the original line formed a side were represented by the extremities of the different repetitions of the line. In most actual cases, however, the external forces can be represented by a series of weights placed on the joints of the upper and lower boundaries of the frame, and a diagram can be drawn representing at once the magnitude and direction of the stresses. The construction of such diagrams is easy, and affords a security against errors, since if any mistake is made the diagram cannot be completed. Since all the weights on the upper boundary of the frame act towards one point at an infinite distance, the lines representing them will form part of a polygon, and since they are all parallel, the lines will be successive portions of the same straight line. The same will be true of the weights on the lower boundary. The vertical lines corresponding to these two sets of weights, together with the forces supporting the whole structure, will form a closed polygon. If the supporting forces are vertical, this polygon will be reduced to a vertical line folded on itself.

The diagram for a roof may be thus constructed:– Let A_1, A_2, &c, be the different divisions of the rafters beginning at the left side of the figure. Let a_1, a_2, &c, be the stresses in these pieces. Let B_1, B_2, &c, be the different pieces

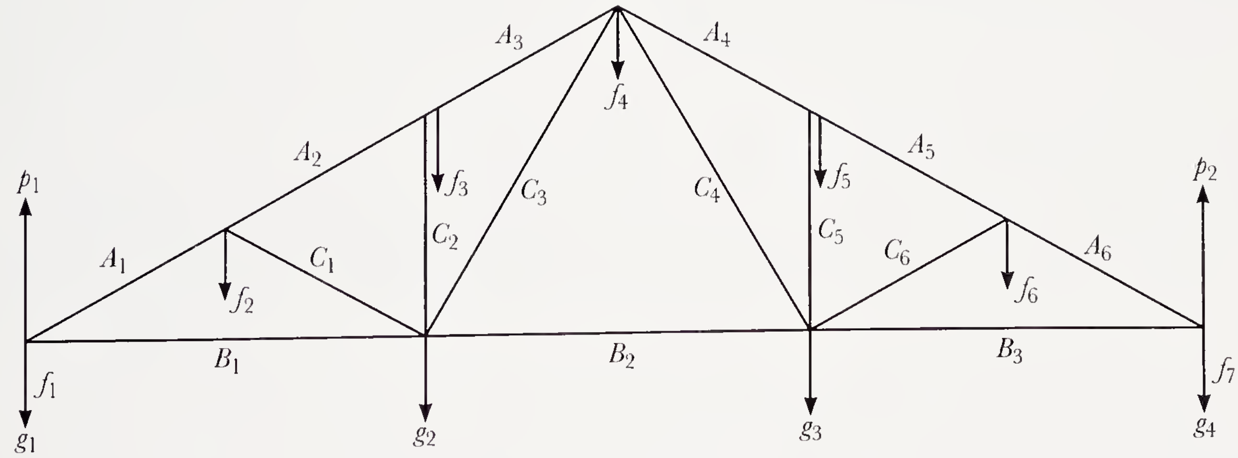

Figure 273,1

of the tie beam, and b_1, b_2, &c, the stresses in them. Let C_1, C_2, &c, be the different pieces of the frame between the rafters and the tie beam, and let c_1, c_2, &c, be the stresses in them. Let f_1, f_2, &c, be the weights applied at the end of the rafters and at the different joints of them, and let g_1, g_2, &c, be the weights at the extremities of the pieces of the tie beam. Let p_1, p_2, be the forces supporting the entire structure. It is easiest to calculate these in the ordinary way by taking moments. To construct the diagram draw a vertical line and measure successive portions upwards representing f_1, f_2, &c. From the bottom of this line draw p upwards (if p is vertical the two lines will coincide). From the top of p measure segments g_1, g_2, &c, downwards in succession. The line joining the bottom of this line with the top of the line of f_1, f_2, &c, will represent p_2. Next, from the point of division between f_1 and f_2 draw a_1 parallel to A_1, and from the points of division between f_2 and f_3 draw a_2 parallel to A_2, and so on. In the same way from the point g_1, g_2 draw b_1 parallel to B_1, and so on. Then, since at the foot of the first rafter the forces are f_1, a_1, b_1, g_1, p, these must form a polygon, and therefore the lines a_1 and b_1 terminate at their point of intersection. We may then go on to determine all the other forces by drawing lines parallel to c_1, c_2, &c, as in the figure, always remembering that forces acting at a point in the roof must form a closed polygon in the diagram, and that the forces along pieces which form a closed polygon in the roof must meet at a point in the diagram.

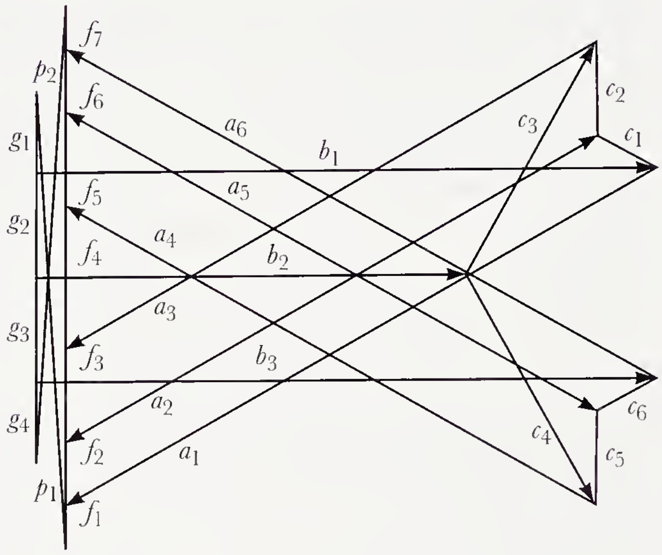

Figure 273,2

APPENDIX: BRITISH ASSOCIATION PAPER ON DIAGRAMS OF FORCES

[SEPTEMBER 1867]

From the *Report of the British Association for 1867*[7]

ON THE THEORY OF DIAGRAMS OF FORCES AS APPLIED TO ROOFS AND BRIDGES

A roof is made up of a series of vertical frames. A diagram of forces is a figure consisting of straight lines, which represent, both in magnitude and direction, the tensions and pressures in the different pieces between the joints of the frame. The pieces of the frame and the weights acting on it are denoted by capital letters, and the corresponding lines of the diagram by small letters. The diagram is constructed by the following rule, which is sufficient for the purpose:– The frame, including the vertical lines representing the weights, and the diagrams of forces, are reciprocal figures, such that every line in the one is parallel to the corresponding line in the other, and every set of lines which meet in a point in the one figure form a closed figure in the other. It follows from this that the weights, which are all vertical forces, are represented by the parts of one vertical line. The first extension of the principle of the diagram of forces was made by Dr. Rankine in his 'Applied Mechanics'.[8] The theory was generalized by the author in the Philosophical Magazine in April 1864.[9] In the present paper it is shown to be connected with the theory of reciprocal polars in solid geometry, and rules for the construction of diagrams are given. The advantage of the method is that its construction requires only a parallel ruler, and that every force is represented to the eye at once by a separate line, which may be measured with sufficient accuracy for all purposes with less trouble than the forces can be found by calculation. It also affords security against error, as, if any mistake is made, the diagram cannot be completed.

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(7) *Report of the Thirty-seventh Meeting of the British Association for the Advancement of Science; held at Dundee in September 1867* (London, 1868), part 2: 156.

(8) W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 137–40.

(9) See note (4).

LETTER TO WILLIAM THOMSON

14 SEPTEMBER 1867

From the original in the University Library, Glasgow[1]

Glenlair
Dalbeattie
1867 Sept 14

Dear Thomson

I send you per book post a copy of my paper on the maintenance of electric currents[2] enclosing stereograms of[3]

I Icosihedron inscribed in Octahedron. This is one of two cases right & left handed. Every face of the Octahedron has in it a face of the Icosihedron with a twist to the right or left hand thus.

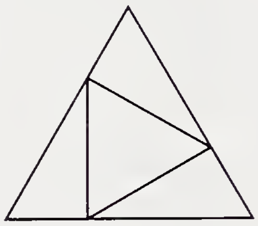

Figure 274,1

The alternate sides of the octahedron are right or left handed, and if the two tetrahedra be formed by producing the sides of the octahedron the one tetrahedron will be righthanded and the other lefthanded with regard to the icosihedron.

II Part of the elliptic paraboloid near its vertex showing the umbilici and lines of curvature.

III Lines of curvature of a hyperbolic paraboloid near the vertex. Both figures are here symmetrical in themselves but give the negative curvature in the stereoscope.

IV Lines of curvature of the ellipsoid.[4]

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(1) Glasgow University Library, Kelvin Papers M 20.

(2) J. Clerk Maxwell, 'On the theory of the maintenance of electric currents by mechanical work without the use of permanent magnets', *Proc. Roy. Soc.*, **15** (1867): 397–402 (= *Scientific Papers*, **2**: 79–85). See Number 268.

(3) The stereograms listed below are preserved in ULC Add. MSS 7655, V, i/11, where a number of the original drawings are also preserved. See Plates VI and VII. Some of the stereograms mentioned here were exhibited to the London Mathematical Society on 23 January 1868: see Number 279. See also Numbers 275 and 277; and on the stereoscope see Number 272 and Plate V. The proof of the stereogram of the elliptic paraboloid carries the following explanation on the *verso*: 'II Lines of Curvature of an Elliptic Paraboloid / General Equation of the confocal system / $\frac{x^2}{p+a}+\frac{y^2}{p-a}+4(z-p) = 0$. / If p, a, q, $-a$, r be in order of magnitude and if q and r be substituted for p, the point of intersection of the three confocal surfaces $p\ q\ r$ is / $x^2 = -2\frac{(p+a)\,(q+a)\,(r+a)}{a}$ / $y^2 = 2\frac{(p-a)\,(q-a)\,(r-a)}{a}$ / $z = p+q+r$. / In the figure $a = 2$, $p = 4$ / q & r from $+2$ to -4.'

(4) Maxwell's proof of this stereogram (see note (3)) carries the following explanation on the

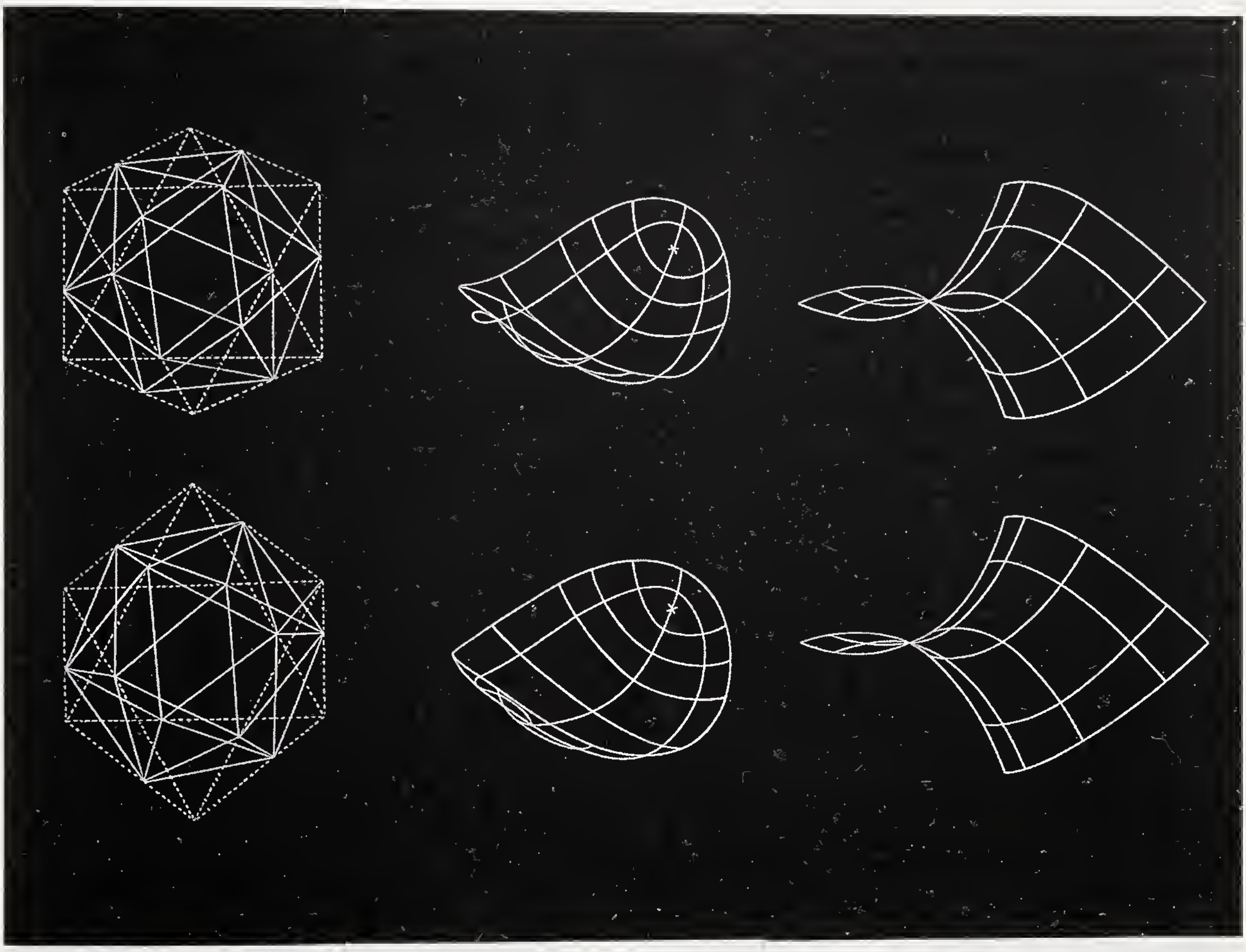
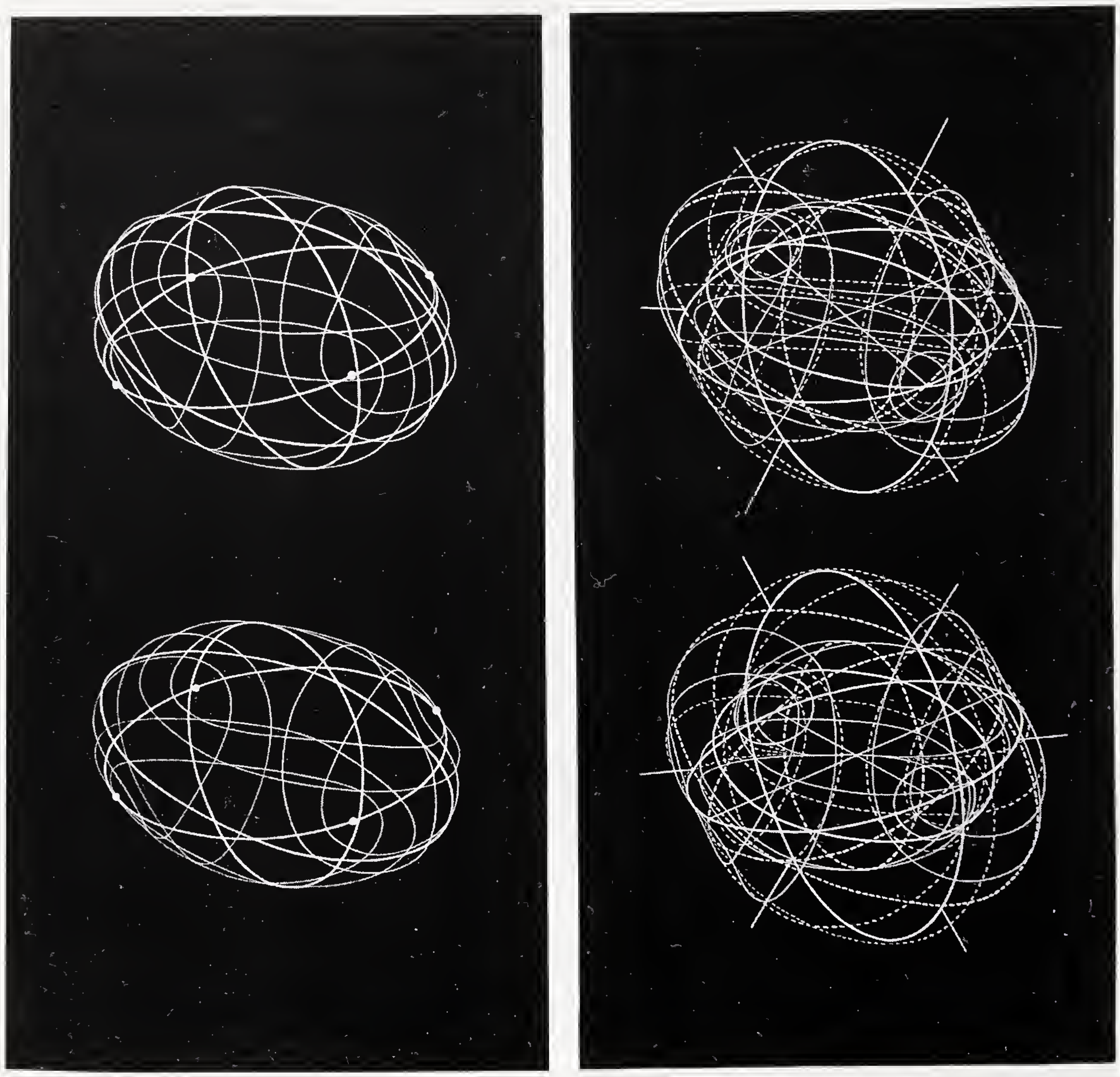


Plate VI. Stereograms (1867) of icosihedron in octahedron, lines of curvature of elliptic paraboloid and lines of curvature of hyperbolic paraboloid (Number 274).



LINES OF CURVATURE OF ELLIPSOID.

FRESNEL'S WAVE SURFACE.

Plate VII. Stereograms (1867) of lines of curvature of ellipsoid and Fresnel's wave surface (Number 274).

V Wave surfaces of the same ellipsoid.[(5)] The dotted lines follow the direction of the planes of polarization at every point and are all spherical curves on the wave surface. The continuous lines are perpendicular to these or in the direction of Fresnels vibration. They are all intersections of the wave surface with ellipsoids similar to IV.

The two lines which cross at the centre pass through the conical points where the inner sheet which is like a pulpit cushion meets the outer sheet which is like an apple with two stalk holes and two blossom holes.

The six spines sticking out are the ends of the principal axes projecting beyond the surface to a fixed distance. They show where the principal sections are.

The other four are figures of the Cyclide or surface all whose lines of curvature are circles and all whose normals pass through two confocal conics (the skeleton curves of a confocal system of quadric surfaces. See my paper in next number of QJ of P & A M).[(6)]

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verso: 'IV. Lines of Curvature of an Ellipsoid / Equation of the Confocal System / $\dfrac{x^2}{\rho^2-a^2}+\dfrac{y^2}{\rho^2-b^2}+\dfrac{z^2}{\rho^2-c^2}=1$. / If ρ, c, μ, b, v, a be in descending order of magnitude and if μ and v be substituted for ρ, the point of intersection of the three confocal surfaces is given by $x^2=-\dfrac{(\rho^2-a^2)(\mu^2-a^2)(v^2-a^2)}{(c^2-a^2)(a^2-b^2)}$ and two other equations for y & z which may be written down from symmetry. In the figure, for the ellipsoid $a^2=0$, $b^2=3$, $c^2=6$, $\rho^2=9$. / For the hyperboloids of one sheet $\mu^2=6$, 5, 4 and 3. / For the hyperboloids of two sheets $v^2=3$, 2, 1 and 0. / The umbilici are indicated by dots.'

(5) Maxwell's proof of this stereogram (see note (3)) carries the following explanation on the *verso*: 'V. Fresnels Biaxial Wave Surface. / Formed by Fresnels construction from the Ellipsoid N°IV. / Points on the outer and inner sheet respectively are found from the equations

$$x_1^2=\frac{(\rho^2-b^2)(\rho^2-c^2)(\mu^2-a^2)(v^2-a^2)}{(c^2-a^2)(a^2-b^2)(\rho^2-\mu^2)}$$

$$x_2^2=\frac{(\rho^2-b^2)(\rho^2-c^2)(\mu^2-a^2)(v^2-a^2)}{(c^2-a^2)(a^2-b^2)(\rho^2-v^2)}$$

with similar equations for y & z. / The straight lines which intersect at the centre are the optic axes joining the two pairs of conical points where the two sheets meet. / The short straight lines are portions of the three principal axes projecting beyond the outer sheet. / The continuous lines are in the direction of vibration at every point and are the intersections of the Wave Surface with ellipsoids similar to N°IV. / The dotted lines are in the direction of the plane of polarization and are the intersections of the Wave Surface with concentric spheres. / The strong lines are the principal sections.' See Number 320 note (8).

(6) J. Clerk Maxwell, 'On the cyclide', *Quarterly Journal of Pure and Applied Mathematics*, **11** (1867): 111–26 (= *Scientific Papers*, **2**: 144–59). The four cyclides here listed are described and illustrated in this paper: on the definition of the 'cyclide' he cites Charles Dupin, *Applications de Géométrie et de Mécanique* (Paris, 1822): 200–10.

VI is a horned cyclide with the skeleton curves and the lines thro which the circular sections pass dotted.
VII a ring cyclide
VIII a spindle cyclide
IX a parabolic ring cyclide.

Here is something which might be considerably extended.

Let A B C D be four bodies then if operation F acts between A and B and produces the effect E between C and D then will the same operation F acting between C & D produce the same effect E between A & B.

Examples of Operations	Effects
Tension or Pressure in the direction AB	Elongation or contraction in CD
Difference of Potential	Electric Current
Difference of Potential	Electric Charge

I have proved this kind of reciprocity for all elastic frameworks(7) (Case I) for systems of linear conductors such as the electric balance and others more complicated(8) (Case II) and Case III is Greens doctrine of the Greenish Function(9) as extended by Riemann(10) & others.(11) If there is any thing which has a rotatory property of conduction &c the prop. does not hold for it.

Yours truly
J. Clerk Maxwell

(7) See Number 273.

(8) See Maxwell, *Treatise*, **1**: 335–6 (§281).

(9) See Maxwell, *Treatise*, **1**: 91–2 (§§88–9) for his discussion of Green's reciprocity theorem and Green's function; and see George Green, *An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism* (Nottingham, 1828): 18–20 (= *Mathematical Papers of the Late George Green*, ed. N. M. Ferrers (London, 1871): 36–9).

(10) Bernhard Riemann, 'Ueber die Fortpflanzung ebener Luftwellen von endlicher Schwingungsweite', *Abhandlungen der Mathematischen Classe der Königlicher Gesellschaft der Wissenschaften zu Göttingen*, **8** (1860): 43–65.

(11) Maxwell may well have had in mind papers by Carl Neumann and Enrico Betti. See Carl Neumann, 'Ueber die Integration der partiellen Differential-gleichung: $\frac{\partial^2\phi}{\partial x^2}+\frac{\partial^2\phi}{\partial y^2}=0$', *Journal für die reine und angewandte Mathematik*, **59** (1861): 335–66, esp. 336–9, where Neumann introduces the 'Greensche Function' (denoted G, as in the *Treatise*, **1**: 113–15 (§101)), and where his discussion is similar to Maxwell's in the *Treatise*. Maxwell refers to Neumann's paper in the *Treatise*, **1**: 234 (§190); and see Number 337. See also Betti's discussion of the 'funzione di Green' in his 'Teorica delle forze che agiscono secondo la legge di Newton e sua applicazione alla elettricità statica', *Nuovo Cimento*, **18** (1863): 385–402; *ibid.*, **19** (1863): 59–75, 77–95, 149–75, 357–77; *ibid.* **20** (1864): 19–39, 121–41; see esp. *ibid.*, **19** (1863): 165–9. Maxwell refers to Betti's paper in his letter to Tait of 11 December 1867 (Number 277).

275

LETTER TO PETER GUTHRIE TAIT

13 NOVEMBER 1867

From the original in the University Library, Cambridge[1]

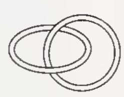
Glenlair
Dalbeattie
1867 Nov 13

Dear Tait

If you have any spare copies of your translation of Helmholtz on 'Water Twists'[2] I should be obliged to you if you could send me one.

I set the Helmholtz dogma to the Senate house in '66,[3] and got it very nearly done by some men, completely as to the calculation nearly as to the interpretation.

Thomson has set himself to spin the chains of destiny out of a fluid plenum[4] as M. Scott set an eminent person to spin ropes from the sea sand,[5] and I saw you had put your calculus in it too.[6] May you both prosper and disentangle your formulae in proportion as you entangle your wurbles. But I fear that the simplest *indivisible* whorl is either two embracing wurbles or a worble embracing itself.[7]

For a simple enclosed worble may be easily split and the parts separated but two embracing worbles preserve each others solidarity thus 

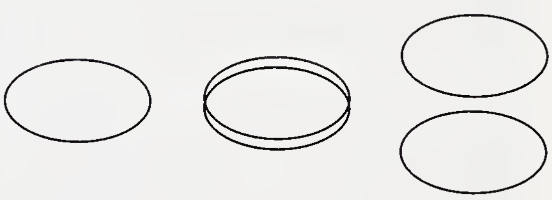


Figure 275,1

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(1) ULC Add. MSS 7655, I, b/6. Previously published (in part) in Knott, *Life of Tait*: 106.

(2) Hermann Helmholtz, 'On the integrals of the hydrodynamical equations, which express vortex-motion', *Phil. Mag.*, ser. 4, **33** (1867): 485–512; trans. by P. G. Tait of Helmholtz's paper 'Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen', *Journal für die reine und angewandte Mathematik*, **55** (1858): 25–55.

(3) Question 5 in the Mathematical Tripos paper on the afternoon of Friday 19 January 1866, confirmed by Maxwell's mark book (ULC Add. MSS 7655, V, k/8(iv)). See Number 254 note (1).

(4) William Thomson's paper 'On vortex motion', *Trans. Roy. Soc. Edinb.*, **25** (1869): 217–60 (= *Math. & Phys. Papers*, **4**: 13–66) was read to the Royal Society of Edinburgh on 29 April 1867; see *Proc. Roy. Soc., Edinb.*, **6** (1867): 167. See Number 295 note (2).

(5) Maxwell may be associating 'the wondrous Michael Scott' of Sir Walter Scott's *The Lay of the Last Minstrel* (canto II, xiii) with the legend recounted in Samuel Butler's *Hudibras* (part I, canto i, *ll.* 157–8): 'And, with as delicate a hand, / Could twist as tough a rope of sand.'

(6) In his paper 'On vortex atoms', *Proc. Roy. Soc. Edinb.*, **6** (1867): 94–105 (= *Math. & Phys. Papers*, **4**: 1–12) Thomson described Tait's demonstration of smoke rings, which had stimulated his speculations.

(7) See also Number 307 esp. note (7).

though each may split into many every one of the one set must embrace every one of the other.

So does a knotted one.

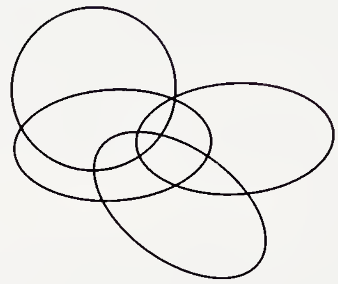


Figure 275,2

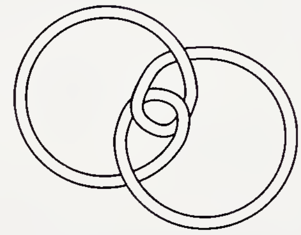
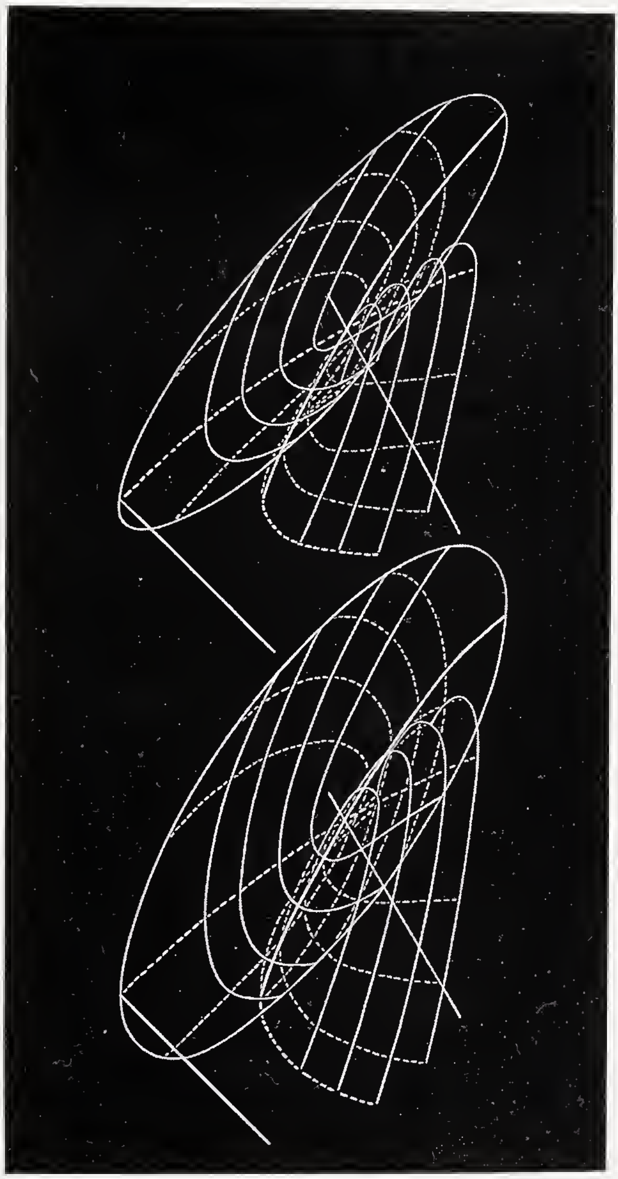


Figure 275,3

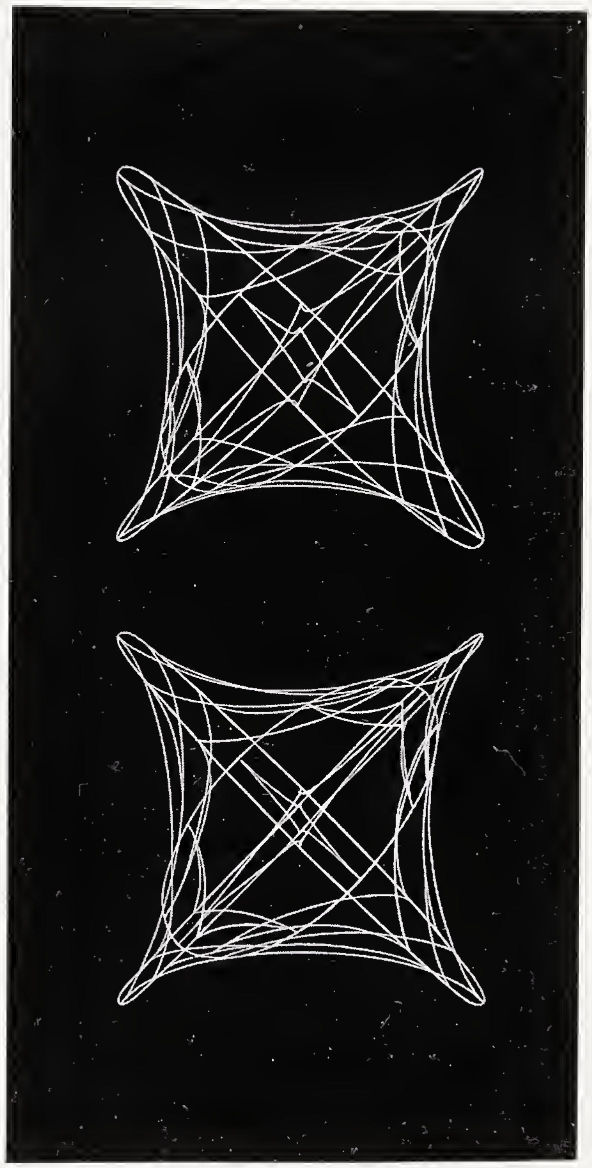
I send you one or two stereograms of Cyclides ellipsoids & a parabolic hyperboloid. I have several more which I will send you when they are more perfect. I have got the Wave Surface and a magnified view of the point of adhesion also the surface of centres of an ellipsoid, &c &c,(8) but the engraver has to make some improvements on them.

Yours truly
J. Clerk Maxwell

(8) See Number 274 and Plates VI, VII and VIII.



CONICAL POINT OF WAVE SURFACE.



CENTRES OF CURVATURE OF ELLIPSOID.

Plate VIII. Stereograms (1867) of conical point of wave surface and centres of curvature of ellipsoid (Number 275).

LETTER TO PETER GUTHRIE TAIT

4 DECEMBER 1867

From the original in the University Library, Cambridge(1)

Glenlair
Dalbeattie
1867 Dec 4

Dear Tait

I have yours of the 27th ult.(2)

Of course
$$x_1 = Ce^{-\frac{k}{2}t}\cos\left(\sqrt{n^2-\frac{k^2}{4}}\,t+\alpha\right)$$
$$y_1 = C'e^{-\frac{k}{2}t}\cos\left(\qquad\qquad +\beta\right)$$

gives your log. spiral when $C = C'$ & $\alpha+\beta = \frac{\pi}{2}$ where $\sqrt{\frac{k^2}{4}-n^2}$ is real, make it ω_2 for short,

---

(1) ULC Add. MSS 7655, I, b/7.

(2) In his letter of 27 November 1867 (ULC Add. MSS 7655, I, a/3) Tait had written: 'Dear Maxwell, / I was sorry not to meet you at Glasgow – for various reasons. / One *selfish* one I must state. A particle resisted *as the vel.*^{y}, and attracted *as the dist*^{ce} moves with uniform $\angle^r$ vel in a log. spiral. The projection of this on any line gives a very pretty geometrical solution of §342 of T & T′. But what does the curve become when $\frac{k^2}{4}-n^2$ is positive? $\left(\text{There is an error in §342: } \frac{k^2}{4}-n^2\right.$ being written for $\left.n^2-\frac{k^2}{4}\right)$. It seems to be a curve derived from a hyperbola as the log. spiral is derived from the circle. Whence the difficulty of giving a geometrical solution in this last case? What is the path? / Your note to T. has some little bearing on an experiment I had been getting ready for – i.e. making an electrified piece of vulcanite into a magnet by spinning it violently. I am nearly ready to make the trial, having got a multiplying train of great power. Can I do anything for you with it? / I am delighted to hear you are going to do a Senate-House Treatise on Electricity. The sooner the better. / Yours truly / P. G. Tait'. Tait's discussion, relating to §342 of Thomson and Tait, *Natural Philosophy*: 276, also relates to §6 of his 'Note on the hodograph', *Proc. Roy. Soc. Edinb.*, **6** (1867): 221–6, esp. 224–6 (read 16 December 1867): 'A point describes a logarithmic spiral with uniform angular velocity about the pole – find the acceleration'. In §342 of their *Natural Philosophy* Thomson and Tait consider the motion of a particle resisted as the velocity; the rate of retardation due to unit velocity is k; n^2 is the rate of acceleration when the displacement is unity; 'then the motion is of the oscillatory or non-oscillatory class according as $k < 2n$ or $k > 2n$'. See Tait's reply to Maxwell's letter (Number 277 note (2)). Maxwell subsequently applied Tait's treatment of the acceleration of a particle describing a logarithmic spiral with uniform angular velocity about the pole, in discussing the damped vibrations of a magnetic needle in *Treatise*, **2**: 336–43 (§§731–42). On the hodograph see Number 472 §1.

then $$x_2 = C e^{-\frac{k}{2}t} \cosh(\omega t + \alpha)$$

and if you choose your conditions

$$y = C e^{-\frac{k}{2}t} \sinh(\omega t + \alpha)$$

where $$\cosh\theta = \frac{1}{2}(e^{\theta} + e^{-\theta}) \ \& \ \sinh\theta = \frac{1}{2}(e^{\theta} - e^{-\theta})$$

above $$x_1^2 + y_1^2 = C^2 e^{-kt}$$

below $$x_2^2 - y_2^2 = C^2 e^{-kt}$$

above $$\frac{y}{x} = \tan\theta_1 = \tanh\omega_1 t$$

below $$\frac{y}{x} = \tan\phi_2 = \tanh\omega_2 t.$$

Therefore – In the 1st case make a set of circles whose radii are r_1 r_2 &c and a set of radii whose angles are θ_1 θ_2 &c.

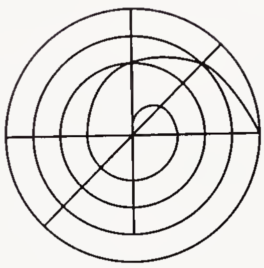


Figure 276,1

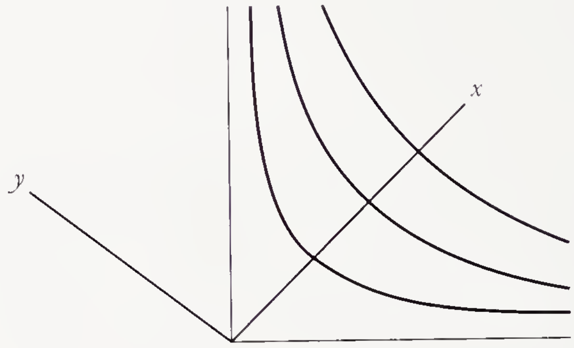


Figure 276,2

In the 2nd case make a set of hyperbolas whose eqns are

$$x_1^2 - y_1^2 = r_1^2$$
$$x_2^2 - y_2^2 = r_2^2$$

&c

and a set of radii whose angles are

$$\phi_1 = \tan^{-1}\frac{e^{\theta_1} - e^{-\theta_1}}{e^{\theta_1} + e^{-\theta_1}} \quad \phi_2 = \tan^{-1}\frac{e^{\theta_2} - e^{-\theta_2}}{e^{\theta_2} + e^{-\theta_2}}.$$

Then the first curve will lie among the circles and radii as the second lies among the hyperbolas and radii.

If this throws any light on the question well, but if not, as I think what does it signify.

I hope to hear of your vulcanite magnet drawing.(3) Thomson mentioned

(3) Tait's reference to Maxwell's 'note to T[homson]' (not extant) in relation to his projected experiment, may relate to Maxwell's discussion in the *Treatise*, **2**: 370 (§770) of the possibility

it in his letter to me of which the style was so entêté that there could be no doubt that it was not uninspired, or else T had been accented.(4)

At present I suppose that the superficial tension of water across a line 1 metre long

is 33.5 *grammes weight* at 43° Fah^{t} and
29 .... at 52°.

But I expect better results soon as at present my optical method is better than my readings and I cannot work with warm water on account of the steam dimming the glass. I expected more difficulty in the measurement of level, and used an indirect method by pouring measured quantities of water into a wide cylinder. I now find that I can measure the height of the surface directly and will require only a few drops to work on.(5)

Many thanks for your 3 pamphlets and for the page printed on one side from T & T′.(6)

I have amused myself with knotted curves for a day or two.(7) It follows from electromagnetism that if ds and $d\sigma$ are elements of two closed curves and r the distance between them and if $l\ m\ n$, $\lambda\ \mu\ \nu$, and $L\ M\ N$ are the direction cosines of $ds\ d\sigma$ & r respectively

then
$$\iint \frac{ds\,d\sigma}{r^2} \begin{vmatrix} L & M & N \\ l & m & n \\ \lambda & \mu & \nu \end{vmatrix}$$
$$= \iint \frac{ds\,d\sigma}{r^2}\left[\left(1-\left|\frac{dr}{ds}\right|^2\right)\left(1-\left|\frac{dr}{d\sigma}\right|^2\right)-\left(r\frac{d^2 r}{ds\,d\sigma}\right)^2\right]^{\frac{1}{2}}$$
$$= 4\pi n$$

that an electrified body in motion produces magnetic effects. Maxwell there discussed measuring the magnetic effect of a 'non-conducting disk revolving in the plane of the magnetic meridian'. Compare Henry Rowland's detection of the magnetic effects of the convection current; see 'Versuche über die elektromagnetische Wirkung elektrischer Convection', *Monatsberichte der Akademie der Wissenschaften zu Berlin* (1876): 211–16.

(4) That is: written by Tait (T′). See Number 277 note (2) for Tait's response, and Maxwell's further comments in his letter to Tait of 23 December 1867 (Number 278).

(5) See Number 292. On Tait's interest in surface tension see his paper 'On some capillary phenomena', *Proc. Roy. Soc., Edinb.*, 5 (1866): 593–4.

(6) Thomson and Tait, *Natural Philosophy*.

(7) Perhaps suggested by reading Helmholtz on 'vortex motion': see Number 275 esp. note (2). On Helmholtz's theorems which express the analogy between hydrodynamics and electromagnetism see Number 295 esp. note (7).

the integration being extended round both curves and n being the algebraical number of times that one curve embraces the other in the same direction.[(8)]

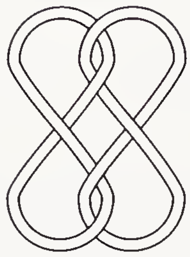


Figure 276,3

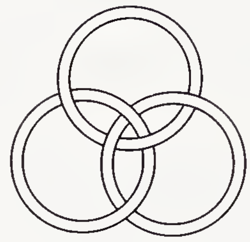


Figure 276,4

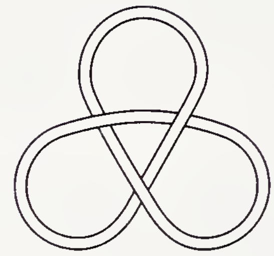


Figure 276,5

If the curves are not linked together $n = 0$ but if $n = 0$ the curves are not necessarily independent. In fig [276, 3] the two closed curves are inseparable but $n = 0$. In fig [276, 4] the 3 closed curves are inseparable but $n = 0$ for every pair of them. Fig [276, 5] is the simplest single knot on a single curve. The simplest equation I can find for it is $r = b + a\cos\frac{3}{2}\theta \quad z = c\sin\frac{3}{2}\theta$
when c is $-^{\text{ve}}$ as in the figure the knot is right handed
when c is $+^{\text{ve}}$ it is left handed.
A right handed knot cannot be changed into a left handed one.[(9)]

$$\text{The curve} \quad \begin{aligned} x &= \sin 2\theta \\ y &= \sin 3\theta \\ z &= \sin(5\theta + \gamma) \end{aligned}$$

is knotted in different degrees according to the value of γ. When $\gamma = 0$ it is not knotted at all when $\gamma = \dfrac{\pi}{3}$ it begins to be knotted and when $\gamma = \dfrac{7}{12}\pi$ it is knotted in a different way but to the same degree.

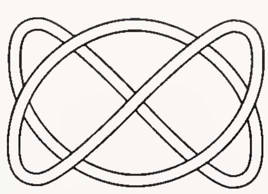


Figure 276,6. $\gamma = 0$ no knot

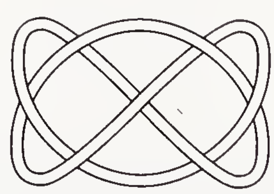


Figure 276,7. $\gamma = \frac{\pi}{3}$

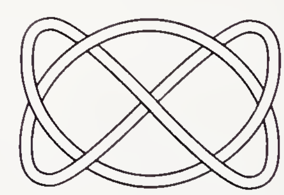


Figure 276,8. $\gamma = \frac{7}{12}\pi$

(8) See also Number 318. For further discussion see the *Treatise*, **2**: 40–1 (§421), where Maxwell makes reference to Gauss' discovery of this integral and discussion of the 'Geometry of Position': see Gauss, *Werke*, **5** (Göttingen, 1867): 605. On the electromagnetic interpretation of this integral, discussed by Maxwell in the *Treatise* §421, see 'Theorem A' of Maxwell's 'Note on the electromagnetic theory of light', in *Phil. Trans.*, **158** (1868): 652–7, esp. 653–4 (= *Scientific Papers*, **2**: 138–9).

(9) On the topology of knots see Johann Benedict Listing, 'Vorstudien zur Topologie', in *Göttinger Studien. 1847. Erste Abtheilung: Mathematische und naturwissenschaftliche Abhandlungen* (Göttingen, 1847): 811–75, esp. 862–4. See Number 370 notes (6) and (7).

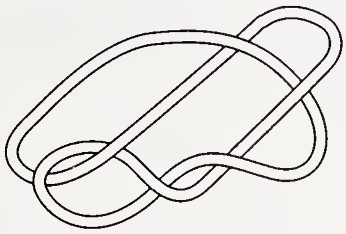


Figure 276,9. $\gamma = \frac{\pi}{3}$, a knot = a righthanded twist of $4\frac{1}{2}\pi$ and then the ends linked together.

Figure 276,10. The twist of $\frac{9}{2}\pi$

Yours truly
J. Clerk Maxwell

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LETTER TO PETER GUTHRIE TAIT

11 DECEMBER 1867

From the original in the University Library, Cambridge(1)

Glenlair
Dalbeattie
1867 Dec 11

Dear Tait

Here is a different construction.(2) Of course a rotatory construction like your log spiral will not do so let us make time a straight line instead of a circle & become progressionists instead of cyclicists let space be vertical time horizontal and let x be the space from origin

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(1) ULC Add. MSS 7655, I, b/8. Published in part in Knott, *Life of Tait*: 213–14.

(2) Reply to Tait's letter (a reply to Number 276) of 6 December 1867 (ULC Add. MSS 7655, I, a/4). 'Dear Maxwell, / Many thanks for your letter, though you don't solve my difficulty about giving frictional or other resistance its true place by *geometrical* methods when too strong. I have tried several times, and got one (luckily the most important case) but the other is hopelessly complex. / Please to remember that you are a fellow of the R.S.E., and be good enough to send us a paper on Knots & their possible equations in 3 dimensions. We devised all your figures (and many more) long ago – (Crum Brown & I, working for Thomson) – but we never tried EQUATIONS. Give us a paper on them like a good fellow; whether for the *Trans.* or merely for the *Proc.*. / Give me also a reference as to your capillary investigations – for I am about to establish a working Laboratory for students, and will be delighted to get any hints as to keeping them to work & *useful* work. / Thomson's letter *was* rather ambiguous, seeing that I dictated one half & he the other, and that it was not written by him but by his nephew, whose hand is somewhat similar to his. / Nevertheless I hope your Treatise on Electricity will go on soon – whips & scorpions notwithstanding. / HALF the edition of our first vol. of Nat. Phil. is already sold!!! It was published only 3 months ago. / You may understand my desire for a solution of the COSH question when I tell you that it would be extremely useful in our smaller volume now going through the press. / Are you sufficiently up to the history of Thermodynamics to critically examine & put right a little treatise I am about to print – and will you kindly apply your critical powers to it? / You would greatly oblige me by doing so, as Clausius & others have cut up very rough about bits referring to them. I dont pretend to know the subject thoroughly and would be glad of your help. The fact seems to me to be that both Clausius & Rankine are about as obscure in their writings as anyone can well be. / Shall we never see you in Edin[h]? / Yours ever, / P. G. Tait / P.S. Ponder this proposition. A man of your *originality*, and *fertility*, and *leisure*, is undoubtedly bound to furnish to the chief Society of his native land, numerous papers, however short.' See Thomson and Tait's treatment of the motion of a particle in a logarithmic spiral in their *Elements of Natural Philosophy. Part I* (Oxford, 1873): 95–6, where they reprint §6 of Tait's 'Note on the hodograph' (see Number 276 note (2)). On the circumstances surrounding the publication of Tait's *Sketch of Thermodynamics* (Edinburgh, 1868) see Number 278 note (2). In seeking to solicit a paper from Maxwell for the Royal Society of Edinburgh, Tait was writing as one of the Secretaries of the Society; see *Proc. Roy. Soc. Edinb.*, **6** (1867): 173.

$$y = x + a\frac{dx}{dt}$$

$$z = y + b\frac{dz}{dt}$$

$$= x + (a+b)\frac{dx}{dt} + ab\frac{d^2x}{dt^2}.$$

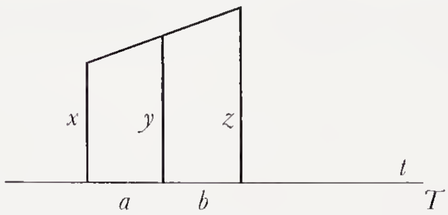


Figure 277,1

Now let us take your case in which

$$\frac{d^2x}{dt^2} = -n^2x - k\frac{dx}{dt}$$

then $z = (1-abn^2)\,x + (a+b-abk)\dfrac{dx}{dt}$.

If
$$a = \frac{1}{2}\frac{k}{n^2} \pm \frac{\sqrt{k^2-4n^2}}{2n^2}$$
$$b = \frac{1}{2}\frac{k}{n^2} \mp \frac{\sqrt{k^2-4n^2}}{2n^2}$$

then $z = 0$ always.
Hence the following construction.
Let OP be the original value of x.
Draw PQ horizontal = unit of time
QR vertical = initial velocity
and join PR & produce.
Make $OA' = a$ $OB' = b$ $OC = a+b$
Draw $B'B$ $A'A$ vertical Join BC AC

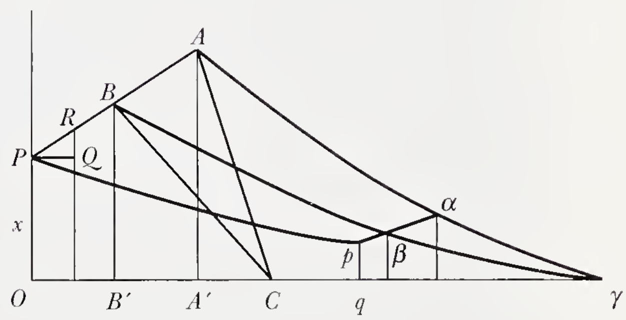


Figure 277,2

From A draw the logarithmic curve $A\alpha$ with const. subtangent $= b$.
From B draw the log curve $B\beta$ with subtangent a.
Let $\gamma\alpha$ & $\gamma\beta$ be tangents from the same point γ on the axis.
Join $\alpha\,\beta$ & produce. Make $\gamma q = a+b$ and draw qp to meet $\alpha\beta$. p is on a curve such that if $Oq = t$ $qp = x$.

To draw the log. curves exactly.
Draw vertical lines at equal intervals h.
Let P_1 be the beginning of the curve. Make

$$\alpha = \frac{h}{1-e^{\frac{-h}{a}}}$$

where a is the true subtangent.

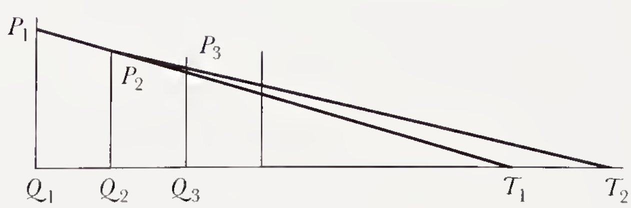


Figure 277,3

Make $Q_1 T_1 = \alpha_1$. Join $P_1 T_1$ cutting Q_2 in P_2. P_2 is a point in the curve. Make $Q_2 T_2 = \alpha_2$ & so on. This gives a series of points in the true curve.

The best way to draw a catenary is to draw two log curves in this way join corresponding points PP' and bisect PP' in Q. Q is a point in the catenary & PP' is a tangent if the log curves are properly drawn wh: is easy.

It is good in drawing curves to have tangents ready made, since a point & its tangent is as good as two points in theory & better in practice.

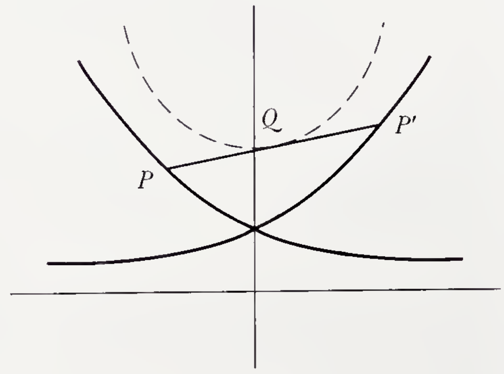


Figure 277,4

With regard to knots I have drawn stereoscopically $x = \sin 2\theta$ $y = \sin 3\theta$ $z = \cos 7\theta$ which is the first case of a real web that I have got.(3)

If the middle crossing be reversed  for  it becomes a knot of the simplest kind.

I have not got any R.S.E. matter on this but if they would like could knit het again. I have considerably improved the theory of reciprocal rectilinear figures & diagrams of forces, which appeared in Phil Mag. Ap. 1864.(4)

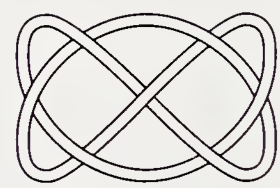


Figure 277,5

I am glad people are buying T & T′. May it sink into their bones! I shall not see it till I go to London. I believe you call Laplaces Coeffts Spherical Harmonics.(5) Good. Do you know that every Sp. Harm. of degree n has n *axes*? I did not till recently. When you know the directions of the axes (or their poles on the sphere) you have got your harmonic all but its strength.(6) For one of the 2nd degree they are the poles of the two circular equipotential lines on the sphere. I have a picture of them.(7)

I do not know in a controversial manner the history of thermodynamics, that is I could make no assertions about the priority of authors without

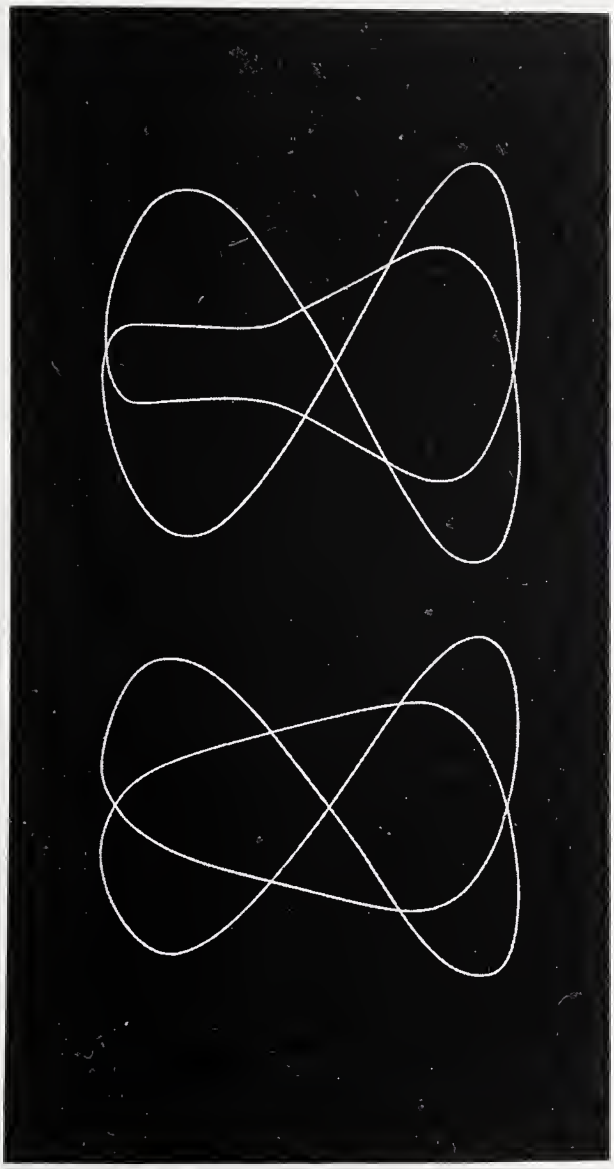
(3) See Plate IX and Number 279: Appendix.

(4) See Number 273, esp. note (4).

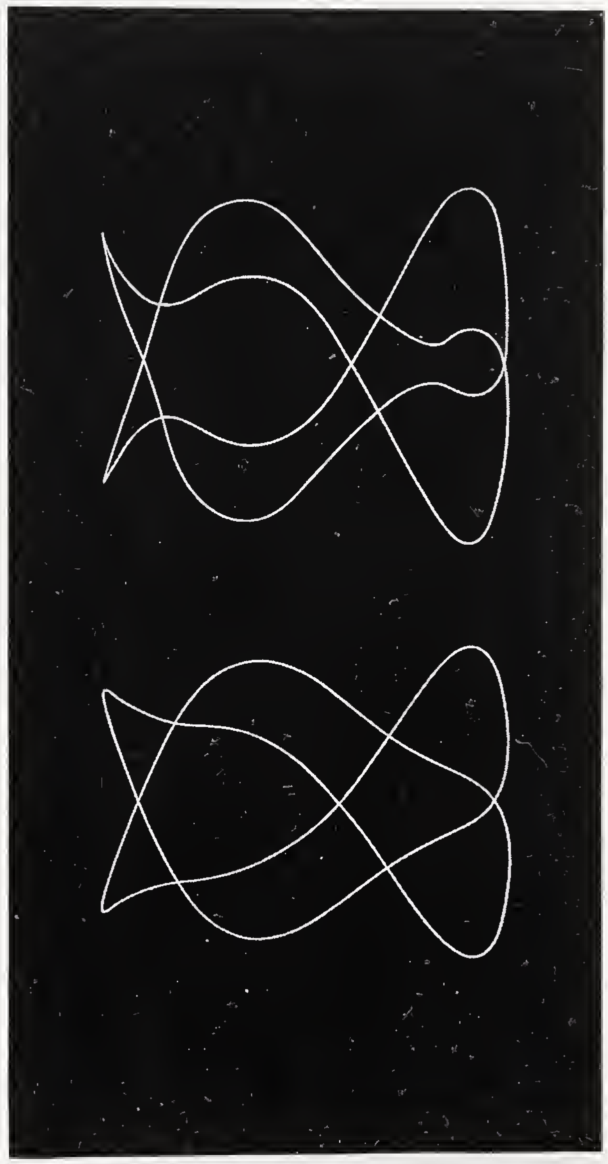
(5) See Number 262, esp. notes (8) and (10). See Thomson and Tait, *Natural Philosophy*: 140; 'The mathematical method of "Laplace's Co-efficients"... here called *spherical harmonic analysis*'.

(6) See Maxwell's discussion in the *Treatise*, **1**: 157–80, esp. 162 (§§128–46, esp. §131), where he refers to Gauss' note on 'Geometrische Bedeutung der Kugelfunctionen', published in his *Werke*, **5** (Göttingen, 1867): 631 (and see Number 276 note (8)). See Numbers 281, 293, 294 and 295.

(7) See Plate X and Number 279: Appendix.

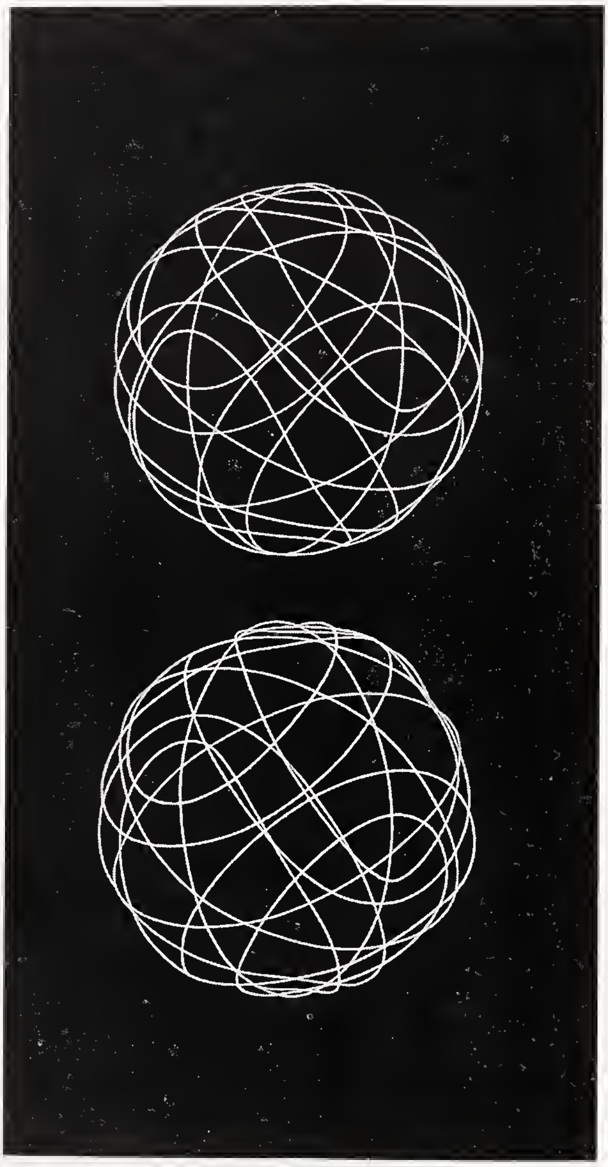


GORDIAN KNOT (2. 3. 5.)

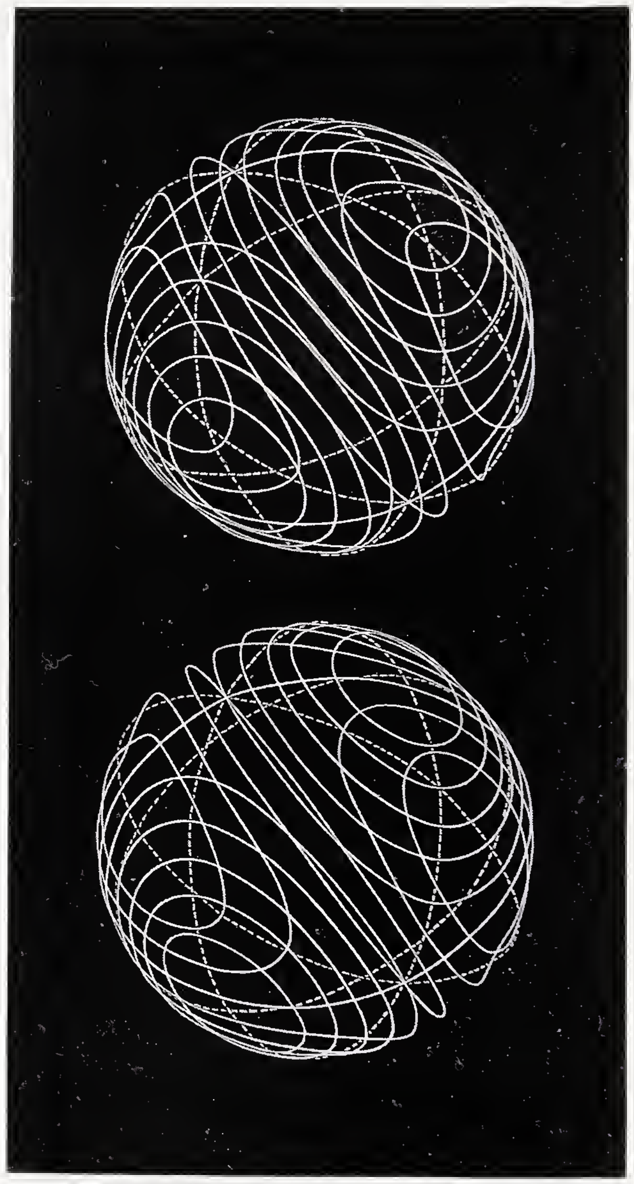


GORDIAN KNOT (2. 3. 7.)

Plate IX. Stereograms (1867) of Gordian knots (Number 277).



CONFOCAL SPHERICAL ELLIPSES.



CONCYCLIC SPHERICAL ELLIPSES.

Plate X. Stereograms (1867) of confocal spherical ellipses and concyclic spherical ellipses, showing spherical harmonics of the second degree (Number 277).

referring to their actual works. If I can help you in any way with your book I shall be glad, as any contributions I could make to that study are in the way of altering the point of view here & there for clearness or variety and picking holes here & there to ensure strength & stability.

As for instance I think that you might make something of the theory of absolute scale of temperature[(8)] by reasoning pretty loud about it and paying it due honour, at its entrance. To pick a hole – say in the 2[nd] law of Θ[cs],[(9)] that if two things are in contact the hotter cannot take heat from the colder without external agency.[(10)]

Now[(a)] let *A* & *B* be two vessels divided by a diaphragm and let them contain elastic molecules in a state of agitation which strike each other and the sides.

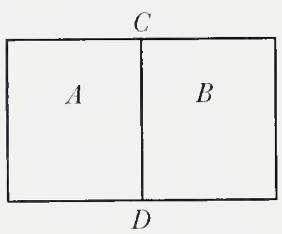


Figure 277,6

Let the number of particles be equal in *A* & *B* but let those in *A* have the greatest energy of motion. Then even if all the molecules in *A* have equal velocities, if oblique collisions occur between them their velocities will become unequal & I have shown that there will be velocities of all magnitudes in *A* and the same in *B* only the sum of the squares of the velocities is greater in *A* than in *B*.[(11)]

When a molecule is reflected from the fixed diaphragm *CD* no work is lost or gained.

If the molecule instead of being reflected were allowed to go through a hole

---

(a) {Thomson} All very well now. But when is then?

(8) See Number 207 note (15).

(9) The term 'thermo-dynamics' was first used by William Thomson, 'On the dynamical theory of heat. Part V. Thermo-electric currents', *Trans. Roy. Soc. Edinb.*, **21** (1854): 123–71, on 123 (= *Math. & Phys. Papers*, **1**: 232). The terms 'thermodynamics' and the 'second law of thermodynamics' became conventional; see W. J. M. Rankine *A Manual of the Steam Engine* (London/Glasgow, 1859): 299, 307. For Thomson's statement of the second law of thermodynamics see his paper 'On the dynamical theory of heat', *Trans. Roy. Soc. Edinb.*, **20** (1851): 261–88, esp. 265 (= *Math. & Phys. Papers*, **1**: 179): 'It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects.'

(10) Compare Thomson's statement of Clausius' version of the second law of thermodynamics: 'It is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature'; Thomson, 'On the dynamical theory of heat': 266 (= *Math. & Phys. Papers*, **1**: 181). Thomson declared that the two forms of the second law of thermodynamics were logically equivalent.

(11) In his papers 'Illustrations of the dynamical theory of gases', *Phil. Mag.*, ser. 4, **19** (1860): 19–32; *ibid.*, **20** (1860): 21–37 (= *Scientific Papers*, **1**: 377–409); and 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88 (= *Scientific Papers*, **2**: 26–78).

in CD no work would be lost or gained, only its energy would be transferred from the one vessel to the other.

Now conceive a finite being[(12)] who knows the paths and velocities of all the molecules by simple inspection but who can do no work, except to open and close a hole in the diaphragm, by means of a slide without mass.

Let him first observe the molecules in A and when he sees one coming the square of whose velocity is less than the mean sq. vel. of the molecules in B let him open the hole & let it go into B. Next let him watch for a molecule in B the square of whose velocity is greater than the mean sq. vel. in A and when it comes to the hole let him draw the slide & let it go into A, keeping the slide shut for all other molecules.

Then the number of molecules in A & B are the same as at first but the energy in A is increased and that in B diminished that is the hot system has got hotter and the cold colder & yet no work has been done, only the intelligence of a very observant and neat fingered being has been employed.

Or in short if heat is the motion of finite portions of matter and if we can apply tools to such portions of matter so as to deal with them separately then we can take advantage of the different motion of different portions to restore a uniformly hot system to unequal temperatures or to motions of large masses. Only we can't, not being clever enough.[(b)]

Is your book on Quaternions out yet?[(13)]

I see $$\lhd = i\frac{d}{dx}+j\frac{d}{dy}+k\frac{d}{dz}$$

and $$V = \lhd F\rho$$

(b) {Thomson} Very good. Another way is to reverse the motion of every particle of the universe and preside over the unstable motion thus produced.[(14)]

(12) William Thomson's term 'demon' – see his 'The kinetic theory of the dissipation of energy', *Nature*, **9** (1874): 441–4, esp. 442n (= *Math. & Phys. Papers*, **5**: 12n) where he ascribes the term to Maxwell – did not receive Maxwell's approbation. In an undated note to Tait (ULC Add. MSS 7655, V, i/11a), which will be published in Volume III (and see Knott, *Life of Tait*: 215), he suggested that Tait 'Call him no more a demon but a valve'; see the Introduction esp. notes (52) and (53). For Maxwell's subsequent discussion of this argument, aimed to show that the second law of thermodynamics is an irreducibly statistical law which applies to systems of molecules, not to the spontaneous fluctuations of individual molecules, see his letter to J. W. Strutt of 6 December 1870 (Number 350) and the *Theory of Heat* (London, 1871): 308–9 (the draft version of this portion of its text being reproduced in Number 350: Appendix).

(13) P. G. Tait, *An Elementary Treatise on Quaternions* (Oxford 1867).

(14) See Thomson's subsequent discussion in his paper 'The kinetic theory of the dissipation of energy'; if 'the motion of every particle in the universe were precisely reversed at any instant, the course of nature would be simply reversed for ever after'. See Number 350 note (4). For Maxwell's discussions of time-reversal see Numbers 286 esp. note (12) and 350.

in your paper in RSE Proceedings 1862

also $$\lhd^2 = -\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right).^{(15)}$$

Lamé (Fonctions Inverses)(16) & (Coordonnées Curvilignes)(17) calls V the 1st differential parameter
and $\Delta^2 F\rho$ the 2nd diffl paramr(18) with other names besides. Good. But –
Betti Nuovo Cimento 1866(19) a good mathematician

calls $$\Delta^2 V = \frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+\frac{d^2V}{dz^2}$$

and $$\Delta V = \overline{\frac{dV}{dx}}\Big|^2+\overline{\frac{dV}{dy}}\Big|^2+\overline{\frac{dV}{dz}}\Big|^{2\,(20)}$$

which is, to say the least, an atrocious combination. Is there any virtue in turning Δ round 30°?(21)

(c)Yours truly
(d)J. CLERK MAXWELL(22)

(c) {Thomson} I should prefer $(+\frac{1}{12}\Delta)$. But the truth is ∇^2 (NB on even keel).
(d) {Tait} C over ☞
O.T. Tatlock(23) has returned the proof(24) corrected & somewhat changed by Rankine. Do you now adopt it, and may I print off with a clear conscience? If not pray Criticize fully (in writing) his remarks on Clausius,(25) that U & the Jewel(26) may get justice. Also say what occurs to you about the enclosed WHICH RETURN speedily. The *mixing* of the gases which is an objection to the text may easily be got over by waiting longer till two (one from each vessel) impinge centrically. What then? / Yrs T′.

(15) P. G. Tait, 'Formulae connected with small continuous displacements of the particles of a medium', *Proc. Roy. Soc. Edinb.*, **4** (1862): 617–23; i, j, k are unit vectors at right angles to each other.

(16) Gabriel Lamé, *Leçons sur les Fonctions Inverses des Transcendantes et les Surfaces Isothermes* (Paris, 1857): 2. See also Maxwell, *Treatise*, **1**: 181–90 (§§147–54) for substantial reference to Lamé's book.

(17) Gabriel Lamé, *Leçons sur les Coordonnées Curvilignes et leurs Diverses Applications* (Paris, 1859): 6.

(18) Lamé, *Coordonnées Curvilignes*: 6; 'paramètres differentiels du premier ordre, et du second ordre, de la fonction-de-point F. On peut les désigner par les symboles $\Delta_1 F$ et $\Delta_2 F$.'

(19) Enrico Betti, 'Teorica delle forze che agiscono secondo la legge di Newton e sua applicazione alla elettricità statica', *Nuovo Cimento*, **18** (1863): 385–402; *ibid.*, **19** (1863): 59–75, 77–95, 149–75, 357–77; *ibid.*, **20** (1864): 19–39, 121–41.

(20) Betti, 'Teorica delle forze', *Nuovo Cimento*, **18** (1863): 389; *ibid.*, **19** (1863): 68.

(21) In his *Quaternions*: 221, 267, 307 Tait wrote ∇. See Number 347.

(22) Tait's reply is dated 13 December 1867 (ULC Add. MSS 7655, I, a/5): 'P.S. We are open to an offer from you to give us an evening's discourse on any subject you like (scientific of course). Name your day & your subject, and we'll book it eagerly. / Dear Maxwell, / 1) Can't you contrive, like your neighbour Dudgeon of Carssen, or like Fox Talbot, to spend a winter now

& then in Edin[h]? In that case we should at once put you on the Council of the R.S.E. and get some good out of you. Also you should have the run of my laboratory (which is shortly to be considerably increased) as well as those of Playfair, Crum Brown, &c. Ponder the point. Good. / 2) Thanks for time horizontal, &[c] & the dodge about drawing logarithmic curves, which may be very useful to me. I fear I can't avail myself of your system for our *elementary* book, for w[h] my spiral is well fitted. / 3) I read the extract of y[r] note about a paper on "Theory of reciprocal rectil[r] figs. & diagrams of forces" to the Council R.S.E. this afternoon; and you are booked for the paper as soon as you can send it. / 4) I don't see why you shouldn't send us a "Note" on "Nots" with a stereogram or two for our Proceedings. It would give you no trouble to do this – though it might bother you to get ready a paper for the Transactions. / 5) I suppose your tension of films goes to the London R.S. – else we should be glad of a few scraps. / 6) I object to your infinitely sharp individual that he *lets his gases mix*, and so spoils the theorem. But let him wait long enough to catch a quick one from the colder medium & a slow one from the hotter w[h] are moving in the same line so as to impinge centrically when he moves the slide. How many Darwinian ages will that require? And, when he has caught these two, won't he have to wait longer for a repetition? Good. / 7) The Quaternions have been out just as long as THE Book, but I have not yet heard how they sell, and whether they have sold anybody yet. / 8) Δ is required in 4[ions] for its finite diff[ce] meaning – so we do $\triangleleft$ or ∇ for the flux. I didn't know that Lamé had a $\triangleleft$, though I knew he had a $\triangleleft^2 = \left(\frac{d}{dx}\right)^2 + \ldots$, and a δ (I think) for Betti's $\left(\frac{dF}{dx}\right)^2 + \ldots$. But it is a long time I looked at his papers. Still if he had a $\triangleleft$, and not merely separate parts of it such as $\frac{dF}{dx}$, &[c] *he must have anticipated Hamilton in discovering 4[ions]*, so I wish you would give me the reference. / 9) If you read the last 20 or 30 pages of my book I think you will see that 4[ions] are worth getting up, for there it is shown that they go into that $\triangleleft$ business like greased lightning. Unfortunately I cannot find time to work steadily at them. / 10) I'll send you a copy of my first two Chapters on Thermodynamics in a day or two, when I hear from Rankine, meanwhile let me know your mind on the graver subjects propounded at the threshold of this note. / y[rs] / P. G. Tait'. Tait is alluding to 'Chapter XI. Physical applications' of his *Quaternions*: 276–311.

(23) See Number 332 note (2).

(24) Of Tait's *Sketch of Thermodynamics*: see Number 278 note (2).

(25) See Tait's discussion of Rankine's 'Thermodynamic function' and Clausius' 'Aequivalenzwerth' as published in his *Sketch of Thermodyamics*: 29. In the second edition of the *Sketch of Thermodynamics* (Edinburgh, $_2$1877): xv Tait admitted that the first edition contained 'paragraphs written for me by Rankine'. See also Number 278 note (2).

(26) James Prescott Joule.

LETTER TO PETER GUTHRIE TAIT

23 DECEMBER 1867

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
1867 Dec 23

Dear Tait

I have received your histories of Thermodynamics & Energetics,[2] and will examine them, along with Robertson on the Unconditioned[3] who holds that our ultimate hope of sanity lies in sticking to metaphysics and letting physics go down the wind.

I have read some metaphysics of various kinds and find it more or less ignorant discussion of mathematical and physical principles, jumbled with a little physiology of the senses. The value of the metaphysics is equal to the mathematical and physical knowledge of the author divided by his confidence in reasoning from the names of things.[4]

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(1) ULC Add. MSS 7655, I, b/9. Published in part in Knott, *Life of Tait*: 215–16.

(2) Two draft chapters of Tait's *Sketch of Thermodynamics* (Edinburgh, 1868), the 'Historical sketch of the dynamical theory of heat' and the 'Historical sketch of the science of energy', were 'printed privately for class use in 1867', according to Knott, *Life of Tait*: 213. These were, presumably, the texts of the 'histories' which Maxwell acknowledged receiving. Tait had already sent copies to Helmholtz, Rankine and Clausius. Helmholtz replied by defending Julius Robert Mayer's contribution to the formulation of the principle of the conservation of energy (see Tait, *Sketch of Thermodynamics*: v–vii). Rankine amended Tait's text (see Number 277 note (25)); while Clausius, as Tait put it, 'cut up very rough' (see Number 277 note (2)). Clausius gave an account of the 'Tendenz des Buches "*Sketch of Thermodynamics*" von Tait' in his *Die mechanische Wärmetheorie*, 2 vols. (Braunschweig, 1876–9), **2**: 324–30. These two draft chapters of Tait's *Sketch of Thermodynamics* were based on two articles, 'The dynamical theory of heat' and 'Energy', published in the *North British Review*, **40** (1864): 40–69, 337–68.

(3) Alexander Robertson, *The Philosophy of the Unconditioned* (London, 1866), a response to John Stuart Mill, *An Examination of Sir William Hamilton's Philosophy and of the Principal Philosophical Questions discussed in his Writings* (London, 1865). The reviewer (probably H. L. Mansel) in the *Contemporary Review*, **2** (1866): 584–9 criticised Robertson's philosophical incompetence. For a sophisticated defence of Hamilton see [H. L. Mansel,] 'The philosophy of the conditioned: Sir William Hamilton and John Stuart Mill', *Contemporary Review*, **1** (1866): 31–49, 185–219.

(4) Tait scathingly dismissed metaphysical arguments on 'what is heat?' and any 'metaphysical pretender to discovery of the laws of nature' in his *Sketch of Thermodynamics*: 1–2. Compare Maxwell's comments on the 'obtrusive antinomies' in Tait's denunciation of metaphysics (while himself making implicit appeal to metaphysical principles), in his review of the second (1877) edition of 'Tait's "Thermodynamics"', *Nature*, **17** (1878): 257–9, 278–80, esp. 257 (= *Scientific Papers*, **2**: 661).

You have also some remarks on the sensational system of philosophising (sensational in the American not the psychological sense). Beware also of the hierophantic or mystagogic style. The sensationalist says 'I am now going to grapple with the Forces of the Universe and if I succeed in this extremely delicate experiment you will see for yourselves exactly how the world is kept going'. The Hierophant says 'I do not expect to make you or the like of you understand a word of what I say, but you may see for yourselves in what a mass of absurdity the subject is involved'.

Your statement however seems tolerably complete considering the number of pages. One or two ideas should be brought in with greater pomp of entry, perhaps.

I do not understand how Verdets discovery that paramagnetic bodies produce rotation of the plane of polarization in the opposite direction to diamagnetic(5) bodies *confirms* Faradays doctrine that a diamagnetic body is only less paramagnetic than the field.(6)

It is a pretty doctrine, but I do not think Faraday thought it certain and Verdets phenomenon appears to me the strongest thing against it. I am myself sorry to part with it.(7)

Webers doctrine is that paramagnetic bodies have ready made electromagnets in them which are set in one direction by magnetic force, and therefore when they are all set parallel there is a limit to magnetization and that diamagnetics have currents set up in them by induction and that these are unopposed by resistance and these will give an opposite polarity.(8)

I do not say this theory is true(9) but if it were Thomsons revolving

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(5) Émile Verdet, 'Recherches sur les propriétés optiques développées dans les corps transparentes par l'action du magnétisme', *Ann. Chim. Phys.*, ser. 3, **52** (1858): 129–63.

(6) Michael Faraday, 'Experimental researches in electricity. – Twenty-sixth series. Magnetic conducting power', *Phil. Trans.*, **141** (1851): 29–84 (= *Electricity*, **3**: 200–73).

(7) See Maxwell's comment on Faraday's theory in 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83, esp. 45 (= *Scientific Papers*, **1**: 180), as 'the most precise, and at the same time the least theoretic statement'. See Maxwell's further discussion in his letter to Thomson of 18 July 1868 (Number 295).

(8) Wilhelm Weber, 'Ueber die Erregung und Wirkung des Diamagnetismus nach den Gesetzen inducirter Ströme', *Ann. Phys.*, **73** (1848): 241–56 (= *Wilhelm Weber's Werke*, 6 vols (Berlin, 1892–4), **3**: 255–68), (trans.) 'On the excitation and action of diamagnetism according to the laws of induced currents', *Scientific Memoirs*, ed. R. Taylor, **5** (London, 1852): 477–88; Weber, 'Ueber der Zusammenhang der Lehre vom Diamagnetismus mit der Lehre von dem Magnetismus und der Elektricität', *Ann. Phys.*, **87** (1852): 145–89 (= *Werke*, **3**: 555–90), (trans.) 'On the connection of diamagnetism with magnetism and electricity', *Scientific Memoirs, Natural Philosophy*, ed. J. Tyndall and W. Francis (London, 1853): 163–99. See Volume I: 363n.

(9) See Maxwell's positive discussion of Weber's theory in his chapter on 'Ferromagnetism and diamagnetism explained by molecular currents' in the *Treatise*, **2**: 418–25 (§§832–45).

diamagnetic sphere would not be a prime mover(10) any more than any other electromagnetic machine consisting of coils revolving in the presence of magnets.

There is a difference between a vortex theory ascribed to Maxwell(11) at p 57, and a dynamical theory of Electromagnetics by the same author in Phil Trans 1865.(12) The former is built up to show that the phenomena are such as can be explained by mechanism. The nature of this mechanism is to the true mechanism what an orrery is to the Solar System.(13) The latter is built on Lagranges Dynamical Equation and is not wise about vortices.(14) Examine the first part which treats of the mutual actions of currents before you decide that Webers is the only hypothesis on the subject.(15)

I hope you will come to some result with your vulcanite magnet.

It will require a great speed and you will require to guard the testing magnet from direct electromagnetic action of the revolving machinery.(16)

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(10) William Thomson, 'On the theory of magnetic induction in crystalline and non-crystalline substances', *Phil. Mag.*, ser. 4, **1** (1851): 179–86, esp. 186 (= *Electrostatics and Magnetism*: 465–80); 'a sphere of matter of any kind, placed in a uniform field of force, and set to turn round an axis fixed perpendicular to the lines of force, cannot be an inexhaustible source of mechanical effect.' See Maxwell's discussion in 'On Faraday's lines of force': 74–6 (= *Scientific Papers*, **1**: 217–19); and Volume I: 416–17, 485.

(11) J. C. Maxwell, 'On physical lines of force', *Phil. Mag.*, ser. 4, **21** (1861): 161–75, 281–91, 338–48; *ibid.*, **23** (1862): 12–24, 85–95 (= *Scientific Papers*, **1**: 451–513). Compare Maxwell's comment in Number 235.

(12) J. Clerk Maxwell, 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512 (= *Scientific Papers*, **1**: 526–97). See Number 238.

(13) Compare A. W. Williamson, 'On the constitution of salts', *Journal of the Chemical Society*, **4** (1852): 350–5, on 351; '[Formulae] may be used as an actual image of what we rationally suppose to be the arrangement of the constituent atoms in a compound, as an orrery is an image of what we conclude to be the arrangement of our planetary system.' This passage is cited by B. C. Brodie, 'The calculus of chemical operations; being a method for the investigation by means of symbols, of the laws of the distribution of weight in chemical change. Part I. On the construction of chemical symbols', *Phil. Trans.*, **156** (1866): 781–859, on 783n. Maxwell had participated in the Chemical Society discussion of Brodie's chemical calculus held on 6 June 1867 (Number 270).

(14) Compare the published text of Tait's *Sketch of Thermodynamics*: 74–5. Alluding to Maxwell's 'particular hypotheses as to molecular vortices... [he observed that Maxwell] seems, however, to have since discarded these hypotheses, and to rely only on the principle of energy applied to investigate the properties of the medium... [and] Lagrange's dynamical equation'.

(15) Wilhelm Weber, 'Elektrodynamische Maassbestimmungen, über ein allgemeines Grundgesetz der elektrischen Wirkung', *Abhandlung bei Begründung der Königlichen Sächsischen Gesellschaft der Wissenschaften... Leipzig* (1846): 211–378 (= *Werke*, **3**: 25–214). See Volume I: 305–6n.

(16) See Maxwell's letter to Tait of 4 December 1867 (Number 276, esp. note (3)).

You wrote me about experiments in the Laboratory. There is one which is of a high order but yet I think within the means and powers of students namely the determination of Joules coefft[(17)] by means of mercury.[(18)] Mercury is $\frac{13.57}{.033}$[(19)] times better than water so that about 9 feet would give 10° Fah. You have a cistern which you keep full to a certain point with mercury by proper fillers and a tube of iron with a strong iron cistern at the bottom into which is let a place to put mercury & dip a thermometer without exposing it to pressure. Out of this is a tube which enters an open cistern and out of this tube the mercury escapes either through a series of plates each having a fine hole in it or through a plug of compressed cotton or otherwise.

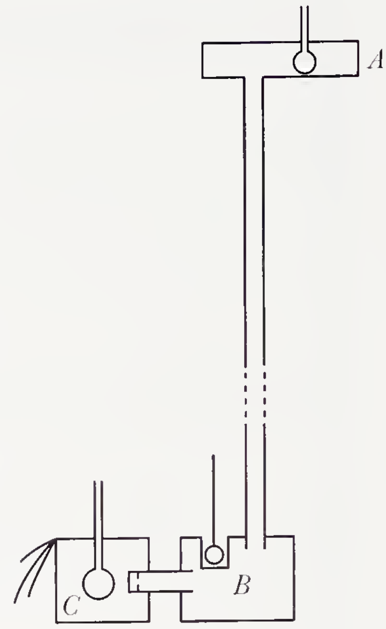

Figure 278,1

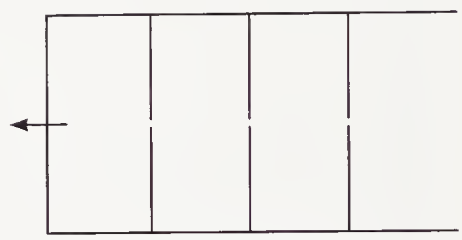
Figure 278,2

The mercury and the tube is thus heated but the heat is all communicated (in time) to the mercury for the part conducted back by the sides of the tube must be small and can be estimated.

The mercury rises and pours over a notch in the open cistern below and students continually carry it up to the upper cistern mixing it with very cold mercury to keep the temperature even.

The experiments required are 1 a comparison of the temperature at *A* and *B* when the mercury flows pretty quick to determine the effect of pressure in altering temperature.

2 Rate of cooling (or heating) of *B* & *C* with given difference of temperature from air

3 difference of level at *A* & *C*. This can be done with pointed screws dipping on the surface

4 diff[e] of temperature at *A* & *C* can be got within $\frac{1}{50}$°F.

I have liberty from Joule to try this experiment but though I have considered the necessaries and do not think them unattainable I have no prospect of doing it.

(17) The mechanical equivalent of heat: see Number 207 note (43).

(18) On this experiment see Number 235.

(19) The specific gravity and specific heat of mercury (relative to water as unity). See W. J. M. Rankine, *A Manual of the Steam Engine* (London/Glasgow, 1859): 555.

I think it a plan free from many mechanical difficulties and in a lofty room with plenty of mercury and strong iron work, and a cherub aloft to read the level & the thermometer and a monkey to carry up mercury to him (called Quicksilver Jack) the thing might go on for hours, the coefficient meanwhile converging to a value to be appreciated only by the naturalist.

Are you aware that if anything converges according to log. to a fixed value v and if $x\ y\ z$ are three equidistant values

$$v = \frac{xz-y^2}{x+z-2y}.$$

I have sent the Secretary of the RSE an article on the arrangement of a prism and a lens of the same material for spectrum observations, as for instance when you are restricted to quartz or rock salt and cannot use achromatic lenses.[20]

I may find something else but this is the first I could lay hands on for the Proceedings.

Yours truly
J. CLERK MAXWELL

(20) J. Clerk Maxwell, 'On the best arrangement for producing a pure spectrum on a screen', *Proc. Roy. Soc. Edinb.*, **6** (1868): 238–42 (= *Scientific Papers*, **2**: 96–100).

DRAFT ON THE PRINCIPLES OF STEREOSCOPIC VISION

circa 1867

From the original in the University Library, Cambridge(1)

STEREOSCOPIC ILLUSTRATIONS OF SOLID GEOMETRY(2)

by J. Clerk Maxwell, M.A., FRSS. L. & E.

If lines drawn from a fixed point to the several points of any figure cut a fixed plane, the several points in which these lines cut the plane are called the projections of the corresponding points of the figure.

If the centre of the pupil of the eye coincide with the fixed point then the direction of the axes of pencils entering the eye will be the same whether they come from the points of the figure or from corresponding points of the projection. Hence the positions of the images of corresponding points on the retina will be the same, and if the distance of the point and of its projection from the eye are large compared with the principal focal length of the eye, the difference of adjustment for distinct vision of the two points will be small.

Hence the appearance of the figure and that of its projection are nearly alike when viewed by an eye placed at the fixed point.

The centres of the right and left eye are about $2\frac{1}{2}$ inches apart generally in a horizontal direction. Hence the projection of a figure taken with respect to one eye will not in general be similar to the projection taken with respect to the other eye. This will be the case only when the original figure is in a plane parallel to that on which it is projected. If we look with both eyes at once at the same plane projection of a figure, we become aware of the similarity and conclude that the figure is a plane one, unless the figure represented is of a form which powerfully suggests solidity.

But if each eye looks at a different plane figure, and if these figures are projections of the same solid figure with respect to the centres of the two eyes, then we become aware of the dissimilarity of the two figures and we find that in order to look at any point we have to make the same motions of both eyes as would be requisite in looking at the corresponding point of the real figure.

(1) ULC Add. MSS 7655, V, b/11.

(2) Very probably a preliminary and aborted draft of 'The construction of stereograms of surfaces', *Proceedings of the London Mathematical Society*, **2** (1868): 57–8 (= *Scientific Papers*, **2**: 101), presented at a meeting of the Society on 23 January 1868.

We also find that the points in the figure which are at the same distance as the point we look at are more distinct than those nearer or farther off, which appear double, and in this way while looking at the pair of projections we may probe or sound the figure so as to obtain an accurate knowledge of its depth as well as its length and breadth.

Two projections of this kind are called a Stereoscopic Pair. The complete study of the theory of binocular vision requires the consideration of the doctrine of corresponding points and of the motions of the eyes. These subjects are ably treated by Prof^r H. Helmholtz in his 'Physiological Optics' (Karsten's Cyclopädie)(3) and his Croonian Lecture on the Motions of the Eye (Proceedings of the Royal Society 186 (4)).(5)

In each eye there is a certain direction called the axis defined by a straight line drawn through the optic centre and a certain point on the retina on which the image of an object is made to fall when we look intently at it. This point may be called the centre of the retina. The centres of the two retinae are corresponding points. The other corresponding points are defined by their having equal coordinates x y measured from axes of which that of x is horizontal in both eyes, and that of y is inclined to the vertical at an angle of about (6) degrees measured outwards, so that the two axes of y would meet at a point about (6) below the level of the eyes.

Each eye is acted on by three pairs of muscles one pair of which tend to turn it about a horizontal axis so as to raise or lower the axis another pair turns it about a vertical axis to the right or left while the third pair tend to turn the eye about the axis of vision. These three motions are mechanically independent but in the actual motion of the eye they are connected by the following law.

There is a certain direction, fixed with regard to the head, which may be called the normal direction of the axis of the eye. This direction is nearly parallel to the median plane of the head and nearly horizontal when the head is in its normal position. A certain position of the whole eye when the axis is in this direction is called the normal position. When the axis of the eye is turned into any other position the position of the eye with respect to rotation about the axis is such that by a simple rotation about an axis perpendicular to the optic axis it could be brought into the normal position.

(3) Hermann Helmholtz, *Handbuch der physiologischen Optik* (Leipzig, 1867): 457–529.

(4) Space in the MS.

(5) Hermann Helmholtz, 'On the normal motions of the human eye in relation to binocular vision', *Proc. Roy. Soc.*, **13** (1864): 186–99. Maxwell almost certainly attended Helmholtz's Croonian Lecture, read on 14 April 1864 (see Number 225).

(6) Space in the MS.

APPENDIX: EXHIBITION OF STEREOGRAMS TO THE LONDON MATHEMATICAL SOCIETY

23 JANUARY 1868

From the *Proceedings of the London Mathematical Society*[7]

[ON STEREOGRAMS OF SURFACES][8]

Mr Maxwell stated that he had prepared most of the specimens exhibited for publication. The members present were enabled, after the meeting, to examine a large number of stereograms by means of a Real Image Stereoscope constructed after Mr Maxwell's directions.[9]

There were on view stereograms of the lines of curvature of the ellipsoid, and its surface of centres; of the wave surface of Fresnel,[10] of confocal spherical ellipses, of concylic spherical ellipses, showing the form of Laplace's coefficient of the second order, of twisted cubics, of Gordian knots of the form

$$x = a\sin pt, \quad y = b\sin qt, \quad z = c\cos rt,^{(11)}$$

and of 4 forms of the cyclide.[12]

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(7) *Proceedings of the London Mathematical Society*, **2** (1868): 58.

(8) The exhibition of stereograms followed the presentation of Maxwell's paper on 'The construction of stereograms of surfaces'.

(9) See Number 272.

(10) See Numbers 274 and 275 and Plates VI, VII and VIII.

(11) See Number 277 and Plates IX and X.

(12) See Number 274 esp. note (6).

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QUESTION TO THE LONDON MATHEMATICAL SOCIETY ON GOVERNORS

23 JANUARY 1868[1]

From the *Proceedings of the London Mathematical Society*[2]

Mr Maxwell asked if any member present could point out a method of determining in what cases all the possible parts of the impossible roots of an equation are negative.[3] In studying the motion of certain governors for regulating machinery,[4] he had found that the stability of the motion depended on this condition, which is easily obtained for a cubic,[5] but becomes more difficult in the higher degrees.[6]* Mr. W. K. Clifford[7] said that, by forming an equation whose roots are the sums of the roots of the original equation taken in pairs and determining the condition of the real roots of this equation being negative, we should obtain the condition required.[8]

* On Governors. Proceedings of the Royal Society, March 5, 1868.[4]

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(1) See note (2).

(2) *Proceedings of the London Mathematical Society*, **2** (1868): 60–1; a meeting held on 23 January 1868.

(3) See Number 219 esp. note (17).

(4) See his paper 'On governors', *Proc. Roy. Soc.*, **16** (1868): 270–83 (= *Scientific Papers*, **2**: 105–20); received on 20 February 1868.

(5) For Thomson's and Jenkin's governors: see Number 219, and 'On governors': 276, 278 (= *Scientific Papers*, **2**: 111, 114).

(6) For the 'combination of governors' (combining the principles of Thomson's and Jenkin's governors) see Number 219 esp. note (9) and 'On governors': 278–9 (= *Scientific Papers*, **2**: 114–15). For the solution to the problem of establishing stability criteria for systems of fifth order, see Number 297: Appendix, esp. note (9).

(7) William Kingdon Clifford, Trinity 1863, second wrangler 1867 (Venn).

(8) For an account of Clifford's interjection see *Mathematical Papers by William Kingdon Clifford*, ed. Robert Tucker (London, 1882): xvi–xvii.

APPENDIX: FRAGMENT OF DRAFT ON GOVERNORS[9]

circa JANUARY 1868[10]

From the original in the University Library, Cambridge[11]

[ON GOVERNORS][12]

[...] or $M'\frac{d^2\theta'}{dt^2}+C\omega\sin 2\beta\frac{d\phi'}{dt} = P-R'-G'\phi'-X\frac{d\theta'}{dt}$.

The nature of the motion is in this case also determined by a cubic equation of which the form is

$$MBn^3+(MY+BX)\,n^2+\{(C\omega\sin 2\beta)^2+XY\}\,n+GC\omega\sin 2\beta = 0.$$

The condition that the possible parts of the impossible roots of this equation should be negative is

$$(MY+BX)\,(\overline{C\omega\sin 2\beta}|^2+XY)-MBGC\omega\sin 2\beta = \text{a positive quantity.}$$

This implies that X and Y must not both vanish or that a damper must be applied either to the main shaft or to the arm of the centrifugal piece. A vessel full of a viscous liquid attached to either of these moveable pieces would answer the purpose if the unavoidable resistances of the parts of the machine are not sufficient.

If we neglect the product XY the condition becomes

$$\left(\frac{X}{M}+\frac{Y}{B}\right)C\omega\sin 2\beta > G.$$

If we increase the power of the break the quantity G is increased and the governor acts more promptly, but if G is increased beyond the value given by the above equation the motion becomes unstable in the form of a continually increasing oscillation.

Instead of allowing the whole force $G\sin\phi$ to act on the break a constant part of it may be taken off by means of a spring so as to diminish the waste of power.

(9) Compare Maxwell's discussion of 'Sir W. Thomson's and M. Foucault's governors' in 'On governors': 276–8 (= *Scientific Papers*, 2: 112–14). (10) See notes (2) and (4).

(11) ULC Add. MSS 7655, V, e/7. Previously published in A. T. Fuller, 'James Clerk Maxwell's Cambridge manuscripts: extracts relating to control and stability – V', *International Journal of Control*, **43** (1986): 805–18, on 805–6.

(12) Compare Maxwell's discussion of Thomson's governor in his letter to Thomson of 11 September 1863 (Number 219).

The governor of M. Foucault[13] is identical in principle with that just described. The centrifugal force of the arm is balanced by the downward force of gravity combined with the upward force of a system of levers, the resultant effect of which is the same as that of the spring in the former case. The break instead of acting by friction acts by admitting air to a centrifugal fan so as to increase and diminish the quantity of work done by the machine. The resultant equations of motion however are of the same form as equations ([....]

(13) Léon Foucault, 'Expression générale des conditions d'isochronisme du pendule régulateur à force centrifuge', *Comptes Rendus*, **57** (1863): 738–40.

LETTER TO WILLIAM THOMSON

20 FEBRUARY 1868

From the original in the University Library, Glasgow(1)

8 Palace Gardens Terrace
London W
1868 Feb 20

Dear Thomson

Many thanks for your letter. Do not study the geometrical statics too much.(2) I have got better results which I hope to send to the R.S.E.(3)

I hope Tait will start the determination of Joules Equivalent with mercury coming down a wide tube from a cistern and flowing through a difficult passage into a lower cistern. One foot of mercury is as good as 400 of water so an experiment in a room will be much better than Niagara falls.(4)

I do not know what authority Tait has for my coming to Edinburgh in winter, as we intend always to be in London as head quarters. I have no doubt that a general reversal of all motions would lead to curious results such as the past becoming future and only part of the future past.

I see by advertisements that M^r^ Esson of Merton Coll Oxford(5) is going to do an Electricity for the Clarendon Press. I only know M^r^ Esson as an observer of the progress of chemical changes along with M^r^ Vernon Harcourt,(6) so I do not know whether his book will be about Greens Theorem or about the influence of electrical action on plants. If the latter I shall go on with the theorem.(7)

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(1) Glasgow University Library, Kelvin Papers, M 21.

(2) J. Clerk Maxwell, 'On reciprocal diagrams in space, and their relation to Airy's function of stress', *Proceedings of the London Mathematical Society*, **2** (1868): 58–60 (= *Scientific Papers*, **2**: 102–4).

(3) J. Clerk Maxwell, 'On reciprocal figures, frames and diagrams of forces', *Trans. Roy. Soc. Edinb.*, **26** (1870): 1–40 (= *Scientific Papers*, **2**: 161–207). See Tait's letter to Maxwell of 13 December 1867 (Number 277 note (22)).

(4) See Number 278.

(5) William Esson, Fellow of Merton College, Oxford, 1860 (J. Foster, *Alumni Oxonienses*, 4 vols. (Oxford, 1888), **2**: 429).

(6) A. Vernon Harcourt and William Esson, 'On the laws of connexion between the conditions of a chemical change and its amount', *Phil. Trans.*, **156** (1866): 193–211; *ibid.*, **157** (1867): 117–37.

(7) Maxwell's first statement that he was writing the *Treatise on Electricity and Magnetism*. For his account there of Green's theorem see the *Treatise*, **1**: 108–13 (§100); and see George Green, *An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism* (Nottingham, 1828): esp. 10 (= *Mathematical Papers of the Late George Green*, ed. N. M. Ferrers (London/Cambridge, 1871): esp. 23). See also Number 274.

I am glad to see a good deal of Spherical Harmonics in T. T′[8] which will relieve me. I do not see however the doctrine that every S. H. of the ith degree has i poles, and is formed by differentiating $\frac{1}{r}$ with respect to these i directions in any order and either leaving the result as it stands or multiplying by r^{2i+1}.[9]

This can be done only for these i poles and no others when the form of the S. H. is given but it is a trouble to find them when i is above 2. I have a stereogram of a S. H. for $i = 2$.[10] The equipotential surface which passes through the centre is of course a cone of the ith degree. I want to find out into how many regions it divides space, or a spherical surface when the i poles are at arbitrary points. It is easy when the surface is cut up into quads by meridians & parallels.

The doctrine of the whole number of points and lines of equilibrium in a field of force depends on this. I *think* that n centres of force have p points and l lines of equilibrium where $p+2l+1 = n$.

Yours truly
J. Clerk Maxwell

(8) Thomson and Tait, *Natural Philosophy*: 140–60.

(9) See Numbers 277 and 388.

(10) See Numbers 277 and 279: Appendix and Plate X.

REPORT ON A PAPER BY JOSEPH DAVID EVERETT ON THE RIGIDITY OF METAL RODS

circa 25 FEBRUARY 1868[1]

From the original in the Library of the Royal Society, London[2]

REPORT ON D^r EVERETT'S THIRD PAPER ON TORSION AND FLEXURE[3]

This paper gives an account of experiments on the same plan as those described in the 2^nd paper (Feb 1867)[4] but on rods of different materials, namely Wrought Iron, Cast Iron, and Copper. I have already reported on the merit of the method of the experiments and on a few defects.[5] A clearer statement is given in this paper of the relative positions of the scale, the mirrors and the telescopes, and of the optical correction required on account of the obliquity of the ray to the plane of the scale.

This might be made smaller if it were worth while by making the plane of the scale perpendicular to the rays reflected from the mirrors when they are both horizontal, and if the scale is on a flat board this is easily done by placing the back of a mirror against the centre of the scale and causing the ray to be reflected back from this mirror to the mirrors on the rod.

In making the observations of torsion I think it would be worth while to observe if any flexure takes place and vice versâ. The scale being divided in both directions, such observations would not only test the proper orientation of the scale but would indicate any inequality or ellipticity in the section of the rod and might be used as illustrations of the theory of combined torsion and flexure by Kirchhoff.[6] See Thomson & Taits Natural Philosophy §588 et seq.[7]

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(1) According to the Royal Society's *Register of Papers Received* Everett's paper was referred to Maxwell on 20 February 1868, and to Stokes on 28 February 1868.

(2) Royal Society, *Referees' Reports*, **6**: 119.

(3) J. D. Everett, 'Account of experiments on torsion and flexure for the determination of rigidities', *Phil. Trans.*, **158** (1868): 363–9. The paper was received by the Royal Society on 13 January 1868, and read on 30 January 1868; see the abstract in *Proc. Roy. Soc.*, **16** (1868): 248.

(4) J. D. Everett, 'Account of experiments on torsion and flexure for the determination of rigidities', *Phil. Trans.*, **157** (1867): 139–53.

(5) Number 269. See also Number 261.

(6) Gustav Kirchhoff, 'Ueber das Gleichgewicht und die Bewegung eines unendlich dünnen elastischen Stabes', *Journal für die reine und angewandte Mathematik*, **56** (1859): 285–313.

(7) Thomson and Tait, *Natural Philosophy*: 437–52.

I consider the publication of the method of these researches as of scientific value. This has already been done in the second paper. I also think that the new results are very valuable and ought to be published for the use of physicists & engineers. The actual values of the observed scale readings afford a means of estimating the accuracy of the experiments and a few examples of the process of deducing the final results enables the reader to understand the course of experiments. In other respects the table of the values of M n k & σ[(8)] for the given specimens condenses the whole substance of the paper. From this table it will be seen that the three substances Steel, Wrought Iron, and Cast Iron have the values of M, n & k in descending order, steel being stiffest in every sense. Not much however is stated about the mode of preparation and the state of annealing or temper, or previous strain of the rods.

The values of Poissons ratio[(9)] are also in a descending order for these three substances which seems to show that neither the stiffness and small plasticity of steel nor the high melting point of wrought iron indicate an approach to the value .250 given by a particular assumption in a molecular theory.[(10)]

The greater values of M & n (the observed rigidities) for copper than for brass deserves attention, as brass is generally supposed harder than copper.

If the method is applicable to lead, it would be worth trying it as Matthiessen finds that lead is remarkably uniform in its properties, to whatever strains and temperatures it has previously been subjected.[(11)]

D^r^ Everett has referred to some unpublished experiments of mine.[(12)] The observations of torsion in these and all other similar experiments is generally satisfactory but those of flexure were made on a plan much inferior to that of D^r^ Everett, and are not to be depended on.[(13)]

D^r^ Everetts experiments on a wooden rod are very interesting as showing that the rigidity which resists a shearing stress in planes perpendicular to the fibres is very much less than that in planes parallel to the fibres. The explanation of the results with fibrous substances given on p. 152 of the 2^nd^ paper[(14)] does not seem to me correct as it refers the phenomenon to the

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(8) In Everett's papers M denotes Young's modulus, n the rigidity, k the resistance to compression, and σ Poisson's ratio.

(9) See Number 261 note (6).

(10) See Number 261, esp. note (7).

(11) A. Matthiessen and M. von Bose, 'On the influence of temperature on the electric conducting power of metals', *Phil. Trans.*, **152** (1862): 1–27, on 15–16.

(12) There is no reference to Maxwell's experiments in the printed text of Everett's paper; but see Number 261, esp. notes (17) and (18).

(13) See Volume I: 180, 193–4.

(14) Everett, 'Account of experiments on torsion and flexure...', *Phil. Trans.*, **157** (1867): 152.

difference of shearing rigidity between planes parallel *or* perpendicular to the length and planes oblique to the length, whereas these last have nothing to do with it and the ratio $\frac{T}{F}$(15) is greater or less than for isotropy as the rigidity in planes parallel to the length is greater or less than in planes perpendicular to the length.

James Clerk Maxwell

(15) In Everett's papers T and F denote the numbers on his scale for torsion and flexure.

LETTER TO GEORGE BIDDELL AIRY

12 MARCH 1868

From the original in the Royal Greenwich Observatory Archive(1)

8 Palace Gardens Terrace
London W
1868 March 12

Dear Sir

I have neither the MS nor a copy of my paper on Governors(2) so that I cannot be certain as to every expression.

I divided the governors considered into

1 those which act by alteration of pressure
2 ——————— by alteration of position of a centrifugal piece
3 ——————— by the motion of a liquid.

After describing the general principle of the second kind and stating briefly how the conditions are fulfilled in Sir W. Thomsons and M. Foucaults governors respectively(3) I said that governors on Mr Siemens principle(4) were used in the Greenwich Observatory and that they essentially consisted of a conical pendulum slightly inclined to the vertical, the angular velocity of which is checked by means of a fan which dips into a liquid as the pendulum diverges from the vertical, and that the driving power of the pendulum and its inclination to the vertical is kept within narrow limits of variation by means of a differential system of wheelwork between the main shaft and the pendulum shaft which works a valve or a break in the prime mover.(5)

(I think it is a break in the Chronograph and a valve in the Equatoreal.)(6)

I did not enter into any detail as I had already explained the general principle and the mathematical theory so far as I understand it of the Greenwich instruments is considerably more difficult than that of some others.

I read several years ago the descriptions of the instruments in the prefaces

(1) Royal Greenwich Observatory Archive, ULC, Airy Papers 6/172, 447R–448R.

(2) J. Clerk Maxwell, 'On governors', *Proc. Roy. Soc.*, **16** (1868): 270–83 (= *Scientific Papers*, **2**: 105–20), read on 5 March 1868.

(3) See Number 280 note (9).

(4) See Number 219 note (10).

(5) Compare Maxwell, 'On governors': 273 (= *Scientific Papers*, **2**: 108).

(6) See the *Astronomical and Magnetical and Meterological Observations made at the Royal Observatory, Greenwich, In the Year 1860* (London, 1862): ix–x, xv–xviii.

to the Greenwich Observations[7] and I saw them at one of the visitations of the Observatory but if you can furnish me with some of the facts related thereto which appear to you most deserving of attention I shall be greatly obliged to you, as it will enable me to make my paper fuller and more accurate with respect to those governors which have been subjected to the longest and the most accurate testing of any now existing.

I remain
Yours truly
J. Clerk Maxwell

The Astronomer Royal

(7) Airy's reports, as Astronomer Royal, of *Observations made at the Royal Observatory, Greenwich*, were published annually, and were prefaced by descriptions of the instruments used at the Observatory.

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LETTER TO PETER GUTHRIE TAIT

12 MARCH 1868

From the original in the University Library, Cambridge[1]

8 P.G.T.[2]
12 March 1868

D^r^ Tait

Yours received. I dispatched the proofs to you yesterday. As regards conduction of heat I have not considered it enough to know whether a deductive method like yours would predict anything about it.[3] I have come to a knowledge of my ignorance of the nature of electrical conduction in metals which is a phenomenon like that of heat, and both very easy to formulate but difficult to conceive.[4]

As regards Clausius he pointed out *gross* mistakes in M.[5] I have no doubt he has some of his own but I have not had patience to find them out, except that he stuck to uniform velocity in the molecules[6] though I proved it impossible and pointed out the only true distribution of velocity.[7] Clausius uniform velocity leads (by sound mathematics) to an expression for mean relative velocity which is unsymmetrical with respect to the components so that you need to know which is the greater of the two velocities and to put it in the right place of the formula.

With respect to Riemann for whom I have great respect and regret,[8] I only lately got either Pogg or Phil Mag[9] from the binder & wrote you a rough note for yourself. I now have him more distinct. Weber says that electrical force depends on the distance and its 1st & 2nd derivatives with respect to t.[10]

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(1) ULC Add. MSS 7655, I, b/10. Previously published in *Molecules and Gases*: 473–4.

(2) Palace Gardens Terrace [London].

(3) See Numbers 293 esp. note (3) and 294.

(4) Compare Maxwell's discussion of the analogy in the *Treatise*, **1**: 297–8 (§§243–5).

(5) Rudolf Clausius, 'Ueber die Wärmeleitung gasförmiger Körper', *Ann. Phys.*, **115** (1862): 1–56; see Number 207 notes (9) and (39).

(6) Rudolf Clausius, 'On the dynamical theory of heat', *Phil. Mag.*, ser. 4, **19** (1860): 434–6.

(7) See Number 207 §4 and note (13), and Number 377 para. (12).

(8) Georg Friedrich Bernhard Riemann had died in 1866.

(9) Bernhard Riemann, 'Ein Beitrag zur Elektrodynamik', *Ann. Phys.*, **131** (1867): 237–43; (trans.) 'A contribution to electrodynamics', *Phil. Mag.*, ser. 4, **34** (1867): 368–72.

(10) For Wilhelm Weber's statement of his electrodynamic force law (which included terms for the relative velocity and acceleration between electric charges e, e') in the form

$$\frac{ee'}{r^2}\left(1-\frac{1}{c^2}\left(\frac{dr}{dt}\right)^2+\frac{2r}{c^2}\frac{d^2r}{dt^2}\right)$$

Riemann says that this is due to the fact that the potential at a point is due to the distribution of electricity elsewhere not at that instant but at times before depending on the distance.

In other words potential is propagated through space at a certain rate and he actually expresses this by a partial diff eq^{n} appropriate to propagation.(11)

Hence either (1) space contains a medium capable of dynamical actions which go on during transmission independently of the causes which excited them (and this is no more or less than my theory divested of particular assumptions)

or (2) if we consider the hypothesis as a fact without any etherial substratum and if A & B are two bodies each of which can vary in electrical power, say each a pair of equal magnets one of which revolves about the middle of the other so that the combination is alternately $= 2$ and $= 0$.

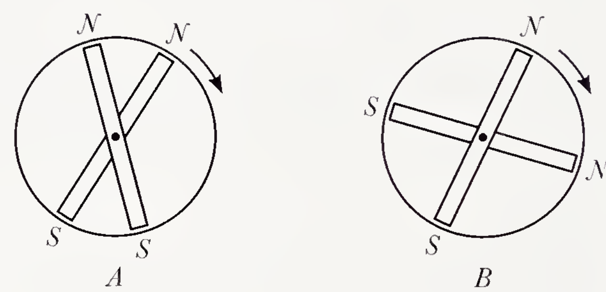

Figure 284,1

Now let things be so arranged that the time of propagation from A to $B = T$ then if the magnetism of A be $A\cos(nt+\alpha)$ and that of B $B\cos(nt+\beta)$ the action of B on A will be $A\cos(nt+\alpha)\,B\cos(nt+\beta-nT)$ into a function of the distance and that of A on B $A\cos(nt+\alpha-nT)\,B\cos(nt+\beta)$ into same function. The difference of these is $F(r)\,AB\sin nT\,(\sin(nt+\beta)-\sin(nt+\alpha))$ that is, action & reaction are not equal & opposite. (I mean pushes and pulls not Hamiltonian action.)(12)

Webers action and reaction are equal but his energy is unreclaimable.(13)

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where r is the distance between the electric charges, see his 'Elektrodynamische Maassbestimmungen, insbesondere Widerstandsmessungen', *Abhandlungen der Königlichen Sächsischen Gesellschaft der Wissenschaften, math.-phys. Klasse*, **1** (1852): 199–381, esp. 259–70 (= *Wilhelm Weber's Werke*, **3**: 301–471). On Weber's interpretation of the constant c see Volume I: 306n, 686n.

(11) Compare Maxwell's discussion of Riemann's theory of electrodynamics in the 'Note on the electromagnetic theory of light' appended to his paper 'On a method of making a direct comparison of electrostatic with electromagnetic force', *Phil. Trans.*, **158** (1868): 643–57, esp. 652 (= *Scientific Papers*, **2**: 137). The paper was received by the Royal Society on 10 June 1868 (see Number 289). Compare also the *Treatise*, **2**: 435 (§862).

(12) W. R. Hamilton, 'On a general method in dynamics', *Phil. Trans.*, **124** (1834): 247–308; *ibid.*, **125** (1835): 95–144. Hamilton's 'principle of varying action' is discussed by Thomson and Tait, *Natural Philosophy*: 231–41.

(13) A criticism originally advanced against Weber's theory by Helmholtz, who indicated that the velocity-dependent terms in Weber's force law conflicted with his own principle of

Riemanns action & reaction between the gross bodies are unequal and his energy is nowhere unless he admits a medium which he does not do explicitly. My action & reaction are equal only between things in contact not between the gross bodies till they have been in position for a sensible time, and any energy is and remains in the medium including the gross bodies which are among it.

Instead of part about A & B read as follows

$$\underset{\longrightarrow}{\overset{X}{\bullet}} \qquad\qquad \underset{\longrightarrow}{\overset{Y}{\bullet}}$$

Let X & Y be travelling to the right with velocity v at a distance a then

$$\text{the force of } X \text{ on } Y \text{ will be } \frac{XY}{a^2}\left(1-\frac{v}{V}\right)^2$$

$$\text{and that of } Y \text{ on } X \qquad \frac{XY}{a^2}\left(1+\frac{v}{V}\right)^2$$

where V is the velocity of transmission of force. If the force is an attraction and if X & Y are connected by a rigid rod X will be pulled forward more than Y is pulled back and the system will be a locomotive engine fit to carry you through space with continually increasing velocity.(14) See Gullivers Travels in Laputa.(15)

Yours truly
J. CLERK MAXWELL

'Erhaltung der Kraft' which required that forces should be functions only of the distance. See Hermann Helmholtz, *Über die Erhaltung der Kraft, eine physikalische Abhandlung* (Berlin, 1847): 63, (trans.) 'On the conservation of force', in *Scientific Memoirs, Natural Philosophy*, ed. J. Tyndall and W. Francis (London, 1853): 114–62, esp. 156. Maxwell had echoed Helmholtz's argument in his paper 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83, esp. 67 (= *Scientific Papers*, **1**: 208); and see also 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 460 (= *Scientific Papers*, **1**: 527) where he alludes to the 'mechanical difficulties' of Weber's theory. See Number 389 for Maxwell's subsequent discussion of the issue.

(14) Compare the similar argument in the 'Note on the electromagnetic theory of light': 652–3 (= *Scientific Papers*, **2**: 137–8), directed not only at Riemann's electrodynamics but also at the paper by L. V. Lorenz, 'Ueber die Identität der Schwingungen des Lichts mit den elektrischen Strömen', *Ann. Phys.*, **131** (1867): 243–63, (trans.) 'On the identity of the vibrations of light with electric currents', *Phil. Mag.*, ser. 4, **34** (1867): 287–301. Compare his discussion of Lorenz in the *Treatise*, **2**: 398 (§805 note).

(15) The island floating in the air in Jonathan Swift's *Gulliver's Travels*; the members of the 'Academy' of Lagado engaged in fanciful scientific enquiries.

LETTER TO WILLIAM ROBERT GROVE(1)

27 MARCH 1868

From the *Philosophical Magazine* for May 1868(2)

8 Palace Gardens Terrace,
W.
March 27, 1868

Dear Sir,

Since our conversation yesterday on your experiment on magneto-electric induction,(3) I have considered it mathematically, and now send you the result. I have left out of the question the secondary coil, as the peculiar effect you observed depends essentially on the strength of the current in the primary coil, and the secondary sparks merely indicate a strong alternating primary current. The phenomenon depends on the magneto-electric machine, the electromagnet, and the condenser.

The machine produces in the primary wire an alternating electromagnetic force, which we may compare to a mechanical force alternately pushing and pulling at a body.

The resistance of the primary wire we may compare to the effect of a viscous fluid in which the body is made to move backwards and forwards.

The electromagnetic coil, on account of its self-induction, resists the starting and stopping of the current, just as the mass of a large boat resists the efforts of a man trying to move it backwards and forwards.

The condenser resists the accumulation of electricity on its surface, just as a railway-buffer resists the motion of a carriage towards a fixed obstacle.

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(1) Lawyer and scientist, FRS 1840 (*DNB*).

(2) J. C. Maxwell, 'On Mr Grove's "Experiment in Magneto-electric induction". In a letter to W. R. Grove, F.R.S.', *Phil. Mag.*, ser. 4, **35** (1868): 360–3, esp. 360–1 (= *Scientific Papers*, **2**: 121–2). Maxwell's letter was communicated to the *Phil. Mag.* by Grove.

(3) W. R. Grove, 'An experiment in magneto-electric induction', *Phil. Mag.*, ser. 4, **35** (1868): 184–5. Grove investigated whether 'the ordinary effects of the Ruhmkorff-coil might be produced by applying to it a magneto-electric machine [dynamo]'. He found that: 'The result was very unexpected. The terminals of the magneto-electric coils being connected with the primary coil of the Ruhmkorff, and the contact-breaker being kept closed so as to make a completed circuit of the primary wire (a condition which would have appeared *à priori* essential to success), no effect was produced; while if the circuit was interrupted by keeping the contact-breaker open, sparks of 0.3 of an inch passed between the terminals of the secondary coil of the Ruhmkorff, and vacuum-tubes were readily illuminated. Here there was in effect no primary coil, no metallic connexion for the primary current; and yet a notable effect was produced.' For description of the Ruhmkorff coil see Gustav Wiedemann, *Die Lehre vom Galvanismus und Elektromagnetismus*, 2 vols. (Braunschweig, 1861), **2**: 836; and Fleeming Jenkin, *Electricity and Magnetism* (London, 1873): 287–8.

Now let us suppose a boat floating in a viscous fluid, and kept in its place by buffers fore and aft abutting against fixed obstacles, or by elastic ropes attached to fixed moorings before and behind. If the buffers were away, the mass of the boat would not prevent a man from pulling the boat along with a long-continued pull; but if the man were to push and pull in alternate seconds of time, he would produce very little motion of the boat. The buffers will effectually prevent the man from moving the boat far from its position by a steady pull; but if he pushes and pulls alternately, the period of alternation being not very different from that in which the buffers would cause the boat to vibrate about its position of equilibrium, then the force which acts in each vibration is due, partly to the efforts of the man, but chiefly to the resilience of the buffers, and the man will be able to move the boat much further from its mean position than he would if he had pushed and pulled at the same rate at the same boat perfectly free.

Thus, when an alternating force acts on a massive body, the extent of the displacements may be much greater when the body is attracted towards a position of equilibrium by a force depending on the displacement than when the body is perfectly free.

The electricity in the primary coil when it is closed corresponds to a free body resisted only on account of its motion; and in this case the current produced by an alternating force is small. When the primary coil is interrupted by a condenser, the electricity is resisted with a force proportional to the accumulation, and corresponds to a body whose motion is restrained by a spring; and in this case the motion produced by a force which alternates with sufficient rapidity may be much greater than in the former case. I enclose the mathematical theory of the experiment,(4) and remain,

Yours truly
J. Clerk Maxwell

(4) Maxwell's appended 'Mathematical theory of the experiment', which forms the substance of his paper 'On Mr Grove's "Experiment in magneto-electric induction"': 361–3 (= *Scientific Papers*, **2**: 122–4), is not reproduced here. In this paper he gives an explanation of resonating alternating current circuits.

LETTER TO MARK PATTISON[1]

7 APRIL 1868

From the original in the Bodleian Library, Oxford[2]

8 Palace Gardens Terrace
London W
1868 April 7

Sir

In the Saturday Review of April 4 is an article on Science & Positivism[3] the writer of which appears to take so much interest in metaphysics science and positivism that I should be obliged if you will communicate to him the following remarks on a portion of the article.

M. Caro, according to the article (for I have not yet seen his own work),[4] uses in his argument the doctrine of the gradual conversion of all kinds of energy into the form of heat, and the ultimate uniform distribution of temperature over all matter.

As the speculation has important consequences I should like to point out to the writer of the article where he may find the data on which it is formed.

1 Fourier, in his great work on the conduction of heat,[5] has given methods by which if we know the temperature of every part of a body at any time and the temperature of the surface at all times we can determine the temperature of any point of the body at any *future* time as arising from the conduction of heat within it. (If the body be supposed to include the universe the condition about the surface is unnecessary.)

Now the formulae of Fourier for predicting the future temperature are equally applicable to determine the *former* temperature of the body in all its parts by simply making the quantity denoting the time a negative quantity.

If in this way we attempt to ascertain the state of the body previous to our observation of it, then (except in particular cases) we find that as we go back we arrive at an epoch at which the temperature varied in a discontinuous manner, and if we seek for the state of the body at any time still farther back we arrive at an impossible result.

Hence the body could not have existed as a solid body and a conductor of

(1) Rector of Lincoln College, Oxford; a regular contributor to the *Saturday Review* (see M. M. Bevington, *The Saturday Review 1855–1868* (New York, 1941): 26).

(2) MS Pattison 56, fols. 438^r–441^v, Bodleian Library, Oxford.

(3) 'Science and positivism', *Saturday Review*, **25** (4 April 1868): 455–6.

(4) Elme Marie Caro, *Le Matérialisme et la Science* (Paris, 1867), reviewed in the *Saturday Review*.

(5) Joseph Fourier, *Théorie Analytique de la Chaleur* (Paris, 1822).

heat before a certain epoch. At that time or after it something must have happened, e.g. two bodies at different temperatures may have been joined together at a certain epoch and the present condition of the compound body will indicate when that was.[6]

This is a purely mathematical result founded however on experimental data. It has been pointed out by Sir W Thomson in several papers on the Secular Cooling of the Earth & Sun.[7]

2 The general doctrine of the dissipation (not the destruction) of energy was first clearly stated by Sir W. Thomson in his papers on the Dynamical Theory of Heat (Trans. Royal Society of Edinburgh 185 $\overline{\scriptstyle 1345}$).[8] It has also been treated at some length and with great labour by Prof. R Clausius of Zurich under the name of 'Entropy'[9] which is an expression for the quantity of energy now rendered unavailable, a quantity always on the increase.[10]

The data are

1st the fact that energy cannot be created or destroyed by physical agency

2nd the fact that energy may change its form in two different ways

(α) in a certain class of conceivable cases the process by which the transfer takes place may be exactly reversed so that everything is at last in the same condition as at first

(β) in another class of cases the process is not reversible by any physical agency e.g. the equalization of temperature by conduction of heat in a body & the production of heat by the electric current when it meets with 'resistance'.

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(6) The reviewer reported Caro's discussion of thermodynamics: 'We should then have to conceive of the existing order of things... as a slow but sure advance towards extinction. And, looking backward, it would... become... infinitely probable that the laws which now regulate the world had been arranged by an intelligent Cause.' The reviewer noted that 'this speculation... is at once metaphysical, and in harmony with the facts of science', and was used by Caro 'in vindication of metaphysical speculation as a form of knowledge', against the claims of positivist thinkers 'to have suppressed metaphysics'. See also Number 339.

(7) William Thomson, 'On the age of the sun's heat', *Macmillan's Magazine*, **5** (1862): 288–93 (= Thomson, *Popular Lectures and Addresses*, 3 vols. (London, 1889–94), **1**: 349–68); Thomson, 'On the secular cooling of the earth', *Phil. Mag.*, ser. 4, **25** (1863): 1–14 (= *Math. & Phys. Papers*, **3**: 295–311).

(8) William Thomson, 'On the dynamical theory of heat', *Trans. Roy. Soc. Edinb.*, **20** (1851): 261–88, 475–82; *ibid.*, **21** (1854): 123–71 (= *Math. & Phys. Papers*, **1**: 174–210, 222–32, 232–91).

(9) Rudolf Clausius, 'Ueber verschiedene für die Anwendung bequeme Formen der Hauptgleichungen der mechanischen Wärmetheorie', *Ann. Phys.*, **125** (1865): 353–400, esp. 390, 400. See Number 483 note (22).

(10) On Maxwell's interpretation of Clausius' concept of entropy see Number 483 esp. notes (19) and (20).

By no contrivance can we arrange an example of class α without introducing processes belonging to class β so that in every action in nature part of the process is not capable of reversion.

This part always tends in one direction to diminish the energy which is available for producing phenomena involving change, and to increase the energy which cannot be so used. The ultimate condition is one of uniform temperature in which everything remains at the same distance from every other thing in so far as these are sensible objects.

I speak of sensible objects because according to a certain theory the phenomena of heat are due to the intestine motion of the small parts of hot bodies.(11)

A uniformly hot body apparently at rest is not on this theory devoid of motion for if we had the means of observing the very smallest parts (I do not speak of atoms) of the body and of distinguishing the smallest intervals of time we should find at a given instant different parts moving in different ways, and if we could lay hold of these parts by machinery we might extract energy from this motion till the whole mass was reduced to stillness.

There is no evidence that this can be done either by direct manipulation or by any physical process, and therefore in the present dispensation there remain a number of irreversible processes, all of which tend in the same direction, and therefore tend of themselves to an end and by reasoning backwards (if we know enough) we should find an epoch before which the present order could not have existed.

The peculiar faith required of a positivist is in the universal validity of laws, the form of which he does not yet know, though he speculates to a certain extent on their results.

A strict materialist believes that everything depends on the motion of matter. He knows the form of the laws of motion though he does not know all their consequences when applied to systems of unknown complexity.

Now one thing in which the materialist (fortified with dynamical knowledge) believes is that if every motion great & small were accurately reversed, and the world left to itself again, everything would happen backwards the fresh water would collect out of the sea and run up the rivers and finally fly up to the clouds in drops which would extract heat from the air and evaporate and afterwards in condensing would shoot out rays of light to

(11) 'The Dynamical Theory of Heat... that heat consists of a motion excited among the particles of bodies'; Thomson, 'On the dynamical theory of heat', *Trans. Roy. Soc. Edinb.*, **20** (1851): 261, who attributes the theory to Humphry Davy. Maxwell subsequently listed Bacon, Boyle, Newton and Cavendish as supporters of the theory: see Number 377 para. 3.

the sun and so on. Of course all living things would regrede from the grave to the cradle and we should have a memory of the future but not of the past.(12)

The reason why we do not expect anything of this kind to take place at any time is our experience of irreversible processes, all of one kind, and this leads to the doctrine of a beginning & an end instead of cyclical progression for ever.

The practical relation of metaphysics to physics is most intimate. Metaphysicians differ from age to age according to the physical doctrines of the age and their personal knowledge of them. Leibnitz is in advance of Descartes Newton so far as he exposes himself is distinct. Locke Berkeley &c differ according to the degree in which they enjoyed the diffusion and dilution of the Galilean and Newtonian doctrines.

The Edinburgh & the Dublin Hamilton(13) differ in their metaphysical power in the direct ratio of their physical knowledge (not the inverse as most people suppose).

On the other hand the effect of the absence of metaphysics may be traced in most physical treatises of the present century.

I have been somewhat diffuse but I happen to be interested in speculations standing on experimental & mathematical data and reaching beyond the sphere of the senses without passing into that of words and nothing more.

I am Sir
Yours truly
J. Clerk Maxwell

(12) Compare Maxwell's letter to J. W. Strutt of 6 December 1870 (Number 350). Maxwell's expression of the reverse movement of the universe may perhaps echo the myth expounded in Plato's *Politicus* (269–70), of which a major edition had been published by Lewis Campbell in 1867. See *The Sophistes and Politicus of Plato*, with a revised text and English notes by Lewis Campbell (Oxford, 1867): [*Politicus*] 45–53; 'The universe is at one time turned by God, but at certain periods is relinquished by him, and turns itself in the opposite direction. ... Yet it has the least possible change of motion, when the direction of its rotation is reversed...guided by its Divine Author, and receives from him a renewal of life and immortality. And again, being let go at the most auspicious moment, it makes countless revolutions by itself, like a huge and perfectly balanced top, revolving on the finest peg.... As the movement of the world, so the order of the ages of man is reversed. And, at the time when the world returns under the Divine care, old age is done away, and men pass through maturity and youth to childhood and infancy, and so pass away.' In his 'Introduction to the Statesman': xxviii–xli, esp. xxxiv Campbell gave a full discussion of the problems surrounding the interpretation of the myth.

(13) Sir William Hamilton, Bart and Sir William Rowan Hamilton, respectively.

LETTER TO MARK PATTISON

13 APRIL 1868

From the original in the Bodleian Library, Oxford[1]

8 Palace Gardens Terrace
London W
1868 April 13

Sir

I have received your letter[2] and will do my best to answer your queries. You must see that my acquaintance with positivism whether as stated by Comte,[3] Littré,[4] or Mill,[5] is but slight.

Comte certainly endeavoured to form a distinct picture of the shape, method and aim of a system of sciences, some of which he considered as in a very immature state at present. This picture he considered as a sort of matrix in which these sciences would probably be developed by future labourers and one of its uses was to prevent them from wasting their efforts on attempts of a kind which would prove abortive. In this part of his work Comte was like other philosophers who try to make their 'Principles of Human Knowledge' practically useful, and using every method – conjecture – imagination & himself for that purpose.

But when he prescribes rules for the study of sciences already formed, as Astronomy, all this part of the philosophic spirit is proscribed, and the astronomer is even forbidden to extend his views beyond the Solar System.

I say therefore that though it is quite the part of a philosopher to try to form a conception of a state of science more developed than the present, and of the methods likely to be most fertile, he who does so assumes that although he does not know *what* will be discovered he has some anticipation of the mould in which future discoveries will be cast.

The statements of certain positivists therefore about human knowledge & science in general are apt to make one think that they are convinced that new truths will fall into the old moulds, even in sciences yet unborn.

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(1) MS Pattison 56, fols. 442^r–448^v, Bodleian Library, Oxford.

(2) Not extant.

(3) Auguste Comte, *Cours de Philosophie Positive*, 6 vols. (Paris, 1830–42). There is a copy of G. H. Lewes, *Comte's Philosophy of the Sciences: being an Exposition of the Cours de Philosophie Positive of Auguste Comte* (London, 1853) in Maxwell's library (Cavendish Laboratory, Cambridge).

(4) Émile Littré, *Auguste Comte et la Philosophie Positive* (Paris, 1863).

(5) John Stuart Mill, *Auguste Comte and Positivism* (London, 1865). Mill and Littré were mentioned in the review 'Science and positivism', *Saturday Review*, **25** (1868): 455–6, which had occasioned Maxwell's correspondence with Pattison: see Number 286, esp. notes (4) and (6).

I have no doubt however that in my last letter(6) I went astray on this subject. I go on to your 3rd query, taking them in the reverse order.

I am not sure whether the 'Matter' against which Berkeley argued has any existence now. I am little satisfied with most of the definitions of it. 'That which is perceived by the senses' is utterly wrong both in excess and defect.(7)

Lucretius says

'facere et fungi sine corpore nulla potest res'.(8) In Dynamics, the science of the motion of matter as affected by forces, matter is defined and measured solely with respect to the force required to move it in a certain manner and the force is likewise defined with respect to matter & motion.(9)

Having defined equal times equal distances and equal velocities, Force is defined to be that which produces change in a bodys velocity and is *measured* by the change which it would produce in the velocity [of] a standard body (Imperial Pound) if it acted on it for a second.

Any other mass on which the same force would produce the same effect is called an equal mass, whatever be its other properties.

This is the definition of equal quantities of matter and it is found to lead to consistent statements.

The measurement of quantity of matter by weight is a secondary method founded on the fact, which Newton and others have carefully verified, that the weight of all bodies known to us is proportional to the quantity of matter in them and independent of the *kind* of matter.

When the word Inertia is used by a physicist since Newton it generally

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(6) Number 286.

(7) Compare Number 294: Appendix.

(8) *Titi Lucreti Cari De Rerum Natura Libri Sex*, ed. and trans. H. A. J. Munro, 2 vols. (Cambridge, $_2$1866), **1**: 57 (Book I, line 443); 'no thing can do and suffer without body', *ibid.*, **2**: 11. On Lucretius see also Maxwell's letter to H. A. J. Munro of 7 February 1866 (Number 257) and Number 377 para. 1. Fleeming Jenkin had recently published an essay reviewing Munro's edition of *De Rerum Natura*: 'The atomic theory of Lucretius', *North British Review*, **48** (1868): 211–42. Jenkin had written to Maxwell on 10 January 1868: 'I send by book post a revise of my atoms article will you write anything you please in pencil on the margin.' (ULC Add. MSS 7655, II/28). In a letter to William Thomson of 20 February Jenkin commented on Maxwell's response: 'Thank you very much for your notes on Lucretius...Munro unearthed Lesage. Maxwell says he has calculated the effect of atoms striking as he described and found that no gravitation would result if the striking atoms rebounded.' (ULC Add. MSS 7342, J 27). Maxwell commented on Lesage's theory in his manuscript for Thomson on the 'Kinetic Theory of Gases' (Number 377, see para. 5 and note (7)). Compare Jenkin's discussion of Lesage in 'The atomic theory of Lucretius': 238–9, where he alluded to the 'dynamical imperfections' of Lesage's hypothesis.

(9) Compare Number 266.

means not metaphysical passivity(10) but a measurable quantity namely the number of pounds in the body.

Suppose that a chemist asserted that the addition of a certain substance (say phlogiston) diminished the weight of a body, a physicist would say that there could not be very much of it in the Solar System or facts would be different.

But if two bodies were found to be of equal mass (by giving them equal and opposite velocities and observing them come to rest after impact) and if phlogiston added to one of them diminished its mass so *measured* then either the mechanical properties of the body and of the phlogiston are not simply added together, or the phlogiston by itself would have negative mass, that is, a force acting on it would cause it to move against the force, which is pure nonsense.

Again on the undulatory theory of light, the medium is supposed to communicate motion from one part to another by the action of elastic force which gradually changes the motion of each portion. This implies that the medium is material and that the number of pounds of it in a cubic mile might be ascertained*(11) though as yet we have no evidence of gravitation acting on it.

These examples are meant to show that the true test of matter is its relation to the force which alters its motion.

When matter is in motion it has two mathematical possessions.

1 Momentum, a directed quantity equal to Mass × Velocity and in the direction of the velocity.

2 Kinetic Energy an absolute positive quantity, without direction measured by $\frac{1}{2}$ Mass × (Velocity)^2.

This was formerly called Vis Viva and the controversy between the Newtonians & Leibnitzians was about which (1) or (2) was the more excellent measure of the Motion of a Body.

* See Thomson on the Value of a Cubic Mile of Sunlight and the density of the Aether Trans R S E 1854.(11)

(10) In his review of the second edition of Thomson and Tait's *Treatise on Natural Philosophy* (Cambridge, 1879), Maxwell rejected their assertion that 'matter has an innate power of resisting external influences', which he terms the 'Manichæan doctrine of the innate depravity of matter'; see J. Clerk Maxwell, 'Thomson and Tait's Natural Philosophy', *Nature*, **20** (1879): 213–16, esp. 214 (= *Scientific Papers*, 2: 779). For comment see P. M. Harman, *Metaphysics and Natural Philosophy* (Brighton, 1982): 140–5.

(11) William Thomson, 'Note on the possible density of the luminiferous medium and on the mechanical value of a cubic mile of sunlight', *Trans. Roy. Soc. Edinb.*, **21** (1854): 57–61 (= *Math. & Phys. Papers*, 2: 28–33).

The *facts* are that in *all* cases of the mutual action of a system of bodies
1 the *geometrical* sum of the momenta reckoned according to their directions remains constant.
2 In certain cases visibly, and in all cases if we could measure it the sum of the kinetic energies taken arithmetically together with the sum of certain other energies called potential energies remains constant.

Berkeley quotes (with disdain) a passage of Torricelli which seems appropriate. 'Matter is nothing but an enchanted vase of Circe, which serves for a receptacle of the force and the momenta of impulse. Power* and impulse† are such subtle abstracts, are quintessences so refined, that they cannot be enclosed in any other vessels but the inmost materiality of natural solids'.(12)

I so far agree with this, that I cannot admit any theory which considers matter as a system of points which are centres of force acting on other similar points, and admits nothing but these forces. For this does not account for the perseverance of matter in its state of motion and for the measure of matter.

I am afraid I have been tiresome on this subject but I consider it of the first importance in physics to know what we mean by matter and how to measure it, and both natural and mental science writers often go astray at the beginning about these things.

You ask about the definition of Energy. Energy is of two kinds, Kinetic and Potential.(13) Energy is the capacity which a body has of doing *work*.(14) A moving body has energy due to its motion called kinetic energy and measured by $\frac{1}{2}$ mass × (velocity)^2.

A body or system of bodies which are connected so that forces act between them tending to alter their relative position is capable of work and this capacity is called Potential Energy. Two heavenly bodies tending to approach or (say) the earth and a weight have potential energy due to gravitation. Electrified bodies have electrical potential. Elastic springs when bent &c &c are examples of potential energy.

* = Energy † = Momentum? in modern language

(12) The passage from Torricelli's *Lezioni Accademiche* is cited in Berkeley's 'De Motu; sive de motus principio et natura, et de causa communicationis motuum', (trans.) 'Concerning motion; or the origin and nature of motion, and the cause of communicating it', in The *Works of George Berkeley, D.D., Bishop of Cloyne*, ed. G. N. Wright, 2 vols. (London, 1843), 2: 83–103, on 86n (exactly as quoted by Maxwell). He subsequently consulted the Torricelli text itself: see Numbers 294 esp. note (29) and 437: Appendix.

(13) The term 'kinetic energy' had been introduced by Thomson and Tait, *Natural Philosophy*: 163; and 'potential energy' by W. J. M. Rankine, 'On the general law of the transformation of energy', *Phil. Mag.*, ser. 4, **5** (1853): 106–17, on 106.

(14) Compare Thomson and Tait, *Natural Philosophy*: 177–8.

Energy of both kinds is capable of exact measurement, and the progress of science at present is in the direction of measuring additional forms of energy.

Now the conception of Kinetic Energy is simply that of a moving mass which can do work till it is stopped.

Potential energy is force acting between bodies and capable of continuing to act between them while they yield to its action.

Now the conception of a body in motion is more fundamental than that of the power of producing motion in other bodies (commonly called force of attraction repulsion &c). Hence when any form of potential energy (such as the elasticity of air) can be explained by motion only, the explanation is a step in advance, and I suppose that gravitation itself will not be satisfactorily understood till it is explained in some way not involving action at a distance.

I have examined several attempts by various speculators, but the dynamics in all were faulty. I have also tried to apply to gravitation the same method which I had found useful in electromagnetism, but I have found no opening for such a theory of gravitation.[15]

I merely state this to show that there is a desire among men to explain action apparently at a distance by the intermediate action of a medium and then to explain the action of the medium as much as possible by its motion, and so to reduce Potential Energy to a form of Kinetic Energy.

Energy is never destroyed, but some of its transformations are not reversible. I have investigated the case of a multitude of molecules moving in a confined space and occasionally deflecting each other from their paths.[16] This is a question in pure dynamics but it gives results which apply with great exactness to the properties of gases.

In whatever way the motion is distributed among the particles at first, it is very quickly distributed according to a certain law according to which the number of particles having velocities between certain limits can be found. There remains always a great difference between the greatest and the least velocities and the molecules are often gaining and losing velocity but a general law of distribution prevails like that of wealth in a nation by which the proportion having so much above or below the average is calculable.

Now in a nation you can pick out the rich people as such, but in a gas you cannot pick out the swift molecules either by mechanical or chemical means.

As a simpler instance of an irreversible operation which (I think) depends on the same principle suppose so many black balls put at the bottom of a box

(15) See Numbers 217 esp. note (11), 238 and 309.
(16) See Number 263.

and so many white above them. Then let them be jumbled together. If there is no physical difference between the white and black balls, it is exceedingly improbable that any amount of shaking will bring all the black balls to the bottom and all the white to the top again, so that the operation of mixing is irreversible unless either the black balls are heavier than the white or a person who knows white from black picks them and sorts them.

Thus if you put a drop of water into a vessel of water no chemist can take out that identical drop again,(17) though he could take out a drop of any other liquid.

(Can you tell me without trouble what is meant by the expression 'idem *numero*' in metaphysical or theological language as when James Bernoulli calls his spiral 'carnis nostrae... post varias alterationes -- ejusdem *numero* resurrecturæ symbolum'.)(18)

I think you are right in thinking that we are likely to arrive at physical indications of a beginning & an end. That end is not a destruction of matter or of energy but such a distribution of energy that no further change is possible without an intervention of an agent who need not create either matter or energy but only direct the energy into new channels.

I do not think that anyone can have a second-hand acquaintance with a physical *principle* such as the ideas of matter force, energy, motion. To every one it must either be a mere word or a true form of thought, whether he is a professional experimenter or a mathematician or a lover of wisdom. The power to understand and assimilate elementary physical ideas (such as those contained in the Definitions and Axioms in Newtons Principia) is not confined to professed mathematicians or experimenters.

Experiments are often made to illustrate these principles but not to prove them, and these illustrations help to explain what is meant, say, by Action and Reaction but no one has proved them equal, or has required any proof, after he knew the meaning of the terms.

All these things therefore, being the foundations of science are as much the property of one man as of another. The discovery of such principles is the work of eminent men like Archimedes, Galileo, Newton, Young, Faraday but when they are illustrated and explained they may be fully understood as a

(17) Compare Maxwell's letter to J. W. Strutt of 6 December 1870 (Number 350).

(18) '[Our curve] will be a symbol of our flesh which will arise the same in number after various changes and finally also death itself'. See Jakob Bernoulli, 'Lineae cycloidales, evolutae, anti-evolutae, causticae, anti-causticae peri-causticae. Earum usus & simplex relatio ad se invicem. Spira mirabilis', *Acta Eruditorum* (May 1692): 207–13, esp. 213 (= *Opera*, 2 vols. (Lausanne/Geneva, 1744), **2**: 491–502, esp. 502). Compare Volume I: 96.

useful mental possession by any one whose mind is sufficiently open to receive them.

At the same time it is possible to become eminent both in mathematics and experiment with a very faulty set of first principles provided they are not often appealed to and so brought to the test.

I think that Newton through himself Desaguliers[(19)] Bentley[(20)] Locke[(21)] Gregory[(22)] and others exercised a very great influence on English thought among men who were neither mathematicians nor astronomers and that Voltaires scientific writings[(23)] produced a very great influence on French thought, though several most important ideas in Newton have been almost lost in the process of transmission, so that I would recommend the reading of Newtons Definitions & Axioms to every scientific man who is not familiar with them ⟨and others will find there the origin of several expressions which are now part of our mother tongue⟩.

If I had taken more time I should have occupied yours less with trying to answer your queries.

I remain
Yours faithfully
J. Clerk Maxwell

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(19) J. T. Desaguliers, *A Course of Experimental Philosophy*, 2 vols. (London, 1734–44).

(20) Richard Bentley, *A Confutation of Atheism from the Origin and Frame of the World* (London, 1693).

(21) Interpreting John Locke's *Essay Concerning Human Understanding* (1690) as 'Newtonian' in inspiration.

(22) David Gregory, *The Elements of Astronomy, Physical and Geometrical*, 2 vols. (London, 1715).

(23) F. M. A. Voltaire, *Lettres Philosophiques* (Amsterdam, 1734).

REPORT ON PAPERS BY FRANCIS BASHFORTH,[1] JAMES ATKINSON LONGRIDGE[2] AND CHARLES WATKINS MERRIFIELD[3] ON THE MOTION OF PROJECTILES

19 MAY 1868

From the original in the Library of the Royal Society, London[4]

REPORT ON PROFESSOR BASHFORTHS PAPER ON THE RESISTANCE OF THE AIR TO THE MOTION OF ELONGATED PROJECTILES[5]

The Author has examined the theory of chronographs as applied to the investigation of the motion of artillery projectiles, and also other methods of determining their velocities.

The method of Robins' ballistic pendulum[6] has the disadvantage that it gives only one velocity for each round, and the initial velocity must be deduced from the supposition that it depends on the known weight of the projectile & that of the powder or that it is the same in successive rounds.[7]

The author therefore has adopted the method of determining the times of transit of the projectile through ten planes at equal distances from each other, thus finding the time corresponding to nine different spaces. He finds that these times may be represented with sufficient accuracy by the formula

$$t = as+bs^2$$ [8]

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(1) St John's 1840, Fellow 1843, Professor of Applied Mathematics to the Advanced Class of Artillery Officers, Woolwich 1864 (Venn, *DNB*).

(2) Trinity 1841; an engineer with interests in gunnery (Venn; and see 'Armstrong, Sir W. G.' (*DNB*)).

(3) FRS 1863, Principal of the Royal School of Naval Architecture and Marine Engineering 1867 (Boase).

(4) Royal Society, *Referees' Reports*, **6**: 17.

(5) Francis Bashforth, 'On the resistance of the air to the motion of elongated projectiles having variously formed heads', *Phil. Trans.*, **158** (1868): 417–41. The paper was received by the Royal Society on 30 January 1868, and read on 20 February 1868; see the abstract in *Proc. Roy. Soc.*, **16** (1868): 261–3.

(6) Described by Benjamin Robins in his *New Principles of Gunnery* (London, 1742); and see W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 548–51.

(7) See Bashforth, 'On the resistance of the air to the motion of elongated projectiles': 418.

(8) Bashforth, 'On the resistance of the air to the motion of elongated projectiles': 437; *s* and *t* denote space and time.

which indicates a resistance proportional to the cube of the velocity and equal to $2bv^3$.(9)

As much labour has already been spent in the interpretation of the readings in the experiments and apparently good results have been obtained I think it worth while to point out the theoretically best value of b according to the method of least squares, supposing equal errors in each observation of time.

$$s_n = nl^{(10)}$$

and let t_n be the time of passing the $n+1^{\text{th}}$ screen then let

$$t_0 - c = \epsilon_0$$
$$t_0 - c - al - bl^2 = \epsilon_1$$
$$\text{-----------}$$
$$t_n - c - nal - n^2bl^2 = \epsilon_n.$$

Let $\quad t_0 + t_1 + \&c + t_n = \sum (t), \quad (t_1 + 2t_2 + \&c + nt_n) = \sum (nt)$

$$(t_1 + 4t_2 + \&c + n^2t_n) = \sum (n^2t)$$

then we have the following equations to determine the best values of a b c

$$\sum (t) = nc + \frac{n(n+1)}{2}al + \frac{n(n+1)(2n+1)}{6}bl^2$$

$$\sum (nt) = \frac{n(n+1)}{2}c + \frac{n(n+1)(2n+1)}{6}al + \frac{n^2(n+1)^2}{4}bl^2$$

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(9) Bashforth had noted in his 'On the resistance of the air to the motion of elongated projectiles': 417, that: 'According to Newton's law [Principia, Book II, Prop. X], the resistance of the air varies as the square of the velocity.... But in spite of grave doubts respecting the accuracy of Newton's law, it has been adopted by most of the eminent mathematicians who have written on the subject'. This was the law as stated in contemporary mechanics texts; and by Maxwell in an 1852 draft on the motion of a body in a resisting medium (see Volume I: 214–15). Bashforth however noted that 'in 1719 John Bernoulli gave equations for finding ... the path &c of a projectile, when the resistance of the air was supposed to vary according to any power of the velocity.' In his *A Mathematical Treatise on the Motion of Projectiles, founded chiefly on the results of experiments made with the author's chronograph* (London, 1873): 45–9, Bashforth explained that his expressions for the path of a projectile on the supposition that the resistance varies according to any power of the velocity, from which he had derived tables on the assumption of a third power law, were based on Bernoulli's method. 'Professor Adams communicated to me the above expressions ... in a letter dated Nov. 13 1866, when the few ballistic experiments I had then made seemed to indicate a cubic law of resistance. Professor Adams at the same time remarked that ... "an equivalent process was given long ago by John Bernoulli".' See Johann Bernoulli, 'Responsio ad non-neminis provocationem', *Acta Eruditorum* (May 1719): 216–26, and 'Operatio analytica per quam deducta est ... solutio', *ibid.* (May 1721): 228–30 (= Johann Bernoulli, *Opera Omnia*, 4 vols. (Lausanne/Geneva, 1742), **2**: 393–402, 513–16).

(10) l is the distance between the screens, which are connected by an electric circuit, passed by the projectiles in Bashforth's experiments.

$$\sum(n^2t) = \frac{n(n+1)(2n+1)}{6}c + \frac{n^2(n+1)^2}{4}al + \frac{n(n+1)}{30}(6n^3+9n^2+n-1)\,bl^2.$$

From these equations we find

$$bl^2 = \frac{30\{(n+1)(n+2)\sum(t) - 6(n+1)\sum(nt) + 6\sum(n^2t)\}}{(n-2)(n-1)\,n(n+1)(n+2)}.$$

It is not difficult after finding b to determine the probability of a term indicating that the resistance increases faster or slower than $2bv^3$. b should be determined for each round and the weight size and kind of projectile given together with the mean velocity at which b was determined. In this way it may be ascertained whether $\frac{2b\,.\,W}{d^2}$[11] is a quantity independent of v and dependent only on the kind of shot.

Professor Bashforth appears to have determined b by eliminating a between three observed times from the equation[12]

$$\frac{t_n}{n-1} - \frac{t_3}{2} = \overline{n-3}\,bl^2$$

or a similar equation. As he has in general nine equations for each round it is well to use the most advantageous combination of them.

The Chronograph

Most chronographs aim at producing uniform motion. Some of these attempt to make the mean motion in long periods constant others to reduce the greatest and least velocities as nearly as possible to the mean.

For the exact measurement of small portions of time the constancy of the mean rate is of small importance but the disturbances of the motion are of great importance. Hence the simpler the mechanism the simpler is likely to be the nature of the motion. A complex mechanism containing rapidly revolving parts and having several degrees of freedom is likely to be liable to a considerable number of superposed oscillations of different periods most of which are unknown and difficult of investigation.

In M^r^ Bashforths Chronograph the moving part may be described as a

(11) W is the weight of the shot, d its diameter; see Bashforth, 'On the resistance of the air to the motion of elongated projectiles': 438.

(12) Bashforth obtains the general equation $t_n/(n-1) = al + \overline{n-1}\,bl^2$; finding numerical values of $t_n/(n-1)$ for each experiment, and taking the difference of two of these quantities, he obtains $t_n/(n-1) - \frac{1}{2}t_3 = \overline{n-3}\,bl^2$; see Bashforth, 'On the resistance of the air to the motion of elongated projectiles': 437.

rigid body revolving round a vertical axis. If care is taken that this axis is a principal axis through the centre of gravity and vertical then the velocity of rotation is affected only by the resistance of the air and friction. If the centre of gravity is not in the axis and if the axis is not vertical there will be a disturbance having a period of one revolution which will be easily detected from the clock records.

If the axis though vertical is not a principal axis or not through the centre of gravity there will be a tendency to make the stand of the instrument oscillate or work round in a circle which also will be easily detected.

The absolute measure of time is entrusted to a pendulum clock as in the Greenwich Chronograph,[(13)] and as each set of observations is the result of a good deal of labour & expense it is worth while to interpolate as Mr Bashforth has done so as to find the true time of each transit on a scale of divisions which constantly increase.

The other methods enumerated are deficient in simplicity of construction and in simplicity of motion.

Electrical Arrangement

The object of the electrical arrangement is that the interval of time between the breaking of the circuit and the motion of the marker shall be the same whatever screen is broken.[(14)]

Let R be the resistance of the coil of the electromagnet
L its coeff^t of self-induction
C the capacity of a foot of covered wire of the connexions
Y the quantity of electricity which passes the electromagnet after the circuit is broken at S, where the distances from L are a & b feet,

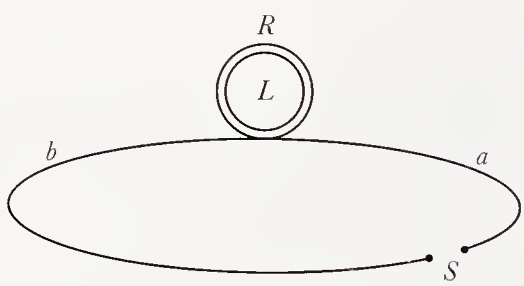

Figure 288,1

then $$\frac{1}{c}\left(\frac{1}{a}+\frac{1}{b}\right)X+R\frac{dX}{dt}+L\frac{d^2X}{dt^2}=0$$

whence $$X = Ce^{nt} \quad \text{where} \quad n = -\frac{R}{2L}\pm\sqrt{\frac{R^2}{4L^2}-\frac{1}{CL}\left(\frac{1}{a}+\frac{1}{b}\right)}.$$

(13) See the 'Description of the galvanic chronographic apparatus', appendix to *Astronomical and Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the Year 1856* (London, 1858).

(14) See note (10) and Bashforth, 'On the resistance of the air to the motion of elongated projectiles': 421–3.

If the connecting wire is kept off the ground C is very small and n will be a large impossible quantity indicating a rapidly decreasing alternating current. The period of alternation will depend on $\frac{1}{a}+\frac{1}{b}$ and therefore on the position of the break, but the rate of decrease depends on $\frac{R}{2L}$ which is independent of the position of the screen. To make the electromagnets act quickly they should be made of bundles of iron wire insulated from each other, and the keepers should never be in contact with them.

Professor Bashforth has published much of the matter of his paper in the Proceedings of the Royal Artillery Institution(15) nevertheless as the methods are good and the results remarkable I think it deserving of publication in the Philosophical Transactions.(16)

II

M^r LONGRIDGE'S PAPER(17)

The author discusses several laws of velocity, and has obtained an expression for the distance in terms of the initial and final velocity when the resistance varies as the p^{th} power of the velocity. He has applied this to the results given in the report of the Special Armstrong & Whitworth Committee(18) for the velocities at 40, 440 and 840 yards and finds $p = 8.747$.

The results were obtained by Navez electroballistic pendulum.(19) I do not know whether the different velocities are those of the same shot but in any case the value of the results must depend on the method employed which is not described.

As an example of an effort to derive working mathematical formulae from

(15) Francis Bashforth, 'Description of a chronograph, adapted for measuring the varying velocity of a body in motion through the air, and for other purposes', *Minutes of Proceedings of the Royal Artillery Institution*, **5** (1866): 161–92; published separately (London, 1866).

(16) In a letter to Stokes of 26 April 1868 (Royal Society, *Referees' Reports*, **6**: 16) W. J. M. Rankine stated that Bashforth's paper was a 'valuable communication, and in every respect eligible for being printed in the Philosophical Transactions'.

(17) J. A. Longridge, 'On the resistance of the air to rifled projectiles' (Royal Society, AP. 50.10). The paper was received by the Royal Society on 13 February 1868, and read on 27 February 1868; see the abstract in *Proc. Roy. Soc.*, **16** (1868): 263–6.

(18) The *Report of the Special Armstrong and Whitworth Committee. Vol. II. Minutes of Evidence, Appendix, and Index* (London, 1866): 500–3 (*Parliamentary Papers* (1866) XLII) includes results of experiments on the velocities of projectiles.

(19) This instrument is described by Bashforth, 'On the resistance of the air to the motion of elongated projectiles': 419; see also Stephen Vincent Benet, *Electro-ballistic Machines and the Schultz' Chronoscope* (New York, 1866): 10–11, 39.

a limited number of facts the paper is valuable but I doubt whether we can have much confidence in any part of the process.[20]

III Mr MERRIFIELD'S PAPER[21]

The data in this case were the elevations required for different ranges in shooting with Metfords match rifle.

If we consider the horizontal motion only and if $\frac{dv}{dt} = -mv^3$ [22] then

$$m = 2\frac{t.v-s}{s^2V}$$

where t is the actual time V the initial velocity and s the space.[23] If the vertical motion is unaffected by the resistance we may find t (which is not observed) as the time in vacuum for an assumed velocity with the given elevation. (This ought to be verified.) I do not know how the initial velocity was found except that it gives consistent results, but I have no doubt that the author had a method for finding it which he could explain. This paper is also an example of deduction from data which are not so complete as might be wished, but the number of experiments, the conciseness of the method and the consistency of the results incline me to believe that this paper ought to be printed in the Transactions.[24]

(a) J. CLERK MAXWELL

(a) {Maxwell} Report on Three Papers on the motion of Rifled Projectiles / J. Clerk Maxwell / 19 May 1868

(20) The paper was not printed in the *Philosophical Transactions*, despite a judgement by W. J. M. Rankine, in a letter to Stokes of 21 April 1868 (Royal Society, *Referees' Reports*, **6**: 162), that it was 'on the whole eligible for publication in the Philosophical Transactions, because it contains a useful series of investigations regarding the application of various formulae for resistance to previously published experiments.' Longridge had himself commented ('On the resistance of the air to rifled projectiles', f. 8) that: 'He was prepared to find the resistance increasing at a higher ratio than the cube of the velocity... but the ninth power staggered him, and he thought that there must be either some error of observation in the results, or that the form of the assumed function was altogether wrong.'

(21) C. W. Merrifield, 'On the law of the resistance of the air to rifled projectiles', *Phil. Trans.*, **158** (1868): 443–6. The paper was received by the Royal Society on 19 March 1868, and read on 23 April 1868; see the abstract in *Proc. Roy. Soc.*, **16** (1868): 321–2.

(22) m is 'the coefficient of resistance'. Merrifield stated that 'I found the resistance to vary as the cube of the velocity'; compare Bashforth's conclusion.

(23) See Merrifield, 'On the law of the resistance of the air to rifled projectiles': 443–4. Maxwell considers only the horizontal motion and ignores the inclination which appears in Merrifield's expression.

(24) A judgement confirmed by W. J. M. Rankine in a letter to Stokes of 26 June 1868 (Royal Society, *Referees' Reports*, **6**: 182), stating the paper to be an 'important contribution... of especial value in connection with two papers on the same subject... lately... read to the Royal Society... highly eligible for publication in the Philosophical Transactions'.

ABSTRACT OF PAPER 'ON A METHOD OF MAKING A DIRECT COMPARISON OF ELECTROSTATIC WITH ELECTROMAGNETIC FORCE; WITH A NOTE ON THE ELECTROMAGNETIC THEORY OF LIGHT'

[10 JUNE 1868][1]

From the *Proceedings of the Royal Society*[2]

ON A METHOD OF MAKING A DIRECT COMPARISON OF ELECTROSTATIC WITH ELECTROMAGNETIC FORCE; WITH A NOTE ON THE ELECTROMAGNETIC THEORY OF LIGHT[3]

By J. Clerk Maxwell, F.R.SS.L. & E.

Received June 10, 1868

(Abstract)

The experiments described in this paper[4] were made in the laboratory of Mr. Gassiot,[5] who placed his great battery of 2600 cells of bichloride of mercury at the disposal of the author. Mr. Willoughby Smith[6] lent his resistance-coils of 1,102,000 Ohms; Messrs. Forde[7] and Fleeming Jenkin lent a sensitive galvanometer, a set of resistance-coils, a bridge, and a key for

(1) The date the paper was received by the Royal Society. The paper was read on 18 June 1868: see note (2).

(2) *Proc. Roy. Soc.*, **16** (1868): 449–50.

(3) Published in *Phil. Trans.*, **158** (1868): 643–57 (= *Scientific Papers*, **2**: 125–43). Reporting on the paper in a letter to Stokes of 16 July 1868 (Royal Society, *Referees' Reports*, **6**: 180) Fleeming Jenkin declared the paper 'eminently suited for publication in the Transactions'; while in a letter to Stokes of 19 October 1868 (Royal Society, *Referees' Reports*, **6**: 181; printed in Wilson, *Stokes–Kelvin Correspondence*, **1**: 336) William Thomson stated that it 'ought to be published by all means in the Transactions'.

(4) Maxwell published a revised account of these 'Experiments on the value of v, the ratio of the electromagnetic to the electrostatic unit of electricity' in the 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-ninth Meeting of the British Association* (London, 1870): 434–8, on 436–8.

(5) John Peter Gassiot: see Number 242 esp. note (8).

(6) Willoughby Smith, Chief Electrician to the Telegraph Construction and Maintenance Co. (Boase).

(7) Henry Charles Forde, telegraph engineer, partner of Fleeming Jenkin (Boase).

double simultaneous contacts; and Mr. C. Hockin(8) undertook the observation of the galvanometer, the adjustment of the resistances, and the

(8) Insight into the technical difficulties of the experiment is provided by a letter from Charles Hockin to Maxwell dated 15 May 1868 (ULC Add. MSS 7655, II/30; first published in I. B. Hopley, 'Maxwell's determination of the number of electrostatic units in one electromagnetic unit of electricity', *Annals of Science*, **15** (1959): 91–108, on 97–8). 'Dear Sir, / I tested all the coils & the galvanometer today. Becker could not let me have room so I took them to S^t^ Mary's. The results most unsatisfactory. The boxes containing Siemens were well enough the errors being not much more than I have some times found after carrying a box of coils from one place to another with all sorts of variations of temperature. But the box containing B.A. units did not at all agree, one coil marked 33,000 had a resistance 100,000 & more – the sum of all boxes being 1,100,000 (1,096,500). I shall do them again tomorrow morning. If the error arises from bad contact anywhere the result is *still not* to come out the same. This brings v from 22×10^7 to $24\frac{1}{2}\times10^7$. / The galvanometer has been altered since I had it last the resistance 45,290 $(1\pm1/200)$. The shunts remaining as before. I expect to get tomorrow results true to 1/1000 the coils being together in a room like M^r^ Gassiotts at a pretty constant temperature. The 31 coil is right & agrees with the coils in Jenkins boxes $(\pm1/2000)$. I made them at different times from different standards so there is no great error there. I fancy any amount of error may be expected in the absolute value of the micrometer screw. Have you any objection to my taking the screw to Mathiessen's to measure it?... / How was the contact ensured with the insulated disc? It seems to me very imperfect & I do not understand how the touching the micrometer screw produced such a deflection now I have seen the arrangement. Was this screw anyhow in *metallic* connection with the suspended system? If so one could understand it better. I find among my notes for the attraction of two uniformly electrified discs of radii c & c', $c \ll c'$ & at a distance h
$\frac{q}{s}\cdot\frac{q'}{s'}\times2\pi^2c^2\left(1-\frac{h}{c'}+\frac{3}{8}\frac{hc^2}{c'^3}-\frac{1}{2}\frac{h^3}{c'^3}\right)$ or $\frac{P^2}{8v^2}\cdot\frac{c^2}{h^2}\left(1-\frac{h}{c'}+\frac{3}{8}\frac{hc^2}{c'^3}-\frac{1}{2}\frac{h^3}{c'^3}\right)$ but do not know if it is right. This correction is very large but the wrong way. G is of little importance. I cannot think that the errors in s & s' can be at all considerable enough to account for the difference of 1 per cent even. / You are right that the small battery (galv.) should have had two coils as well as the other as at first proposed. With one coil & one needle one is altogether free of errors arising from changes of relative sensitiveness in the two needles & a change in the magnetism of the single needle does not appear. I do not think however that an error of this kind can be the cause of the difference – I found when the galvanometer was made that when a very strong current was passed through it the correctness of the instrument was impaired slightly as a differential instrument & therefore had that little moveable coil made at the back. A very slight motion of this coil I have found sufficient to correct after using the instrument for weeks for all sorts of work the correction of the order 1/10,000 or so. / Also the reasonable agreement of the values of the two coils on different days would seem to preclude the idea of 10 or more per cent error on this account. I will make some experiments on this tomorrow. When a galv. with one needle is rendered astatic by a needle at the top so as to be 1000 times as sensitive as we had the galvanometer a strong current will double or half its sensitiveness. But this is not like the case in question. It is essential to know how a very strong current affects the ratio of the coils & how far a change in level does so this I will find out & let you know. Jenkin tells me he wants galv. as you said on 29^th^. I will return it in time. / Yours truly / C. Hockin.' The letters s and s' denote the areas of the discs, P the potential

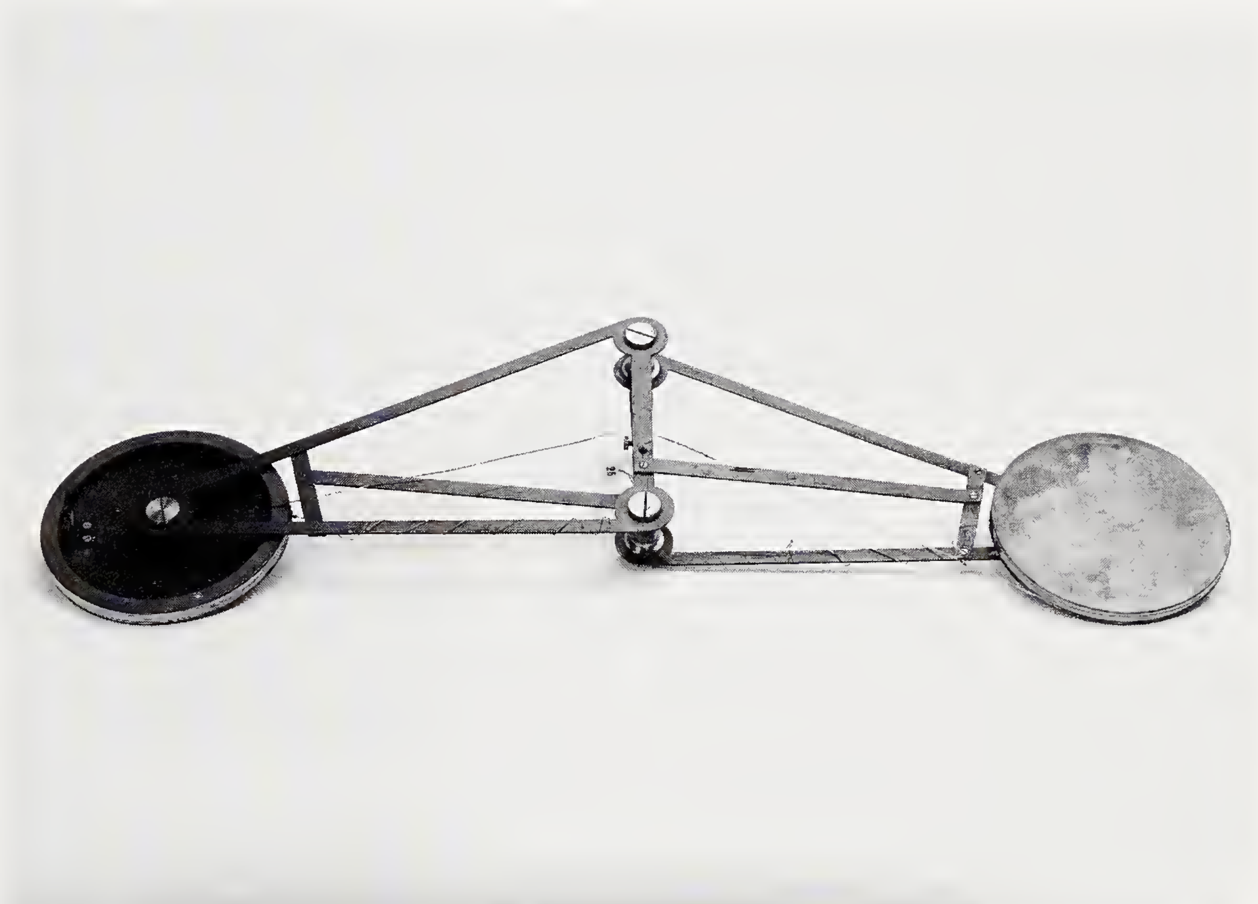

Plate XI. The torsion balance arm (1868) from Maxwell's apparatus for the determination of the ratio of the electrostatic to the electromagnetic unit of electricity (Number 289).

testing of the galvanometer, the resistance-coils and the micrometer-screw. The electrical balance itself was made by Mr. Becker.(9)

The experiments consisted in observing the equilibrium of two forces, one of which was the attraction between two disks, kept at a certain difference of potential, and the other was the repulsion between two circular coils, through which a certain current passed in opposite directions. For this purpose one of the disks, with one of the coils attached to its hinder surface, was suspended on one arm of a torsion-balance, while the other disk, with the other coil behind it, was placed at a certain distance, which was measured by a micrometer-screw.(10) The suspended disk, which was smaller than the fixed disk, was adjusted so that in its position of equilibrium its surface was in the same plane with that of a 'guard-ring', as in Sir W. Thomson's electrometers,(11) and its position was observed by means of a microscope directed on a graduated glass scale attached to the disk. In this way its position could be adjusted to the thousandth of an inch, while a motion of much smaller extent was easily detected.

An exactly similar coil was placed at the other end of the torsion-balance, so as to get rid of the effects of terrestrial magnetism.

It was found that though the suspended disk and coil weighed about half a pound, a very slight want of equality between the opposing forces could be detected, and remedied by means of the micrometer.

The difference of potential between the disks was maintained by means of Mr. Gassiot's great battery. To measure this difference of potential, it was made to produce a current through Mr. Willoughby Smith's resistance-coil, and the primary coil of the galvanometer shunted with a variable resistance.

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difference between them. Following a notebook entry dated 'May 28th [1868]' recording data, Maxwell jotted: 'Memorandum of Improvements. / 1 object glass tube of microscope to be made longer and firmer. / 2 a guard round the suspended discs in metallic connexion with the case and the mercury cup. / 3 Hoopers compound removed. / 4 Index of micrometer put right. / 5 Selenium resistance?'. (King's College London Archives, Maxwell Note Book (3)).

(9) Carl Becker: see Number 214 note (5).

(10) See Number 243.

(11) Thomson's guard-ring electrometer measured the potential between two discs at different potentials. One disc is fixed; and a central portion of the second disc is separated from the rest to form the attracted disc, the outer ring forming the remainder of the disc being fixed and forming the guard ring. Force is measured on the central part of the discs, where it is regular. Thomson described his electrometer in his 'Report on electrometers and electrostatic measurements', *Report of the Thirty-seventh Meeting of the British Association for the Advancement of Science; held at Dundee in September 1867* (London, 1868): 489–512, esp. 497–501 and Plate 6, Fig. 11 (= *Electrostatics and Magnetism*: 281–6). The instrument is described, and Thomson's figure reproduced, in *Treatise*, **1**: 266–71 (§§216–18). See also G. Green and J. T. Lloyd, *Kelvin's Instruments and the Kelvin Museum* (Glasgow, 1970): 20–1, and frontispiece, and Number 459 notes (4) and (5).

The current in the coils was maintained by a Grove's battery,[(12)] and was led through the secondary coil of the galvanometer.

One observer, by means of the micrometer-screw, altered the distance of the disks till the suspended disk was in equilibrium at zero. At the same time the other observer altered the shunt, till the galvanometer-needle was also in equilibrium. The micrometer reading and the resistance of the shunt were then set down as the results of the experiment.

The mean of twelve satisfactory experiments, at distances varying from .25 to .5 inch, gave for the ratio of the electromagnetic to the electrostatic unit of electricity –[(13)]

$$
\begin{aligned}
v &= 27.79 \text{ Ohms, or B. A. units.}\\
&= 277{,}900{,}000 \text{ metres per second.}\\
&= 174{,}800 \text{ statute miles per second.}^{(14)}
\end{aligned}
$$

This value is considerably lower than that found by MM. Weber and Kohlrausch by a different method, which was 310,740,000 metres per second.[(15)] Its correctness depends on that of the B. A. unit of resistance, which, however, cannot be very far from the truth, as it agrees so well with Dr. Joule's thermal experiments.[(16)]

It is also decidedly less than any estimate of the velocity of light, of which the lowest, that of M. Foucault, is 298,000,000 metres per second.[(17)]

In a note to this paper[(18)] the author gave his reasons, in as simple a form

(12) On Grove's voltaic battery (consisting of zinc and platinum plates) see W. R. Grove, 'On a small voltaic battery', *Phil. Mag.*, ser. 3, **15** (1839): 287–93.

(13) The ratio of electrical units is expressed in terms of the resistance of the British Association standard coil: see Number 235 note (3). In the 'Report of the committee on standards of electrical resistance', *Report of the Thirty-fourth Meeting of the British Association* (London, 1865): 345–67, table facing 349, the coil forming the British Association unit, denoted the 'B.A. unit or Ohmad' had a value of '10,000,000 metres/second according to experiments of Standard Committee'.

(14) In the British Association report on 'Experiments on the value of v': 438, and in the paper 'On a method of making a direct comparison of electrostatic with electromagnetic force' as published in *Phil. Trans.*, **158** (1868): 651, Maxwell corrected these values: 'Mean value of v = 28.798 Ohms, or B.A. units, or 288,000,000 metres per second, or 179,000 statute miles per second.' (*Scientific Papers*, **2**: 135).

(15) See Number 238 esp. notes (22) and (23).

(16) J. P. Joule, 'Determination of the dynamical equivalent of heat from the thermal effects of electric currents', in the 'Report of the committee on standards of electrical resistance', *Report of the Thirty-seventh Meeting of the British Association* (London, 1868): esp. 512–22. Determining the mechanical equivalent of heat by measuring the heat generated by a current flowing through a resistance, Joule provided a check on the value of the B.A. unit of resistance.

(17) See Number 238 esp. note (25).

(18) *Phil. Trans.*, **158** (1868): 652–7 (= *Scientific Papers*, **2**: 137–43).

as he could, for believing that the ratio of the electrical units, and the velocity of light, are one and the same physical quantity, pointing out the difference between his theory and those of MM. Riemann(19) and Lorenz,(20) which appear to lead to the same conclusion.

(19) See Maxwell's letter to Tait of 12 March 1868 (Number 284, esp. note (9)).
(20) See Number 284 note (14).

REPORT ON A PAPER BY ALFRED DES CLOISEAUX[1] ON THE OPTICAL PROPERTIES OF CRYSTALS

circa LATE MAY 1868[2]

From the original in the Library of the Royal Society, London[3]

REPORT ON A PAPER ON THE DISPERSION OF THE OPTIC AXES IN HARMOTOME AND WÖHLERITE BY M. DES CLOISEAUX[4]

On account of my ignorance of crystallography and of the recent researches on the optical properties of crystals I am not able to judge of the originality of this paper and can only form a rough estimate of its merits.

If the crystallographic axes of a mineral are all at right angles and coincide with the three principal axes of the wave surface for any kind of light they will probably coincide in direction for all kinds of light and the optic axes will be in the plane containing the extreme axes and will be equally inclined to these axes on opposite sides.[5]

If the ratio of the axes of 'elasticity' is different for different kinds of light there will be 'dispersion' of the optic axes[6] the axes, as the light is made to vary will move in their own plane through equal and opposite angles. If they meet, they will then open out in a plane at right angles to the first.

(1) Eminent crystallographer, author of *Manuel de Minéralogie* (Paris, 1862–93), Foreign Member of the Royal Society, 1875; see *Proc. Roy. Soc.*, **63** (1898): xxv–xxviii.

(2) According to the Royal Society's *Register of Papers Received* Des Cloiseaux's paper was referred to Maxwell on 13 May 1868, and approved for publication on 2 July 1868.

(3) Royal Society, *Referees' Reports*, **6**: 100.

(4) A. L. O. Des Cloiseaux, 'New researches upon the dispersion of the optic axes in harmotome and wöhlerite, proving these minerals to belong to the clinorhombic (oblique) system', *Phil. Trans.*, **158** (1868): 565–75. The paper was received by the Royal Society on 12 March 1868, and read on 23 April 1868; see the abstract in *Proc. Roy. Soc.*, **16** (1868): 319–21. Harmotome (baryta-harmotome or morvenite) is a barium and aluminium silicate, and wöhlerite a silicon–zirconium niobate of calcium and sodium; see Henry Watts, *A Dictionary of Chemistry*, 5 vols. (London, 1863–9), **3**: 12–13, **5**: 104.

(5) The case of orthorhombic crystals.

(6) The expression was familiar in the literature; see James MacCullagh, 'On the dispersion of the optic axes, and of the axes of elasticity, in biaxial crystals', *Phil. Mag.*, ser. 3, **21** (1842): 293–7. For a brief discussion see G. G. Stokes, 'Report on double refraction', *Report of the Thirty-second Meeting of the British Association for the Advancement of Science* (London, 1863): 253–82, esp. 271–4 (= *Papers*, **4**: 157–202). See also the bibliography in Émile Verdet, *Leçons d'Optique Physique*, 2 vols. (Paris, 1869–70), **2**: 212–16; and Verdet's discussion in *ibid.*: 180–8.

But if the crystallographic axes are not at right angles,[7] the axes of elasticity cannot coincide in direction with them but their directions as well as their ratios may depend on the kind of light.[8] As the directions of these axes remain at right angles, their motion must be one of rotation.

A rotation about the mean axis will make the bisectors of the optic axes revolve in their own plane.

This rotation is the least likely to occur on account of the difference of the extreme axes, which causes a considerable variation of optical properties to produce but a small rotation.

A rotation about an extreme axis will be shown by 'twisted dispersion' seen in the plane perpendicular to this axis, and 'horizontal dispersion'[9] that is a displacement perpendicular to the line joining the extremities of the axes as seen in the plane perpendicular to the other principal axis.

'Twisted dispersion' may be expected most frequently in plates normal to the *acute* bisector of the optic axes and 'horizontal dispersion' in plates normal to the obtuse bisector on account of a small difference of properties in directions in the plane of the first plate producing considerable rotation, the principal elasticities being nearly equal. This is the case in Harmotome but in Wöhlerite the rotation is about the obtuse bisector.

These properties show that there are not three rectangular axes of absolute symmetry in either of these bodies and the same is deduced from the action of heat on these crystals, which in one case appears to depend on the previous as well as the actual temperature.

The kind of light employed in the study of this kind of dispersion is not mentioned. A mixture of light from the extreme ends of the spectrum may be obtained by Dove's Dichrooscope[10] by the use of small blue glass of proper thickness, by the use of a flame coloured with two substances or by direct superposition of the prismatic colours. I have found a mixture of three kinds of light belonging to the red green and blue parts of the spectrum produced by a prism useful in making achromatic combinations and I should think such a mixture useful in detecting 'twisted dispersion'.

The investigation is evidently a careful one and the crystals of obscure

(7) As in the case, considered by Des Cloiseaux, of examples of monoclinic crystals, termed 'clinorhombic' by Des Cloiseaux, the French term being 'clinorhombique' (see Verdet, *Leçons d'Optique Physique*, **2**: 182–3).

(8) A possibility which had been noted by Stokes in his 'Report on double refraction': 274.

(9) Terms employed by Des Cloiseaux, 'New researches upon the dispersion of the optic axes in harmotome and wöhlerite': 570.

(10) H. W. Dove, 'Das Dichrooskop', *Ann. Phys.*, **110** (1860): 265–78; (trans.) 'The dichrooscope', *Phil. Mag.*, ser. 4, **20** (1860): 352–60.

characters, and I have no doubt of the propriety of publishing the paper in the Transactions if Prof^{r} Miller is also satisfied.(11)

J. CLERK MAXWELL

(11) William Hallowes Miller, Professor of Mineralogy at Cambridge, Foreign Secretary of the Royal Society, 1856–73 (Venn, *DNB*), had communicated Des Cloiseaux's paper to the Royal Society.

REPORT ON A PAPER BY ROBERT MOON[1] ON THE IMPACT OF COMPRESSIBLE BODIES

8 JULY 1868

From the original in the Library of the Royal Society, London[2]

REPORT ON M^r MOON'S PAPER 'ON THE IMPACT OF COMPRESSIBLE BODIES'[3]

This paper consists of three parts. The first part has already been published in the 'Proceedings'[4] and appears to contain all the essential matter of the paper. The second part consists of a translation of the first part into mathematical language, and the third part contains remarks on the equations of propagation of waves in an elastic body.

The object of the paper appears to be explained in a note at the end at least I have not been able to gather it from anything else in it. The author several years ago endeavoured to prove the existence in fluids of a force of resistance which is not taken into account in the ordinary theory of fluid motion.[5] If I recollect it right the force suggested was either of the nature of the 'rigidity' of elastic solids (a property admitted by others in the luminiferous medium) or similar to 'viscosity' in liquids. I am sorry I cannot now refer to what M^r Moon published and he has given no reference to it.

The investigation in this paper was undertaken 'to meet the objection of an eminent mathematician' that 'the velocities of the surfaces of contact of contiguous laminae are necessarily equal'.[6] I do not see how this objection can be overturned, or what bearing anything in the paper has on it, or how it is an objection to anything in the paper.

The case which the author has first described is that of two cylinders having the same axis one of which is initially at rest while the other has a velocity

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(1) Queens' 1834, Fellow 1839, Inner Temple 1838 (Venn).

(2) Royal Society, *Referees' Reports*, **6**: 190.

(3) Robert Moon, 'On the impact of compressible bodies, considered with reference to the theory of pressure' (Royal Society, AP. 50.11). The paper was received by the Royal Society on 22 April 1868, and read on 28 May 1868; see *Proc. Roy. Soc.*, **16** (1868): 411–14. The paper was communicated by J. J. Sylvester.

(4) See note (3).

(5) Robert Moon, 'On the theory of internal resistance and internal friction in fluids, and on the theories of sound and of auscultation', *Proc. Roy. Soc.*, **9** (1858): 223–7.

(6) Moon, 'On the impact of compressible bodies', ff. 15–16.

which as the cylinder is compressible may be different at different points. He then takes the case in which, at the moment of contact the front end of the impinging cylinder has no velocity but the parts behind have a forward velocity increasing as we go back along the cylinder. The author has said nothing whatever about the physical properties of the cylinder and he seems carefully to avoid the use of such words as force, pressure &c substituting for them 'transference of momentum'. For anything we are informed of in the paper, the subsequent motion of the different particles of the cylinder might be unaffected by their mutual action, and the hindmost particles might go on through those in front and through the cylinder at rest without disturbing them.

In the actual case, momentum will be transferred from the hindmost particles to those in front, which is neither more nor less than saying that forces will act between certain parts of the bodies. The relation between the amount of these forces and the relative state of these parts constitutes the 'rigidity' elasticity or plasticity of the body, about which the most scrupulous silence is maintained in the paper, but which nevertheless are absolutely necessary to prevent the one body from going through the substance of the other.

It is impossible to treat the question without knowing something of these forces. If we suppose them like those of elasticity, then in the case before us we have a disturbance confined to the first cylinder which will be propagated to the second by known laws. For an excellent statement of the consequences of impact in elastic cylinders see Thomson & Taits Nat. Phil. §§303, 304.(7) A perusal of the statement there given will justify the authors last remark that 'the extraordinary difficulty which the subject opposes to our apprehension when approached in this direction may be entirely obviated by contemplating it from a different point of view'.(8)

If M^r^ Moons 'extraordinary difficulty' had arisen from the metaphysics of motion or of matter, or if he had explained his objections to the use of the words force, pressure & elasticity; or if he had shown that some received theory does not agree with experiment, I should have had some difficulty in deciding, but as it seems to me that the difficulty of solving a dynamical problem without any consideration of forces or anything equivalent is of a

(7) Thomson and Tait, *Natural Philosophy*: 212–13; on collision in the case of 'compressibility with perfect elasticity'.

(8) Moon, 'On the impact of compressible bodies', f. 16; for 'point of view' read 'quarter'.

kind which I should be sorry to see overcome, I think it would be well not to print the paper in the Philosophical Transactions.[9]

JAMES CLERK MAXWELL

Glenlair
Dalbeattie
1868 July 8

(9) A judgement echoed in a letter from Thomas Archer Hirst to Stokes of 5 October 1868 (Royal Society, *Referees' Reports*, **6**: 189): 'Apart from the fact, therefore, that I hold these views to be unsound, further publication appears to be unnecessary.'

292

MANUSCRIPT ON THE MEASUREMENT OF SURFACE TENSION

circa SUMMER 1868[1]

From the original in the University Library, Cambridge[2]

ON AN INSTRUMENT FOR MEASURING THE SUPERFICIAL TENSION OF LIQUIDS

by J. Clerk Maxwell, F.R.S.L. & E.

The equilibrium of a liquid at its bounding surface may be determined by assuming that the surface is in a state of tension the tension depending only on the nature of the liquid, and being equal in all directions. The agreement of this theory with phenomena in the case of liquids in capillary tubes, has been investigated by various observers. The form of the surface near a vertical solid plane has been delineated by Felici*[3] the shape of drops has been studied by Waterston†[4] & Bashforth[5] and the tension of films has been directly measured by Van der Mensbrugghe§.[6]

Without entering into any theory of the cause of this tension such as that which has been given by Laplace[7] I intend to point out a method of determining the magnitude of the force in absolute measure.[8]

* Nuovo Cimento[3]

† Phil Mag[4]

§ Bulletin de l'Académie Royale de Belgique ser 2 tom xxii[6]

(1) See Number 293.

(2) ULC Add. MSS 7655, V, e/8. Previously published in I. B. Hopley, 'Clerk Maxwell's apparatus for the measurement of surface tension', *Annals of Science*, **13** (1957): 180–7, where the microscope (Plate XII) is identified.

(3) Read: Enrico Betti, 'Teoria della capillarità', *Nuovo Cimento*, **25** (1867): 81–105, 225–37 (see *Scientific Papers*, **2**: 548n).

(4) J. J. Waterston, 'On capillarity and its relation to latent heat', *Phil. Mag.*, ser. 4, **15** (1858): 1–19.

(5) Francis Bashforth, 'On capillary attraction', *Report... of the British Association... [for] 1862* (London, 1863), part 2: 2–3.

(6) G. Van der Mensbrugghe, 'Sur la tension des lames liquides', *Bulletin de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles*, ser. 2, **22** (1866): 308–28; *ibid.*, **23** (1867): 448–65; (trans.) 'On the tension of liquid films', *Phil. Mag.*, ser. 4, **33** (1867): 270–82; *ibid.*, **34** (1867): 192–202.

(7) P. S. de Laplace, *Traité de Mécanique Céleste*, 5 vols. (Paris, An VII [1799]–1825), supplements 'Sur l'action capillaire' and 'Supplément à la théorie de l'action capillaire' to Book X, Volume 4 (= *Oeuvres Complètes de Laplace*, 14 vols. (Paris, 1879–1912), **4**: 349–498).

(8) See also Maxwell's article on 'Capillary action' in *Encyclopaedia Britannica* (9th edn), **5** (Edinburgh, 1876): 56–71 (= *Scientific Papers*, **2**: 541–91).

Plate XII. Maxwell's microscope (1868) adapted for the measurement of the surface tension of liquids (Number 292).

For this purpose I take two portions of the free surface of the liquid one convex and the other concave and measure the difference of level of these surfaces and their radii of curvature.

The liquid is contained in a small vessel in the lid of which slide two vertical tubes. One of the tubes contains a simple diaphragm with a hole $\frac{1}{6}$ inch diameter. When this is dipped into the liquid and then raised above the general level the liquid adheres to the diaphragm and its upper surface within the hole becomes concave. The other tube contains a diaphragm in which is placed a short vertical tube $\frac{1}{6}$ inch diameter. When this is pressed down into the vessel the liquid rises in the short tube and forms a convex bead. To determine the position and curvature of these two surfaces the vessel is placed upon an accurately levelled plane surface above which a microscope is made to work vertically by means of a rack.

The microscope consists of an achromatic object glass of about $1\frac{1}{2}$ inch focal length and an eyeglass of about 1 inch focal length. At 7 inches from the object glass is placed a frame containing two spiders lines at right angles. Below this an opening is made in the side of the microscope in which slide the following parts – a tube at the end of which is placed a piece of parallel glass inclined 45° to the axis. Within this is another tube with a diaphragm having a slit in it along and across which are stretched fine spider lines. A mirror to reflect the light into this tube is placed outside. The tubes with the glass and the slit are so arranged that the virtual image of the cross lines in the slit formed by reflexion in one of the surfaces of the glass plate coincides accurately in position with the intersection of the cross lines in the axis of the tube. This is ensured by taking off the upper part of the instrument and examining the coincidence of the virtual image with the real cross lines by means of the microscope.

When the instrument is in use, the light enters the side tube and is reflected down the axis of the microscope by the parallel glass and after passing through the object glass falls on the surface of the liquid. It is then reflected and seen through the eye glass of the microscope. A real image of the cross lines of the slit is formed in the same place as that of the other cross lines at a definite distance below the object glass. If this point coincides either with the surface or with the centre of curvature of a spherical reflector the rays after reflexion will form an image of the cross lines of the slit coinciding in position with the real cross lines in the axis and may be examined along with them through the eye glass. When the point coincides with the surface of the fluid the image is erect and is undisturbed by slight movements of the fluid but when the point coincides with the centre of curvature the image is inverted and is rendered invisible by slight tremors of the fluid.

It is easy by means of this instrument to determine the radius of curvature of a spherical surface to .002 inch. It is necessary however that the coincidence of the virtual image of the lines in the slit with the actual lines in the axis of the microscope should be well observed. If these points instead of coinciding are distant x along the axis, the images of the points below the axis will be distant by y where $x = 13y$ in this instrument. If a and b are the scale readings when the erect and inverted image respectively are seen distinctly and if r is the radius of curvature of the reflecting surface

$$a-b = \pm\sqrt{r^2+y^2}.$$

Since r is seldom more than $\frac{1}{3}$ inch in the experiments it is necessary to make y very small in order to be able to take $a-b$ as the true value or r.

When the instrument was first constructed I introduced the light behind the cross lines in the axis and observed their coincidence with their own reflected image. In this case when the image is erect the coincidence is perfect and the reflected image cannot be seen at all being hidden by the cross lines themselves. The method I have now adopted admits of greater optical power and is easier in working but I have yet to ascertain which method gives the most accurate results, and to determine the actual values of the superficial tension of various liquids bounded either by air or by other liquids at various temperatures.

LETTER TO PETER GUTHRIE TAIT

14 JULY 1868[1]

From the original in the University Library, Cambridge[2]

Fixed Point of Reference} = {Glenlair Dalbeattie
Lat 55° Long. 4 1868 .534

D^r P. G. T.

Use no more the above triliteral expression on the outside of your communications as the expression Palace Gardens Terrace will not avail in communicating with me.

Your hypothesis about the law of conductivity[3] would be a sound one if the state of steady flow of heat were not a state involving the continual dissipation of energy. It may however be *true* though unsound. Does it agree with the *general* equations of steady flow as well as with those in one dimension? that is, Is the distribution of temperature in any case of steady flow such that if it were arrested at any instant the recoverable energy would be a min:.

I have improved my machine for finding the superficial tension of liquids. I find it decreases in water with the temperature very considerably and rapidly, say about 7.7 grammes to the metre at 69° & 5.6 at 100 °F.[4]

I am trying to get the temperature correctly by immersing my machine in hot water and I have succeeded in preventing the glass from being dimmed by the steam. I shall also try soap bubbles to test the effect of the soap.

I am doing figures of equipotential surfaces & lines of force.[5] The figure for Thomson Electrical Images[6] is very pretty if well done. I am also doing a few cases of conduction &c in plane sheets (what Rankine calls plane water

(1) See Maxwell's date: '1868 .534', where 0.534 of the year (366 days) = day 196 = 14 July.

(2) ULC Add. MSS 7655, I, b/11.

(3) See P. G. Tait, 'On the dissipation of energy', *Proc. Roy. Soc. Edinb.*, **6** (1868): 309–11; 'If an infinite plate be kept permanently heated in layers, each of equal temperature throughout – the temperature rising gradually from one side to the other – the hypothesis is made that the temperature of any three contiguous layers (of equal thickness) so adjust themselves that the least possible energy can be restored from the system of three.' See also Numbers 284, 294 and 296.

(4) See Number 292.

(5) Compare Figs. I–V appended to the *Treatise*, **1**.

(6) See Number 301.

lines)[7] and sections of the low spherical harmonics,[8] and *maps* of the surface harmonic of the 6th order as given in T.T′.[9]

I had a letter from Joule who says he is game to do the equivalent of heat by friction of mercury.[10] For whom do you want my vote, Jupiter I know but who is Vulcan? not he who fell on ice, is it?[11] & what the vote.[12] Send me the Vortices here.[13]

Yours
J.C.M.

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(7) W. J. M. Rankine, 'On plane water-lines in two dimensions', *Phil. Trans.*, **154** (1864): 369–91. See Number 223.

(8) Compare Figs. V–IX appended to the *Treatise*, **1**.

(9) Thomson and Tait, *Natural Philosophy*: 627–9.

(10) Maxwell's experiment described in his letter to Tait of 23 December 1867 (Number 278).

(11) William Thomson on 22 December 1860; see S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 412.

(12) See Number 294.

(13) See Number 294 esp. note (2).

LETTER TO PETER GUTHRIE TAIT

18 JULY 1868

From the original in the University Library, Cambridge[1]

Glenlair
18 July 1868

Dr T′

I have received Thomson on Vortices[2] which I will return in a little, as I am reading it with pains. I do not see the Comptes Rendus, nor do I perceive, without the aid of Bertrand, the 'legère faute' in H^2.[3] In fact I consider it impossible to commit one at the beginning of such a theory. You must either tell a 'rousing whid'[4] or be infallible. If equations 3 & 3a have anything the matter with them, may ξ stick in H^2s throat.[5] I not only believe them myself but set them to senate house men who did them.[6] I will send T a theory of the system of ring vortices with a common axis[7] if Kissingen will find him.[8]

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(1) ULC Add. MSS 7655, I, b/12.

(2) Thomson to Tait, 5 July 1868 (see Number 295 note (2)); note Thomson's paper 'On vortex motion', *Trans. Roy. Soc. Edinb.*, **25** (1869): 217–60 (= *Math. & Phys. Papers*, **4**: 13–66).

(3) Joseph Bertrand, 'Théorème relatif au mouvement le plus général d'un fluide', *Comptes Rendus*, **66** (1868): 1227–30 (read 22 June 1868). Bertrand had criticised the argument of Hermann Helmholtz's paper 'Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen', *Journal für die reine und angewandte Mathematik*, **55** (1858): 25–55, which had been translated by P. G. Tait as 'On the integrals of the hydrodynamical equations, which express vortex-motion', *Phil. Mag.*, ser. 4, **33** (1867): 485–512. Bertrand claimed that '[Helmholtz] a commis, dès le début de son Mémoire, une légère inadvertance qui, en lui faisant attacher à la condition d'intégrabilité signalée plus haut une importance tout à faire exagérée, entache d'erreur tous les résultats'. Defending his treatment of the condition of rotation of a fluid element ('Sur le mouvement le plus général d'un fluide', *Comptes Rendus*, **67** (1868): 221–5) Helmholtz replied: 'Si l'expression $(udx+vdy+wdz)$ [where at a point x, y, z in a liquid u, v w are the components of the velocity] est une différentielle exacte, il n'y a pas de rotation dans la partie du fluide correspondant. Si cette expression n'est pas une différentielle exacte, il y a rotation.' (on 222).

(4) Robert Burns, 'Death and Dr Hornbook', i (a lie) (*OED*).

(5) Helmholtz, 'On the integrals of the hydrodynamical equations, which express vortex motion': 491–2. Equations (3) and (3a) determine the variations of the angular velocities ξ, η, ζ during the motion of a fluid element. In further response to Bertrand's continued criticisms, Helmholtz pointed out (*Comptes Rendus*, **67** (1868): 754–7) that 'M. Stokes a dit, en parlant des quantités qui, dans les théorie des solides élastiques, correspondent aux quantités ξ, η, ζ de mon Mémoire: "Ces quantités expriment les rotations de l'élement du moyen..."'; see G. G. Stokes, 'On the dynamical theory of diffraction', *Trans. Camb. Phil. Soc.*, **9** (1849): 1–62, on 12 (= *Papers*, **2**: 243–327).

(6) See Numbers 254 note (1) and 275.

(7) Number 295.

(8) See S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 526; 'Kissingen having been recommended to Lady Thomson for its waters'.

I highly approve of Vulcan as the right man for Glasgow & Aberdeen[9] but I have been in correspondence with the Registrar & find I have no right to vote. If I can do anything else for Smith, I will. If I had influence in Greenock I would help Moncrieff there.[10]

On the back of this you will see a few of the equipotential & lines of force for an electrified point acting on an uninsulated sphere.[11] These are spent lines, I have done them better.

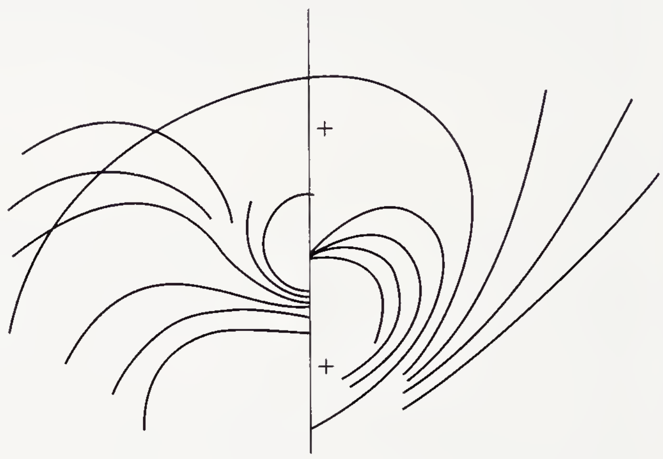

Figure 294,1

I send you another of plane (2 dimension) lines and one of the 1st spherical harmonic,[12] and a few remarks in pencil on chap II of T & T′.

W. P. Taylor Esqre has sent me several diagrams of girders with their forces which I will tack to my paper on diagrams of forces for the R. S. E. W. P. T. is an independent discoverer of the method along with Rankine.[13]

I do not believe that the conductivity of a body can be found from its sp. h. & temperature by your method.[14] Your method may be expressed thus.

Given the surface temperature of a body, then its actual temperature for steady flow of heat will be so distributed that the amount of energy recoverable at any instant from *certain portions* of the body will be a minimum. Now the final temperature of a system of unequally heated bodies does not depend on their collocation neither does the recoverable energy.

But conduction essentially depends on collocation, so that in order to apply the hypothesis you must *select* certain portions of the body (3 slices &c).

For if you take the whole body with given surface temperature the recoverable energy will be least when the whole body *except the surface* is at uniform temperature.

(9) The Act of Parliament 31 and 32 Vict., c. 48, conferred the franchise on the General Councils of the Universities of Aberdeen and Glasgow conjointly.

(10) Archibald Smith, Glasgow University 1828, senior wrangler 1836, Hon. LL.D. Glasgow 1864 (Venn, *DNB*), as prospective Liberal candidate for the Parliamentary seat of Aberdeen and Glasgow Universities; and James Moncreiff (see Volume I: 393n), elected MP for Aberdeen and Glasgow Universities in November 1868 (*DNB*).

(11) Maxwell's fragmentary sketch is reproduced as Figure 294,1.

(12) Figures 293,4 and 3 (for which latter compare the *Treatise*, **1**: Fig. V), respectively.

(13) See Number 334.

(14) See Number 293 esp. note (3).

Your lead cylinder looks a good form of exp^t.(15) How do you find the distance of your copper–iron junctions from the axis. Do you slice the cylinder at last? or do you use optical means to find where the bottoms of your pits are. If you do the rest of the job well you will require good measures of these distances. You should also if possible distribute your pits thus so as to be distant from each other. Each pit produces a disturbance $P\frac{1}{r}\sin\theta$ in the temperature about it on neighbouring pits, so separate them well.

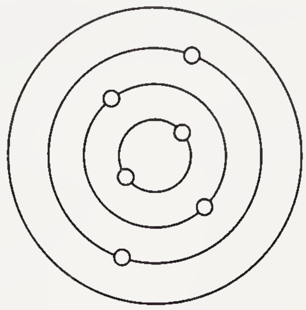

Figure 294,2

How do T & T′ divide the Harmonics (Her Majestys Ships) between them. I had before getting hold of T & T′ done mine for electricity but I should be delighted to get rid of the subject out of that book except in the way of reference to T & T′. My method is to treat them as the neighbourhood of singular points in potential systems, those of positive degree being p^ts of equilibrium and those of negative being infinite points.(16) (There is a relation between the numbers of each kind in any system.)

I then show that a complete harmonic of the ith degree has always i axes of which the directions are definite and then i poles & a constant give the $2i+1$ variable quantities of the harmonic.(17)

The complete H can also be expressed *as usual* as the sum of a set of H^s $i-s$ of whose poles are clubbed together while the other s are placed at equal distances round the equator.(18)

He may also be treated as the sum of a set of zonal H's with different poles.(19)

I have also an expression for $\Sigma Y_i\, Y'_i$ over the sphere where Y_i & Y'_i are of the same order but have different systems of poles.(20)

Also a prospectus of the theory as applied to closed surfaces of any form not spherical.(21)

Yours truly
J. Clerk Maxwell

(15) Tait's experiments on thermo-electricity, reported in his 'First approximation to a thermo-electric diagram', *Trans. Roy. Soc. Edinb.*, **27** (1873): 125–40.

(16) See Number 281; and *Treatise*, **1**: 157–8 (§128).

(17) See *Treatise*, **1**: 160–2 (§130).

(18) See *Treatise*, **1**: 163–5 (§132).

(19) See *Treatise*, **1**: 175–6 (§143) and Figs. VI–IX appended to the volume.

(20) See *Treatise*, **1**: 169–70 (§136–7). Y_i, Y'_i are surface harmonics.

(21) See *Treatise*, **1**: 178–80 (§146).

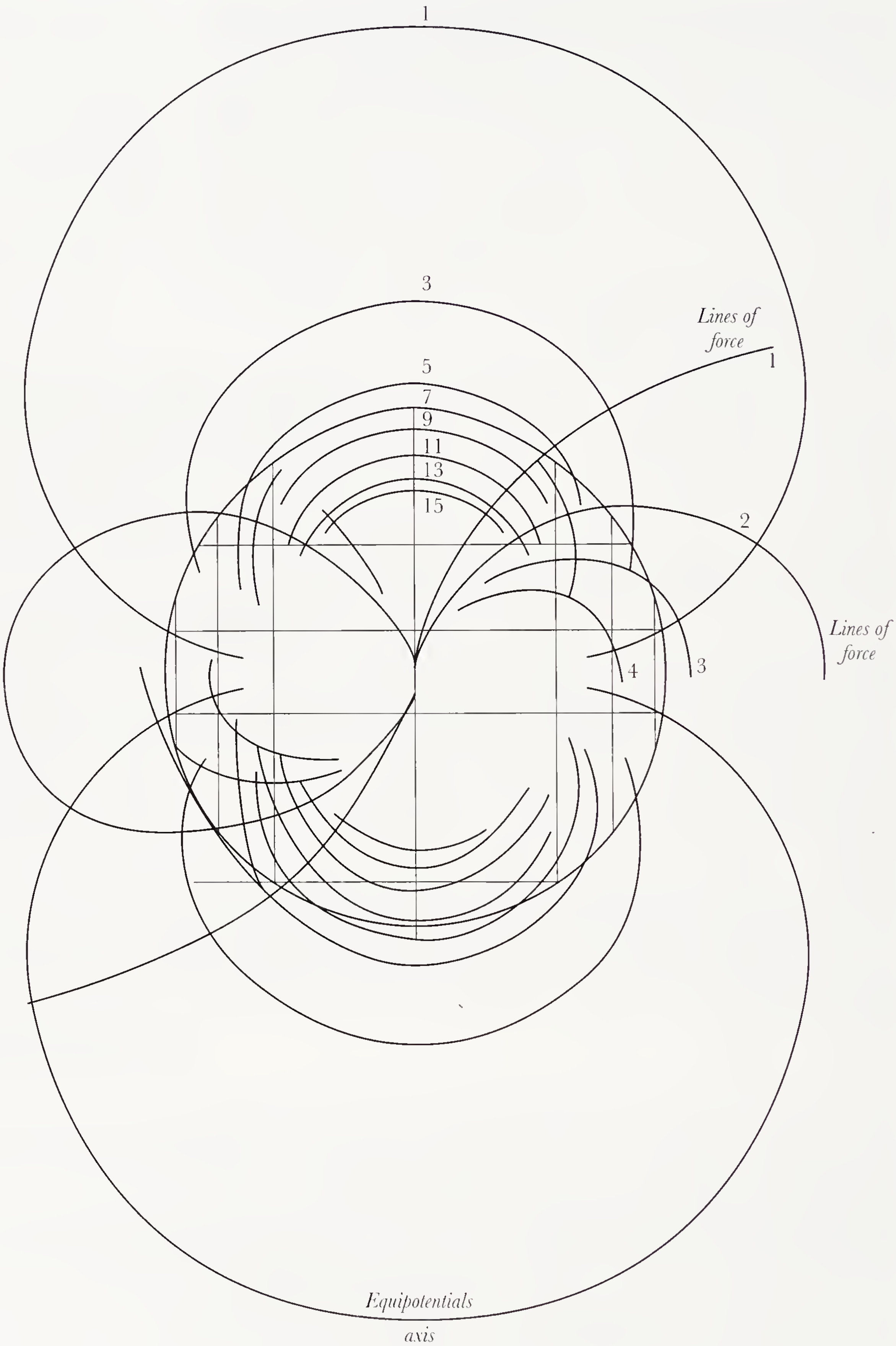

Figure 294,3. Section of 1[st] Harmonic Solid

APPENDIX: COMMENTS ON THOMSON AND TAIT'S *NATURAL PHILOSOPHY*, CHAPTER II

JULY 1868[22]

From the original in the University Library, Cambridge[23]

T+T′ ERRATA CHAP II[24]

§206 – at least in the beginning of the subject[25] – some[tr(26)] even –
§207 Matter is *never* perceived by the senses.[27] According to Torricelli, quoted by Berkeley 'Matter is nothing but an enchanted vase of Circe fitted to receive Impulse and Energy, essences so subtle that nothing but the inmost nature of material bodies is able to contain them'.[28] This from memory but I mean to look up Torricelli himself when I am at a seat of learning.[29]

Be careful as to stating anything to be a direct object of sense till you know what Bain says[30] and then use your discretion.

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(22) See Maxwell's reference in his letter to Tait.

(23) ULC Add. MSS 7655, V, h/12. Previously published (in part) in Knott, *Life of Tait*: 195.

(24) 'Chapter II. Dynamical laws and principles', in Thomson and Tait, *Natural Philosophy*: 161–2 (§§206–8).

(25) Compare *Natural Philosophy* (§206): 'The introduction to the *Principia* contains in a most lucid form the general foundations of Dynamics. The *Definitiones* and *Axiomata sive Leges Motûs*, there laid down require only a few amplifications and additional illustrations, suggested by subsequent developments, to suit them to the present state of science'. (26) translation.

(27) Compare *Natural Philosophy* (§207): 'We cannot, of course, give a definition of *Matter* which will satisfy the metaphysician, but the naturalist may be content to know matter as *that which can be perceived by the senses*'. In the second edition of their *Treatise on Natural Philosophy* (Cambridge, 1879): 219 Thomson and Tait retained this definition. On this occasion Maxwell criticised them publicly; see his review 'Thomson and Tait's Natural Philosophy', *Nature*, **20** (1879): 213–16, esp. 214 (= *Scientific Papers*, **2**: 779). See Number 287 note (10).

(28) Maxwell accurately quotes the passage, as reproduced in Berkeley's 'De Motu', in his letter to Mark Pattison of 13 April 1868 (Number 287, see esp. note (12)), here quoting from memory.

(29) In a notebook entry, probably dating from 1869 (Notebook (3), Maxwell Papers, King's College London Archives) he transcribed the passage from the *Lezioni Accademiche D'Evangelista Torricelli* (Florence, 1715): 25, from 'Della forza della percosa. Lezioni Quarta': 'Torricelli Evangelista Lezioni Accademiche Firenze 1715. En verescit Galicæus alter. L. IV p. 25. Questo è ben certo che la materia per se stessa è morta, e non serve se non per impedire, e resistere alla virtù operante. La materia altro non è, che un vaso di Circe incantato, il quale serve per ricettacolo della forza, e de' momenti dell' impeto. La forza poi, e gl'impeti sono astratti tanto sottili, son quintessenze tanto spiritose, che in altre ampolle non si posson racchindere, fuor che nell' intima corpulenza de' solidi naturali.' See Number 437: Appendix and note (48).

(30) Alexander Bain, *Mental and Moral Science. A Compendium of Psychology and Ethics* (London, 1868): 27–66 (on sensation).

§208 Newtons statement[(31)] is meant to distinguish matter from space or volume, not to explain either matter or density.
Def. The Mass of a body is that factor by which we must multiply the velocity to get the momentum of the body, and by which we must multiply the half square of the velocity to get its energy.

Hence if we take the Xchequer lb as unit of mass (which is made of platinum) and if we find a piece of copper such that when it and the Xchequer lb move with equal velocity they have the same momentum (Describe experiment) then the copper has a mass = 1 lb.

You may place the two masses in a common balance (which proves their *weights* equal) you may then cause the whole machine to move up or down. If the arm of the balance moves || itself the *masses* must also be equal.

Some illustration of this sort (what you please) is good against heresy, in the doctrine of the Mass. Next show examples of things which are not matter, though they may be moved and acted on by forces. 1 The path of a body, 2 Its axis of rotation 3 the form of a steady motion 4 an undulation (sound or light) &c 5 Boscovich's centres of force.[(32)] Next things which are matter such as the luminiferous aether, and if there be anything capable of momentum & kinetic energy.

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(31) Compare *Natural Philosophy* (§208): '*The Quantity of Matter* in a body, or, as we now call it, the *Mass* of a body, is proportional, according to Newton, to the *Volume* and the *Density* conjointly. In reality, the definition gives us the meaning of density rather than of mass'.

(32) Boscovich's theory of matter as mathematical points, or centres of force, was widely noticed by British physicists in the period, especially in Scotland (though his views were not always accurately described). A possible source for Maxwell may have been the lengthy account given by John Robison in his *A System of Mechanical Philosophy*, 4 vols. (Edinburgh, 1822), **1**: 267–368. Maxwell himself, in his article on 'Atom', in *Encyclopaedia Britannica* (9th edition), **3** (Edinburgh, 1875): 36–49, esp. 37 (= *Scientific Papers*, **2**: 448–9), later gave a good account of Boscovich's theory of matter as described in the *Theoria Philosophiæ Naturalis* (Rome, 1758; Vienna, $_2$1763); and see his comments in Numbers 287 and 437.

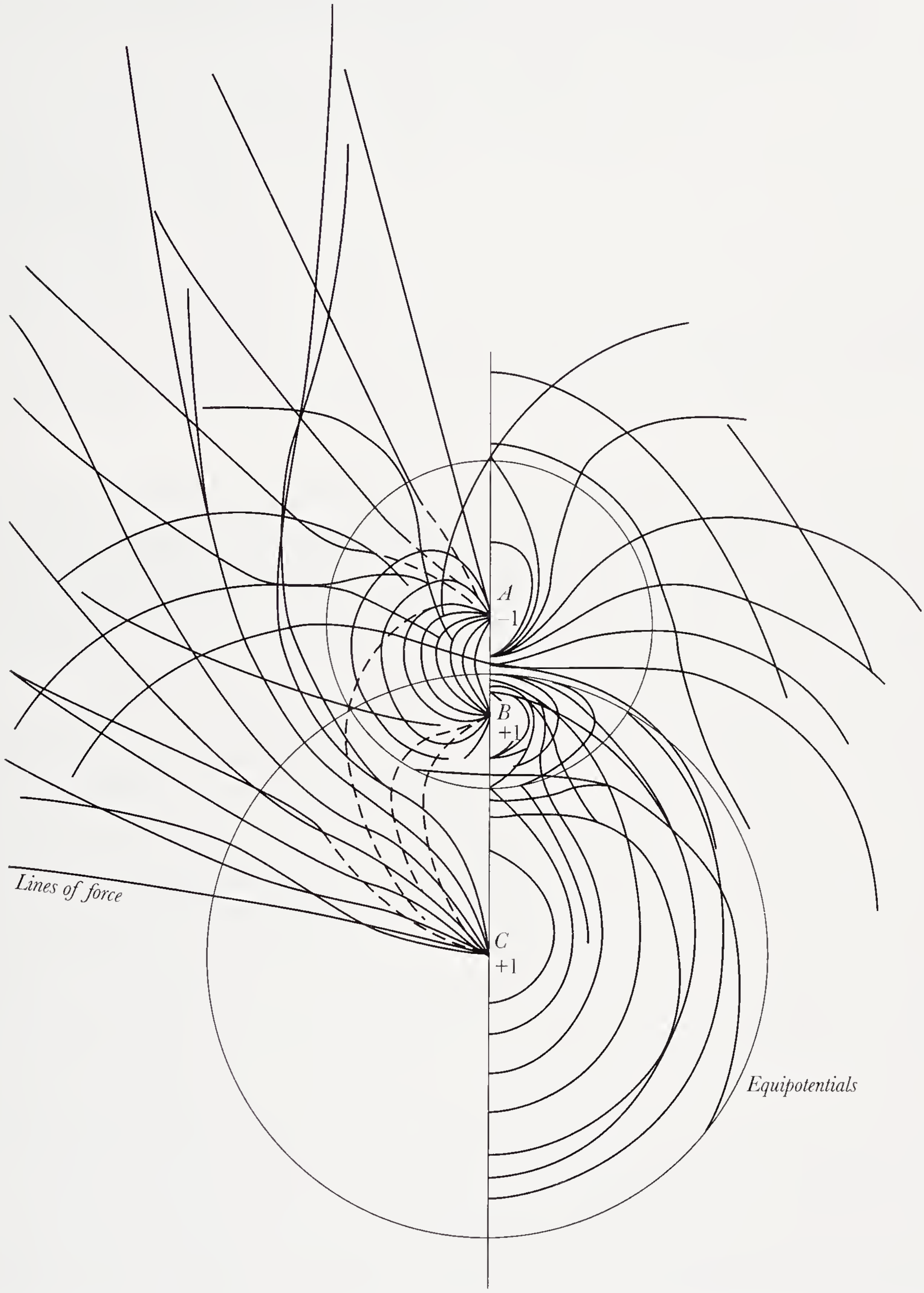

Figure 294,4. Lines in 2 dimensions. Small circle impermeable or if you please large circle perfect conductor and charged with double portion of opposite electricity to *A*.

LETTER TO WILLIAM THOMSON

18 JULY 1868

From the original in the University Library, Cambridge(1)

Glenlair
Dalbeattie
1868 July 18

(a)

Dear Thomson,

Tait sent me a letter of yours on Vortices.(2)

(a) {Tait} See p 6 for Query from T′.

(1) ULC Add. MSS 7342, M 102.

(2) In a letter to Tait of 5 July 1868 (Glasgow University Library, Kelvin Papers T 90), which Tait had forwarded to Maxwell, Thomson had discussed Helmholtz's 1858 paper on vortex motion as well as his own current paper on the subject (see Number 294 notes (2) and (3)). The relevant portions of Thomson's long and complex letter contain the following statements: 'I proposed to begin with irrotational motion and show the reform in its theory required by the footnote of H^2 tr^n p. 488. I should have begun with division irrotational $\begin{cases}\text{non-cyclic}\\ \text{cyclic}\end{cases}$. Cyclic require a core with double or multiple continuity. Consider an infinite liquid, or one contained in a very large fixed boundary at infinitely great distance in all directions from the field of motion and having no multiple continuity. Any number of rigid, flexible, more or less imperfectly elastic solids, frictional if they rub on one another, to be in the field of mot^n.... Apply any forces to these solids.... When the forces have ceased to act what they have done is called the *impulse* of the motion that super venes. The central axis of the system, treated Poinsotically, is the line (? or axis?) of the impulse, the resultant along it is the resultant impulse: the couple round it is the impulsive couple.... *I* The resultant impulse, its line & the couple round it, remain constant for ever (i.e. as long as no new impressed forces act, and no influence is produced by the solids coming near the infinitely distant boundary)./... Going back to *I*, let the solids be asunder, and let each, being flexible, have the proper motion given to it to make the core of any desired vortex & then let all become liquid. The component impulse in any line is equal to the sum of the projections on a plane $perp^l$ to that line, of all infinitesimal vortex filaments of equal cyclic constants, into which the whole rotationally moving parts of the fluid may be divided. Hence *I* shows that the centre of gravity of the whole area, of projection on a plane $perp^l$ to the line of impulse remains fixed; & the amount of the area constant: also that the sum of the moments of positive & negative areas of $proj^n$ on planes through the line of impulse, properly chosen to show the impulsive couple, is constant (or something to this effect). H^2's (9) expresses that the sum of the areas of $proj^n$ on the plane $\perp$ the line of impulse is constant. I am trying for a generalisation of (9)a or (9)b which should give the law of translation of a vibrating vortex, or group of vortices, but as yet don't see through it although it looks as if it should become transparent. / If you think it worthwhile and think he would make it out, you might send the above to Maxwell, as on a recent occasion he manifested a spirit of inquiry regarding vortices. But in any case secure him for Smith as this puts me in mind he must be surely on the Aberdeen list.' Thomson was referring

I am sorry I have no vote at Aberdeen or I should support A. Smith whom I consider w^{d} make the best university member to be found,(3) as he would rather ascertain what is wanted and compare plans than run off on any special hobby & so lose the confidence of the House.

I have not seen Bertrands refutation of Helmholtz(4) so I will proceed as if he were still existing.

From eqn^{n} 9b he deduces the velocity of translation of one ring $v = \dfrac{K}{4\pi h\mathfrak{M}R^2}$.(5) Hence we must find K. This is a question in electromagnetics & done in my paper thereon(6) or as follows.(7) First find N(8) for a cylindric vortex of rotation = velocity ζ and radius b from the eq^{n} $\dfrac{d^2N}{dx^2}+\dfrac{d^2N}{dy^2} = 2\zeta$.(9)

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to Tait's recent English translation of Helmholtz's paper on vortex motion, 'On the integrals of the hydrodynamical equations, which express vortex-motion', *Phil. Mag.*, ser. 4, **33** (1867): 485–512, esp. 488n, which reads: 'In complexly-connected spaces ϕ [the velocity potential] may have more values than one; and for multiple-valued functions which satisfy the above differential equation [Laplace's equation] Green's fundamental theorem does not hold; and hence a great number of its consequences which Gauss and Green have deduced for magnetic potential functions also fail, since the latter, from their very nature, can have but single values.' Thomson's reference to this passage and to equations (9) in Helmholtz's paper provides the context for Maxwell's response to Thomson's request.

(3) See note (2) and Number 294 notes (9) and (10).

(4) See Number 294 esp. notes (3) and (5), an issue alluded to by Thomson in his letter to Tait (see note (2)) to which Maxwell here responds. Thomson had observed that 'It is a pity that H^2 is all wrong and we all dragged so deep in the mud after him'.

(5) Helmholtz, 'On the integrals of the hydrodynamical equations, which express vortex-motion': esp. 509, his treatment of circular vortex filaments whose planes are parallel to xy and whose centres are symmetrical about the axis of z. In this equality K is the *vis viva* (kinetic energy) of the moving mass of fluid, h its density, R the mean radius of the rings, and $\mathfrak{M}$ the mass of a slice of fluid cut by a plane.

(6) J. Clerk Maxwell, 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 486–91 (= *Scientific Papers*, **1**: 562–8).

(7) In his 'On the integrals of the hydrodynamical equations': 486–7 Helmholtz noted '[the] remarkable analogy between the vortex-motion of fluids and the electro-magnetic action of electric currents... the velocities of the fluid elements are represented by the forces exerted on a magnetic particle by closed electric currents which flow partly through the vortex-filaments in the interior of the fluid mass, partly on its surface, their intensity being proportional to the product of the section of the vortex-filament and the angular velocity'. Compare also Maxwell's letter to Tait of 4 December 1867 (Number 276, esp. note (8)).

(8) L, M, N are potential functions.

(9) Helmholtz's equation for the potential of straight vortex filaments; see his 'On the integrals of the hydrodynamical equations': 503.

Within the cylinder $\mathcal{N} = A + \tfrac{1}{2}\zeta(r^2 - b^2)$

Outside $\mathcal{N} = A' + \zeta b^2 \log\frac{r}{b}$.

Since these coincide at the surface $A = A' =$ surface value of $\mathcal{N}$.

$$\text{Now } K = \frac{1}{2}\sum(\mathcal{N}\zeta) = \frac{1}{2}l\int_0^b 2\pi r dr\left(A + \frac{1}{2}\zeta(r^2 - b^2)\right)\zeta \qquad l = \text{length}$$

$$= \frac{1}{2}l\pi\zeta b^2\left(A - \frac{1}{4}\zeta b^2\right).$$

Now $\pi\zeta b^2$ is what H[2] calls $\mathfrak{M}$ and A is the surface value of $\mathcal{N}$.

In the straight cylinder the $\mathcal{N}$ outside depends only on $\mathfrak{M}$ so that we may suppose that in a ring of nearly circular section $\mathcal{N}$ will, outside be nearly that due to a linear ring of equal cyclic const. (or $\mathfrak{M}$).

Now for a linear ring radius a_1, at a distance r from it measured so that the distance of this point from the axis is a_2 we have various expressions for M.

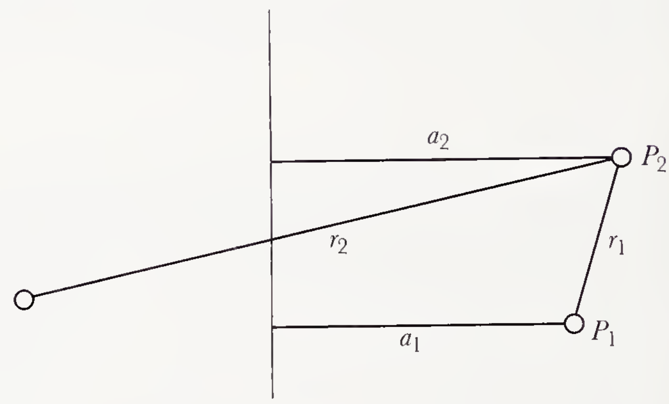

Figure 295,1

⟨Helmholtz⟩ The potential of the ring P_1 on P_2 when each carries unit electric current is

$$M = 2\pi\sqrt{aa_2}\left\{\left(c - \frac{2}{c}\right)F_c + \frac{2}{c}E_c\right\}^{(10)} \quad \text{where } c = \frac{2\sqrt{a_1 a_2}}{r_2}$$

or $$M = 4\pi\sqrt{aa_2}\frac{1}{c_1}\left(E_{c_1} - F_{c_1}\right) \quad \text{where } c_1 = \frac{r_2 - r_1}{r_2 + r_1}$$

or $$\left.\begin{aligned} M = 4\pi a \log\frac{8a}{r}&\left\{1 + \frac{1}{2}\frac{a - a_2}{a} + \frac{1}{16}\frac{3b^2(a_1 - a)^2}{a^2} - \&c\right. \\ -4\pi a&\left\{2 + \frac{1}{2}\frac{a - a_2}{a} + \frac{1}{16}\frac{b^2 - 3(a - a_2)^2}{a^2} - \&c\right. \end{aligned}\right\} \begin{array}{l} b = \text{distance} \\ \text{of planes.} \end{array}$$

The first terms of this give $M = 4\pi a\left(\log\frac{8a}{r} - 2\right)$.

(10) F_c and E_c are the complete elliptic integrals of the first and second orders for the modulus c. See Number 262 note (3).

Now at the surface $r = b$ and $\mathfrak{M}M = 2\pi aN$, hence

$$A = 2\left(\log\frac{8a}{b} - 2\right)\mathfrak{M}$$

and $$K = 2a\mathfrak{M}^2\left(\log\frac{8a}{6} - \frac{7}{4}\right).$$

Of this $2a\mathfrak{M}^2\left(\log\frac{8a}{6} - 2\right)$ is due to the irrotational motion and $2a\mathfrak{M}^2\left(\frac{1}{4}\right)$ to the rotational motion of the core itself.

If in my paper on the Electromagnetic Field you make

$$\begin{array}{cccccccccc} -2\pi p, & -2\pi q, & -2\pi r; & \alpha & \beta & \gamma; & \frac{F}{\mu} & \frac{G}{\mu} & \frac{H}{\mu}\text{(11)} \\ \xi & \eta & \zeta; & u & v & w; & L & M & N\text{(12)} \end{array}$$

the upper line being my notation the lower line will be Helmholtz.

To generalize (9) of H² (13)

Let ds & ds' be elements of two filaments the strengths of which are m & m' & direction cosines $\alpha\ \beta\ \gamma$, $\alpha'\ \beta'\ \gamma'$ then the value of L due to ds will be

$$-\frac{1}{2\pi}\frac{m\,ds\,\alpha}{r}$$

and that of u $$\frac{m}{2\pi r^3}ds(\gamma y - \beta z).$$

Now if A' is the projection of the area of s' in the plane yz the increment of Am' per unit of time due to the motion of ds' is

$$\begin{aligned} m'(v\gamma' - w\beta')\,ds' &= \frac{mm'\,ds\,ds'}{2\pi r^3}\{\alpha\gamma' z - \gamma\gamma' x - \beta\beta' x - \alpha\beta' y\} \\ &= \frac{mm'\,ds\,ds'}{2\pi r^3}\{\alpha(\alpha' x + \beta' y + \gamma' z) - x(\alpha\alpha' + \beta\beta' + \gamma\gamma')\}. \end{aligned}$$

(11) Maxwell, 'A dynamical theory of the electromagnetic field': 480–2 (= *Scientific Papers*, **1**: 554–6); p, q, r are electrical currents, α, β, γ magnetic forces, F, G, H the components of electromagnetic momentum, and μ the coefficient of magnetic induction.

(12) Helmholtz, 'On the integrals of the hydrodynamical equations': 487, 491, 496; ξ, η, ζ are the angular velocities of a fluid element, u, v, w the rectangular components of the velocity, and L, M, N potential functions.

(13) Helmholtz, 'On the integrals of the hydrodynamical equations': 508. Helmholtz's equation (9) is the integral $\iint \sigma\rho\,(d\rho/dt)\,d\rho\,d\lambda = 0$, for space filled with an infinite number of vortex rings, where σ is the angular velocity of a vortex ring, ρ its radius and λ its distance from a plane parallel to xy.

Now $(\alpha' x+\beta' y+\gamma' z)\, ds = rdr$ so that the integral of the first term round a closed curve is zero. If we consider the increment of mA due to ds' we shall obtain the second term with the sign of x reversed so that

$$\frac{d}{dt}mA+\frac{d}{dt}m'A' = 0 \quad \text{(This is eq}^{\text{n}}\text{ (8) of H}^2\text{)}^{(14)}$$

proved for filaments rings of any form, or if $A_1\ A_2\ A_3$ be the projections of the areas of rings of strength $m_1\ m_2\ m_3$ then

$$m_1 A_1 + m_2 A_2 + m_3 A_3 = \text{const.}$$

Next multiply the area ⟨swept out⟩ subtended normal to x by the element ds' by its strength m' and by its velocity u and we get

$$2\,\delta A m' u = \frac{mm'\, ds\, ds'}{2\pi r^3}\,(\gamma y-\beta z)\,(\gamma' y-\beta' z)$$

whence $$2\delta(Au+Bv+Cw)\,m' = mm'\, ds\, ds' \left\{\frac{\alpha\alpha'+\beta\beta'+\gamma\gamma'}{2\pi r} - \frac{(\alpha x+\beta y+\gamma z)\,(\alpha' x+\beta' y+\gamma' z)}{2\pi r^3}\right\}.$$

The second term disappears on integration and the first term becomes $-\int(L\alpha'+M\beta'+N\gamma')\, m'\, ds'$ or simply the energy due to the relation of m to m'.

Hence $(A'\bar{u}+B'\bar{v}+C'\bar{w})\, m' = \frac{1}{2}$ relative energy of m & m' where $\bar{u}\,\bar{v}\,\bar{w}$ are the mean velocities (vel of centre of gravity) of the filament m.

This agrees with H^2 provided the filament moves parallel to itself but for other cases I must examine further as I have not studied it up.

Since the velocity of translation of a ring vortex is greater than that of the fluid thro its centre, the portion of fluid which travels with it is always a ring & not a simply connected body. To draw it, draw the lines of flow referred to the ring as fixed & combine with a set of lines whose distances from the axis are as $1\ \sqrt{2}\ \sqrt{3}$ &c (lines of parallel flow).

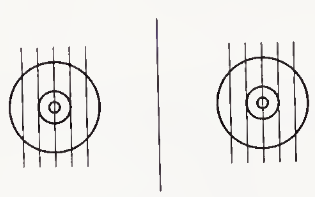

Figure 295,2

the resultant

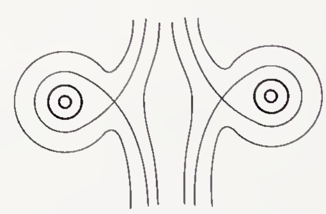

Figure 295,3

(14) Helmholtz, 'On the integrals of the hydrodynamical equations': 507. Equation (8) is $m\tau\chi+m_1\tau_1 g = 0$, for two vortex filaments m and m_1 at the points χ, z and g, c; where τ, τ_1 are the velocities in the direction of the radii χ and g; and $m = \sigma d\chi\, dz$ and $m_1 = \sigma dg\, dc$ (where σ is the angular velocity of the rings).

Have I told you that Hockin & I tried exp[ts] on electrical equilibrium[(15)] with 2600 cells of Gassiots[(16)] and get

$$v = 28.798 \text{ ohms or } 178{,}800 \text{ miles a second}^{(17)}$$

prob error $\frac{1}{6}$ per cent. on 12 exp[ts] varying from 1000 to 2600 cells and distances from 12 to 25 fiftieths of an inch. One difficulty was in obtaining equilibrium for more than a few seconds between the effects of the great battery & those of 9 Groves[(18)] which were used for the dynamometer effect. The *observation* of the unstable equilibrium was easy enough, as is seen by the agreement of results.

Let me know when you are in the W. of Scotland again. Address Glenlair.

Correspondence between Vortices & Electric Currents[(19)]

Electric Currents	Vortices
Strength of Current $\times 2\pi$	$=$ $-$strength of Vortex
Magnetic intensity at a point	$=$ velocity of fluid
Coefficient of magnetic induction (μ)	$= 4\pi \times \text{density} = 4\pi h$
$\dfrac{\text{Electromagnetic momentum at a point}}{\mu}$	$=$ resultant of $L\ M\ N$
energy of the system	$=$ energy of the system
$= \dfrac{1}{2}\sum (Fp + Gp + Hr)\, dV$	$= -\dfrac{1}{h}\sum (L\xi + M\eta + N\zeta)\, dV$
$= \dfrac{\mu}{8\pi}(\alpha^2 + \beta^2 + \gamma^2)\, dV$	$= \dfrac{1}{2}h\sum (u^2 + v^2 + w^2)\, dV.$
N° of lines of mag force through a closed curve	$=$ quantity of fluid passing through closed curve in unit of time.
Force on a circuit resolved parallel to x due to any other system	$=$ Rate of increase of area of ring vortex projected on a plane normal to x due to the action of other vortices.

This is the equation (9) generalized, and since the forces between two circuits are such that action & reaction are equal & opposite, the sum of the areas of two vortex rings is not altered by their mutual action.

(15) See Number 289.

(16) See Number 289 note (5).

(17) See Number 289 note (14).

(18) See Number 289 note (12).

(19) Compare Maxwell's analogy of the flow of an incompressible fluid to lines of force in his paper 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83 (= *Scientific Papers*, **1**: 155–229). See Volume I: Numbers 83, 84, 85 and 87.

$$\left.\begin{array}{l}\text{Number of lines due to an}\\ \text{element } ds' \text{ which pass through}\\ \text{a closed curve } S\end{array}\right\} = 6\pi \times \left\{\begin{array}{l}\text{Rate at which solid space is}\\ \text{generated by a conical surface}\\ \text{whose base is } S \text{ and vertex } ds.\end{array}\right.$$

Given a point P and a line S moving in any manner. Draw a conical surface with P for vertex & S for base. This surface will sweep over a certain space as S moves. If S is a closed plane curve of area A normal to x and if this plane is distant x from P then the whole space (conical) is $\frac{1}{3}Ax$ and the rate of sweeping is $\frac{1}{3}\left(A\frac{dx}{dt}-x\frac{dA}{dt}\right)$ in *this particular* case. It is less simple in other cases. If 6 times this quantity is multiplied by the strength of the circuit in the closed curve, we get the energy due to the action of the element ds' on the closed circuit S that is

$$\left(4A\frac{dx}{dt}-\frac{x\,dA}{dt}\right)m = \text{energy of } ds' \text{ on } S.$$

Similarly $$\left(4A'\frac{dx'}{dt}-x'\frac{dA'}{dt}\right)m' = \text{energy of } ds \text{ on } S'.$$

This is eq$^{\text{n}}$ (9a) of H^2.[(20)] Thus in algebra.

Let the element ds move with velocity $v_1(u\ v\ w)$ which is inclined θ_1 on the right side of ds. It describes in unit time a parallelogram $v_1\,ds\sin\theta_1$. The pyramid on this base with vertex at origin is $P=\frac{1}{3}p_1\,v_1\,ds\sin\theta_1$ where p_1 is the perp. on the parallelogram & P the pyramid and if $dx\ dy\ dz$ be the components of ds

$$3P = (vz-wy)\,dx+(wx-uz)\,dy+(uy-vx)\,dz.$$

If $$u=\frac{\gamma'y-\beta'z}{2\pi r^3}m' \quad v=\frac{\alpha'z-\gamma'x}{2\pi r^3}m' \quad w=\frac{\beta'x-\alpha'y}{2\pi r^3}m'$$

$$\begin{aligned}3P &= \frac{1}{2\pi r^3}\{(\alpha'dx+\beta'dy+\gamma'dz)\,(x^2+y^2+z^2)\\ &\quad -(xdx+ydy+zdz)\,(\alpha'x+\beta'y+\gamma'z)\}\\ &= \frac{1}{2\pi r}(\alpha'dx+\beta'dy+\gamma'dz)-\end{aligned}$$

(b)

I fear I have got into a mess with the benign equation in the attempt to make it suit non-circular rings.

(b) {Tait} Wherefore I have room to say that I wish particularly to know when you will be home – as they want me in Ireland – and I may as well waste my time there as here. Price, Kitchin, & M$^{\text{c}}$M. are *all* dunning me about S.B, 2$^{\text{nd}}$ Ed. of I, and II. Everything is at a standstill – / T′.

(20) Helmholtz, 'On the integrals of the hydrodynamical equations': 508.

I want to know what you think of T′ in his book on Heat §125 where he says that Verdets discovery that paramagnetics act oppositely on light from diamag[s](21) constitutes a proof that the polarity of both classes of bodies is the same.(22) This certainly requires explanation which I would be glad of, for myself & for the Heat Book.(23) He also gives your proof of the impossibility of a diamagnetic acquiring its reverse polarity gradually like a paramagnetic.(24) But Webers diamagn[c] hypothesis of induced molecular currents(25) does not lead to your absurd conclusion(26) any more than our Kings College coil could be made a perpetual motion.

T′ also speaks of the 'Fact established by Faraday'(27) that a diamag. takes the same polarity as a paramag. in the same position. I cannot find that Faraday thought he had established this as a fact. He certainly showed that the lines of magnetic forces, as related to induction of currents run in the same general direction in bismuth Iron & steel (that is, not in the opposite direction)(28) but he also showed that in a steel magnet placed in the opposite direction to its natural one the lines are as in bismuth provided the dominant magnetic force is strong enough compared with the steel magnet.(29) In the Exp Res. Vol III p 528 he gives reasons against the 'magnetic fluid' theory of the reverse polarity of Bismuth(30) which I do not think apply to the

(21) Émile Verdet, 'Recherches sur les propriétés optiques développées dans les corps transparents par l'action du magnétisme', *Ann. Chim. Phys.*, ser. 3, **52** (1858): 129–63.

(22) Maxwell is commenting (see Number 299) on the proofs of Tait's *Sketch of Thermodynamics* (Edinburgh, 1868): 72; in the published text Tait changed the argument to read: 'It seems most probable, notwithstanding this discovery of Verdet's, that the rotations constituting the magnetic force, in a diamagnetic body, are in the same direction, but of less amount, than in the surrounding medium'.

(23) See Number 278.

(24) See Tait, *Sketch of Thermodynamics*: 72–3, and W. Thomson, 'On the theory of magnetic induction in crystalline and non-crystalline substances', *Phil. Mag.*, ser. 4, **1** (1851): 179–86 (= *Electrostatics and Magnetism*: 465–80).

(25) Weber had supposed that the currents in paramagnetic molecules circulated in the opposite direction to the induced currents in diamagnetics: Wilhelm Weber, 'Ueber die Erregung und Wirkung des Diamagnetismus nach den Gesetzen inducirter Ströme', *Ann. Phys.*, **73** (1848): 241–56; Weber, 'Ueber der Zusammenhang der Lehre vom Diamagnetismus mit der Lehre von dem Magnetismus und der Elektricität', *ibid.*, **87** (1852): 145–89. See also Number 278, esp. note (8).

(26) Thomson, 'On the theory of magnetic induction': 186, and see Number 278 note (10).

(27) Deleted from the published text.

(28) Michael Faraday, 'Experimental researches in electricity. – Twenty-second series. On the crystalline polarity of bismuth (and other bodies), and on its relation to the magnetic form of force', *Phil. Trans.*, **139** (1849): 1–41 (= *Electricity*, **3**: 83–136).

(29) Michael Faraday, 'On some points of magnetic philosophy', *Phil. Mag.*, ser. 4, **9** (1855): 81–113 (= *Electricity*, **3**: 528–65, esp. 560–1 (§3357)).

(30) Faraday, 'On some points of magnetic philosophy', in *Electricity*, **3**: 534–7 (§§3309–12).

'induced current' hyp. He also gives a theory of the influence of media(31) which is first rate and Verdets discovery is the only objection which I can find against it. If you or T′ can get over Verdet I shall be much obliged to you for simplifying the theory of magnetism.(32)

I am drawing maps on stereographic projection of your tables of the 6th Harmonic (Surface of Sphere).(33) I mean to combine these so as to get some notion of the appearance of combinations of the selected terms, and the principal modifications of the general harmonic.

Any n^{th} Harmonic may be regarded as either

1° the sum of a series of not more than $2n+1$ of the zonal tesseral & sectorial forms having one axis common.

2° the sum of a number of zonal harmonics with different axes

3° A single harmonic got by differentiating $\dfrac{M}{r}$ n times in n given directions whose poles are the n poles of the harmonic.(34) It is completely defined by the const. M & the $2n$-spherical coords. of the n poles, and this be done only 1 way.

Yours

J. Clerk Maxwell

(31) Faraday, 'On some points of magnetic philosophy', in *Electricity*, **3**: 537–41 (§§3313–17).

(32) See also Ole Knudsen, 'The Faraday effect and physical theory, 1845–1873', *Archive for History of Exact Sciences*, **15** (1976): 235–81, esp. 259–60.

(33) See Number 293 esp. note (9).

(34) M/r is the potential of a point of charge M at a distance r. See Number 277 esp. note (6).

LETTER TO PETER GUTHRIE TAIT

circa 20 JULY 1868[1]

From the original in the University Library, Cambridge[2]

Pray direct & dispatch
enclosed to T.[3]

D^r^ T′

I do not think it necessary to explore Bertrand in the wake of H^2, the latter seems to me tolerably unassailable.[4] I have been transforming electromagnetic props into vortical ones, e.g. 'Two electric circuits act on one another with equal and opposite forces' becomes 'Two ring vortices of any form affect each others area so that the sum of the projection of the two areas on any plane remains constant'.[5]

/ I notice what H^2 says about conduction of heat & agree with him that something might be done from an investigation of irregularly distributed motion,[6] but not with you that it could be got from a maximum question, not involving the connexions of the parts.[7] But perhaps I do not understand you as you stand in print and probably you will yourself enlarge your standing ground.

Matthiessen finds general similarity between the conductivities of metals for heat & for electricity.[8] He also finds a wonderful equality in the effect of temperature on electric conductivity in all metals[9] *except* IRON & Thallium.[10]

But non metallic bodies conduct heat proportionally much better than

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(1) See note (3); and also Numbers 293 and 295.

(2) ULC Add. MSS 7655, I, b/13.

(3) Probably Number 295.

(4) See Number 293 esp. notes (3) and (5).

(5) See Number 295.

(6) See Hermann Helmholtz, 'Über discontinuirliche Flüssigkeits-Bewegungen', *Monatsberichte der... Akademie der Wissenschaften zu Berlin* (1868): 215–28. Helmholtz noted the analogy between the discontinuous motion of fluids and the conduction of heat.

(7) P. G. Tait, 'On the dissipation of energy', *Proc. Roy. Soc. Edinb.*, **6** (1868): 309–11; see Number 293 note (3).

(8) Augustus Matthiessen, 'On the electric conducting power of the metals', *Phil. Trans.*, **148** (1858): 383–7.

(9) A. Matthiessen and M. von Bose, 'On the influence of temperature on the electric conducting power of metals', *Phil. Trans.*, **152** (1862): 1–27, esp. 24.

(10) A. Matthiessen and Carl Vogt, 'On the influence of temperature on the electric conducting-power of thallium and iron', *Phil. Trans.*, **153** (1863): 369–83. Compare *Treatise*, **1**: 416–17 (§360).

electricity and gutta percha,[11] glass &c[12] conduct electricity the better the *warmer*.

With respect to conductivity of irregular motions both Clausius & I make that of gases increase with temperature. I make it proportional to absolute T $\left(\text{you make it in solids} \propto \frac{1}{T}\right)$.[13] See dynamical theory of gases Phil Trans 1867.[14]

I will write you about your treatise[15] at earliest but

(1) I, personally, am satisfied with the book as a development of T′ and as an account of a subject where the ideas are new and as I well know almost *unknown* to the most eminent scientific men. It is a great thing to get them expressed any how and I think you have done it intelligibly as well as accurately.

But with respect to the bits of matter I sent you do you not think there are breaches of continuity between some, e.g. the statement about dynamical theories,[16] and the context, if they do not actually contradict the context at least the N.B. Review part of it.[17] If you disagree with anything of mine, out with it, for it is better to go into print having one opinion rather than with two opinions to throw the reader into perplexity.

2 I shall see what case Clausius has.[18]

3 Who is Charles[19] that I might believe in him. Is he B. Charles K & M, or is it he ycleped the Great[20] to whom Gan says of Almonte

'His sight / He kept upon the standard, and the laurels
In fact & fairness are his earning, Charles'.[21]

I do not know where to find Charles. Give reference.

J. C. M.

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(11) Fleeming Jenkin, 'On the insulating properties of gutta percha', *Proc. Roy. Soc.*, **10** (1860): 409–15.

(12) Heinrich Buff, 'Ueber die electrische Leitfähigkeit des erhitzten Glases', *Annalen der Chemie und Pharmacie*, **90** (1854): 257–83.

(13) See Tait, 'On the dissipation of energy': 310.

(14) See Number 263.

(15) Tait's *Sketch of Thermodynamics* (Edinburgh, 1868), in proof: see Number 299.

(16) See Tait, *Sketch of Thermodynamics*: 49, on 'Dynamical theories in general'.

(17) The first two chapters of Tait's *Sketch of Thermodynamics* were based on two articles published in the *North British Review*, **40** (1864): 40–69, 337–68.

(18) See Number 278 note (2).

(19) On J. A. C. Charles see Tait, *Sketch of Thermodynamics*: iv; '[Charles] discovered that the coefficient of dilatation is nearly the same in all permanent gases.' See Number 373.

(20) Blessed Charles King and Martyr (King Charles I) and Charlemagne, respectively.

(21) Byron's translation of the first canto of the 'Morgante Maggiore di Messer Luigi Pulci', *ll.* 110–12. See Number 373.

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LETTER TO JOHN TYNDALL

23 JULY 1868

From the original in the Library of Imperial College, London[1]

Glenlair
Dalbeattie
1868 July 23

Dear Tyndall

M^r C. Hockin tells me he is a candidate for your lectureship at the School of Mines. As I have worked with him for five years, I have had opportunities of forming a judgment of his scientific character. In particular, he worked at the determination of the B A unit of electrical resistance with Jenkin and me and then by himself,[2] and this spring he helped me in finding the ratio of the two electrical units (which according to me gives the velocity of light).[3]

You know the kind of difficulties which are always turning up in such investigations, the hunting for sources of discrepancies, the hitches in the working of new apparatus, the mathematical difficulties, and not least the physical difficulty of keeping up the spirit of accuracy to the end of a long and disappointing days work.[4]

I therefore need say no more to you than that I think Hockins scientific 'bottom' and patience such, that united as it is with a power of seeing the essentials of an experiment and securing *them* first, and with very extensive mathematical knowledge and an unreserved devotion to science, he is safe to do more than *any* young man I know for the promotion of natural knowledge.

Besides this he has taken care to make himself acquainted with engineering and telegraphy under Jenkin, and has studied chemistry with Matthiessen,[5] and in everything he has done he has reduced his observations and planned his experiments with a mathematical ability and a familiarity with abstract subjects, which I have seldom seen combined with actual manipulation.

If he were placed in any scientific institution like the School of Mines, I am certain that his only object would be to do honour to the institution both by

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(1) Huxley Papers, Vol. I: letter 53, Imperial College, London.

(2) See Number 222 note (3).

(3) Number 289.

(4) Hockin's commitment is revealed in his letter to Maxwell of 15 May 1868 on his work in finding the ratio of electrical units: see Number 289 note (8). His mathematical abilities are revealed in his solution of the problem of establishing stability criteria for governors of the fifth order: see Appendix *infra* and note (9).

(5) Augustus Matthiessen was a member of the British Association electrical standards committee: see Number 245 note (8).

sound teaching, and by experimental researches, on which he seems to have set his heart.[6]

I remain
Yours truly
J. CLERK MAXWELL

APPENDIX: STABILITY CRITERIA FOR GOVERNORS OF THE FIFTH ORDER

circa LATE JULY 1868[7]

From the original in the University Library, Cambridge[8]

HOCKIN ON QUINTIC EQUATION[9]

$$x^5+px^4+qx^3+rx^2+sx+t = 0.^{(10)}$$

That the real parts of all roots should be $-^{\text{ve}}$.
1^{st} If all the roots are real $p\ q\ r\ s\ t$ each $+^{\text{ve}}$.

Let α, β, γ, δ, ϵ be the roots.
$\text{I} = \alpha+\beta+\gamma+\delta+\epsilon = -p \quad \therefore p$ must be $+^{\text{ve}}$.
Let $(\alpha+\beta)\,(\alpha+\gamma)\ldots$ 10 factors $=$ II.
$\text{II} = (pq-r)\,(rs-qt)-(ps-t)^2$
$(\alpha+\beta+\gamma)\,(\alpha+\beta+\delta)\ldots$ 10 factors $=$ III.
$\text{III} = (pq-r)^2pr+(pq-r)\,(rs-p^2t)+p(ps+2t)\,(pr-s)+p^4(pt-sq)+t(rq-t)$

(6) In a letter to Maxwell of 27 July 1868 (ULC Add. MSS 7655, II/31), published in A. T. Fuller, 'James Clerk Maxwell's Cambridge manuscripts: extracts relating to control and stability – V', *International Journal of Control*, **43** (1986): 805–18, esp. 807–8, Hockin wrote that: 'Forde and Jenkin will be engineers to the French Atlantic cable if it is made & they offer me work.... This makes it necessary for me to make haste & find out if possible whether I have a chance of getting the appointment I wrote you about.' Hockin accepted the Atlantic cable appointment: see his letter to Maxwell of 11 March 1870 (ULC Add. MSS 7655, II/34) and Number 324 note (4).

(7) See note (9).

(8) ULC Add. MSS 7655, V, k/9, f. 12r.

(9) Maxwell's notebook entry, a solution to the problem of establishing stability criteria for governors of the fifth order, which he had raised at a meeting of the London Mathematical Society on 23 January 1868 (Number 280), is transcribed from Hockin's letter of 27 July 1868 (see note (6)). Hockin's stability criteria are discussed and proved by Fuller, 'Maxwell's Cambridge manuscripts': 809–17.

(10) The characteristic polynomial for systems of fifth order: see Number 219 note (17).

$\mathrm{IV} = (\alpha+\beta+\gamma+\delta)\,(\alpha+\beta+\gamma+\epsilon) \quad \therefore 5$ factors

$= -(ps-t)-p^2(pq-r)$

V $\alpha\beta\gamma\delta\epsilon = t.$

Conditions of real parts $-^{\text{ve}}$

$(\mathrm{II}+^{\text{ve}})\ (\mathrm{III}+^{\text{ve}})\ (\mathrm{IV}\ -^{\text{ve}})\ \mathrm{V}\ +^{\text{ve}}$.(11)

(11) In the immediately following entry in his notebook (see note (8), on f. 12^{v}), Maxwell drafted a problem on stability criteria for fourth-order systems: 'Shew how to find the sum of the nth powers of the roots of any equation. / Shew that the continued product of all the sums of all pairs of roots of the equation / $x^4+px^3+qx^2+rx+s=0$ / is $pqr-p^2s-r^2$ / and that if this quantity is positive and all the roots are impossible, the real parts of these roots will be of the sign of $-p$.' He subsequently set this problem (with minor changes of wording) as a question in the 1869 Mathematical Tripos: see *The Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 486.

REPORT ON A PAPER BY GEORGE GABRIEL STOKES ON THE COMMUNICATION OF VIBRATION TO A GAS

28 JULY 1868

From the original in the Library of the Royal Society, London[1]

REPORT ON PROF. STOKES PAPER 'ON THE COMMUNICATION OF VIBRATION FROM A VIBRATING BODY TO A SURROUNDING GAS'[2]

In this paper a solution is given of the problem of wave propagation in three dimensions in an elastic fluid when the vibrations are excited either by a sphere or an indefinitely long cylinder.[3] The results are applied to the explanation of a phenomenon observed by Leslie that the sound of a bell appeared exceedingly enfeebled when the bell was struck in hydrogen gas.[4]

When a sound-wave is produced in an elastic fluid by the symmetrical expansion or contraction of a spherical surface from or to the centre, the amplitude of the excursions at any distance from the centre considerable with respect to the wave-length is nearly proportional to the reciprocal of the distance from the centre of the sphere.

If the same expansion or contraction of the spherical surface were to occur when it is surrounded by a perfectly incompressible fluid the motion would be propagated instantaneously to all parts of space but the extent of the motion would be inversely as the square of the distance from the centre.

If the expansion of the spherical surface is the same at all points it is expressed as a spherical Harmonic[5] of degree zero; if the sphere without

(1) Royal Society, *Referees' Reports*, **6**: 269.

(2) G. G. Stokes, 'On the communication of vibration from a vibrating body to a surrounding gas', *Phil. Trans.*, **158** (1868): 447–63 (= *Papers*, **4**: 299–324). The paper was received by the Royal Society and read on 18 June 1868; see the abstract in *Proc. Roy. Soc.*, **16** (1868): 470–1 (= *Papers*, **3**: 299–300).

(3) Compare Stokes, 'On the communication of vibration': 448–9; 'I have taken the two cases of a vibrating sphere and a long vibrating cylinder, the motion of the fluid in the latter case being supposed to be in two dimensions. The sphere is chosen as the best representative of a bell.... The cylinder is chosen as the representative of a vibrating string.'

(4) John Leslie, 'On sounds excited in hydrogen gas', *Trans. Camb. Phil. Soc.*, **1** (1821): 267–8.

(5) Following Thomson and Tait, *Natural Philosophy*: 140; see Number 277 esp. note (5). Stokes, 'On the communication of vibration': 450–1, refers to 'Laplace's Functions'.

changing its form vibrates about its mean position the motion is expressed by a harmonic of the first degree; if the diameters of the sphere expand & contract so that the sphere becomes, when vibrating, an ellipsoid the motion is expressed as a harmonic of the second degree. Any motion of the surface however complicated can be expressed by a series of such harmonics and the effect of each may be considered separately.

If the fluid is supposed incompressible, the amplitude of the vibrations is inversely proportional to the $n+2^{th}$ power of the distance supposing the vibrations of the sphere expressed by a harmonic of the n^{th} order. The direction of this motion is radial at some points and tangential at others and regions of outward motion are separated from regions of inward motion by lines of tangential motion.

But if the fluid is elastic and if we confine our attention to those sound-waves which have advanced several wavelengths from their source we shall find them approximating to ordinary sound waves of compression & dilatation travelling with constant velocity and therefore having a common system of normals, and diverging from centres not far removed from the centre of the disturbance. The amplitude of the vibrations will in this case be inversely as the distance from the centre.

If the direction of the normal passes at a distance p from the centre of the sphere, then the radial component of the vibration at a great distance from the centre will vary as $\frac{1}{r}\frac{\sqrt{r^2-p^2}}{r}$ or as $\frac{1}{r}$ and the tangential component as $\frac{1}{r}\frac{p}{r}$ or as $\frac{1}{r^2}$. The magnitude of the vibrations at a distance from the sphere therefore depends principally on what takes place close to the sphere.(6) If the dimensions of the regions of the sphere which move in a concurrent manner are great compared with the wavelength, the case will approximate to that of the symmetrical motion and the amplitude will be as the reciprocal of the radius from the first.

If however the linear dimensions of the concurrent regions are small compared with a wave length the motion of the fluid near the sphere will be like that of an incompressible fluid, the fluid moving tangentially from the expanding to the contracting regions, so that the amplitude will diminish according to a higher inverse power of the distance till the wave has diverged so that its radius of curvature is great compared with the wave length.

Professor Stokes has applied his analysis to investigate the motion of the fluid generally so as to exhibit the whole motion when the mode of

(6) Stokes, 'On the communication of vibration': 452.

propagation is very different near the centre and far from it and to show that hydrogen, on account of the greater elasticity and greater length of the waves as compared with the dimensions of the sounding body will transmit disturbance to a distance in a smaller proportion than air of the same density and that when rarified air is filled up with hydrogen so as to increase its density, the amount of disturbance carried off as sound may be greatly diminished. Hence hydrogen introduced among air in the neighbourhood of a vibrating body will act as a kind of lubricator and will keep it going for a longer time. On the other hand it is said that a hot body surrounded by hydrogen cools more rapidly than in air (probably from more rapid connexion, possibly, in some much smaller degree from greater conductivity).

Professor Stokes also considers the effect of preventing the tangential motion of the fluid by radial septa,(7) and has shown how to exhibit the result by means of a tuning fork in a very simple and easily observed manner.(8)

He has also extended his calculus to the case of vibrating strings & made a practical application of the result to a phenomenon observed by himself.(9) In his investigations he has made use of various methods of treating spherical harmonics and infinite series some of which he has previously investigated in the Cambridge Transactions.(10) He has also made no account of the initial circumstances of the fluid before the vibration began.(11) He has thus avoided much trouble which in investigations about periodic disturbances is

(7) Stokes, 'On the communication of vibration': 452–3.

(8) Stokes, 'On the communication of vibration': 463; to exhibit 'the increase of sound produced by the stoppage of lateral motion'.

(9) Stokes, 'On the communication of vibration': 462–3; to explain 'a peculiar sound of extremely high pitch' produced when telegraph wires were 'thrown into vibration by the wind, and a number of different vibrations, having different periodic times, coexisted'.

(10) G. G. Stokes, 'On the numerical calculation of a class of definite integrals and infinite series', *Trans. Camb. Phil. Soc.*, **9** (1850): 166–87 (= *Papers*, **2**: 329–57). Stokes gave a method for calculating a definite integral discussed in a paper by G. B. Airy, 'On the intensity of light in the neighbourhood of a caustic', *Trans. Camb. Phil. Soc.*, **6** (1838): 379–402. In a letter to W. H. Miller, commenting on Stokes' result, Airy remarked that 'I am glad that Mr Stokes has made something of that unmanageable integral'. Responding to Airy's queries, raised in the letter to Miller, Stokes wrote to Airy on 12 May 1848 (see Larmor, *Correspondence*, **2**: 158–60). Stokes subsequently developed his method in a paper 'On the discontinuity of arbitrary constants which appear in divergent developments', *Trans. Camb. Phil. Soc.*, **10** (1857): 106–28 (= *Papers*, **4**: 77–109). Shortly before presenting his paper 'On the communication of vibration' to the Royal Society, he had written a 'Supplement to a paper on the discontinuity of arbitrary constants which appear in divergent developments', *Trans. Camb. Phil. Soc.*, **11** (1868): 412–25 (= *Papers*, **4**: 283–98).

(11) As Stokes had noted in 'On the communication of vibration': 449, in this respect his method differed from that employed by S. D. Poisson, 'Sur les mouvements simultanés d'un pendule et de l'air environnant', *Mémoires de l'Académie Royale des Sciences*, **11** (1832): 521–81.

invariably useless as it is in all cases of steady dissipation of energy, whether the dissipation be due to conversion of energy into heat within the system or as in this case to its propagation to an indefinite distance in its original form.

I consider this paper as an important contribution to Mathematics and to Acoustics and as worthy of being printed in the Philosophical Transactions.(12)

JAMES CLERK MAXWELL

Glenlair
28 July 1868

(12) In a report to 'The President and Council of the Royal Society', dated 16 July 1868 (Royal Society, *Referees' Reports*, **6**: 267), G. B. Airy strongly recommended publication: 'The fundamental equations of the theory are well known, and had been pursued to their consequences on the supposition of spherical expansion of waves, and partially on that of oscillation of spherical waves; and in a degree not quite so complete, for the corresponding cases when the movement is limited to two dimensions. Professor Stokes however has gone into the investigation with the utmost generality, so as to include not only the cases where the motions in the direction of radius vector examined at successive points through the circumference of a circle are + − (the oscillation above-mentioned), and where the motions are + − + − (the movement produced by a bell), but also the cases where the motions in any number of sectors are so divided as to produce any number of repetitions of the signs + − + − + − &c. For the successful working out of these cases, Professor Stokes is indebted to his unrivalled skill in applying abstract mathematics to physical problems; it appears here particularly in the command of Laplace's Functions, in the utilization of imaginary expressions, and in the delicate treatment of the difficulties of analysis in the cases of motion in two dimensions. The numerical results are elaborated, and the conclusion cannot be resisted, that Leslie's experiment is explained by Stokes' theoretical conclusion on the deadening effect of lateral motion of particles of gas when that gas is light and highly elastic.'

The *verso* of the first folio of Maxwell's report is endorsed: 'I recommend that Prof Stokes's paper be at once ordered for printing. W. A. Miller / I recommend that the paper by Prof Stokes be at once ordered for printing. W. H. Miller'.

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LETTER TO PETER GUTHRIE TAIT

3 AUGUST 1868

From the original in the University Library, Cambridge[1]

Glenlair
1868 Aug 3

Dear T′

I return the letter of T and your prooves.[2] I have made a few marks on the latter which being in some measure repetition of what I did before, you may class as remarks.

But I shall remark also on what is to come. Have you given any evidence that when heat is communicated from one body to another by conduction, the one body loses as much as the other gains. This is the first axiom in measurement of a thing but especially when the thing is not a thing it requires proof.

I would put both bodies into a calorimeter and observe that the resultant effect was the same whether the one had had thermal intercourse with the other or not.

e.g. a ball at 100 °C will melt the same ultimate quantity of ice whether it be enclosed in a shell at 0 °C or not. In the first case there is conduction which is eliminated in the second.

[2] Have you a definition of temperature. *I* say 'Temperature is the thermal state of a body considered with respect to its power of exchanging heat with other bodies'.[3]

[3] In your § 'What is Heat', you should eliminate the doctrine of Locke that Heat is a sensation or idea existing only in the mind and then only when it is felt.[4] The heat in your book is only found in bodies, and is detected only by thermometers. I do not think that radiant heat is heat at all as long as it is radiant.

With respect to my electrical treatise the Clarendon people have I believe accepted it.

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(1) ULC Add. MSS 7655, I, b/14.

(2) Of Tait's *Sketch of Thermodynamics* (Edinburgh, 1868).

(3) Compare J. Clerk Maxwell, *Theory of Heat* (London, 1871): 1–4 for a development of the argument.

(4) See the text as published of the *Sketch of Thermodynamics*: 1; 'it matters not to us what... Locke and Descartes imagined, with regard to the nature of heat'; and Number 278 note (4).

I am writing out the Kinematic part (Ohms law and theory of Conduction).[(5)]

For electrolysis see (besides Faraday[(6)] Miller[(7)] &c) Thomson Phil Mag 1851,[(8)] expounded by Max & Jenk. Brit Ass. Reports 1863 §54[(9)] and followed up, very well for a Frenchman by Georges Salet, *Laboratory* July 7, 1867.[(10)] The subject looks temptingly simple but is not altogether so as yet.

My view of the energetics of magneto electric induction is to be found in the 1st part of my paper on the Field,[(11)] and no where else.

Rankine in a very short statement in the Phil Mag on Conservation has expressed several things very well about energy, force and effect.[(12)]

N.B. There are two kinds of Dissipation of E. one of which is possible in a strictly conservative universe provided it is ∞, namely the propagation of undulations to ∞ from a vibrating body.

Stokes has just sent to the R.S. a paper on a sphere, vibrating in S.H. in an elastic fluid.[(13)] When the elasticity of the fluid is great as in Hydrogen & the wave length therefore great as compared with the dimensions of the vibrating regions of the sphere the motion is nearly that of an incompressible fluid & little sound is sent off to ∞.

Thus a bell makes less noise in a mixture of air and H. than in the rarified air without the H., which is less dense than the mixture. The other dissipation is conversion to heat. Either kind causes a steady periodic driving power to

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(5) Maxwell, *Treatise*, **1**: 295–306 (§§241–54).

(6) Michael Faraday, 'Experimental researches in electricity. – Fifth series. On electro-chemical decomposition', *Phil. Trans.*, **123** (1833): 675–710; Faraday, 'Seventh series. On electro-chemical decomposition', *ibid.*, **124** (1834): 77–122; and Faraday, 'Eighth series. On the electricity of the voltaic pile', *ibid.*, **124** (1834): 425–70 (= *Electricity*, **1**: 127–64, 195–258, 259–321).

(7) William Allen Miller, *Elements of Chemistry: Theoretical and Practical. Part I. Chemical Physics* (London, $_4$1867): 450–553.

(8) William Thomson, 'On the mechanical theory of electrolysis', *Phil. Mag.*, ser. 4, **2** (1851): 429–44 (= *Math. & Phys. Papers*, **1**: 472–89).

(9) J. Clerk Maxwell and Fleeming Jenkin, 'On the elementary relations between electrical measurements', *Report of the Thirty-third Meeting of the British Association for the Advancement of Science; held at Newcastle-upon-Tyne in August and September 1863* (London, 1864): 130–63, esp. 158, '§54. Electromotive force of chemical affinity' (= *Phil. Mag.*, ser. 4, **29** (1865): 436–60, 507–25).

(10) Georges Salet, 'Affinity and electricity', *Laboratory*, **1** (1867): 248–50.

(11) J. Clerk Maxwell, 'A dynamical theory of the electromagnetic field', *Phil. Trans.*, **155** (1865): 459–512, esp. 459–66 (= *Scientific Papers*, **1**: 526–36).

(12) W. J. Macquorn Rankine, 'On the phrase "potential energy" and on the definitions of physical quantities', *Phil. Mag.*, ser. 4, **33** (1867): 88–92.

(13) See Number 298.

produce a motion converging to a steady periodicity (without arbitrary functions). See a question about a vibrating disk in a tube Senate House 1867.(14)

Yours truly
J. Clerk Maxwell

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(14) See *The Cambridge University Calendar for the Year 1867* (Cambridge, 1867): 492; 'Form the differential equation for the propagation of sound in a uniform tube; and explain under what circumstances it may be expressed approximately in linear form. / In a uniform tube of indefinite length is placed a disc which fills it and makes n complete vibrations in a second, their amplitude being c: another disc of mass M is placed at a distance l from the first, and is supported by a spring, whose elasticity is such that the disc, if vibrating freely, would make m vibrations in a second: shew that after a sufficient time has elapsed for the excursions of the air in the tube beyond the second disc to become uniform their amplitude will be

$$c' = c\cos\beta(1-2\sin\beta\sin\gamma+\sin^2\beta)^{-\frac{1}{2}}$$

where $\tan\beta = \pi\dfrac{Mn}{\rho v}\left(\dfrac{m^2}{n^2}-1\right)$ and $\gamma = \beta+4\pi\dfrac{ln}{v}$, ρ being the density of air, and v the velocity of sound. / Find the values of l for which c' is a maximum or minimum, and shew that the maxima are greater and the minima smaller the greater the value of $\tan\beta$.'

ON THE ABSORPTION AND DISPERSION OF LIGHT[(1)]

circa AUGUST 1868[(2)]

From the original in the University Library, Cambridge[(3)]

[DRAFT QUESTION FOR THE MATHEMATICAL TRIPOS][(4)]

Shew from dynamical principles that if the elasticity [of] a medium is such that a tangential displacement η of one surface of a stratum of thickness x calls into action a force of restitution equal to $a\dfrac{\eta}{x}$ per unit of area, then the equation of propagation of waves of such tangential displacements is

$$\rho\frac{d^2\eta}{dt^2} = a\frac{d^2\eta}{dx^2}$$

and deduce the velocity of propagation.

Suppose that every particle of this medium is connected with another atom in such a manner that if the particle were kept at rest the atom would vibrate about it p times in a second. η is the displacement of the particle and $\eta+\zeta$ that of the atom a force $mp^2\zeta+mR\dfrac{d\zeta}{dt}$ acts on the particle and an equal and opposite force on the atom.

Show that if ρ and σ be the mass of the medium and of the atoms respectively in unit of volume the equations of motion are

$$\rho\frac{d^2\eta}{dt^2}-E\frac{d^2\eta}{dx^2} = \sigma p^2\zeta+\sigma R\frac{d\zeta}{dt} = -\sigma\left(\frac{d^2\eta}{dt^2}+\frac{d^2\zeta}{dt^2}\right).$$

Shew that if a disturbance of the form $\eta = e^{-lx}\cos\left(nt-\dfrac{nx}{v}\right)$ can be propagated through this medium where l is the coefficient of absorption and v is the velocity of propagation then if $1-\dfrac{p^2}{n^2} = Q\cos\alpha$ and $\dfrac{R}{n} = Q\sin\alpha$

(1) Maxwell's suggestion of the mode of action of forces acting on matter in the ether may have been prompted by reading Stokes' paper 'On the communication of vibration from a vibrating body to a surrounding gas': see Number 298, and his comments in Numbers 299 and 460.

(2) See note (1) and Appendix *infra*.

(3) ULC Add. MSS 7655, V, k/9, ff. 14–15.

(4) See Appendix *infra*.

and if V be the velocity in the first medium

then $QO = 1$ and $OP = \dfrac{\sigma}{\rho Q}$ and $POD = \alpha$

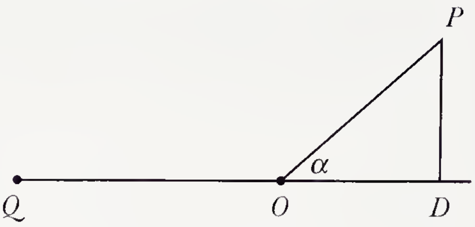

Figure 300,1

$$\frac{1}{v^2} = \frac{1}{2V^2}(QP+QD) \quad \text{or} \quad \frac{1}{V^2}(QP-QD)$$

$$\text{and} \quad \frac{1}{l^2} = \frac{n^2}{2V^2}(QP-QD) \quad \text{or} \quad \frac{n^2}{V^2}(QP+QD).$$

Hence there is either a velocity less than V with a small absorption or a very great velocity with a very great absorption. We must take the 1st case. Also since OP is greatest when Q is least that is when $n = p$ the absorption is then greatest and the irregularity of refraction is also greatest.

If $R = 0$ $\quad v^2 = V^2\dfrac{1}{1+\dfrac{\sigma}{\rho}\dfrac{1}{1-\dfrac{p^2}{n^2}}}$. If p is small v diminishes as n increases or refrangibility increases as wave length diminishes.(5)

APPENDIX: MATHEMATICAL TRIPOS QUESTION

circa LATE 1868

From the *Cambridge Calendar for 1869*(6)

Shew from dynamical principles that if the elasticity of a medium is such that a tangential displacement η (in the direction of y) of one surface of a stratum of thickness a calls into action a force of restitution equal to $E\dfrac{\eta}{a}$ per unit of area, then the equation of propagation of such displacements is

$$\rho\frac{d^2\eta}{dt^2} = E\frac{d^2\eta}{dx^2}.$$

Suppose that every particle of this medium is connected with an atom of other matter by an attractive force varying as the distance, and that there is also a force of resistance between the medium and the atoms varying as their

(5) See Maxwell's comment on the anomalous dispersion of light in his report on the paper by J. W. Strutt (Lord Rayleigh) on 'Some general theorems relating to vibrations' (Number 460). For further discussion see Number 461.

(6) *The Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 502. See Number 460 note (15).

relative velocity, the atoms being independent of each other: shew that the equations of propagation of waves in this compound medium are

$$\rho\frac{d^2\eta}{dt^2}-E\frac{d^2\eta}{dx^2}=\sigma\left(p^2\zeta+R\frac{d\zeta}{dt}\right)=-\sigma\left(\frac{d^2\eta}{dt^2}+\frac{d^2\zeta}{dt^2}\right),$$

where ρ and σ are the quantity of the medium and of the atoms respectively in unit of volume, η is the displacement of the medium, and $\eta+\zeta$ that of the atoms, $\sigma p^2\zeta$ is the attraction, and $\sigma R\frac{d\zeta}{dt}$ is the resistance to the relative motion per unit of volume.

If one term of the value of η be $Ce^{-\frac{x}{l}}\cos n\left(t-\frac{x}{v}\right)$, shew that

$$\frac{1}{v^2}-\frac{1}{l^2n^2}=\frac{\rho+\sigma}{E}+\frac{\sigma n^2}{E}\frac{p^2-n^2}{(p^2-n^2)^2+R^2n^2},$$

$$\frac{2}{vln}=\frac{\sigma n^2}{E}\frac{Rn}{(p^2-n^2)^2+R^2n^2}.$$

If σ be small, one of the values of v^2 will be less than $\frac{E}{\rho}$ and if R be very small v will diminish as n increases, except when n is nearly equal to p, and in the last case l will have its lowest values. Assuming these results interpret them in the language of the undulatory theory of light.

LETTER TO WILLIAM THOMSON

19 AUGUST 1868

From the original in the University Library, Glasgow[1]

Ardhallow
Dunoon
19 August 1868

Dear Thomson

I intend to be in Glasgow on Tuesday morning and will see if you are at Whites[2] and when I have got my own things done I will look you up between White and College. I shall study zero matter presently. Meantime I am doing Greens Theorem applied to conduction in bodies with 9 variable coeffts of resistance,[3] and making bodies with 6 coeffts out of linear conductors.[4] I think that the fact that you cannot produce the rotatory property by any arrangement of linear conductors ought to prove it nonexistent.[5] But it is good to be able to solve questions as if there were such a thing.

If u v w be components of the current and X Y Z of the electromotive force

$$\begin{aligned} u &= r_1 X + p_3 Y + q_2 Z \qquad & X &= R_1 u + Q_3 r + P_2 w \\ v &= q_3 X + r_2 Y + p_1 Z & Y &= P_3 u + R_2 v + Q_1 w \\ w &= p_2 X + q_1 Y + r_3 Z & Z &= Q_2 u + P_1 v + R_3 w \end{aligned}$$

then p q r will be conductivities
P Q R—— resistances.

If the ps and qs are equal there is no rotatory property and in that case the Ps & Qs are also equal.[6]

The ellipsoid of conductivity is

$$r_1 x^2 + r_2 y^2 + r_3 z^2 + (p_1+q_1) yz + (p_2+q_2) zx + (p_3+q_3) xy = \boxed{D}\, r^2$$

where $\boxed{D} = r_1 r_2 r_3 + 2(p_1+q_1)(p_2+q_2)(p_3+q_3)$
$$-\tfrac{1}{4}(r_1(p_1+q_1)^2 + r_2(p_2+q_2)^2 + r_3(p_3+q_3)^2).$$

The ellipsoid of resistance is got by putting big letters for little.
When there is no rotatory property these ellipsoids are polar reciprocals.
When there is rotation they are not.

(1) Glasgow University Library, Kelvin Papers, M 22.
(2) James White, instrument-maker in Glasgow.
(3) See Maxwell, *Treatise*, **1**: 372 (§323).
(4) See the *Treatise*, **1**: 373 (§324).
(5) See the *Treatise*, **1**: 349–50 (§303).
(6) Compare the *Treatise*, **1**: 345–6 (§§297–8).

If C is a current from the origin & V the potential

$$V = \frac{C}{4\pi r}$$

r being the quantity above.(7)

May I make use of your report on Electrometers(8) for my chapter on Instruments?(9)

Have you anything new in Electrical Imagery.(10) Betti,(11) who is otherwise good, does not seem to have got beyond the double series of images in the case of 2 spheres.(12) His method of Bicircular coordinates is very ill adapted for the purpose in 3 dimensions. It is first rate in 2.

I have a chapter on Conduction in 2 dimensions which yields neat results applicable to exp^{t} and capable of development ad ∞.(13)

Why do you talk of the time integral of a force.(14) Why not say Impulse and take Impulse of a Force = Momentum of a System or Body as the general eq^{n} of Dynamics.(15)

I have not heard Hockin lecture in other respects he is most fit.(16)

Yours truly

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(7) Compare the *Treatise*, **1**: 348 (§301).

(8) William Thomson, 'Report on electrometers and electrostatic measurements', *Report of the Thirty-seventh Meeting of the British Association for the Advancement of Science; held at Dundee in September 1867* (London, 1868): 489–512 (= *Electrostatics and Magnetism*: 260–309).

(9) See the *Treatise*, **1**: 254–87 (§§207–29).

(10) See Number 310, and Maxwell's discussion of electric images in the *Treatise*, **1**: 191–225 (§§157–81). The theory had been developed by William Thomson and was familiar to Maxwell in 1855 (see Volume I: 321). See William Thomson, 'On the mathematical theory of electricity in equilibrium [Parts III–VI]', *Camb. & Dubl. Math. J.*, **3** (1848): 141–8, 266–74; **4** (1849): 276–84; **5** (1850): 1–9; the 'Extraits de deux lettres addressées à M. Liouville', *Journal de Mathématiques Pures et Appliquées*, **12** (1847): 256–64; and Thomson, 'On the mutual attraction or repulsion between two electrified spherical conductors', *Phil. Mag.*, ser. 4, **5** (1853): 287–97; **6** (1853): 114–15 (= *Electrostatics and Magnetism*: 52–85, 146–54, 86–97).

(11) Enrico Betti, 'Teorica delle forze che agiscono secondo la legge di Newton e sua applicazione alla elettricità statica', *Nuovo Cimento*, **18** (1863): 385–402; **19** (1863): 59–75, 77–95, 149–75, 357–77; **20** (1864): 19–39, 121–41.

(12) Betti, 'Teorica delle forze', *Nuovo Cimento*, **20** (1864): 19–39. See the *Treatise*, **1**: 215n (§172).

(13) See Part II, Chapter VI of the *Treatise*, **1**: 329–37 (§§273–84).

(14) See Thomson and Tait, *Natural Philosophy*: 207; the 'time-integral' of a force will 'measure...the whole momentum which it generates in the time in question'.

(15) See Maxwell's procedure in the *Treatise*, **2**: 184–94 (§§553–67).

(16) See Number 297.

LETTER TO WILLIAM THOMSON

5 SEPTEMBER 1868

From the original in the University Library, Glasgow[1]

Ardhallow
Dunoon
1868 Sept 5

Dear Thomson

A perfect electrical machine[2] should have a set of insulated carriers, a pair of inductors a pair of receivers and a pair of regenerators.[3]

If the coeff[ts] of capacity of two conductors are such that

$$E_1 = (P+Q)\,V_1 - QV_2$$
$$E_2 = -QV_1 + (Q+R)\,V_2$$

then if $V_1 = V_2$, E_1 will be PV.

If the second conductor nearly surrounds the first P will be small.

If $V_1 = 0 \quad E_1 = -QV_2$.

If the second conductor is not very near the first and does not surround it Q will be small.

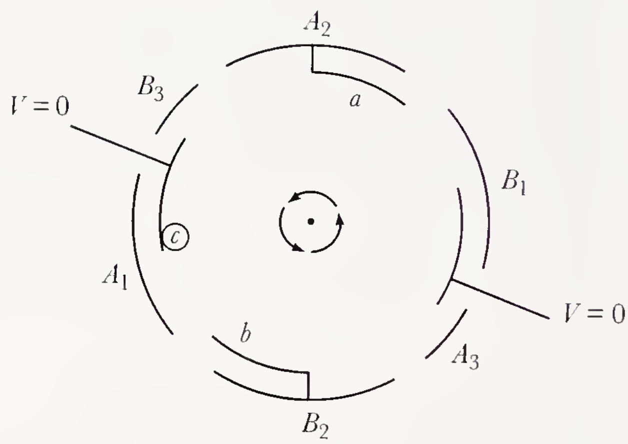

Figure 302,1

Now let A_1 be an inductor, at potential A_1 c a carrier touching an earth-spring in the middle of A_1

c will carry off a charge $= -Q_1 A_1$.

Let B_2 be a receiver, at potential B_2 opposite to A_1.

Let b be the end of a spring connected with B_2.

Let $P_2'\ Q_2'\ R_2'$ be the coeff[ts] corresponding to a carrier at d the end of the spring,

---

(1) Glasgow University Library, Kelvin Papers, M 23.

(2) Thomson had described a machine he had constructed for multiplying electrical charges (in which the carriers are drops of water which charge an inductor which itself charges another stream of drops), in a paper presented to the Royal Society on 20 June 1867. See William Thomson, 'On a self-acting apparatus for multiplying and maintaining electric charges, with applications to illustrate the voltaic theory', *Proc. Roy. Soc.*, **16** (1867): 67–72 (= *Electrostatics and Magnetism*: 319–25). See also Thomson's subsequent paper 'On Mr C. F. Varley's reciprocal electrophorus', *Phil. Mag.*, ser. 4, **35** (1868): 287–9 (= *Electrostatics and Magnetism*: 337–9), where he acknowledged that Maxwell had pointed out to him Varley's priority in the principle of the regenerating instrument. See the *Treatise*, **1**: 256–60 (§§209–11).

(3) Essentially a draft of the *Treatise*, **1**: 260–2 (§§212–13).

$P_2\, Q_2\, R_2$ the same when the carrier is at the middle of B_2. Then if V be the potential of the carrier when just at b

$$-Q_1 A_1 = (P'_2 + Q'_2)\, V - Q'_2 B_2$$

or

$$V = \frac{Q'_2 B_2 - Q_1 A_1}{P'_2 + Q'_2}.$$

If the spring is so arranged that $Q_1 A_1 + P'_2 B_2 = 0$ then $V = B_2$ and there will be no spark at contact.

Let the carrier remain in contact with b till it comes to the middle of B_2, it will leave contact with a charge $P_2 B_2$ so that B_2 receives

$$-(Q_1 A_1 + P_2 B_2) \text{ from the carrier.}$$

Let the carrier with charge $P_2 B_2$ proceed to the middle of the regenerator A_3 and let its potential be V

then

$$P_2 B_2 = (P_3 + Q_3)\, V - Q_3 A_3.$$

If

$$P_2 B_2 + Q_3 A_3 = 0$$

then $V = 0$ and there will be no spark when the carrier touches the earth spring in A_3.

Let the carrier proceed to the middle of B_1 still touching the earth spring and let it leave the spring at the middle of B_1 with a charge

$$-Q_1 B_1$$

which it carries to the receiver A_2 and so on so that it can communicate to the receiver a charge

$$-(Q_1 B_1 + P_2 A_2).$$

We may suppose $A_1\, A_2\, A_3$ connected & at pot. V
$B_1\, B_2\, B_3$ ——— $-V$

then the conditions of no spark become

$$Q_1 = P'_2$$
$$P_2 = Q_3$$

and the ratio of increase of potential is

$$\frac{1}{K}(Q_1 - P_2)$$

where K is the capacity of the whole system in connexion with one electrode.

Hence Q_1 must be large, that is the inductor must closely surround the carrier and P_2 must be small, that is the receiver must closely surround the carrier.

Q_3 must be equal to P_2 and therefore small, that is the regenerator must exert a small induction on the carrier.

P_1' must be equal to Q_1. This settles the position of the end of the springs *a* & *b*.

Now when the carrier is in the middle of A_1 its potential is 0 and if it reached the middle of B_2 without touching the spring it would be

$$-\frac{Q_1+Q_2}{P_2+Q_2}V.$$

Now since Q_1 is greater than P_2 this value is numerically greater than V. Hence there must be some place between the middle of A_1 & the middle of B_2 where the potential is $-V$ and where contact may take place without a spark.

Helmholtz' paper on Discontinuous Motion(4) has been forwarded to me. He makes an electrical application of which I shall avail myself. I had already in 2 dimensions found the good of expressing x and y in terms of ϕ & ψ the functions of potential and of flow.(5)

Betti has sent me an electromagnetic hypothesis.(6) He supposes a closed current to be like a circular magnet with a periodic variation of strength, and that the potential is propagated with a certain velocity.(7)

C Neumann has an elaborate hypothesis about emissive and receptive potentials(8) which I must study before I can understand it.(9)

Have you any easy way of calculating the case of two *unequal* spheres in contact? I get a definite integral which I suppose is connected with Euler's.(10)

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(4) Hermann Helmholtz, 'Über discontinuirliche Flüssigkeits-Bewegungen', *Monatsberichte der... Akademie der Wissenschaften zu Berlin* (1868): 215–28.

(5) Helmholtz, 'Über discontinuirliche Flüssigkeits-Bewegungen': 223; 'Die Curven ψ = Const. sind die Strömungslinien der Flüssigkeit, und die Curven ϕ = Const. sind orthogonal zu ihnen. Letztere sind die Curven gleichen Potentials'. See Number 303, esp. note (15).

(6) Enrico Betti, 'Sopra elettrodinamica', *Nuovo Cimento*, **27** (1868): 402–7.

(7) For comment see the *Treatise*, **2**: 436–7 (§864).

(8) Carl Neumann, 'Resultate einer Untersuchung über die Principien der Elektrodynamik', *Nachrichten von der Königl. Gesellschaft der Wissenschaften und der Georg-August-Universität zu Göttingen* (1868): 223–35.

(9) For comment see Number 327 esp. note (12) and the *Treatise*, **2**: 435–6 (§863).

(10) See Maxwell's treatment in the *Treatise*, **1**: 219–21 (§175) of the problem of any two spheres in contact, where he remarks that the values of the integrals 'are not, so far as I know, expressible in terms of known functions'. He is referring to the Eulerian integrals: see A. M. Legendre, 'Traité des intégrales Eulériennes' in his *Traité des Fonctions Elliptiques et des Intégrales Eulériennes*, 2 vols (Paris, 1825–6), **2**: 365–530; and D. F. Gregory, *Examples of the Processes of the Differential and Integral Calculus* (Cambridge, 1841): 461–7. In the second edition of the *Treatise*, **1**: 255–7 (published posthumously in 1881) Maxwell revised §175, giving a solution using the gamma function or 'second Eulerian integral' (Gregory, *Examples*: 461; Legendre, *Traité*, **2**: 365), and making reference to Legendre's *Traité des Fonctions Elliptiques*, **2**: 438 (on the gamma function). Thomson had made use of this function in a paper 'On certain definite integrals suggested by problems in the theory of electricity', *Camb. & Dubl. Math. J.*, **2** (1847): 109–22 (= *Electrostatics and Magnetism*: 112–25).

$$\begin{aligned} \text{Let } p_1 &= x_{11}\,q_1 + x_{12}\,q_2 \quad + x_{1n}\,q_n \\ p_2 &= x_{21}\,q_1 + x_{22}\,q_2 \quad + \&c \end{aligned}$$

$$\begin{aligned} q_1 &= y_{11}\,p_1 + y_{12}\,q_2 \quad + \&c \\ q_2 &= y_{21}\,p_1 + y_{22}\,q_2 \quad + \&c \end{aligned}$$

where p_1 p_2 are the potentials and q_1 q_2 the charges of the bodies of a system then I have proved that if $2Q = p_1 q_1 + p_2 q_2 + \&c$ where Q is the electrical energy then if by the motion of the system Q change from Q_1 to Q_2 while the charges are constant the work required to move the system is $Q_2 - Q_1$.

But if the potentials are maintained constant then the work required to move it is $Q_1' - Q_2'$ and at the same time $2(Q_1' - Q_2')$ is restored to the batteries for maintaining [the] potentials. (This is true even if x_{12} is not equal to x_{21}.)

303

LETTER TO WILLIAM THOMSON

12 SEPTEMBER 1868

From the original in the University Library, Glasgow[1]

Ardhallow
Dunoon
1868 Sept 12

Dear Thomson

I have just made up a theory of your disk and guard ring[2] opposed to a parallel large plate.

Radius of disk $= R$

of circular aperture in guard ring $R+B$

distance of opposed surfaces $= A$.

I begin with the conjugate functions

$$x_1 = e^{\phi} \cos \psi \quad \text{and} \quad y_1 = e^{\phi} \sin \psi$$
$$\text{and} \quad x_2 = e^{-\phi} \cos \psi \qquad y_2 = -e^{-\phi} \sin \psi.$$

$$\left(\text{Conjugate functions are such that } \frac{d\phi}{dx} = \frac{d\psi}{dy}, \frac{d\phi}{dy} = -\frac{d\psi}{dx}.\right)^{(3)}$$

Then putting

$$2x' = x_1 + x_2 = (e^{\phi} + e^{-\phi}) \cos \psi.$$
$$2y' = y_1 + y_2 = (e^{\phi} - e^{-\phi}) \sin \psi$$
$$x' \;\&\; y' \text{ are conjugate to } \phi \;\&\; \psi.$$

The curves of ϕ are confocal ellipses, those of ψ hyperbolas.[4]

Next let
$$x = b \log \sqrt{(x'^2 + y'^2)}$$
$$y = b \tan^{-1} \frac{y'}{x'}.$$

(1) Glasgow University Library, Kelvin Papers, M 24.

(2) See Number 289 note (11).

(3) See the *Treatise*, **1**: 227 (§183) for Maxwell's more explicit definition of conjugate potential functions in terms of the Cauchy–Riemann differential equations. See Bernhard Riemann, 'Allgemeine Voraussetzungen und Hülfsmittel für die Untersuchung von Functionen unbeschränkt veränderlichen Grössen', *Journal für die reine und angewandte Mathematik*, **54** (1857): 101–4; and Riemann, 'Bestimmung einer Function einer veränderlichen complexen Grösse durch Grenz- und Unstetigkeitsbedingungen', *ibid.*: 111–15.

(4) See the *Treatise*, **1**: 237–8 (§192) and Fig. X appended to the volume. The functions ϕ and ψ are the potential and stream functions. See Number 337 for further discussion.

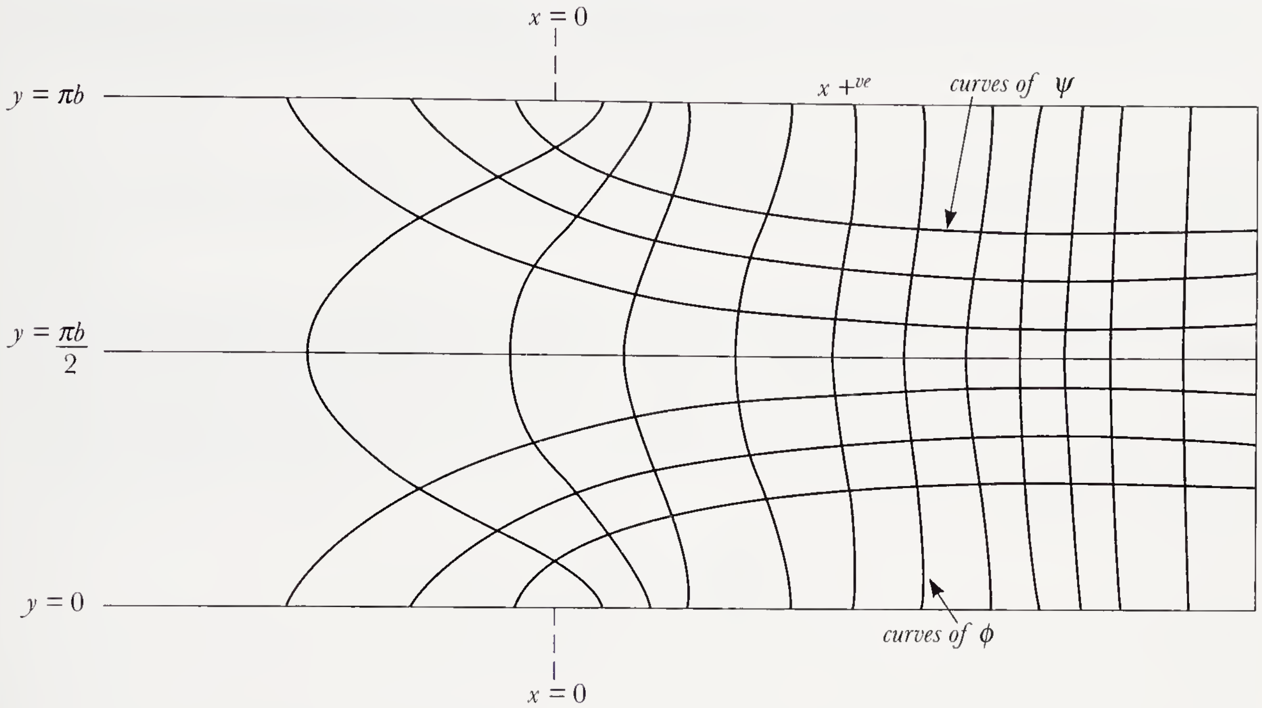

Figure 303,1

Then as you showed me in Dec 1864[5] and as I showed in my paper on Viscosity of Air in 1865[6] the curves are as shown over the page.[7]

The curves of ϕ, as ϕ increases tend to become straight lines parallel to y.

They have undulations of breadth $B = \pi b$ and depth $D = \frac{1}{2}\log\frac{e^{\phi}+e^{-\phi}}{e^{\phi}-e^{-\phi}}$ and the maximum value of x is $b\log\frac{1}{2}(e^{\phi}+e^{-\phi})$.

When ϕ is great this becomes $a = b\phi - b\log 2$ where $x = a$ is the equation of a plane at potential ϕ

$$b\phi = a + b\log 2.$$

Hence if a series of planes normal to y at distance $B = \pi b$ are cut off by the plane $x = 0$ and a plane $x = a$ opposed to their edges the electrical conditions are the same as those of two planes at distance $a + \frac{\log 2}{\pi} + B$.

(If instead of the set of planes there is a plate of corrugated zinc with grooves of breadth B and depth D of the shape nearly of the curves on

(5) See Number 240. Maxwell was calculating the friction between a rotating and fixed discs.

(6) J. Clerk Maxwell, 'On the viscosity or internal friction of air and other gases', *Phil. Trans.*, **156** (1866): 249–68, esp. 261–3 and Plate XXI, Fig. 9 (= *Scientific Papers*, **2**: 16–18, and Plate IX, Fig. 9).

(7) See the *Treatise*, **1**: 238 (§193) and Fig. XI appended to the volume.

opposite page then if A is the least distance of the opposed surfaces and

$$\alpha = \frac{B}{\pi}\log_\epsilon \frac{2}{1+e^{-\pi\frac{D}{B}}}$$

the corrected distance of the plates will be $A+\alpha$. When $D = \infty$ $\alpha = \frac{\log_\epsilon 2}{\pi}B$.)[8]

If there is only one groove, the opposed plane will be more electrified than if there were many and therefore the sides of the groove will be more electrified and there will be less diminution of effect due to the single groove. Hence if S be the flat surface and S' the surface cut away by the groove the capacity will be

$$\frac{S}{4\pi A}+\frac{S'}{4\pi(A+\alpha')}$$

where α' is less than α by a small fraction of itself.[9]

Next let us pass to the case of a system of cylinders with edges opposed to a circular disk. Let the figure revolve about one axis $y = -R$.
Laplaces equation will have the form[10]

$$\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+\frac{1}{R+y}\frac{dV}{dy}+4\pi\rho = 0.$$

If we make $V = \phi$ we may determine ρ the volume-density required to produce this potential. Since $\frac{d\phi}{dy}$ is small except near the edges of the cylinders we may suppose this additional distribution of electricity to be an additional charge on them. If we put

$$N = \frac{2\log 2}{\pi^2}\left\{\int_{-\infty}^{+\infty}\log\left(2^n+\sqrt{4^n+1}\right)dn - \int_0^\infty \log\left(2^n+\sqrt{4^n-1}\right)dn\right\} = \frac{1}{16}$$

(8) Compare Maxwell's discussion of Thomson's guard-ring electrometer in the *Treatise*, **1**: 245–6 (§201), for a similar argument. See also the correction (added 28 December 1868) to his paper 'On a method of making a direct comparison of electrostatic with electromagnetic force', *Phil. Trans.*, **158** (1868): 643–57, on 656n (= *Scientific Papers*, **2**: 142n).

(9) Compare the *Treatise*, **1**: 243–4 (§199).

(10) More correctly, as Maxwell states in the *Treatise*, **1**: 244 (§200), Poisson's equation. See the *Treatise*, **1**: 226–7 (§182), where he states the potential equations of Laplace and Poisson for the distribution of electricity in a space of two dimensions: 'The equation of Poisson may be written $\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+4\pi\rho = 0$' (where V is the potential and ρ the surface-density of the charge).

then if E is the charge on the positive outer side of one of the cylinders due to the superficial density σ that due to the volume density ρ will be

$$E\mathcal{N}\frac{B}{R}.$$

There will be a negative charge on the negative side due to the volume density.

If we now suppose these charges removed from the surrounding space and placed on the surface the change of position will be but small and the effect on the opposed plate negligible.

The capacity therefore of a disc of radius R surrounded by a guard ring with aperture radius $R+B$ and opposed by a large plate of distance A is

$$\frac{R^2}{4A}+\frac{1}{2}\frac{RB}{A+\alpha'}\left(1+\mathcal{N}\frac{B}{R}\right).^{(11)}$$

α' is less than $\frac{\log_\epsilon 2}{\pi}B$ but probably little less.

$\mathcal{N}$ is a numerical quantity which I mean to calculate. The attraction is found by multiplying this by $\frac{V^2}{2A}$. What do you think of the lawfulness of condensing the supposed nebulous electricity on the surface of the conductor. The nebulosity is squeezed out of space by bending the field round the axis. We go to Glenlair for a week but return end of next week, 18 Sept.

I find $\mathcal{N}=\frac{1}{16}$.

I have found $\mathcal{N}=\frac{1}{16}$[(a)] [(12)] so that the capacity of the disk is

$$\frac{R^2}{4A}+\frac{1}{2}\frac{RB+\frac{1}{16}B^2}{A+\frac{\log_\epsilon 2}{\pi}B}.$$

I also find the capacity of a thin circular disk radius R without guard ring between two infinite plates at distance B on each side of it

$$\frac{R^2}{2B}+\frac{\log_\epsilon 2}{\pi}R-\frac{1}{4}B.^{(13)}$$

(a) {Thomson} $\frac{1}{2}$

(11) Compare the *Treatise*, **1**: 244 (§200) for a revised argument.
(12) See Maxwell's correction in his next letter to Thomson (Number 306).
(13) See the *Treatise*, **1**: 245 (§200).

If the middle disk is of thickness 2β, then in the second term we must substitute for $\log_\epsilon 2$

$$\log_\epsilon\left(2\cos\frac{\pi}{2}\cdot\frac{\beta}{B+\beta}\right)$$

where $2(B+\beta)$ is the distance between the outer plates.

This I made use of for Viscosity experiments(14) but did not know how to treat the curvature of the edge.

$$\begin{matrix}\text{Helmholtz} \\ \text{conjugate functions}\end{matrix}\quad\begin{cases}x = \phi + e^{\phi}\cos\psi \\ y = \psi + e^{\phi}\sin\psi^{(15)}\end{cases}$$

gives the case of *two* parallel plates not a series or of one plate opposed to another much larger.(16)

Here is a thing in conjugate functions.

If G and H are conjugate functions of x & y two other conjugate functions E & F of x & y may be found such that

$$2\left(\overline{\frac{dH}{dx}}\Big|^2 - \overline{\frac{dH}{dy}}\Big|^2\right) = 2\left(\overline{\frac{dG}{dy}}\Big|^2 - \overline{\frac{dG}{dx}}\Big|^2\right) = \frac{d^2F}{dx^2} - \frac{d^2F}{dy^2}$$

$$= -2\frac{d^2E}{dxdy} = 4\frac{dH}{dx}\frac{dH}{dy} = 4\frac{dG}{dx}\frac{dG}{dy} = 2\frac{d^2F}{dxdy} = \frac{d^2E}{dx^2} - \frac{d^2F}{dy^2}.$$

The question is – how to do it.

Yours truly
J. Clerk Maxwell

(14) Maxwell, 'On the viscosity or internal friction of air and other gases': 263 esp. note (= *Scientific Papers*, **2**: 18), where he had remarked on the application to 'the calculation of the electrical capacity of a condenser in the form of a disk between two larger disks at equal distance from it'.

(15) Hermann Helmholtz, 'Über discontinuirliche Flüssigkeits-Bewegungen', *Monatsberichte der... Akademie der Wissenschaften zu Berlin* (1868): 215–28, esp. 224. In a notebook entry (Maxwell Notebook (3), King's College London Archives) Maxwell transcribed the equations as stated by Helmholtz: '$x = A\phi + Ae^{\phi}\cos\psi$ / $y = A\psi + Ae^{\phi}\sin\psi$'. See Number 302 esp. note (5).

(16) Helmholtz, 'Über discontinuirliche Flüssigkeits-Bewegungen': 227–8, on the distribution of electricity on two parallel infinitely long plane strips. See Maxwell's discussion in the *Treatise*, **1**: 246–7 (§202) and Fig. XII appended to the volume.

304

DRAFTS ON TOPOLOGY

circa SEPTEMBER 1868(1)

From the originals in the University Library, Cambridge(2)

[GEOMETRY OF POSITION](3)

[1] *A quantity is said to be a function of one or more variables when if the variables are given, the quantity can be determined.*

In physical questions, it is not necessary that we should be able to express the quantity as a function of a definite mathematical form applying to every value of the variables. It is sufficient if by any means, when the variables are given we can determine the quantity. Thus the function may have a certain form when the variables are within certain limits while beyond these limits the form of the function may be different. There are mathematical methods of expressing such a function by a single formula but we shall not find them necessary.

A function of the coordinates of a point is called a function of the position of the point or more simply a function of the point.

A quantity is said to be a continuous function of its variables when if the variables alter continuously from one set of values to another the quantity passes from its original to its final value through all the intermediate values.

It is not necessary that the form of the function should remain the same provided it does not pass suddenly from one value to another.

Cor. If a quantity is a continuous function of its variables its first differential coefficients with respect to these variables are finite but not necessarily continuous.

On Curves

The coordinates of a point on a curve are continuous functions of the length of the curve reckoned from a fixed point on the curve in a direction which is assumed as the positive direction along the curve.

(1) See Number 306.

(2) ULC Add. MSS 7655, V, d/10.

(3) The MS is endorsed 'Geometry of Position'. For this term see the *Treatise*, **1**: 16 (§18), and **2**: 41 (§421), and see Numbers 276 note (8) and 373 esp. note (10). The manuscript derives from Maxwell's reading of Riemann and Helmholtz: see note (4) and Number 305. On Maxwell's topological arguments in Numbers 304, 305 and 306, compare Number 318 and the *Treatise*, **1**: 16–17 (§18), where he refers to the work of Johann Benedict Listing.

A curve in physics may be limited by its two extremities, or it may extend to infinity in either direction or in both or it may return into itself in which case it is called a closed curve.

The different branches of a mathematical curve are not considered parts of the same physical curve because a point cannot pass along the curve from one branch to the other.

Any loop of a curve may be considered physically a closed curve.

When the position of a point on a closed curve is expressed in terms of its distance s measured along the curve from a fixed point, then if l is the whole length of the closed curve the travelling point will arrive at the same place when s becomes $s+l$ or $s+nl$ where n is any whole number.

Hence the position of the point in space is a periodic function of s and s is a function of the point of many values, these values forming an infinite arithmetical series.

On Surfaces

A Surface may either be infinite or it may be bounded by a closed curve or it may be a Closed Surface.

If a surface defined by a mathematical equation has several sheets either separate from each other or touching only at points or along lines then each sheet may be considered in physics as a separate surface.

A Closed surface is a finite surface enclosing a space so that a point cannot pass from within the surface to the space outside without passing through the surface.

A Closed surface may either be simply connected[(4)] like that of a sphere or complexly connected like that of a ring or of a solid body pierced with holes.

In a simply connected surface every closed curve drawn on the surface divides the surface into two parts so that a point cannot travel on the surface from the one part to the other without crossing the closed curve.

In a doubly connected surface[(5)] as that of a ring one closed curve may be drawn on the surface without disconnecting the surface.

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(4) The term is taken from Riemann's expression 'einfach zusammenhangende Fläche' in his paper 'Lehrsätze aus der analysis situs für die Theorie der Integrale von zweigliedrigen vollständigen Differentialen', *Journal für die reine und angewandte Mathematik*, **54** (1857): 105–10. Riemann's expression was adopted by Helmholtz in his paper 'Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen', *ibid.*, **55** (1858): 25–55, esp. 27. In Tait's translation of Helmholtz's paper, 'On the integrals of the hydrodynamical equations, which express vortex-motion', *Phil. Mag.*, ser. 4, **33** (1867): 485–512, esp. 486, 'simply-connected' surfaces is used as a rendition of Riemann's expression.

(5) Compare Riemann, 'Lehrsätze aus der analysis situs': 110; 'zweifach zusammenhangende Fläche'.

In the case of the ring, a closed curve drawn either longitudinally along the ring or transversely round its section does not separate one part of the surface from the other.

In a surface of n connexions $n-1$ closed curves may be drawn so that [it] may be possible to pass from any one point to any other without crossing any of these curves.(6)

Any Finite Space is bounded by one or more closed Surfaces. A Connected space is such that a point may branch from any one position within it to any other without crossing its boundary.

[2] TO DETERMINE THE COLLIGATION OF SYSTEMS OF CLOSED CURVES IN SPACE(7)

Let any system of closed curves in space be given and let them be supposed capable of having their forms changed in any continuous manner, provided that no two curves or branches of a curve ever pass through the same point of space, we propose to investigate the necessary relations between the positions of the curves and the degree of complication of the different curves of the system.

Let the system of curves as it exists at any instant be projected on a plane. Then if the different closed curves of the projected system do not intersect each other they are independent closed curves and if the projection of any one of them does not intersect itself it is a simple closed curve.

If the curves as projected on the plane appear to intersect each other we have to determine whether this indicates a real colligation of the curves or merely an overlapping or a reducible complication.

Let A, B, C &c be the different closed curves and $a\,b\,c$ &c their projections on the plane. Let a travelling point P start from a given point of a and travel completely round the curve. Let the points where the projection of A intersects the projections of itself or of other curves be called $a_1\ a_2 \dots a_n$, in the order in which the point P arrives at these points. When a intersects itself, P will arrive twice at the same point which will therefore be counted twice and have two different symbols. When a intersects another closed curve the number of intersections must be even. Hence n is always an even number for each closed curve.

(6) Compare Riemann, 'Lehrsätze aus der analysis situs'; and see Helmholtz, 'On the integrals of the hydrodynamical equations': 486n; 'An n-ly connected space is thus one which can be cut through by $n-1$, but no more, surfaces, without being separated into detached portions'.

(7) See Number 317.

Similarly let the intersections of the curve b the projection of B with the projections of the other curves be denoted by b_1, b_2, &c and so on for the other curves.

Now let us consider any one of the intersections, say a_p and let the other symbol of this point as it is a point on the intersected curve be b_q. Then if we draw a normal to the plane from this point it will pass through a point A_p on A and a point B_q on B.

If A_p is on the positive side of B_q we shall write the symbol of the normal $\dfrac{A_p}{B_q}$ but if A_p is on the negative side of B_q we shall write it $\dfrac{B_q}{A_p}$.

If the intersection a_p is of a with itself and if its other symbol is $a_{p'}$ we should similarly have a symbol $\dfrac{A_p}{A_{p'}}$ or $\dfrac{A_{p'}}{A_p}$ according to the relative positions of A_p and $A_{p'}$.

In this way we shall find for every point of intersection of the projected curves a symbol composed of the two symbols of the point, one above the other, the upper one denoting the curve which is on the positive side of the other.

If at any point of the projection three or more curves intersect the point will count for every combination of these curves two and two, for by altering the position of the curves the multiple point will be resolved into simple intersections of every pair of curves.

We have now to determine whether the number of these intersections can be diminished by continuous motion of the curves A, B, C without one curve cutting through itself or another.

If the number of apparent intersections of the curve a can be diminished it must be by two of the intersections coalescing and disappearing. For in the continuous motion of the curves the points a_1 a_2 &c move continuously and can only disappear in one way, namely by two intersecting curves changing their position so as no longer to intersect.

Let α β γ represent the number of points on each of the closed curves and l the total number of lines, then $l = \alpha+\beta+\gamma+\&c$.

Let s be the total number of intersections $s = \frac{1}{2}(\alpha+\beta+\gamma+\&c) = \frac{1}{2}l$.

Let f be the number of unit enclosed areas bounded by these lines $f = s+1$.

Let n be the number of sides of any polygon. Then since every line is a side either of two finite polygons or of a finite polygon and of the part of the plane external to them all

$$2l = n_1+n_2+\&c+n_{f+1}$$

or

$$4s = \text{sum of } s+2 \text{ integers.}$$

Hence some of the polygons must have less than four sides.

(1) Let us first consider polygons of one side, that is a curve forming a loop and intersecting itself. The symbol of the intersection is $\dfrac{A_p}{A_{p\pm1}}$.

In this case the intersection may be made to disappear by uncoiling the curve without interfering with its continuity.

Hence all intersections of the form $\dfrac{A_p}{A_{p\pm1}}$ may be eliminated and the symbols A_p and $A_{p\pm1}$ may be omitted from the cycle of the curve A.

(2) Polygons of two sides are formed by the intersection of two curves or two loops of the same curve.

If the symbols of the intersections are of the form $\dfrac{A_p}{B_q}$ and $\dfrac{A_{p\pm1}}{B_{q\pm1}}$ as in the upper figure then the two loops may be separated and the symbols belonging to them may be cancelled but if the symbols of intersection are of the form $\dfrac{A_p}{B_q}$ and $\dfrac{B_{q\pm1}}{A_{p\pm1}}$ the curves are linked together and cannot be separated without moving other parts of the system. The curve B may evidently be a different part of the curve A.

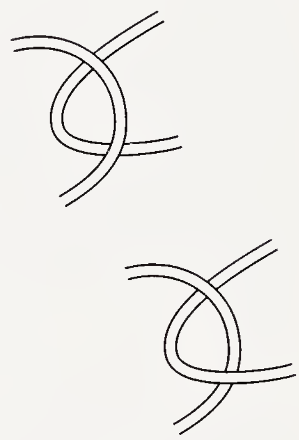

Figure 304,1

(3) Polygons of three sides must be of the forms

$$\left(\frac{A_p}{B_q}, \frac{A_{p\pm1}}{C_r}, \frac{B_{q\pm1}}{C_{r\pm1}}\right) \quad \text{or} \quad \left(\frac{A_{p\pm1}}{B_q}, \frac{B_{q\pm1}}{C_r}, \frac{C_{r\pm1}}{A_p}\right)$$

 or 

that is either one of the curves is above or below both the others and the curves may be arranged in order of position or each curve is above one of its companions and below the other.

In the first case any one curve can be moved past the intersection of the other two without disturbing them. In the second case this cannot be done and the intersection of two curves is a bar to the motion of the third in that direction.

When in passing round the triangle in the direction of the hands of a watch each curve is nearer than the preceding and farther away than the following curve the triangle is said to be right handed. When the reverse is the case it is said to be left handed.

(4) If in a polygon of any number of sides the curve forming one of the sides lies either above both the adjacent curves or below them both the curve forming that side may be moved away and the number of sides reduced.

Hence every polygon must be such that going round it in the direction of the hands of a watch every side is either above the preceding and below the following side, in which case it is right handed or the reverse.

If a polygon is partly right handed and partly left handed it may be reduced. Every right handed polygon is bounded by left handed polygons.

305

DRAFTS ON CONTINUITY AND TOPOLOGY

circa SEPTEMBER 1868[(1)]

From the originals in the University Library, Cambridge[(2)]

[1] ON PHYSICAL CONTINUITY AND DISCONTINUITY[(3)]

The idea of physical continuity is best conceived under the example of the continuous existence of matter in time and space.

A material particle, during the whole time of its existence must have a determinate position. Hence its path is a continuous line and its coordinates are continuous functions of the time.

We are thus led to the definition of the physical continuity of a function. A function is physically continuous within certain limits provided its differential coefficients with respect to its variables remain finite within those limits.

The idea of mathematical continuity refers rather to the form of the function than to its particular values, whereas a function may be physically continuous though its form may be different for different values of the variables.

The 'continuity' which is defined by the 'Equation of Continuity' is the continuous existence of the moving particles of a medium, not the continuity of the form of the functions expressing their velocity &c.

Most important applications of the idea of physical continuity to geometry have been made by Riemann (Crelle[(4)] [(5)]). The ideas of Riemann have been employed by Helmholtz[(6)] Betti[(7)] Thomson[(8)] &c in physical researches

(1) See Number 306.

(2) ULC Add. MSS 7655, V, d/12.

(3) An early draft of the *Treatise*, **1**: 6–7 (§7).

(4) Bernhard Riemann, 'Lehrsätze aus der analysis situs für die Theorie der Integrale von zweigliedrigen vollständigen Differentialen', *Journal für die reine und angewandte Mathematik*, **54** (1857): 105–10.

(5) Space in the MS.

(6) Hermann Helmholtz, 'Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen', *Journal für die reine und angewandte Mathematik*, **55** (1858): 25–55.

(7) Enrico Betti, 'Teorica delle forze che agiscono secondo la legge di Newton e sua applicazione alla elettricità statica', *Nuovo Cimento*, **18** (1863): 385–402; *ibid.*, **19** (1863): 59–75, 77–95, 149–75, 357–77; *ibid.*, **20** (1864): 19–39, 121–41. See especially Betti's discussion of Green's theorem in terms of the distribution of potential in simply-connected space (*Nuovo Cimento*, **19** (1863): 59–75). Compare also Maxwell in the *Treatise*, **1**: 108–111 (§100).

(8) William Thomson, 'On vortex motion', *Trans. Roy. Soc. Edinb.*, **25** (1869): 217–60, esp. 243 (= *Math. & Phys. Papers*, **4**: 13–66), where he adapts 'the terminology of Riemann, as known to me through Helmholtz'. Thomson's paper was read to the Royal Society of Edinburgh on 29 April 1867.

and I have found it necessary for my own purposes to employ a system of nomenclature of spaces, surfaces and lines which I shall now explain.

A line, a surface, or a space is said to be continuous when a material point can travel from any one point to any other without leaving the line surface or space.

If two lines, surfaces or spaces are continuous with each other they are physically one, if they are not, they are physically distinct. Thus the two branches of an hyperbola are physically distinct, but the three sides of a triangle are physically one line.

Limits of Spaces

Spaces are limited by surfaces, surfaces by lines and lines by points. A surface which limits a space must be either closed or infinite. In either case it is called a complete surface.

If we confine ourselves to finite spaces, they are separated from infinite space by a single closed surface which we may call the *external* surface. If the space has any other limits these must be defined by closed surfaces all of which are within the external surface and are external to each other.

If the space is infinite the only condition of its limits is that they are complete surfaces excluding each other.

Continuity of Spaces

Let any closed curve be drawn on the limiting surface and let a surface be drawn within the space bounded by the closed curve then in the case of a space of simple continuity this surface will divide the space into two distinct regions so that a point cannot travel from one to the other without crossing the surface.

Any solid body without any holes through it is an example of simple continuity. Now let a hole be bored through the solid converting it into a ring, and let a surface be drawn meeting the limiting surface along one side of the hole and round one side of the solid. A point can still travel from one side of this surface to the other by going round the other [2][(9)] side of the hole. If n holes had been bored through the solid, n such surfaces may be drawn without separating one part of the space from the rest. Such a space would be

(9) There are two folios in ULC Add. MSS 7655, V, d/12 – the first draft printed here as §2, and the revise printed as an appendix to Number 308 – which present alternative versions of the argument.

called if we follow the method of Riemann, an $(n+1)$ly connected space.(10) I prefer however for reasons which will appear as we proceed to call it an n-cyclic space.

If a finite space bounded by a single continuous surface is n-cyclic the bounding surface is also n-cyclic and the infinite space outside the surface is also n-cyclic as far as that bounding surface is concerned. If we consider the finite space as solid with n holes in it, then the infinite space has n channels by which it embraces the finite space and the finite space has also n channels by which it embraces the infinite space.

If the expression $Xdx+Ydy+Zdz = dV$ be a complete differential at every point within the finite space then in a simply connected space which we may call acyclic V can only have one value for each point of space but in an n-cyclic space V may have values infinite in number of the form

$$A = V_0+p_1 P_1+ \dots +p_n P_n$$

where V_0 is one of the values and $p_1 \dots p_n$ are integral numbers positive negative or zero and $P_1 \dots P_n$ are the values of

$$\int\left(X\frac{dx}{ds}+Y\frac{dy}{ds}+Z\frac{dz}{ds}\right)ds$$

taken round a closed curve drawn round each of the n channels belonging to the finite space.(11) The quantities $P_1 \dots P_n$ may be called the cyclic constants. They are important in the theory of Vortices and in Electromagnetism.(12)

If a space be bounded by several surfaces the number of cycles belonging to the space will be the sum of the number of cycles belonging to the different bounding surfaces.

[3] ON SURFACES

A surface may be either a complete surface, or it may be bounded by lines. A finite surface, if complete must be a closed surface and if bounded its boundaries must be closed curves.

The surface of an n-cyclic space is an n-cyclic surface. On such a surface $2n$ closed curves may be drawn without separating any one part of the surface from any other. For n closed curves may be drawn on the inside of the surface each round one of the channels of the finite internal space without destroying

(10) Compare Riemann's expression '$(n+1)$fach zusammenhangende Fläche' in his 'Lehrsätze aus der analysis situs'.

(11) See Number 318.

(12) Compare Maxwell's discussion of 'Stokes' theorem' in his letters to Stokes and Tait of 11 January and 4 April 1871 (Numbers 351 and 366).

the continuity of the inner side of the surface and n closed curves may be drawn on the outside of the surface round the n channels of the infinite external space without destroying the continuity of the outside of the surface, and a point which moves in the surface itself can still pass from any one point to any other without crossing any of these $2n$ lines.

Now consider a closed n-cyclic surface and let a closed curve be drawn upon it. If the curve surrounds one of the channels say of the internal space the surface becomes $(n-1)$ cyclic. It also ceases to be a complete surface and becomes a surface with two boundaries, the two sides of the curve drawn on it.

But if the curve does not surround a channel it cuts off a portion of the surface from the rest so that we have now two surfaces, each with a single boundary of which the one may be n'-cyclic and the other $(n-n')$ cyclic n' having any value from 0 to n.

If m closed curves are drawn on an n-cyclic surface none of which surrounds a channel or cuts off a channel from the rest of the surface, the surface remains n-cyclic with m boundaries. On such a surface $2n$ closed curves may be drawn together with $m-1$ lines from one boundary to another without destroying the continuity of the surface.

LETTER TO WILLIAM THOMSON

28 SEPTEMBER 1868

From the original in the University Library, Glasgow[1]

Ardhallow
Dunoon
1868 Sept 28

Dear Thomson

Can you get me one ticket or two to see the laying of the foundation of the new College?[2]

In my last letter I made a mistake in the correction for curvature of the interval between the disk & guard-ring.[3] The capacity of the disk ought to be

$$\frac{R^2}{4A}+\frac{(R+\frac{1}{2}B)B}{4(A+\alpha)}$$

where R = radius of disk B = breadth of interval A = distance of opposed surfaces α a quantity less than $B\dfrac{\log_e 2}{\pi}$.

I have been making a statement about the continuity discontinuity periodicity and multiplicity of functions generally and of lines surfaces & solids. Here is the upshot in connected form.[4]

Take a solid without any hollows in it or holes through it. It is a simply connected space bounded by one simply connected closed surface. Now bore n holes right through the solid. It is now a space of n connections bounded by an n-ly connected closed surface.

The infinite space outside is also n-ly connected. Now let there be m hollow spaces within the solid and let these be bounded by closed surfaces whose connexions are $n_1\ n_2\ n_3 \ldots n_m$. Then the solid will be an $(m+1)$-ly *bounded* space and its connexions will be $n+n_1+\&c+n_m$.[5]

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(1) Glasgow University Library, Kelvin Papers, M 25.

(2) The new building of the University of Glasgow: see Number 308 note (2).

(3) Number 303; and see Thomson's annotation.

(4) See Numbers 304 and 305.

(5) Compare Maxwell's correction in his letter to Thomson of 7 October 1868 (Number 308).

Surfaces
are either complete or bounded

Complete surfaces are either closed or infinite. A material point cannot get from one side of a complete surface to the other without passing through it. The boundary of any space is a complete surface. The boundary of an n-ly connected space is an n-ly connected surface. Now let an n-ly connected surface gradually collapse till its inner surfaces meet and the closed surface becomes a double surface everywhere enclosing an infinitely small space by a finite area. Considered in its genesis this is the limit of an n-ly connected closed surface.

Considered in its present state it is an n-ly *bounded* surface, that is, a surface bounded by n closed curves.

We cannot call any one of these curves the external and the rest the internal boundaries as in the boundaries of solids unless we are dealing with plane surfaces only.

A spherical surface with n holes in it is an n-ly bounded surface. A pair of trousers is triply bounded. A surface may be n-ly connected and n'-ly bounded, say the electroplating of a toast rack for $\overline{n-3}$ slices with n' places worn out.

Lines are complete or bounded

Complete lines are closed or infinite. Let the holes in an n-ly bounded surface be enlarged till they nearly and at length quite reach each other, we shall have an n-ly connected line or closed line of n loops.

Now let m of these loops collapse into single lines we shall have $n-m$ loops and m branches with abrupt ends. Such a line is $(n-m)$-ly connected and m-ly bounded.

I have got a 'wheel of life' made by White[(6)] with concave lenses instead of slits, the focal length being equal to the diameter of the wheel. This makes each picture appear to stand still as long as it is visible.[(7)] The light and

(6) See Number 301 note (2).

(7) Maxwell's 'zoetrope' (developed from an instrument popular in the 1860s) is described in the *Life of Maxwell*: 484–5. The instrument and some of the drawings described by Maxwell are preserved in the Cavendish Laboratory, Cambridge: see Plates XIII and XIV. Alluding to the lenses with which Maxwell improved the design, Campbell referred (*Life of Maxwell*: 37n) to a question for the Cambridge Mathematical Tripos on the morning of Thursday 7 January 1869, question xx, set by Maxwell as Junior Moderator. See *The Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 482; 'A lens is moving with velocity p perpendicular to its axis and an object at a distance a from the lens is moving with velocity q across the axis in the opposite

distinctness are much improved but the pictures appear half size. This may if required be corrected by holding a reading glass outside. White is now making another.

I have drawn Rankines Waves[8] in section with long plants of seaweed moving to shew the motion of the water at different depths. Also 3 Helmholtz Rings[9] threading through each other. 4th figure of Lancers[10] &c, generation and growth and final bursting of Volvox Globator,[11] dance of tadpoles in the curve $x = \sin 3\theta \quad y = \sin 2\theta$. I used to draw figures for the old disks with slits but found it was useless to draw them neatly on account of the dimness. By means of the lenses everything is quite distinct.

Yours truly
J. Clerk Maxwell

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direction. Find the focal length of the lens that to an eye on the other side of the lens the object may appear at rest'.

(8) See Number 223; and Rankine, 'On waves in liquids', *Proc. Roy. Soc.*, **16** (1868): 344–7.

(9) See Number 307 and Plate XIII.

(10) A form of quadrille (*OED*). Maxwell's drawing (see note (7)) shows dancers.

(11) A spherical green alga formed of a colony of cells in a gelatinous mass.

307

LETTER TO WILLIAM THOMSON

6 OCTOBER 1868

From the original in the University Library, Glasgow(1)

Ardhallow
Dunoon
Oct 6 1868

Dear Thomson

Many thanks for the ticket to Platform O(2) which arrived here yesterday and also for the first 6 pages of your paper on Vortex motion(3) which looks as if it was going to begin simply.

There are several curious misspellings but I suppose you have corrected them as they are scored.

In the foot notes you make an hypothesis about a mass of 20.5×10^6 grammes which I suppose to be the mass referred to by Mr Crum(4) at the Western Club, and that if the wind had been as good as on Sept 26 you could have looked us up here, and Mrs McCunn(5) says she would have been happy to have given you a bed and continues to keep the same at your disposal in case you should be able to make the passage.

We leave this on the 13th but I hope to see you in Glasgow on Thursday and to arrange either so or according to the other portion of your note explaining a contrary (not opposite) plan.

What you say about a uniform field of force proves that the lines of force are straight not that they are parallel. To prove them parallel you must show that a tube made of a ruled surface (otherwise a scroll) cannot be of uniform area of section unless it is prismatic or cylindric. I will look up Phil. Mag.(6)

H^2's 3 rings do as the 2 rings in his own paper that is those in front expand and go slower those behind contract and when small go faster and thread through the others.(7) I drew 3 to make the motion more slow and visible not

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(1) Glasgow University Library, Kelvin Papers, M 26.

(2) For the laying of the foundation stones for the new building of the University of Glasgow: see Number 308 note (2).

(3) William Thomson, 'On vortex motion', *Trans. Roy. Soc. Edinb.*, **25** (1869): 217–60 (= *Math. & Phys. Papers*, **4**: 13–66).

(4) Alexander Crum, Thomson's brother-in-law.

(5) Maxwell's sister-in-law (see Volume I: 537).

(6) Thomson was probably alluding to a discussion of lines of force by G. J. Stoney, 'On the experiment of Mahomet's coffin', *Phil. Mag.*, ser. 4, **36** (1868): 188–92.

(7) Hermann Helmholtz, 'On the integrals of the hydrodynamical equations, which express

Plate XIII. Maxwell's zoetrope or 'wheel of life' (1868), showing Helmholtz's vortex rings threading through each other (Number 307).

that I have solved the case of 3 rings more than to get a rough notion about this case and to make the sum of the three areas const. I have made them fat when small and thin when big.

A binocular wheel of life would require to be on a horizontal axis, the pictures would be outside the lenses but the amount of care required to draw a presentable stereoscopic pair of pictures is about 10 times that required for a presentable wheel of life with 13 pictures so that I estimate the ratio of trouble at $\frac{130}{n}$ where n is the ratio of expectation of an accurate picture in a stereoscope to same in Wheel.

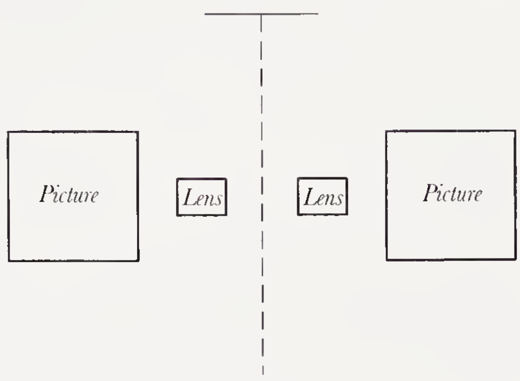

Figure 307,1

I have done a few more wheels.

1. Motion of 6 heavy balls in a vertical circle showing how the line joining opposite balls passes through a fixed point (pole of the line of height due to velocity) and how the triangle of 3 alternate balls touches a fixed circle and how lines forming opposite sides of the hexagon meet in the line of height due to velocity.
2. A fountain with a ball rolling on the top of the jet and throwing off drops which change colour as they pass through the rainbow positions.
3. Leapfrog of boys who give backs and leap alternately, cycle of 2 revolutions.
4. Acrobats male & female going opposite ways 25 positions of one and 27 of the other.
5. Growth of pines.

I am going to do uniform sliding motion ($u = Cy$) and the motion of a perfect fluid (with motes in it to make it visible) about a moving cylinder radius 1.(8)

---

vortex motion', *Phil. Mag.*, ser. 4, **33** (1867): 485–512, esp. 510, on the interaction of two circular vortex rings; 'We can now see generally how two ring-formed vortex-filaments having the same axis would mutually affect each other, since each, in addition to its proper motion, has that of its elements of fluid, as produced by the other. If they have the same direction of rotation, they travel in the same direction; the foremost widens and travels more slowly, the pursuer shrinks and travels faster, till, finally, if their velocities are not too different, it overtakes the first and penetrates it. Then the same goes on in the opposite order, so that the rings pass through each other alternately.' See Plate XIII.

(8) See Number 310 and Plate XIV.

If in this case $\phi = r\left(r-\frac{1}{r}\right)\sin\theta$ is the stream function(9)

$$\frac{dr}{dt} = \frac{1}{r}\frac{d\phi}{d\theta} = \frac{1}{r^2}\sqrt{r^4-(2+\phi^2)\,r^2+1}$$

whence
$$t = \int\frac{r^2dr}{\sqrt{r^4-(2+\phi^2)r^2+1}}$$

which may be done by elliptic functions. This gives the time as a function of r and ϕ the stream function so that we can find the position, say, of a row of particles after the cylinder has past, which, before, were in a straight line.

Mrs McCunn and Mrs Maxwell have just got tickets & I hope to see you on Thursday and arrange a meeting if possible.

Yours truly
J. Clerk Maxwell

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(9) Compare Helmholtz's expression 'die Strömungslinien der Flüssigkeit' (see Number 302 note (5)) and Rankine's term 'stream-line' (see Number 337 esp. note (4)); and Maxwell's definition of the 'stream function' in his paper 'On the displacement in a case of fluid motion', *Proceedings of the London Mathematical Society*, **3** (1870): 82–7, on 83n (= *Scientific Papers*, **2**: 209n). See Number 311.

308

LETTER TO WILLIAM THOMSON

7 OCTOBER 1868

From the original in the University Library, Glasgow[1]

Ardhallow
Dunoon
Oct 7 1868

Dear Thomson

The Senate sent me an invitation to Platform O for which I thank you.[2] M^rs^ Maxwell and M^rs^ M^c^Cunn have got tickets for platform A from Prof. Blackburn.[3] ⟨If you have a ticket for O for M^rs^ Maxwell she would prefer being with me but if it is not convenient she has friends in A. I will call at the Western Club early on Thursday.⟩

Yours truly
J. Clerk Maxwell
over

I find that I made a mistake about the connectedness of hollow solids.[4]

If the solid is bounded by m surfaces of which one is external and the rest internal and if the connectedness of these are

$$n_1 \; n_2 \ldots n_m$$

Then if n_1 belongs to the external surfaces it introduces n_1 connexions into the solid, but if n_2 belongs to an internal surface it introduces only n_2-1 *new* connexions.

Hence the whole number of connexions is $n_1+n_2-1+n_3-1 \qquad +n_m-1$

$$=\sum(n)-m+1.$$

If a surface is entirely composed of triangular facets then if it is singly connected, the sides of the triangles determine the form of the surface and have no conditions among themselves except limiting ones. If the surface is n-ly connected there are $6n$ conditions about the lengths of the sides.

If the sides meeting in p points are broken p degrees of freedom are introduced.

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(1) Glasgow University Library, Kelvin Papers, M 27.

(2) For the ceremonial laying of the foundation stones, by the Prince and Princess of Wales, of the new building of the University of Glasgow at Gilmorehill, Glasgow on 8 October 1868.

(3) Hugh Blackburn, Professor of Mathematics at Glasgow University (Venn). See Volume I: 238n.

(4) See Number 306.

APPENDIX: A DRAFT REVISE ON HOLLOW SOLIDS

circa OCTOBER 1868

From the original in the University Library, Cambridge(5)

[...] side of the hole. If $n-1$ holes had been bored through the solid $n-1$ surfaces may be drawn without separating one part of the space from the rest. Such a space is said to be n-ly continuous or to have n connexions. A ring is doubly continuous, a figure of 8 triply continuous and so on.

The external surface of a space has the same degree of continuity as the space itself, and the space outside the surface has the same degree of continuity so far as that surface is concerned.

Next let us consider the continuity of a space bounded by m complete surfaces. If all the surfaces are of simple continuity the space is of simple continuity, but if any one has continuity of the n^{th} degree, $n-1$ degrees of continuity are added to the space hence if the degrees of continuity of the m bounding surfaces are

$$n_1, n_2 \dots n_m$$

the space bounded by them will have

$$n_1 + n_2 + \&\text{c} + n_m - m + 1 \text{ degrees of continuity.}$$

Continuity of Surfaces

A Surface is said to have n degrees of continuity when $n-1$ closed curves may be drawn upon it without destroying its continuity.

Limits of Surfaces

A finite surface can be limited by m closed curves which must exclude each other, but no one is necessarily the external limit unless the surface is plane.

If a surface of m limits is regarded as a stratum of infinitesimal thickness, the stratum is a space of m degrees of continuity.

(5) ULC Add. MSS 7655, V, d/12. For the first draft of this folio see Number 305 §2.

FROM A LETTER TO WILLIAM HUGGINS

13 OCTOBER 1868

From Campbell and Garnett, *Life of Maxwell* (2nd edn)[1]

Ardhallow
Dunoon
Oct 13/68

My dear Sir

I sympathise with you in your great sorrow. Though my own mother was only eight years with me, and my father became my companion in all things, I felt her loss for many years, and can in some degree appreciate your happiness in having so long and so complete fellowship with your mother. I have little fear, however, that the nearness to the other world which you must feel will in any way unfit you for the work on which you have been engaged, for the higher powers of the intellect are strengthened by the exercise of the nobler emotions....

Your identification of the spectrum of comet II with that of carbon is very wonderful.[2] The dynamical state of comets' tails is most perplexing,[3] but the chemistry and activity of their heads leads to new questions. With respect to the transparency of a heavenly body, I think it indicates scattered condition rather than gaseity. A cloud of large blocks of stone is much more transparent than air of the same average density. Such blocks in a nebula would never be themselves seen, but perhaps if they were often to encounter each other, the results of the collision would be incandescent gases, and might be the only visible part of the nebula.

...Any opinion as to the form in which the energy of gravitation exists in space is of great importance, and whoever can make his opinion probable will have made an enormous stride in physical speculation. The apparent universality of gravitation, and the equality of its effects on matter of all kinds are most remarkable facts, hitherto without exception; but they are purely experimental facts, liable to be corrected by a single observed exception. We

(1) *Life of Maxwell* (2nd edn): 260–1.

(2) William Huggins, 'Further observations on the spectra of some of the stars and nebulæ, with an attempt to determine therefrom whether these bodies are moving towards or from the earth, also observations of the spectra of the sun and of Comet II., 1868', *Phil. Trans.*, **158** (1868): 529–64, esp. 557–62. Huggins included (pp. 532–5) the text of Maxwell's letter of 10 June 1867 (Number 271).

(3) An issue discussed by Huggins, 'Further observations on the spectra of some of the stars and nebulæ': 563–4. See Maxwell's letter to G. P. Bond of 25 August 1863 (Number 217).

cannot conceive of matter with negative inertia or mass; but we see no way of *accounting* for the proportionality of gravitation to mass by any legitimate method of demonstration. If we can see the tails of comets fly off in the direction opposed to the sun with an accelerated velocity, and if we believe these tails to be matter and not optical illusions or mere tracks of vibrating disturbance, then we must admit a force in that direction, and we may establish that it is caused by the sun if it always depends upon his position and distance. I therefore admit that the proposition that the sun repels comets' tails is capable of proof; but whether he does so by his ordinary attractive power being changed into repulsion by a change of state of the matter of the tail is another question.[4] Now, it seems ascertained by simple observations with telescopes that the coma is formed by successive explosions out of the nucleus, mostly on the side of the sun, and that the formation of the tail depends on the coma, though the substance is invisible in the state of passing from the coma to the tail. Then, by your observations, the nucleus and coma have light of their own, probably due to carbon in some gaseous form; but the tail's light being polarised in the plane of the sun is due to him. Hence the head is fire and the tail smoke. The head obeys gravitation, which is exerted on it with precisely the same intensity as on all other known matter, solid or gaseous. The tail appears to be acted on in a contrary way. If the comet consisted of a mixture of gravitating and levitating matter, and is analysed by the sun, then before the emission of the tail the acceleration due to gravitation should be less than on a planet at the same distance; the more complete the discharge of tail the greater the intensity of gravitation on the remaining head.

N.B. – To understand the dynamics of the tail, the motion in space of particular portions of it must be studied.

[J. Clerk Maxwell]

(4) In his paper 'Further observations on the spectra of some of the stars and nebulæ': 564, Huggins suggested that: 'It may be that this apparent repulsion takes place at the time of the condensation of the gaseous matter of the coma, into the excessively minute solid particles of which the tail probably consists.... Perhaps it would be too bold a speculation to suggest that, under the circumstances which attend the condensation of the gaseous matter into discrete solid particles, the division may be pushed to its utmost limit, or nearly so. If we could conceive the separate atoms to be removed beyond the sphere of their mutual attraction of cohesion, it might be that they would be affected by the sun's energy in a way altogether different from that of which we have been hitherto the witnesses upon the earth.'

LETTER TO WILLIAM THOMSON

16 OCTOBER 1868

From the original in the University Library, Glasgow[1]

Ardhallow Oct 16 1868

Dear T.

I have been trying the cap on today and have got the density at the vertex or in other words the state of the poll – as thus[2]

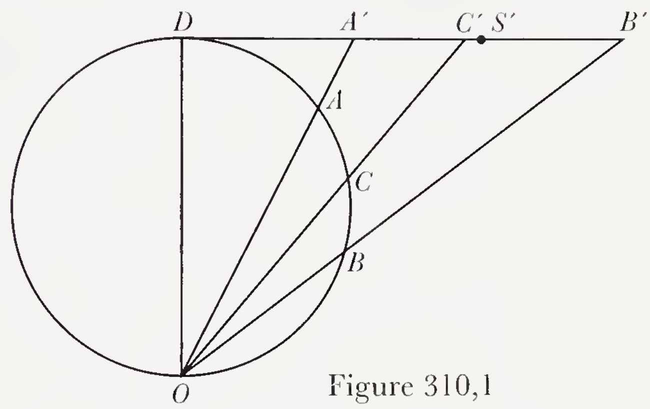

Figure 310,1

1 on a sphere $4\pi\rho = \dfrac{\text{potential}}{\text{radius}}$.

Flatten it to a disk $4\pi\rho = \dfrac{\text{potential}}{\sqrt{\text{rad}^2 - \text{dist}^2 \text{ from centre}}}$ on each side.[3]

2 Let $A'B'$ be the disk S' the centre, AB the cap C its pole.

Let $DOC = \theta \quad COA = COB = \alpha$

$DO = D \quad CO = x = D\cos\theta \quad CA = CB = a = D\sin\alpha.$

Then $DC = D\tan\theta \quad DA' = D\tan(\theta-\alpha) \quad DB' = D\tan(\theta+\alpha)$

whence $S'A' = D\dfrac{\sin\alpha\cos\alpha}{\cos(\theta+\alpha)\cos(\theta-\alpha)}$

---

(1) Glasgow University Library, Kelvin Papers, M 28.

(2) Maxwell is here considering the distribution of electricity on a portion of a spherical surface bounded by a small circle. Thomson had first communicated the results of his investigation of this problem (by his method of electric images) in a letter of 16 September 1846 to Joseph Liouville, published in the 'Extraits de deux lettres addressées à M. Liouville', *Journal de Mathématiques Pures et Appliquées*, **12** (1847): 256–65, esp. 263–4 (= *Electrostatics and Magnetism*: 152–4). Thomson's correspondence with Maxwell on his method of investigation is not extant, but he published his 'Determination of the distribution of electricity on a circular segment of plane or spherical conducting surface, under any given influence' (which is dated January 1869) in his 1872 reprint of his papers on *Electrostatics and Magnetism*: 178–91. In the *Treatise*, Maxwell's 'Application of electrical inversion to the case of a spherical bowl' (*Treatise*, **1**: 221–5 (§§176–81)), follows Thomson's published account of the problem: see Maxwell's letters to Thomson of 17 August and 1 October 1869 (Numbers 326 and 327).

(3) For this expression for the surface-density of electricity on an infinitely-thin circular disc, see George Green, 'Mathematical investigations concerning the laws of the equilibrium of fluids analogous to the electric fluid, with other similar researches', *Trans. Camb. Phil. Soc.*, **5** (1833): 1–63, esp. 61. This result is cited by Thomson in his 1869 paper on the 'Distribution of electricity on a circular segment...' (*Electrostatics and Magnetism*: 179).

$$\text{and } DS' = D\frac{\sin\theta\cos\theta}{\cos(\theta+\alpha)\cos(\theta-\alpha)}$$

$$\text{and } C'S' = D\frac{\sin\theta\sin^2\alpha}{\cos(\theta+\alpha)\cos(\theta-\alpha)}.$$

$$4\pi \text{ Density at } C' = 4\pi\rho' = \frac{V}{\sqrt{S'A'^2 - C'S'^2}}$$

$$= \frac{V}{D}\frac{\cos(\theta+\alpha)\cos(\theta-\alpha)}{\sin\alpha\sqrt{1-\sin^2\alpha\cos^2\theta}}$$

$$= \frac{V}{D}\frac{\dfrac{x^2-a^2}{D^2}}{\dfrac{a}{D}\sqrt{1-\dfrac{a^2x^2}{D^4}}}$$

$$= \frac{V}{aD^2}\frac{x^2-a^2}{\sqrt{1-\dfrac{a^2x^2}{D^4}}}.$$

Density at C, on the spherical surface $\rho = \rho\dfrac{D^3}{x^3} = \dfrac{VD}{4\pi a}\dfrac{1}{x^3}\dfrac{x^2-a^2}{\sqrt{1-\dfrac{a^2x^2}{D^4}}}$.

Potential at same point $= \dfrac{VD}{x}$. Hence a charge $-VD$ placed at O would reduce the potential of the cap to zero, and would induce the above density at C on both sides of the surface.

If CO is constant C being fixed the part cut off by the path of O is $\pi D\dfrac{x^2}{D} = \pi x^2$. Hence if there is a distribution such that the density is σ the quantity between x and $x+dx$ will be $2\pi\sigma x\,dx$. Substitute this for $-VD$ in the expression for ρ and we get

$$d\rho = -\frac{\sigma}{2a}\frac{x^2-a^2}{x^2\sqrt{1-\dfrac{a^2x^2}{D^4}}}dx$$

for the part of the density at C due to a uniform density over the sphere between x & $x+dx$.

The integral is $$-\frac{\sigma}{2}\left\{\frac{a}{x}\sqrt{1-\frac{a^2x^2}{D^4}}+\frac{D^2}{a^2}\sin^{-1}\frac{ax}{D^2}\right\}$$

when $x = D$ this becomes $$-\frac{\sigma}{2}\left\{\frac{a}{D}\sqrt{1-\frac{a^2}{D^2}}+\frac{D^2}{a^2}\alpha\right\}$$

when $x = a$ it becomes $$-\frac{\sigma}{2}\left\{\sqrt{1-\frac{a^4}{D^4}}+\frac{D^2}{a^2}\sin^{-1}\frac{a^2}{D^2}\right\}.$$

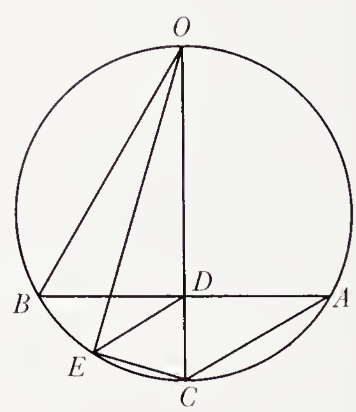

Figure 310,2. Make $EC = DC$, join OE, OB.

Whole density at poll due to induction of uniform cap

$$=\frac{\sigma}{2}\left\{\sqrt{1-\frac{a^2}{D^2}}\left(\sqrt{1+\frac{a^2}{D^2}}-\frac{a}{D}\right)-\frac{D^2}{a^2}(\text{angle } BOE)\right\}=\rho.$$

P.S. Integral from x_1 to $x_2 = -\frac{\sigma}{2}D^2\left(\frac{\sin(\gamma_1-\gamma_2)}{x_1x_2}-\frac{\gamma_1-\gamma_2}{a^2}\right)$

where $\sin\gamma_1 = \frac{ax_1}{D^2}$ and $\sin\gamma_2 = \frac{ax_2}{D^2}$.

Now plaster the whole over to a density σ so that $4\pi\sigma = \frac{2V}{D}$ then the inductor cap will be annihilated, the induced cap will be raised to pot. V and a density σ will be added on the outside.

Hence the outside density will be $\left.\begin{matrix}\rho+\sigma\\ \rho\end{matrix}\right\}$ at the vertex.
inside ———

$$\rho \text{ outside} = \frac{V}{4\pi D}\left\{\frac{D^2}{a^2}(\text{angle } BOE)-\sqrt{1-\frac{a^2}{D^2}}\left(\sqrt{1+\frac{a^2}{D^2}}-\frac{a}{D}\right)\right\}+\frac{V}{2\pi D}$$

$$\rho \text{ inside} = \frac{V}{4\pi D}\left\{\frac{D^2}{a^2}(\text{angle } BOE)-\sqrt{1-\frac{a^2}{D^2}}\left(\sqrt{1+\frac{a^2}{D^2}}-\frac{a}{D}\right)\right\}$$
$$-\sin\alpha.\sin BOE^{(4)}$$

If D is very great BOE becomes $\frac{a}{D}-\frac{a^2}{D^2}$ and $\rho = \frac{V}{4\pi a}$ on both sides.

If $a = \cos\beta$ where β is a small angle $OB = D\sin\beta$
$OE = D\sin\beta\sqrt{2-\sin^2\beta}$.

$$\rho \text{ inside} = \frac{V}{4\pi D}\left\{\frac{1}{1-\sin^2\beta}\{\sin^{-1}(\sin\beta\sqrt{2-\sin^2\beta})-\beta\}\right.$$
$$\left.-\sin\beta(\sqrt{2-\sin^2\beta}-\sqrt{1-\sin^2\beta})\right\}$$
$$=\frac{V}{4\pi D}\left\{\frac{1}{\cos^2\beta}\{BOE-\cos\beta\sin BOE\}\right\}.$$

Now BOE is a small angle equal to $(\sqrt{2}-1)\beta$ nearly and if $BOE = \gamma$

$$\langle BOE-\cos\beta\sin BOE\rangle = \gamma-\cos\beta\sin\gamma = \gamma\left(\frac{\beta^2}{2}-\frac{\gamma^2}{3}\right) = \frac{4\sqrt{2}-3}{6}\beta^3$$

(4) Compare Thomson's result stated in his letter to Liouville of 16 September 1846, proved in his 1869 'Distribution of electricity on a circular segment...' (*Electrostatics and Magnetism*: 184–5), and summarised by Maxwell in the *Treatise*, **1**: 223–4 (§ 180).

a result similar to Greens[(5)] if I recollect but exact up to the approx[n]

$$\text{inside } \rho = \frac{V}{4\pi D}\frac{4\sqrt{2}-3}{6}\beta^3.$$

To get the density *anywhere* the integration is more difficult. I propose for the point P to take the point Q where PT QT are tangent planes meeting in the plane AB. Then I divide the caps by circles such that at any point R, $\frac{QR}{PR} = n$ and the plane PQR cuts PT in a line making an angle ϕ with PT.

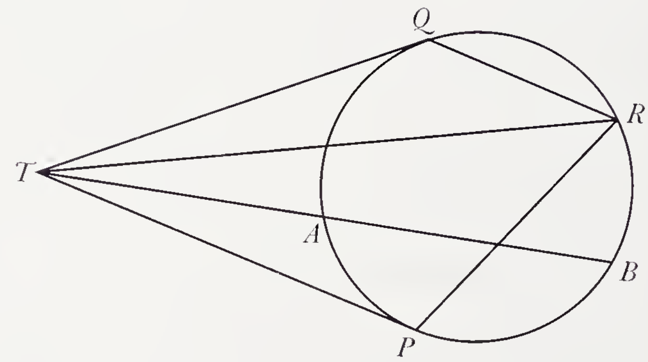

Figure 310,3

I have made some more wheels of life.[(6)] A harp string a fiddle string a piano wire and a sound wave also a cylinder going through a liquid sideways. There are streaks of paint in the liquid like this lapping round the successive cylinders.[(7)] The unlearned pronounce it lively. We go to Edin[h] on the 20[th] and Glenlair 22[nd].

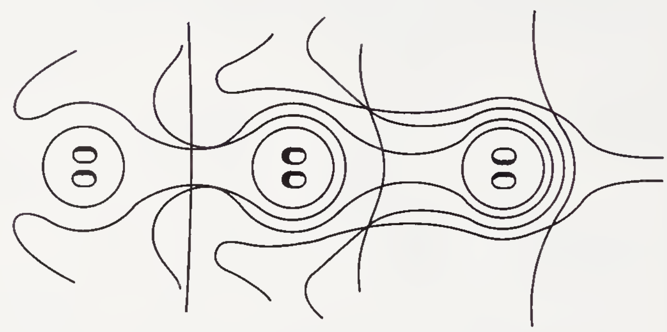

Figure 310,4

If you are in London do not seek us at 8 Palace Gardens Terrace as we are not there. I will tell you where we are, probably Bayswater. The Moderators have just begun work.[(8)]

Yours truly
J. Clerk Maxwell

I see no mistake in the result on last page at the bottom.[(9)] It seems not to agree with yours $f = D$ $\quad a = a$ $\quad r = x$?[(10)]

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(5) Green, 'Mathematical investigations concerning the laws of the equilibrium of fluids': 60–3.

(6) See Numbers 306 and 307.

(7) See Plate XIV and Number 311; and J. Clerk Maxwell, 'On the displacement in a case of fluid motion', *Proceedings of the London Mathematical Society*, **3** (1870): 82–7, esp. Fig. 1 (= *Scientific Papers*, **2**: 211). Thomson sketched curves lapping round a cylinder on the letter.

(8) Maxwell had been appointed Moderator for the 1869 Mathematical Tripos; see *The Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 472.

(9) The expression for the 'whole density at poll due to induction of uniform cap'.

(10) See Thomson's symbols in his letter to Liouville of 16 September 1846, his 1869 'Distribution of electricity on a circular segment', and Maxwell's account in the *Treatise*, **1**: 221–5 (§§176–81).

Plate XIV. Maxwell's zoetrope or 'wheel of life' (1868), showing the motion of a cylinder through a liquid (Number 310).

LETTER TO WILLIAM THOMSON

30 OCTOBER 1868

From the original in the University Library, Glasgow[1]

Glenlair
Dalbeattie
1868 Oct 30

Dear Thomson

I got your letter about St Andrew's.[2] Swan[3] & Campbell[4] have also written. One great objection is the East Wind which I believe is severe in those parts. Another is that my proper line is in working not in governing, still less in reigning and letting others govern.

I have settled the case of the water through which a cylinder passes.[5]

r = distance from axis of cylinder θ angle of r

a = radius $\psi = \left.\begin{matrix}\langle\text{velocity}\rangle \\ \text{action}\end{matrix}\right\}$ potential[6] ϕ = stream potential[7]

$$u = \frac{d\psi}{dx} = \frac{d\phi}{dy} \qquad v = \frac{d\psi}{dy} = -\frac{d\phi}{dx}$$

$$\psi = x\left(1+\frac{a^2}{r^2}\right) \quad \phi = y\left(1-\frac{a^2}{r^2}\right) = \left(r-\frac{a^2}{r}\right)\sin\theta$$

$$\frac{dr}{dt} = \dot{r} = \frac{1}{r}\frac{d\phi}{d\theta} = \left(1-\frac{a^2}{r^2}\right)\cos\theta = \left(1-\frac{a^2}{r^2}\right)\left(1-\frac{\phi^2}{\left(r-\frac{a^2}{r}\right)^2}\right)^{\frac{1}{2}}$$

(1) Glasgow University Library, Kelvin Papers, M 29. Previously published in A. T. Fuller, 'James Clerk Maxwell's Glasgow manuscripts: extracts relating to control and stability', *International Journal of Control*, **43** (1986): 1593–1612, esp. 1600–3.

(2) About the vacant post of Principal of the United College of St Salvator and St Leonard in the University of St Andrews, following the resignation of James David Forbes. See Numbers 312, 313, 314 and 315.

(3) William Swan, Professor of Natural Philosophy at St Andrews (*DNB*); see Volume I: 398n.

(4) Lewis Campbell was Professor of Greek at St Andrews.

(5) See Numbers 307 and 310; and Plate XIV. The discussion which follows is a preliminary version of Maxwell's paper 'On the displacement in a case of fluid motion', *Proceedings of the London Mathematical Society*, **3** (1870): 82–7 (= *Scientific Papers*, **2**: 208–14).

(6) In 'On the displacement in a case of fluid motion': 83 Maxwell uses the term 'velocity-potential', following Helmholtz: see Number 254 note (5).

(7) See Number 307 esp. note (9).

$$\frac{dt}{dr} = \frac{r^2}{\sqrt{r^4-(2a^2+\phi^2)r^2+a^4}}.$$

Hence we can find t the time of passage along a stream line indicated by a constant value of ϕ between two values of r.

Make
$$\sqrt{4a^2+\phi^2}+\phi = 2\beta$$
$$\sqrt{4a^2+\phi^2}-\phi = 2c\beta$$

or
$$c = \frac{a^2}{\beta^2}$$

then $t = \displaystyle\int \frac{r^2dr}{\sqrt{r^2-\beta^2}\sqrt{r^2-c^2\beta^2}}.$

Put $r = \dfrac{\beta}{\sin\psi}$[8] $\quad t = \displaystyle\int \frac{d\psi^{(9)}}{\sin^2\psi\sqrt{1-c^2\sin^2\psi}}$

$$t = \beta\cot\psi\sqrt{1-c^2\sin^2\psi}+\beta(E_c(\psi)-F_c(\psi))$$
$$= \frac{\sqrt{r^4-(2a^2+\phi^2)\,r^2+a^4}}{r}+\frac{1}{2}(\sqrt{4a^2+\phi^2}+\phi)\,(E_c(\psi)-F_c(\psi)).$$

After a complete passage a particle whose original distance from the plane of motion of the axis of the cylinder is y is translated forward along x a distance $2a\dfrac{1}{\sqrt{c}}(F_c-E_c)$ where $c = \dfrac{4a^2}{(\sqrt{4a^2+y^2}+y)^2}$ & F_c & E_c are complete elliptic functions[10] (for when $r = \infty \quad \phi = y$).

Hence when the cylinder moves from $-\infty$ to $+\infty$ *every* particle moves forward. But if the fluid is in a fixed vessel a portion equal in volume to the cylinder must go backward. Hence our case must be that of an infinitely large vessel not fixed but free and having a momentum equal to that of the cylinder in its infinite mass.[11]

---

(8) ψ is here and in sequel an angular variable.

(9) Read: $-\beta d\psi$.

(10) Compare Number 295 esp. note (10).

(11) Maxwell's paper 'On the displacement in a case of fluid motion' was read to the London Mathematical Society on 10 March 1870. On 8 April 1870 Robert Tucker, Secretary of the Society, wrote to Maxwell about its publication in the *Proceedings*. 'Both referees pronounce very warmly in favour of publication – one writes we could get a little more on the subject from the author & that he could be persuaded to give a drawing of the curve for the case considered in the last page calculated from the =[ns] – by aid of Legendre's tables – it might involve, he says, too much labour, to draw accurately from the calculations. He suggests my returning it to you for the consideration of the slight pencil alterations in it – so that if you approve of the same you can make them yourself before I send the ms on to the printers (the Council on two such recommendations being sure to print). / I return the ms with this & trust you will return it to me, as soon as possible – leaving the tracing of the curve to a future occasion if it involves

N.B. Let RA be a circular ring vortex radius a P an equal ring at distance y then the quantity of fluid which flows through P in unit of time is

$$4\pi\sqrt{aa'}\frac{1}{\sqrt{c}}(F_c - E_c) \text{ where}$$

$$c = \frac{PR-PA}{PR+PA} = \frac{4a^2}{(\sqrt{4a^2+y^2}+y)^2}.$$

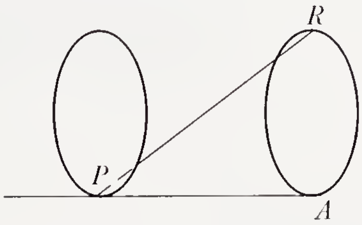

Figure 311,1

So the expression does for both cases.

Here is another electrical prop. founded on Green.[12]

Let A_1 A_2 A_3 &c be conductors, A_3 &c at potential 0. Let a charge E placed on A_1 induce a charge nE on A_2 placed in communication with the ground. Then if A_1 is insulated free of charge and A_2 is charged to potential V, the potential of A_1 will be nV where n is the same ratio as before.

I find some curious cases of unstable motion of tubes through which liquid flows.

Let A = moment of inertia of tube full of solid liquid about O.

$B = 2$ area of tube $\times \rho$.

$C = \int \rho \frac{ds}{k}$ where k is the section $OP = r$

k = section of opening at P α angle $\widehat{OP}$ direction of stream then if $A + r^2k^2C - 2Brk\cos\alpha$ is negative there will be oscillations of increasing amplitude. Unfortunately this quantity is always positive which I have only just found out. It reaches zero only when the tube is a circle with centre at O, no mass of tube itself and no radial piece from O.

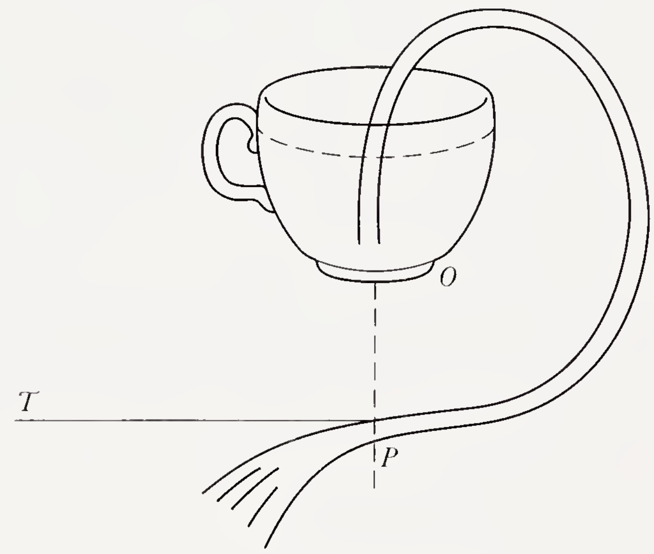

Figure 311,2

Yours truly
J. Clerk Maxwell

difficulty. / Faithfully yours / R. Tucker Hon. Sec^{y}. / I thought this course of letting you know the referees' views would be more satisfactory to you than for you to correct the proof.' (ULC Add. MSS 7655, II/35). It seems likely that Figs. 2 and 3 of 'On the displacement in a case of fluid motion', showing the paths of particles at different distances from the cylinder, were added to the MS of the paper at the referee's suggestion.

(12) See Maxwell's discussion of the proposition in the *Treatise*, **1**: 92 (§89).

FROM A LETTER TO LEWIS CAMPBELL

3 NOVEMBER 1868

From Campbell and Garnett, *Life of Maxwell*(1)

Glenlair
Dalbeattie
3 November 1868

I have given considerable thought to the subject of the candidature, and have come to the decision not to stand.(2) The warm interest which you and other professors have taken in the matter has gratified me very much, and the idea of following Principal Forbes had also a great effect on my feelings, as well as the prospect of residing among friends; but I still feel that my proper path does not lie in that direction – Your afft. friend,

J. Clerk Maxwell

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(1) *Life of Maxwell*, 345–6.
(2) See Numbers 311, 313, 314 and 315.

313

LETTER TO WILLIAM ROBERT GROVE

7 NOVEMBER 1868

From the original in the Library of the Royal Institution, London[1]

Glenlair
Dalbeattie
7 Nov 1868

Dear Sir

I have received an invitation from two thirds of the Professors of the United College of St Salvator and St Leonard, St Andrews, to come forward as a candidate for the office of Principal of that College, of which D^r J. D. Forbes has given in his resignation.[2]

They wish a scientific man to succeed Brewster[3] and Forbes and have done me the honour to think me qualified.

I have therefore become a candidate and as far as I know there is no other professedly scientific man in the field.

If you are of the opinion that I am qualified for the situation and could bring my claims in any way before the Home Secretary or Lord Advocate, I should esteem it a great favour.

The vacancy occurs on the 11th November and Government will probably lose no time in making the appointment.[4]

I have paid so little attention to the political sympathies of scientific men that I do not know which of the scientific men I am acquainted with have the ear of the Government. If you can inform me, it would be of service to me.

I remain
Yours truly
J. Clerk Maxwell

W. R. Grove Esq^re F.R.S.

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(1) Grove Papers, The Royal Institution, London. First published in C. Domb, 'James Clerk Maxwell in London 1860–1865', *Notes and Records of the Royal Society*, **35** (1980): 67–103, on 96–7.

(2) See Numbers 311, 312, 314 and 315.

(3) Sir David Brewster had held the office of Principal until 1860, being succeeded by Forbes; see Volume I: 623.

(4) There was an election in November 1868, Disraeli's Conservative government being replaced by Gladstone's Liberal ministry in early December.

LETTER TO GEORGE BIDDELL AIRY

9 NOVEMBER 1868

From the original in the Royal Greenwich Observatory Archive(1)

Glenlair
Dalbeattie
Nov 9 1868

Dear Sir

Dr J D Forbes has resigned the Principalship of the United College of St Salvator & St Leonards, St Andrews.

I have received a letter signed by two thirds of the Professors of the United College inviting me to become a candidate for the vacant office, which is in the gift of the Crown. Principal Tulloch(2) of St Mary's College, the Vice Chancellor of the University concurs in the invitation.

They are of the opinion that the successor of Sir David Brewster and of Principal Forbes should be a scientific man and they have done me the honour of assuring me that in that respect my appointment would be acceptable to them.

I have therefore become a candidate and as far as I know there is no other professedly scientific man in the field.

If you are of opinion that I am qualified for the situation you would confer on me a great favour if you could assist me in bringing my claims before the Home Secretary(3) either directly or otherwise.

The vacancy occurs on the 11th Nov. and Government will probably lose no time in making the appointment.(4)

I remain
Yours truly
J. Clerk Maxwell

The Astronomer Royal
Greenwich

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(1) Royal Greenwich Observatory Archive, ULC, Airy Papers 6/5, 409R–410R.

(2) John Tulloch (*DNB*).

(3) On this procedure compare Maxwell's application to Marischal College in 1856: see Volume I: 392–403.

(4) In his reply of 12 November 1868 (Airy Papers 6/5, 411R–V) Airy explained his position as Astronomer Royal: 'If it is in my power to assist you in your views regarding the office of Principal at St Andrews, I will do my best. But in this stage of the matter, there is difficulty arising from my connexion with the Government. As a general rule...I cannot suggest to the Government a name for an office.... If any Department of the Government should apply to me for an opinion, I should be free.'

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LETTER TO WILLIAM THOMSON

9 NOVEMBER 1868

From the original in the University Library, Glasgow[(1)]

Glenlair
Dalbeattie
1868 Nov 9

Dear Thomson

When I last wrote I had not been at St Andrews.[(2)] I went last week, and have gone in for the Principalship. If you can certify my having been industrious &c since 1856, or if you can tell me what scientific men are conservative or still better if you can use any influence yourself in my favour pray do so. 6 Professors out of 9 have memorialized the Ld Adv. & Home Sec.[(3)] for me together with Principal Tulloch the V. Chancellor. Of the other 3, one Prof Shairp, is a candidate[(4)] and one, Prof. Bell[(5)] does not approve of memorials at all and is neutral.[(6)] I have written to Sabine[(7)] Airy[(8)] Stokes[(9)] and Grove.[(10)]

Yours truly
J. Clerk Maxwell

(1) Glasgow University Library, Kelvin Papers M 30. Previously published in C. Domb, 'James Clerk Maxwell in London 1860–1865', *Notes and Records of the Royal Society*, **35** (1980): 67–103, on 97–8.

(2) Compare Number 311; and see Numbers 312, 313 and 314.

(3) Edward Strathearn Gordon and Gathorne Hardy (*Whitaker's Almanac*).

(4) John Campbell Shairp, Professor of Humanity, appointed Principal of the United Colleges at St Andrews in November 1868.

(5) Oswald Bell, Professor of Anatomy and Medicine.

(6) The other seven professors were: Lewis Campbell (Greek), W. L. F. Fischer (Mathematics), Thomas Spencer Baynes (Logic), Robert Flint (Moral Philosophy), William Swan (Natural Philosophy), W. M^c^Donald (Civil History), and M. Foster (Chemistry) (*Whitaker's Almanac*).

(7) General Edward Sabine, President of the Royal Society 1861–71. This letter is not extant.

(8) Number 314.

(9) This letter is not extant.

(10) Number 313.

LETTER TO WILLIAM THOMSON

7 DECEMBER 1868

From the original in the University Library, Glasgow(1)

Glenlair
Dalbeattie
1868 Dec 7

Dear Thomson

If you ever see White would you ask him why he neither sends me a 'wheel of life'(2) nor answers my two letters asking him why he does not, during 7 weeks.

Can you give me a good *elementary* Thermodynamics *Problem*. I have not the opportunity of setting book work so I am trying to get in a problem which shall be thermodynamically easy. If it is mathematically difficult it will be no fault in the eyes of the Cambridge men, though I would prefer it easy myself.

I think of asking a question about the deduction of the dilatation per degree of temperature at constant pressure from the intrinsic energy expressed as a function of two variables either v & t or v and ϕ.(3)

I think it is important to insert the wedge by the thin end and to 'hold the eel of science by the tail'. Great mental inertia will be called into play if the new ideas are not fitted on to the old in a continuous manner.

If you could give me a hint of a problem I would thank you.(4)

Yours truly
J. CLERK MAXWELL

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(1) Glasgow University Library, Kelvin Papers, M 31.

(2) See Numbers 306, esp. note (7), 307 and 310.

(3) Maxwell set a question on the compression of gases for the Cambridge Mathematical Tripos on the morning of Thursday, 21 January 1869, question (11). See *The Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 499.

(4) Thomson's reply is dated 14 December 1868: 'Dear Maxwell / x(α) Experiment shows that the specific inductive capacity (electrostatic) of glass is increased by elevation and diminished by depression of temperature. Hence prove that if the charge of a charged Leyden phial be slowly increased, the glass becomes thereby cooled. / (β) Given a non-charged Leyden jar of capacity c at temperature t of absolute thermodynamic scale. Prove that when it is partially charged with a quantity Q of electricity the quantity of heat which must be given to its glass to prevent lowering of temperature is / $\frac{1}{2}\frac{Q}{c^2}.\frac{1}{c}\frac{dc}{dt}.\frac{t}{J}$ / J denoting the dynam[l] equivalent of the thermal unit. / (γ) Experiment gives approximately (not very, but very roughly) for the augmentation of sp. ind. capac., $\frac{1}{100}$ percent of its own amount, for a rise of temp[re] 1° Cent. Find how much a Leyden jar weighing 200 grammes, of mean spec. heat .2 (glass and metal all included) is cooled from initial temp[re] of 45° Cent, when it is slowly charged to potential 50 (acc[g]

gramme-centimetre-second fund[1] units). The value of J in terms of corresponding units is 416000. / N.B. (α) is a *very* elementary problem (γ) a problem less elementary (β) is perhaps too transparently book work (Tait §209). / Why not also give them a liquid film? & if you don't want the 2$^{\text{nd}}$ Thermodyn$^{\text{c}}$ law give them the ? to how thin before work is done = latent heat of evaporation. Will you not look in here on your way S & see my electrostatic thermo-multiplier 500 divisions to the 1° Cent. Yours WT'. (ULC Add. MSS 7655, II/32). In discussing example (β) Thomson refers to Tait's *Sketch of Thermodynamics* (Edinburgh, 1868): 115–16 (§209). Maxwell did not use any of Thomson's examples as examination questions.

MANUSCRIPT ON THE TOPOLOGY OF SURFACES

29 DECEMBER 1868

From the original in the University Library, Cambridge(1)

[ON THE GEOMETRY OF SURFACES](2)

Infinite space is divided into c cells or separate regions each of simple continuity.(3) The surface which separates two contiguous cells is called a face and each face is defined by the portions of the cells which are in contact. The boundary between two contiguous faces is called an edge. At every edge three or more cells meet. The extremities of an edge are called summits.(4) At every summit four or more edges faces and cells meet. Let c be the number of cells, f the number of faces e the number of edges and s the number of summits, then let one of the faces be destroyed so as to throw two cells into one.(5)

Let us suppose this face to have n edges and n angles and let us in the first place suppose that all these edges are formed by the meeting of three cells only. Then the destruction of this face has diminished the number of cells by one. It has also obliterated the n edges of the face and as each of these was a boundary of two faces the number of faces is thereby reduced by n, and if we include the destroyed face by $n+1$. We have also n summits obliterated,

(1) ULC Add. MSS 7655, V, d/12.

(2) See Number 318; and compare Maxwell's early manuscript drafts on topology: Numbers 304, 305, 306 and 308. This draft relates to a question Maxwell set for the Cambridge Mathematical Tripos, on the morning of Thursday, 21 January 1869, question (4). See *The Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 497–8; 'Infinite space is divided into a number of regions, one of which encloses all the rest: every surface of contact between two distinct regions is called a face, every line in which three or more faces meet is called an edge, and every point at which four or more edges meet is called a summit; shew that if all the regions and faces have continuous boundaries and have no multiple connexions, the sum of the numbers of regions and edges will be equal to the sum of the numbers of faces and summits.' The draft also has relation to his paper on 'Topographical geometry', read to the London Mathematical Society on 10 March 1870 (see the *Proceedings*, **3** (1870): 82). The paper was presented to the Liverpool Meeting of the British Association in September 1870 (Number 345), and published as 'On hills and dales', *Phil. Mag.*, ser. 4, **40** (1870): 421–7 (= *Scientific Papers*, **2**: 233–40).

(3) Compare Maxwell's term 'simply connected', derived from Riemann: see Number 304, esp. note (4). Maxwell terms loops or closed paths 'cycles': see Number 318 note (4).

(4) A term used subsequently in 'On hills and dales': 423 (= *Scientific Papers*, **2**: 235).

(5) The terms in which Maxwell discusses the problem suggests his reading of a paper by Arthur Cayley, 'On the partitions of a close', *Phil. Mag.*, ser. 4, **21** (1861): 424–8.

and as each of these was a division between two edges which are now thrown into one, there are n edges.

Hence c has become $c-1$ f has become $f-n-1$ e has become $e-2n$ and s, $s-n$ but the quantity

$$c-f+e-s$$

has not been altered by the destruction of the face.

If m contiguous edges of the face belong each to more than three cells the destruction of the face will not obliterate them, or throw the faces which they separate into one.

Let us now consider infinite space as divided into R regions of which r_1 have one continuous boundary r_2 have two boundaries... and r_m have m boundaries. Also let ρ_1 of these have one cycle ρ_2 two cycles... and ρ_n n cycles.

Let the boundaries of these regions be made up of F faces of which f_0 are complete faces f_1 have one boundary f_2 two... and f_m m boundaries also let ϕ_1 be monocyclic ϕ_2 dicyclic... ϕ_n n-cyclic.

Let the boundaries of these faces consist of E edges of which η are closed curves without any divisions.

Let these edges be terminated by S points called summits.

Then if

$$\sum(mr_m) = r_1+2r_2+\ldots+mr_m$$
$$\sum(n\rho_n) = \rho_1+2\rho_2+\ldots+n\rho_n$$
$$\sum(mf_m) = f_1+2f_2+\ldots+mf_m$$
$$\sum(n\phi_n) = \phi_1+2\phi_2+\ldots+n\phi_n$$

then $\sum(mr_m)-\sum(n\rho_n)-2F+\sum(mf_m)+2\sum(n\phi_n)+E-\eta-S = C = 0.$

To prove this we shall first show that this quantity remains unaltered when any part of the system is destroyed or any new part added.

Let a single closed line be drawn in space. This increases E by one and also increases η by one so that C remains the same.

Let this line be divided into s parts by s points of division. We have then $E = s$ and $S = s$ and C remains unaltered.

(1) C is not altered by dividing a line into segments by points. For if the line is a complete closed one without any divisions the first division adds one to S and takes one from η and every division of a terminated line adds one to S and one to E. Hence C is not altered by adding or removing points of division.

(2) C is not altered by drawing a line upon a face. For if it is drawn between two points on the same boundary of the face it divides the face into two so that F and E are each increased by one.

If it is drawn between two points on different boundaries, it reduces the number of boundaries by one so that $\sum (mf_m)$ is diminished by one and E is increased by one and C remains the same.

If it is a closed curve not surrounding any cyclic channel then it divides the face into two and adds one to the number of boundaries of the part outside so that F and $\sum (mf_m)$ are each increased by one and so are E and η so that C still remains the same.

If a closed curve is drawn round one of the cyclic channels it adds two boundaries (the two sides of the curve) and diminishes the number of cycles, so that $\sum mf_m$ is increased by two, $2\sum (n\phi_n)$ is diminished by two and E and η are each increased by one so that C remains the same.

Hence C is not altered by drawing any lines on any faces or removing them.

(3) C is not altered by drawing a new face.

For let the new face be bounded entirely by the single boundary of a region then if it does not close a cyclic channel it divides the region into two. If the region had originally m boundaries, the sum of the boundaries of the two parts will be $m+1$ so that $\sum mr_m$ is increased by one. If the region had originally n cycles the sum of the cycles of the two parts will still be n so that $\sum (n\rho_n)$ remains the same.

At the same time $2F$ is increased by two and $\sum mf_m$ by one so that C remains the same.

Now let the new face close a cyclic channel. Then $\sum mr_m$ remains the same but $\sum n\rho_n$ is diminished by one $2F$ is increased by two and $\sum mf_m$ by one so that C is the same.

We have supposed that the new face is bounded only by the external boundary of the region. Let us examine what would be the result in either of the above cases if the new face had been also bounded by an internal boundary of the region. In this case $\sum (mr_m)$ would have been diminished by one and $\sum (mf_m)$ would have been increased by one as compared with the former cases so that C would still be the same.

Lastly let the new face be entirely disconnected from the boundaries of the region and let it be n-cyclic.

The region outside acquires one new boundary and n new cycles and the new region has one boundary and n cycles so that $\sum mr_m$ is increased by 2 and $\sum n\rho_n$ by $2n$.

At the same time $2F$ is increased by 2 $\sum (mf_m)$ remains the same and $2\sum (n\phi_n)$ is increased by $2n$ so that C remains the same.

Hence C remains the same whatever points lines or surfaces are drawn in or taken away from the system.

Now in the case of a single n cyclic surface we have two regions each with one boundary and n cycles so that $\sum mr_m = 2$ $\sum n\rho_n = 2n$ $2F = 2$, $\sum mf_m = 0$ $2\sum(n\phi_n) = 2n$ $E = 0$ $\eta = 0$ $S = 0$ so that in this case $C = 0$.

Hence C is equal to 0 in every case.

If all the regions have single boundaries $\sum(mr_m) = R$.

If they are all acyclic $\sum(n\rho_n) = 0$.

If all the faces are singly bounded and acyclic

$$2F - \sum mf_m - 2\sum(n\phi_n) = F.$$

In this case none of the edges can be closed lines so that $\eta = 0$.

Hence if all these conditions are fulfilled, the general equation becomes

$$R - F + E - S = 0.$$

If there is only one finite region bounded by an n-cyclic surface having F singly bounded faces E edges and S points

$$2 - 2n - F + E - S = 0.$$

When $n = 0$ as in acyclic figures we get the well known equation[(6)]

$$2 - F + E - S = 0$$

between the faces, edges and summits of a polyhedron.

Dec 29 1868

(6) Euler's equation: see Number 318 note (3).

COMMENTS ON J. B. LISTING'S PAPER 'DER CENSUS RÄUMLICHER COMPLEXE'[1]

11 FEBRUARY 1869

From the *Proceedings of the London Mathematical Society*[2]

[LISTING'S 'SURVEY OF SPATIAL COMPLEXES']

Mr. Clerk-Maxwell next drew attention to J. B. Listing's paper in the 10th vol. of the Göttingen Transactions, on the kinds of Cyclosis in lines, surfaces, and regions of space.[3] If p points are joined into a system by l lines, then since $p-1$ lines are sufficient for this purpose, the remaining $K = l-p+1$ lines give K independent closed paths.[4] Any other closed path must be compounded of these. If we call s the distance travelled by a point along any path, and[5]

$$L = \int\left(X\frac{dx}{ds}+Y\frac{dy}{ds}+Z\frac{dz}{ds}\right)ds,$$

the line-integral of the quantity, whose components are X, Y, Z, along the path, then if the line-integrals round each of the k cycles are $k_1 \dots k_k$, the value of L from any one point to any other is

$$L = L_0+n_1 k_1+n_2 k_2+\dots+n_k k_k,$$

where $n_1, n_2 \dots n_k$ are integral numbers.

As an instance, if

$$X = \frac{dw}{dx}, \quad Y = \frac{dw}{dy}, \quad Z = \frac{dw}{dz},$$

where w is the solid angle subtended at the point xyz by a closed curve, then if one of the cycles of the curve along which the line-integral is taken is enlinked with this closed curve, the corresponding value of k is 4π.

This will be the case if $L = \iint \frac{u}{r^3}\,ds\,ds'$,

(1) Remarks made at a meeting of the London Mathematical Society, 11 February 1869.

(2) *Proceedings of the London Mathematical Society*, **2** (1869): 165–6.

(3) Johann Benedict Listing, 'Der Census räumlicher Complexe oder Verallgemeinerung des Euler'schen Satzes von den Polyëdern', *Abhandlungen der Math. Classe der Königlichen Gesellschaft der Wissenschaften zu Göttingen*, **10** (1861): 97–182.

(4) Following Listing, Maxwell terms 'a loop or closed path ... a Cycle' (*Treatise*, **1**: 16 (§18)). The existence of cycles is termed 'Cyklose' by Listing, 'Der Census räumlicher Complexe': 111, 181, which Maxwell translates as 'Cyclosis'.

(5) See Number 305 esp. note (12).

where $$\frac{u^2}{r^2} = \left[1-\left(\frac{dr}{ds}\right)^2\right]\left[1-\left(\frac{dr}{ds'}\right)^2\right]-\left(r\frac{d^2r}{ds\,ds'}\right)^2,$$

r being the distance from a point on the closed curve s to a point on the closed curve s', and the integral is taken round both curves. This integral is always $4\pi n$, and is a criterion of the curves being linked together or not, depending only on the relations of r, s and s'.[6]

(6) See Number 276 esp. note (8).

REPORT ON A PAPER BY HENRY MOSELEY[1] ON THE MOTION OF GLACIERS

2 MARCH 1869

From the original in the Library of the Royal Society, London[2]

REPORT ON CANON MOSELEY'S PAPER ON THE MECHANICAL POSSIBILITY OF THE DESCENT OF GLACIERS BY THEIR WEIGHT ONLY[3]

A considerable part of this paper including the general reasoning and the experimental investigation has been printed in the Proceedings.[4] The remainder consists chiefly of a mathematical determination of the work of shearing performed in the interior of a glacier and the work of friction at its surface as compared with the work performed by gravity during the same time.[5] This investigation if correct leads to the conclusion that the amount of work consumed in shearing the ice within the glacier and in rubbing its surface over the channel is greater, even in the most favourable case, than the work done by gravity. But this work is done, therefore there must be something in action besides gravity. The author does not in this paper state what this is, but he has given his views in former papers.[6]

We have therefore principally to consider the calculation of the work of shearing, and this both physically and mathematically. The author has determined experimentally the strength of ice to resist shearing force, and has

(1) St John's 1821, Professor of Natural Philosophy at King's College, London 1831–44, FRS 1839, Canon of Bristol 1853 (Venn, *DNB*).

(2) Royal Society, *Referees' Reports*, **6**: 191.

(3) Henry Moseley, 'On the mechanical possibility of the descent of glaciers, by their weight only' (Royal Society, AP. 51.8). The paper was received by the Royal Society on 21 October 1868, and read on 7 January 1869; see the abstract in *Proc. Roy. Soc.*, **17** (1869): 202–8.

(4) See note (3).

(5) Moseley subsequently published a corrected version of the mathematical portion (ff. 17–27) of his paper in the May 1869 number of the *Philosophical Magazine*; see Henry Moseley, 'On the mechanical impossibility of the descent of glaciers by their weight only', *Phil. Mag.*, ser. 4, **37** (1869): 363–70.

(6) Moseley had maintained that the descent of glaciers was the result of the successive elongation and contraction of sheets of ice, caused by changes of temperature. See Henry Moseley, 'On the descent of glaciers', *Proc. Roy. Soc.*, **7** (1855): 333–42; Moseley, 'On the motion of a plate of metal on an inclined plane, when dilated and contracted; and on the descent of glaciers', *ibid.*, **11** (1861): 168–77, esp. 177, where he contested Forbes' theory of glacier viscosity (see notes (11) and (12)) as explaining the descent of glaciers.

found that it requires about 75 lb weight per square inch to shear across a cylinder of ice made by stuffing a mould with ice and hammering it in. The radius of the cylinder was measured to the hundred thousandth part of an inch.

The author calls this coefficient of strength to resist shearing the *unit* of shear, an expression which I do not approve. Nothing is said about the time occupied in shearing the cylinder but I suppose it was cut in two as the requisite weights were applied.

Now if in any glacier the weight of the ice and the slope were great enough to shear the ice at any point right through then the glacier would descend,(7) and if the slope were a little greater the glacier would slide away with an accelerated velocity till the additional resistance due to concussion on the rocks reduced its speed. That is to say, it would be an avalanche. Glaciers which are not avalanches move very slowly so that the kind of resistance to deformation by which their motion is regulated must be one which depends on the *rate* of deformation. The simplest hypothesis which we can make is that the shearing force is proportional to the rate of deformation.(8) This is the hypothesis about the friction of fluids on which Stokes made his investigation of their action on pendulums.(9) It is probably true for all gases for water, oil, treacle, pitch, asphalt.

On the other hand, hard sharp sand has a resistance to shearing force which is independent of time, and seems to be proportional to the normal pressure as in friction of solid surfaces.

Soft clay, wet sand, mortar, and sanded asphalt have a combination of these two properties.

Another class of bodies such as broken ice and water mixed together, loaf

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(7) From his experiments Moseley concluded that the 'unit of shear of ice...is therefore 75 lbs.' On the basis of the rate of descent of a glacier observed by John Tyndall in his paper 'On the physical phenomena of glaciers. Part I. Observations on the Mer-de-Glace', *Phil. Trans.*, **149** (1859): 261–78, Moseley's mathematical calculation gave the result that 'the unit of shearing force of the ice could not have been more than 1.3193 lb.' See Moseley, 'On the mechanical possibility of the descent of glaciers, by their weight only', *Proc. Roy. Soc.*, **17** (1869): 207.

(8) In his report on Moseley's paper (Royal Society, *Referees' Reports*, **6**: 192), dated 24 February 1869, W. J. M. Rankine noted: 'It may be remarked that the coefficient of resistance to shearing, or "Unit of Shear", as it is termed by the Author, is treated as being independent of the velocity with which the shearing motion goes on – in other words, it is assumed to be as great for an insensibly small velocity of shearing as for the considerable velocity with which the shearing must have taken place in the experiments from which the coefficient was deduced.'

(9) George Gabriel Stokes, 'On the effect of the internal friction of fluids on the motion of pendulums', *Trans. Camb. Phil. Soc.*, **9**, part 2 (1851): [8]–[106] (= *Papers*, **3**: 1–136). See Volume I: 597–8.

sugar and syrup, have a kind of plasticity depending on time but arising from different causes.

Bodies of the first class are called Viscous.[10] Some of them have the property of being sticky, but this is not implied in the use of this word by Forbes[11] and others.[12]

The word viscous is also applied to other bodies in which long continued force produces effects of a different kind from the same force acting for a short time.

Some bodies appear capable of resisting a small force of deformation for an indefinite time, though a greater force produces deformation at a rate depending on the force. Lead is probably of this kind and ice does not appear to alter its form under the action of small forces.

In the investigation in the paper, no notice is taken of the possible effect of time in allowing deformations to take place which would be prevented from happening at once by the strength of the ice.

The hypothesis on which the work of shearing seems to be calculated is that the surfaces, after being shorn asunder immediately freeze together so that the work done in sliding one on the other is equal to the shearing force multiplied by the distance of sliding. I should think that this hypothesis would apply well to the case of sharp sand or detritus, which we know lies permanently on a slope much steeper than that of a glacier, but when it comes to a steeper place it rushes down as a landslip, with a velocity like that of a falling body.

There seems to be a somewhat unnecessary complication in the calculation of the work of shearing on this principle, and I am doubtful whether it would be desirable to print it at full as a mathematical investigation, especially as the result, as stated in the part already printed follows readily from the statement of the hypothesis.

There is a note at p. 27 in which the viscosity of a fluid is treated but there

(10) See Number 252.

(11) J. D. Forbes, *Travels in the Alps of Savoy and of other parts of the Pennine Chain; with Observations on the Phenomena of Glaciers* (Edinburgh, 1843): 365; and Forbes, 'Illustrations of the viscous theory of glacier motion', *Phil. Trans.*, **136** (1846): 143–55, 157–75, 177–210, esp. 143 where he defines a glacier as a 'semifluid or viscous mass in motion'.

(12) Compare Thomson and Tait, *Natural Philosophy*: 591n; 'Forbes' theory is merely the proof by observation that glaciers have the property that mud (heterogeneous), mortar (heterogeneous), pitch (homogeneous), water (homogeneous), all have of changing shape indefinitely and continuously under the action of continued stress.' John Tyndall, 'On the physical phenomena of glaciers': 272–8, and in his *The Glaciers of the Alps* (London, 1860): 311–14, 325–7, had contested Forbes' theory of glacier viscosity, denying that ice possesses 'the "gluey tenacity" which the term viscous suggests' (on 327). See also Number 477.

is no statement of what is meant by $\mu^{(13)}$ either in the interior or at the surface so that the value of the investigation is not easily understood.

On the whole I think that the reprinting of the whole paper with the omitted parts would but slightly increase the value of what we have in the Proceedings.(14)

James Clerk Maxwell
2nd March 1869
7 Kildare Terrace W.

(13) Moseley's 'unit of shear'. In the note on f. 7 of his manuscript (see note (3)) Moseley seems to be considering variations in μ in the interior of a glacier.

(14) Recording his 'dissent from [Moseley's]...conclusion', Rankine recommended publication in the *Philosophical Transactions*: 'Supposing that it should eventually be proved, that the shearing resistance of ice is a function of the velocity of shearing increasing towards a limit – viz: the coefficient found by the Author's experiments, – and that gravity alone is sufficient to account for the descent of glaciers, it will still be desirable that the details of Canon Moseley's investigation should be on record.' (see note (8)).

LETTER TO ARTHUR CAYLEY[1]

12 APRIL 1869

From the original in the Library of Trinity College, Cambridge[2]

86 Hereford Road
Westbourne Grove W
12 April 1869

Dear Sir

I have only just returned from Paris and found your paper of suggestions for surfaces lying here.[3] I send it on to Sylvester.[4]

Surface E, a tubular surface with a parabolic axis,[5] would I think be easily constructed by means of its circular sections which are lines of curvature, and the other lines of curvature pass through corresponding points of the circles.

Draw the parabola and a series of normals at proper intervals on a plane cut off the normals at a given length on each side. Draw the equidistant curves and the evolute of the parabola.

Make a number of semicircles of the proper size and place them on the normals at right angles to the plane.

When the normals intersect, the planes of these circles intersect, so that in the 'interesting' part of the figure there would be a great deal of honeycombing.

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(1) Trinity 1838, Sadlerian Professor of Pure Mathematics at Cambridge 1863 (Venn).

(2) Trinity Portraits, Vol. C, page 50 *verso*.

(3) In his paper 'On the cyclide', *Quarterly Journal of Pure and Applied Mathematics*, **11** (1867): 111–26 (= *Scientific Papers*, **2**: 144–59), Maxwell drew stereoscopic diagrams of four varieties of the cyclide using his real image stereoscope (see Number 274). In a letter of 20 April 1868 (ULC Add. MSS 7655, II/29) Cayley had written to acknowledge receipt of some stereoscopic drawings. 'Dear Sir / I was very much obliged for the stereoscopic drawing of Steiner's surface – besides the ellipses, – & (I suppose less easily drawable) the intersections by the concentric spheres – I do not know if there are any other easily traceable curves on the surface. The axes are of course nodal lines – with two real sheets within the surface – and without any real sheets outside it – and I think it would bring out the stereoscopic figure if the axes were drawn upon it, and that rather more strongly than the other lines of the figure. I remain dear Sir / Yours sincerely / A. Cayley.' Maxwell very probably sent Cayley the figures in connection with his paper 'On the construction of stereograms of surfaces', *Proceedings of the London Mathematical Society*, **2** (1868): 57–8 (= *Scientific Papers*, **2**: 101), read to the London Mathematical Society on 23 January 1868, when stereograms of surfaces were exhibited; see Number 279 and Plate V.

(4) The paper mentioned by Maxwell was probably Cayley's 'On the quartic surfaces $(* \between U, V, W)^2 = 0$', *Quarterly Journal of Pure and Applied Mathematics*, **11** (1870): 15–25. J. J. Sylvester was editor of the *Journal*.

(5) In his paper 'On the quartic surfaces': 21 Cayley discussed the case of a parallel surface of a paraboloid.

This part should be done separately by parallel sections the circular sections being drawn afterwards.

At the Conservatoire des Arts et Métiers there is a set of ruled surfaces capable of transformation, each line being a silk cord stretched by a separate weight. They work very well when they are quite dry and when not more than two strings pass through the same hole. The intersections of two such surfaces are indicated by beads through which two lines pass. There is also a cubic scroll by Michel Chasles[(6)] with cones & conics belonging to it. M. Tresca[(7)] has given me a plaster cast of Fresnels wave surface,[(8)] both the space between the sheets and the internal nucleus.

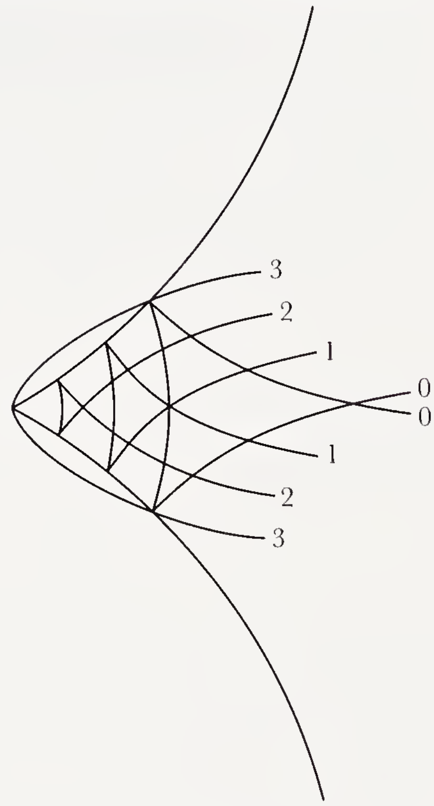

Figure 320,1

It would not be difficult to construct the symmetrical form of Steiner's surface[(9)] which you set a problem on.[(10)] Make a set of ellipses with constant major axis = a

(6) See Number 267 note (4).

(7) Henri Edouard Tresca, professor of mechanics at the Conservatoire des Arts et Métiers.

(8) The surface whose radii determine the speeds of wave fronts; see Augustin Fresnel, 'Mémoire sur la double réfraction', *Mémoires de l'Académie Royale des Sciences*, **7** (1827): 45–176, esp. 126–49. Cayley had published on the subject; see his papers 'On the wave surface', *Quarterly Journal of Pure and Applied Mathematics*, **3** (1860): 16–22; and 'Note on the wave surface', *ibid.*, **3** (1860): 142–4. See Number 274 and Plate VII.

(9) 'Steiner's surface' (suggested by Jacob Steiner), a fourth order surface such that on the surface there lie three double straight lines that intersect in a triple point, that has the property that each of its tangent planes cuts it in a pair of conics, had been made public by Karl Weierstrass in the *Monatsberichte der Akademie der Wissenschaften zu Berlin* (1863): 337–8. Cayley had published a paper on the surface: 'Note sur la surface du quatrième ordre de Steiner', *Journal für die reine und angewandte Mathematik*, **64** (1865): 172–4. Maxwell made a jotting 'Schröter on Steiner Surface' in a notebook (King's College, London Archives, Maxwell notebook (3)); see H. Schröter, 'Über die Steiner'sche Fläche vierten Grades', *Monatsberichte ... Berlin* (1863): 520–38. See Plate V.

(10) In his Smith's Prize paper of 3 February 1869 Cayley had set the following problem: 'Two tangents on a conic are harmonically related to a second conic: find the locus of the intersection of the two tangents. In plane geometry the angle in a semicircle is, in spherical geometry it is not, a right angle: show how these conditions follow from the solution of the above problem.'; see the *Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 511, and Cayley's 'A "Smith Prize" paper; solutions', *Oxford, Cambridge and Dublin Messenger of Mathematics*, **5** (1869): 40–64.

and minor axis $a \sin 2\theta$. Place them with their minor axes coinciding with a fixed line and one extremity of the axis with a fixed point and with the plane of the ellipse inclined θ to a fixed plane. Then the ellipses are lines on Steiners surface.

After this week, address Glenlair
Dalbeattie
NB.

Yours truly
J. CLERK MAXWELL

LETTER TO WILLIAM THOMSON

12 MAY 1869

From the original in the University Library, Cambridge(1)

A. Cornu has set up a divided ring electrometer wh. he keeps charged with a dry pile.(2)

Glenlair
Dalbeattie
May 12 1869

Dear Thomson

I am finishing what I have to say on Statical Electricity & I should be glad to hear from you on some points.

1 I think I am right in saying that Poisson,(3) Plana(4) &c though they determined the distribution on a sphere due to an electrified point did not discover that the potential due to this distribution on the outside is that due to the image of the point and that this simplification of the theory, together with the whole theory of electrical inversion belongs to you.(5)

2nd The other problems solved by a general method (analogous to the harmonics) are

α Ellipsoids of revolution (planetary & ovary)(6) by J. Neumann, Crelle 37(7)

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(1) ULC Add. MSS 7342, M 103. Previously published in Larmor, 'Origins': 736–8.

(2) Alfred Cornu was a professor at the École Polytechnique, Paris, whom Maxwell had presumably met on his visit to Paris: see his letter to Cayley of 12 April 1869 (Number 320). On Cornu's interest in electrometers see his paper 'Sur les mesures électrostatiques', *Journal de Physique Théorique et Appliquée*, **1** (1872): 7–17, 87–98, 241–6.

(3) Siméon Denis Poisson, 'Mémoire sur la distribution de l'électricité à la surface des corps conducteurs', *Mémoires de la Classe des Sciences Mathématiques et Physiques de l'Institut de France* (année 1811): 1–92, 163–274.

(4) Jean Plana, 'Mémoire sur la distribution de l'électricité à la surface de deux sphères conductrices complètement isolées', *Memorie della Reale Accademia delle Scienze di Torino*, ser. 2, **7** (1845): 71–401.

(5) Compare the *Treatise*, **1**: 191–5 (§155). On Thomson's method of electric images see Numbers 301, esp. note (10), and 310.

(6) Maxwell defines these terms in the *Treatise*, **1**: 187–8 (§§151–2), as ellipsoids which are figures of revolution about their conjugate and transverse axes, respectively. The terms 'ellipsoides planétaires' and 'ellipsoides ovales' are used by Gabriel Lamé, *Leçons sur les Fonctions Inverses Transcendantes et les Surfaces Isothermes* (Paris, 1857): 19, to which Maxwell refers below and in the *Treatise*, **1**: 181n (§147).

(7) Read: F. E. Neumann, 'Entwickelung der in elliptischen Coordinaten ausgedrückten

β Indefinite right cylinder Kirchhoff, Crelle 48.(8)

Has Liouville or any one else done it for the ellipsoid of 3 axes, except in the case of the charged ellipsoid in infinite field and the uncharged ellipsoid in field of uniform force.

In all the cases that have been done the position of a point in space is determined by three functions $\rho\ \mu\ \nu$. The equation of the conducting surface is

$$\rho = a.$$

For all internal points ρ is between 0 and a. For all external points ρ is between a and b which may be ∞.

The values of the potential are of the form

$$R\ M\ N$$

where R, M, N are functions of $\rho\ \mu\ \nu$ respectively.

For each value of M & N there are two values of R which fulfil Laplaces eq$^{\text{n}}$(9) one which vanishes at ∞ and another which does not become infinite within the surface.

M & N must be of the nature of periodic functions. See Lamé on Inverse Functions.(10)

The case of 2 dimensions has been pretty well solved by C. Neumann, Crelle 1861.(11)

Have you published anything about the theory of electrical images in the figure formed by four spheres which cut at right angles?(12)

For electrification to potential unity, if the radii are $r_1\ r_2\ r_3\ r_4$ the images are
At centre of 1$^{\text{st}}$ sphere, charge $= r_1$ (four points).

---

reciproken Entfernung zweier Punkte in Reihen, welche nach den Laplace'schen $Y^{(n)}$ fortschreiten; und Anwendung dieser Reihen zur Bestimmung des magnetischen Zustandes eines Rotations-Ellipsoids, welcher durch vertheilende Kräfte erregt ist', *Journal für die reine und angewandte Mathematik*, **37** (1848): 21–50.

(8) Gustav Kirchhoff, 'Ueber den inducirten Magnetismus eines unbegrenzten Cylinders von weichem Eisen', *Journal für die reine und angewandte Mathematik*, **48** (1854): 348–76.

(9) For the potential function V, $\nabla^2 V = 0$.

(10) Lamé, *Leçons sur les Fonctions Inverses Transcendantes*: 36–44. Compare the *Treatise*, **1**: 181–4 (§§147–9).

(11) Carl Neumann, 'Ueber die Integration der partiellen Differential-gleichung: $\frac{\partial^2\phi}{\partial x^2}+\frac{\partial^2\phi}{\partial y^2} = 0$', *Journal für die reine und angewandte Mathematik*, **59** (1861): 335–66. Compare the *Treatise*, **1**: 234–5 (§190).

(12) Compare the *Treatise*, **1**: 211 (§170), where Maxwell outlines his treatment of this case.

At foot of perp from 3rd centre on the line between 1 & 2

$$\text{charge} = -\frac{1}{\sqrt{\dfrac{1}{r_1^2}+\dfrac{1}{r_2^2}}} \text{ (six points of this kind).}$$

At foot of perp from 4th centre on plane of 1 2 & 3

$$\text{charge} = +\frac{1}{\sqrt{\dfrac{1}{r_1^2}+\dfrac{1}{r_2^2}+\dfrac{1}{r_3^2}}} \text{ (four such points).}$$

At intersection of these four perpendiculars

$$\text{charge} = -\frac{1}{\sqrt{\dfrac{1}{r_1^2}+\dfrac{1}{r_2^2}+\dfrac{1}{r_3^2}+\dfrac{1}{r_4^2}}} \text{ (one point).}$$

The surface for which the potential due to these 15 points is unity consists of the 4 spheres. Of course the case of this figure influenced by a point comes out by inversion.[13]

Three spheres give the same expressions[14] and so do two.

If there are two spheres, centres A, B, radii α and β and a point O distant a from A & b from B the density at a point P on the sphere A induced by unit of electricity at O is

$$\sigma = \frac{1}{4\pi}\frac{a^2-\alpha^2}{\alpha r^3}\left(1-\frac{\beta^3 r^3}{(\beta^2 r^2+(b^2-\beta^2)\,(p^2-\beta^2))^{\frac{3}{2}}}\right)$$

radii of spheres α & β distance $AB = \sqrt{\alpha^2+\beta^2}$

$$OA = a \quad OB = b \quad OP = r \quad BP = p.^{(15)}$$

I have been doing the charge on a proof plane in the form of a disk of radius a and thickness b.[16] I find that the charge is greater than that of the circle it

(13) On the method of 'electrical inversion', obtained from the geometrical method of inversion, see the *Treatise*, **1**: 199–203 (§§162–4). On the geometrical method of inversion see also Volume I: 484–5; and George Salmon, *A Treatise on the Higher Plane Curves* (Dublin, 1852): 306–7.

(14) See the *Treatise*, **1**: 210–11 (§169) for an analogous treatment of this case.

(15) See the *Treatise*, **1**: 207–9 (§168) (and Fig. IV appended to the volume) for the same result.

(16) On the theory of Coulomb's proof plane (a disc attached to the surface of a conductor) see the *Treatise*, **1**: 277–81 (§§223–5).

covers in the proportion of

$$1+8\frac{b}{a}\log\frac{8\pi a}{b} \text{ to } 1.^{(17)}$$

The thickness is supposed small compared with the radius and the radius small compared with the radius of curvature of the electrified surface to which the disk is applied.

I am now describing your absolute electrometer[(18)] & the quadrant d°.[(19)] To what class do you refer Coulombs Torsion Balance?[(20)]

Where are you to be found this summer. I may be at Ardhallow, Dunoon, between May 17 and May 31, and after that at Glenlair. How is the reprint of your electrical papers getting on?[(21)]

Yours truly
J. Clerk Maxwell

(17) The value derived in the *Treatise*, **1**: 281 (§225).

(18) Thomson's guard-ring electrometer: see Number 289 esp. note (11).

(19) Thomson quadrant electrometer was described in his 'Report on electrometers and electrostatic instruments', *Report of the Thirty-seventh Meeting of the British Association* (London, 1868): 489–512, esp. 490–7 and Plate V (= *Electrostatics and Magnetism*: 262–81, with considerable augmentations). The instrument is described in the *Treatise*, **1**: 271–4 (§219).

(20) In his 'Report on electrometers and electrostatic instruments': 490, Thomson considered electrometers as falling under three classes: 'repulsion', 'symmetrical' (an example being the quadrant electrometer) and 'attracted disc' (an example being the guard-ring electrometer). Thomson made no mention of Coulomb's torsion balance, which Maxwell describes in the *Treatise*, **1**: 263–6 (§215). Coulomb described his electrometer in his paper 'Sur l'électricité et le magnétisme', *Mémoires de l'Académie Royale des Sciences* (année 1785): 569–77, 578–611, 616–38.

(21) The reprint of Thomson's papers on *Electrostatics and Magnetism* was published in 1872.

LETTER TO WILLIAM THOMSON

5 JUNE 1869

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
June 5 1869

Dear Thomson

I am anxious to know whether your views about contact electricity[2] are those which I attribute to you.

In the first place what is the potential of a metallic conductor. Is this it?[3] Make a hollow place in the conductor and place the end of the electrode of an electrometer in the hollow space, and carry off a great number of small particles from the end of the electrode to an infinite distance, then the potential indicated by the electrometer differs from that of the conductor by a constant quantity.

2nd The electromotive force from one metal to another is $J\Pi$ where Π is the Peltier effect[4] as defined in your RSE paper.[5] Now Π is a function of the temperature and of the 2 metals in contact and is such that at any one temperature for metals a b c

$$\Pi_{bc}+\Pi_{ab}+\Pi_{ca} = 0.$$

Hence $$\Pi_{ab} = P_a-P_b \quad \Pi_{bc} = P_b-P_c \quad \Pi_{ca} = P_c-P_a$$

where P_a P_b P_c are functions of the temperature for each metal.[6]

Also in any one metal owing to difference of temperature the difference of potentials of any two points is equal to the difference of the values of a

(1) ULC Add. MSS 7342, M 104. First published in Larmor, 'Origins': 738–9.

(2) William Thomson, 'On a self-acting apparatus for multiplying and maintaining charges, with applications to illustrate the voltaic theory', *Proc. Roy. Soc.*, **16** (1867): 67–72 (= *Electrostatics and Magnetism*: 319–25). See the *Treatise*, **1**: 299–300 (§§246–8).

(3) See the *Treatise*, **1**: 276 (§222).

(4) J. C. A. Peltier, 'Nouvelles expériences sur la caloricité des courants électriques', *Ann. Chim. Phys.*, ser. 2, **56** (1834): 371–86.

(5) William Thomson, 'On the dynamical theory of heat. Part V. Thermo-electric currents', *Trans. Roy. Soc. Edinb.*, **21** (1854): 123–71, esp. 133 (= *Math. & Phys. Papers*, **1**: 232–91); 'Π_1, Π_2, Π_3 &c denote the amounts (positive or negative) of heat absorbed at [the different junctions]... by a positive current of unit strength during the unit of time'.

(6) See the *Treatise*, **1**: 302–4 (§§250–1).

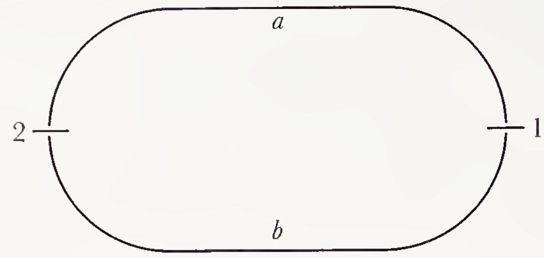

Figure 322,1

function Q of the temperature so that for a circuit of two metals a and b of which the junctions are at temperatures 1 and 2 the total electromotive force is

$$F = P_{a_1} - P_{b_1} + Q_{b_1} - Q_{b_2} + P_{b_2} - P_{a_2} + Q_{a_2} - Q_{a_1}$$
$$= P_{a_1} - Q_{a_1} - (P_{b_1} - Q_{b_1}) - (P_{a_2} - Q_{a_2}) + (P_{b_2} - Q_{b_2}).$$

Hence if we make $P_{a_1} - Q_{a_1} = R_{a_1}$ and so on

$$F = R_{a_1} - R_{a_2} - R_{b_1} + R_{b_2}.$$

If every P was equal to the corresponding Q thermo electric currents would be impossible but the Peltier effect at a junction might still exist so that by thermal experiments we might discover both the Ps and Qs in the way that you discovered the Qs.(7)

Could the Qs be measured by finding the difference of potential of air near hot & cold parts of the same piece of metal?

Here is my theory of the state of a dielectric called electric polarization.(8)

Draw any closed curve and through the points of this curve draw lines of inductive action so forming a tube of inductive action.(9) Draw two equipotential surfaces cutting the tube and enclosing between them a cell.

---

(7) Thomson, 'Thermo-electric currents': 141–5.

(8) On Maxwell's theory of dielectric polarisation, and the 'displacement of the electricity', see J. C. Maxwell, 'On physical lines of force. Part III. The theory of molecular vortices applied to statical electricity', *Phil. Mag.*, ser. 4, **23** (1862): 12–24, esp. 14–19 (= *Scientific Papers*, **1**: 491–6). See Maxwell's letters to Faraday and Thomson of 19 October and 10 December 1861 (Volume I: 683–9, 692–8). Maxwell had reformulated his concept of electric polarisation in Theorems C and D of his 'On a method of making a direct comparison of electrostatic with electromagnetic force; with a note on the electromagnetic theory of light', *Phil. Trans.*, **158** (1868): 643–57, esp. 654 (= *Scientific Papers*, **2**: 139). In the *Treatise* he develops this account of electric polarization: see the *Treatise*, **1**: 57–65, 131–4 (§§59–62, 109–11).

(9) Compare Maxwell's geometrical imagery in his paper 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83 (= *Scientific Papers*, **1**: 155–229). See Volume I: 337–50, 357–61.

The resultant electromotive force in the small cell is $\frac{A-B}{AB}$ in the direction perpendicular to A or B and from A towards B.

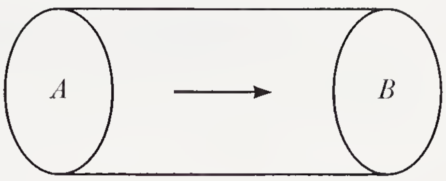

Figure 322,2

The electric displacement is a linear function of the electromotive force(10) and is parallel to the surface of the tube. In fluid dielectrics the displacement is in the direction of the electromotive force and is proportional to it.

The displacement through any section of the tube is the same and is

$$\frac{A-B}{AB} D\,(\text{area}) = Q$$

where D is the specific inductive capacity.(11)

Besides the internal displacement, which consists in a quantity of electricity Q being forced through every section from A towards B we must suppose a superficial distribution of Q on section A and $-Q$ on section B.(12) When the polarization is solenoidal(13) these superficial electrifications destroy one another except at the surface of the dielectric.(14)

The energy of the cell is $\frac{1}{2}FQ$(15) and its mechanical stress is a tension $= \frac{1}{2}\frac{FQ}{\text{volume}}$ on the surfaces A and B and a pressure of equal numerical value on the sides of the tube.(16)

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(10) See the *Treatise*, **1**: 58–60 (§§59–60); Number 231 (equation (12)); and Maxwell's 'On a method of making a direct comparison...': 654–6 (= *Scientific Papers*, **2**: 139–42).

(11) On this Faradayan concept see the *Treatise*, **1**: 50 (§52).

(12) Compare Maxwell's discussion of the Leyden jar in the *Treatise*, **1**: 133 (§111). On Maxwell's theory of charge in the *Treatise* see J. Z. Buchwald, *From Maxwell to Microphysics* (Chicago/London, 1985): 20–40.

(13) See William Thomson's discussion of the 'solenoidal' distribution of magnetism in his paper 'A mathematical theory of magnetism', *Phil. Trans.*, **141** (1851): 243–85, esp. 270, 273 (= *Electrostatics and Magnetism*: 341–404). Thomson considers a magnet divided into an infinite number of 'solenoids' (a term derived from Ampère; see Number 410 note (19)), tubes 'which are either closed or have their ends in the bounding surface...the equation $\frac{d\alpha}{dx}+\frac{d\beta}{dy}+\frac{d\gamma}{dz}=0$ expresses that the distribution of magnetism is solenoidal', where α, β, γ are the components of magnetisation at any internal point x, y, z.

(14) Compare the *Treatise*, **1**: 133 (§111); 'all electrification is the residual effect of the polarization of the dielectric'.

(15) See Number 231; Maxwell, 'On a method of making a direct comparison...': 656 (= *Scientific Papers*, **2**: 142); and the *Treatise*, **1**: 63–4 (§62).

(16) Compare the *Treatise*, **1**: 126–31 (§§107–8).

It follows from this theory that the movements of electricity are like those of an incompressible fluid[17] and that charging a body and putting it into a cubic foot of air does not increase the quantity of electricity in the cubic foot.

Yours truly
J. C. MAXWELL

(17) Compare the *Treatise*, **1**: 64 (§62); 'the motion of electricity is subject to the same condition as that of an incompressible fluid ... it follows from this that every electric current must form a closed circuit'. See his comment on closed circuits in Theorem D of 'On a method of making a direct comparison ...': 654 (= *Scientific Papers*, **2**: 139).

LETTER TO GEORGE GABRIEL STOKES

26 JUNE 1869

From the original in the University Library, Cambridge(1)

Glenlair
Dalbeattie
June 26, 1869

Dear Sir

As you have studied series of periodic functions(2) I should like to know from you whether there is any method of finding the coefficients of such a series

$$S = A_0 + A_1 \cos\theta + A_2 \cos 2\theta + \ldots + A_n \cos n\theta$$
$$+ B_1 \sin\theta + B_2 \sin 2\theta + \ldots + B_n \sin n\theta$$

so that between the limits $\theta = 0$ & $\theta = \alpha$ the series S shall have values given in terms of θ,

and between the limits $\theta = \alpha$ and $\theta = 2\pi$ some other series depending on S such as $\frac{dS}{d\theta}$ shall have values given in terms of θ.

For instance, can we find the values of the coefficients A in the series such that $A_0 + A_1 \cos\theta + A_2 \cos 2\theta + \ldots + A_n \cos n\theta$ shall be constant $= C$ for values of θ of the form $\theta = 2\pi n \pm \phi$ when ϕ is less than α and at the same time $A_1 \cos\theta + 2A_2 \cos 2\theta \ldots + nA_n \cos n\theta$ shall be zero for values of θ not within the above limits?

The question arises from the application of Fouriers theorem to the determination of the potential of an electrified grating consisting of a number of parallel strips all in one plane the breadth of each being $2a\alpha$ and the intervals between them being $2(\pi - \alpha)\,a$.

The potential is of the form

$$V = A_0 + A_1 e^{-\frac{y}{a}} \cos\frac{x}{a} + \&\mathrm{c} + A_n\, \mathrm{e}^{-n\frac{y}{a}} \cos\frac{nx}{a}$$

with the conditions $V = C$ on the strips and $\frac{dV}{dy} = 0$ in the intermediate spaces.

I have succeeded by a different method (that of transformation by

(1) ULC Add. MSS 7656, M 426. First published in Larmor, *Correspondence*, **2**: 29–30.

(2) George Gabriel Stokes, 'On the critical values of the sums of periodic series', *Trans. Camb. Phil. Soc.*, **8** (1848): 533–83 (= *Papers*, **1**: 236–313).

conjugate functions)[(3)] in finding the effect of interposing a grating of fine parallel wires at potential zero between a plane at potential zero and an electrified parallel plane.[(4)]

If E be the electrification produced through the grating and E' that produced when the grating is away then

$$\frac{E'}{E} = 1 + \frac{\dfrac{1}{\alpha}}{\dfrac{1}{b_1}+\dfrac{1}{b_2}}$$

where b_1 & b_2 are the distances between the grating and the two planes and α is a line equal to $\dfrac{a}{2\pi}\log_e\left(\dfrac{1}{2}\operatorname{cosec}\dfrac{\pi c}{a}\right) = \alpha$ a being the distance between the axes of the wires and c their radius.

This is only true when a is much greater than c and when b_1 and b_2 are each much greater than a.

I have since got a method of finding the complete solution for cylindrical wires[(5)] in a series of the form $A_0 F + A_1 \dfrac{dF}{dy} + A_2 \dfrac{d^2F}{dy^2} + \&c$, where

$$F = \log\left(e^y + e^{-y} - 2\cos x\right).$$

Can you tell me the value of the infinite series

$$\frac{1}{1.(1+\alpha)} + \frac{1}{2.(2+\alpha)} + \frac{1}{3.(3+\alpha)} + \&c,$$

where α is any fraction?

Excuse my troubling you with these questions, I wish you to answer them only if you can do so without trouble.

Yours truly
J. Clerk Maxwell

(3) See Number 303 esp. note (3).
(4) See the *Treatise*, **1**: 248–51 (§§203–5); and Numbers 324 and 326.
(5) Compare the *Treatise*, **1**: 251–3 (§206).

324

LETTER TO GEORGE GABRIEL STOKES

8 JULY 1869

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
8 July 1869

Dear Sir

I am very much obliged to you for your information about Fouriers series &c.[2]

I am afraid I shall not be able to attend the meeting of the B.A. at Exeter[3] which I hope will be a great success. Jenkin is I believe with the Great Eastern[4] and will probably be busy for some time. Balfour Stewart knows most about the resistance experiments and D^r Joule has got the whole subject up independently in a most thorough manner so as to correct several numerical mistakes in the report.[5]

I know how to reduce the problem I sent you to a case of integration by a method which Thomson has applied to the complete solution of the electrification of a segment of a spherical shell.[6]

I first find by this method the distribution on an arc of a cylindric surface and then putting this in cylindric coordinates r & θ, the functions are periodic in θ. Then putting $r = ce^{\frac{2\pi x}{a}}$ and $\theta = \dfrac{2\pi y}{a}$, the functions become suited for rectangular coordinates and periodic in y.[7]

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(1) ULC Add. MSS 7656, M 427. Previously published (in part) in Larmor, *Correspondence*, **2**: 30–1.
(2) See Number 323.

(3) The 39th meeting of the British Association for the Advancement of Science, held at Exeter in August 1869.

(4) Fleeming Jenkin was engaged in laying the French Atlantic Cable aboard the *Great Eastern*, at Brest; see S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 552.

(5) Jenkin, Joule and Stewart were members, with Maxwell, of the British Association's Committee on Standards of Electrical Resistance; see their brief 'Report' in the *Report of the Thirty-ninth Meeting of the British Association* (London, 1870): 434–8, which included Maxwell's 'Experiments on the value of v, the ratio of the electromagnetic to the electrostatic unit of electricity' (on which see Number 289 note (13)), giving his results on units and the figure of his apparatus as published in his paper 'On a method of making a direct comparison of electrostatic with electromagnetic force', *Phil. Trans.*, **158** (1868): 643–57, esp. 646, 651 (= *Scientific Papers*, **2**: 129, 135). The values given in the abstract (Number 289) differ: see Number 289 note (14).

(6) See Number 310 note (2).

(7) See the *Treatise*, **1**: 248–9 (§204): theory of a grating of parallel wires; and see Number 326.

The curves on the opposite page(8) represent the equipotential surfaces & lines of force due to an infinite series of *wires* of which one is given at A placed parallel to a plane on the left hand which has an equal and opposite charge on its opposed surface.

Yours truly

J. Clerk Maxwell

(8) Compare Fig. XIII appended to the first volume of the *Treatise*.

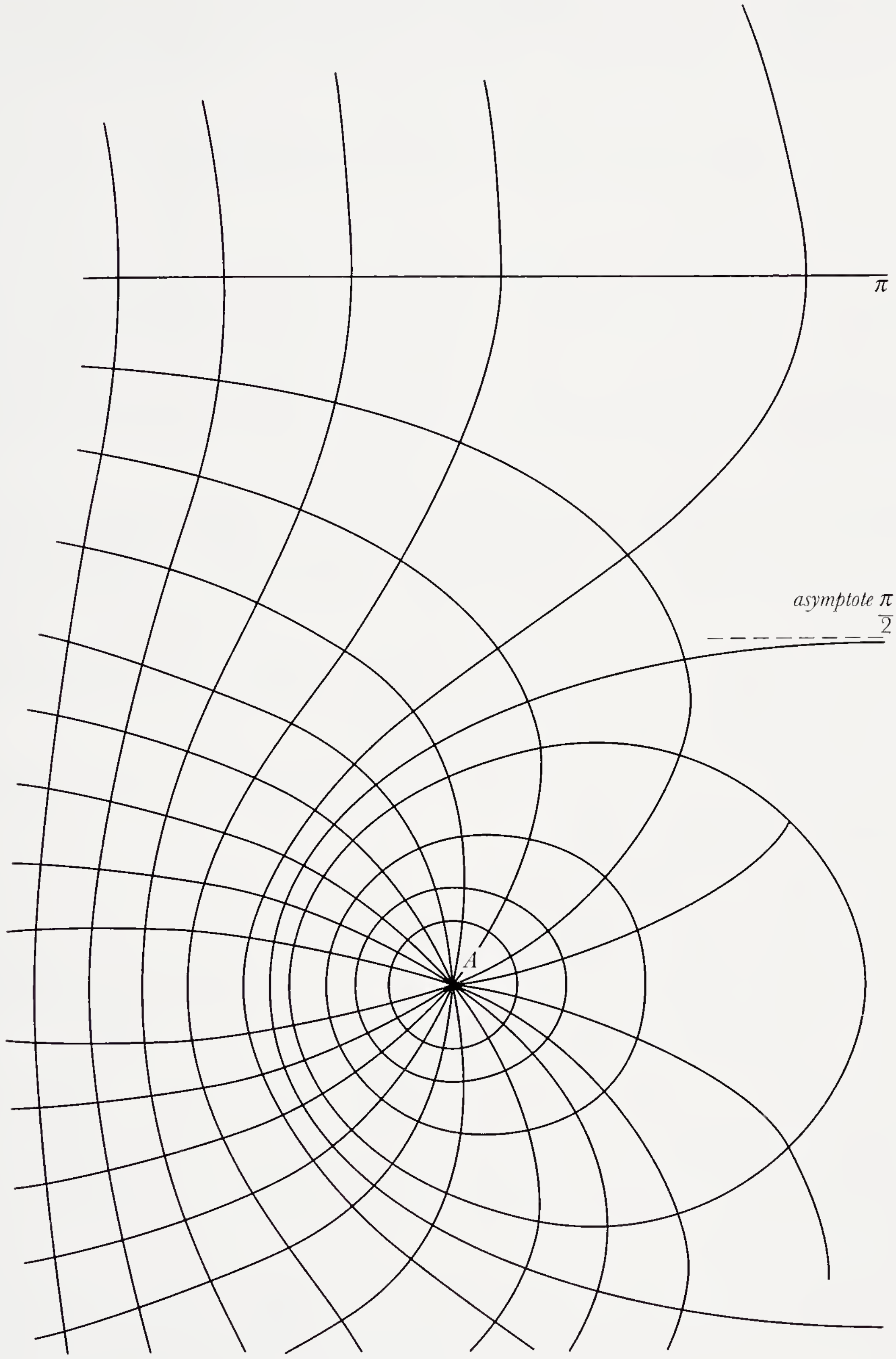

Figure 324,1

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REPORT ON A PAPER BY NORMAN MACLEOD FERRERS[1] ON THE MOTION OF A RIGID BODY

14 AUGUST 1869

From the original in the Library of the Royal Society, London[2]

REPORT ON THE REVD. N. M. FERRERS' PAPER

NOTE ON PROF SYLVESTERS REPRESENTATION OF THE MOTION OF A FREE RIGID BODY BY THAT OF A MATERIAL ELLIPSOID[3]

Professor Sylvester in the paper to which this is a note has shown that the motions of different rigid bodies under different circumstances are related to one another in a simple manner.[4] Among other remarkable results he has found that the motion of a rough ellipsoid whose centre is fixed and whose surface rolls without sliding on a fixed plane is identical with that of a free body moving about the fixed point provided A B and C the moments of inertia of the ellipsoid are in the proportion of $\frac{G}{a^4}+\frac{H}{a^2}$, $\frac{G}{b^4}+\frac{H}{b^2}$ and $\frac{G}{c^4}+\frac{H}{c^2}$ where a b c are the semiaxes of the ellipsoid.[5]

---

(1) Caius 1847, Fellow 1852 (Venn).

(2) Royal Society, *Referees' Reports*, **6**: 124.

(3) N. M. Ferrers, 'Note on Professor Sylvester's representation of the motion of a free rigid body by that of a material ellipsoid whose centre is fixed, and which rolls on a rough plane', *Phil. Trans.*, **160** (1870): 1–7. Under the title 'Note on Professor Sylvester's representation of the motion of a free rigid body by that of a material ellipsoid rolling on a rough plane', Ferrers' paper was received by the Royal Society on 29 May 1869, and read on 17 June 1869; see the abstract in *Proc. Roy. Soc.*, **17** (1869): 471–2. In recommending publication in a report dated 14 July 1869 (Royal Society, *Referees' Reports*, **6**: 123), Arthur Cayley suggested that: 'A slight change in the Title appears necessary – if the specification "rolling on a fixed plane" be inserted it is quite as necessary to state that the centre is a fixed point.' Ferrers adopted this emendation; and the paper as published in *Phil. Trans.* contains several lengthy additions dated February 1870.

(4) J. J. Sylvester, 'On the motion of a free rigid body acted on by no external forces', *Phil. Trans.*, **156** (1866): 757–79.

(5) Sylvester, 'On the motion of a free rigid body': 761–2. Maxwell here adopts the symbols employed by Ferrers; G and H are the angles between the principal axes of the ellipsoid and the 'instantaneous axis' (the line of contact). Sylvester's paper employed concepts derived from Louis Poinsot's *Théorie Nouvelle de la Rotation des Corps* (Paris, 1851), where the rotation of a body about an axis that varies in its position about a fixed point is represented by a cone whose summit is at this point, which rolls without sliding on the surface of another cone, whose vertex coincides with this point. The rotation of a body is represented by the 'central ellipsoid' which has its axes coincident with the principal axes of inertia of the body. Maxwell had employed Poinsot's theory

The moments of inertia of the free body must be in the proportion of $\frac{1}{a^2}\ \frac{1}{b^2}$ and $\frac{1}{c^2}$ and the two bodies must have the same initial motion.

Professor Sylvester has investigated the pressure and friction between the ellipsoid and the rough plane at the point of contact. M^r^ Ferrers has also calculated the pressure and friction by a different process, in the course of which he has obtained an interesting relation between the absolute motion of the point of contact and the angular momentum resolved parallel to the fixed plane, this resolved part being proportional to the velocity of the point of contact and perpendicular to it.

In fact if h be the angular momentum about any axis parallel to the fixed plane and v the velocity of the point of contact resolved perpendicular to this axis

$$h = Gpv.^{(6)}$$

This is a remarkable result in the pure kinematics of a rolling ellipsoid and forms the basis of M^r^ Ferrers dynamical method.

In calculating the force exerted between the fixed plane and the end of the instantaneous axis it should be stated that this force is necessarily indeterminate since any force in the direction of the axis itself may be combined with it without altering its effect. Both Prof Sylvester & M^r^ Ferrers assume (what is the most convenient assumption) that the direction of the friction is perpendicular to the line joining the point of contact with the foot of the perpendicular from the fixed point. I find that if $X\ Y\ Z$ are the components of the force and if we put

$$K = \frac{G\,(A\omega_1^2+B\omega_2^2+C\omega_3^2)^{(7)}}{\dfrac{G}{p^2}+H}$$

$$X = K\frac{1}{a^2}\left(\frac{1}{a^2}-\frac{1}{b^2}-\frac{1}{c^2}\right)x+Lx$$

---

in his paper 'On a dynamical top', *Trans. Roy. Soc. Edinb.*, **21** (1857): 559–70 (= *Scientific Papers*, **1**: 248–62); see Volume I: 499–500.

(6) Here G is the mass of a particle situated at the point of contact of the ellipsoid and rough plane, p is the distance from the centre of the ellipsoid to the plane (these are Ferrers' symbols). Maxwell has generalised equations (8) of Ferrers' 'Note on Professor Sylvester's representation of the motion of a free rigid body': 4, where he states relations for the component angular momenta of the ellipsoid.

(7) ω_1, ω_2, ω_3 represent the component angular velocities of the ellipsoid about its principal axes.

$$Y = K\frac{1}{b^2}\left(\frac{1}{b^2}-\frac{1}{c^2}-\frac{1}{a^2}\right)y+Ly$$

$$Z = K\frac{1}{c^2}\left(\frac{1}{c^2}-\frac{1}{a^2}-\frac{1}{b^2}\right)z+Lz.$$

The coefficient L is indeterminate as far as the given conditions are concerned.

If however we assume that the friction is in the direction stated above

$$Xx\left(1-\frac{p^2}{a^2}\right)+Yy\left(1-\frac{p^2}{b^2}\right)+Zz\left(1-\frac{p^2}{c^2}\right) = 0$$

or
$$K\left(\frac{2}{p^2}-\left(\frac{1}{a^2}+\frac{1}{b^2}+\frac{1}{c^2}\right)\right)+L\,(r^2-p^2) = 0.$$

The friction then becomes

$$F = \frac{Kp}{q}\frac{(b^2-c^2)\,(c^2-a^2)\,(a^2-b^2)}{a^4b^4c^4}xyz$$

where $\quad r^2 = x^2+y^2+z^2 \quad$ and $\quad p^2+q^2 = r^2$

r being the instantaneous semiaxis p the perpendicular on the fixed plane and q the distance of the point of contact from the foot of the perpendicular. This expression is not subject to the ambiguity of sign which is found in the expressions given by Prof Sylvester & M^r Ferrers with which however it agrees.(8) I find the pressure

$$P = p\left(\frac{Xx}{a^2}+\frac{Yy}{b^2}+\frac{Zz}{c^2}\right)$$

$$P = \frac{K}{pq^2}\left\{\left(\frac{1}{a^2}+\frac{1}{b^2}+\frac{1}{c^2}\right)q^2+2\left(\frac{1}{a^2}+\frac{1}{b^2}+\frac{1}{c^2}-\frac{1}{p^2}\right)p^2-2\frac{a^2+b^2+c^2-r^2}{a^2\,b^2\,c^2}p^2\,r^2\right\}.$$

This agrees with Prof. Sylvester but not with M^r Ferrers.(9) M^r Ferrers takes advantage of the result $h = Gpv$, which indicates that as far as motion about axes parallel to the fixed plane is concerned (on which alone the pressure depends) the angular momentum of the body is the same as that of a body of mass G, moving so as always to coincide with the point of contact. Hence if R is the resolved part of the force on the latter body in the direction of q (towards the foot of the perpendicular)

(8) Sylvester, 'On the motion of a free rigid body': 764; Ferrers, 'Note on Professor Sylvester's representation of the motion of a free rigid body': 5.

(9) Sylvester, 'On the motion of a free rigid body': 766; Ferrers, 'Note on Professor Sylvester's representation of the motion of a free rigid body': 5.

$$P = R\frac{p}{q} = \frac{p}{q}G\left(\frac{d^2 q}{dt^2} - n^2 q\right)^{(10)}$$

which agrees with M^r Ferrers result.[11]

In my copy of Poinsot I find $\dot{\phi} = \frac{h}{k} + \frac{(h^2-a^2)\,(h^2-b^2)\,(h^2-c^2)}{kh^3 v^2}$[12] instead of M^r Ferrers $n = \lambda - \alpha\beta\gamma\frac{\lambda^2}{\mu^2}$[13] and I found independently of Poinsot that the sign is $+$ and not $-$.

This should be looked into but I have not time at present. I think the method might be made clearer by a division of the mass of the body into two parts corresponding to G and H so as to show that the external forces are due to G only and also by a comparison of one part of the external force to the force acting on a body $= G$ moving as the point of contact moves.[14]

The calculation of n and P should be reexamined and then I think the paper deserving of being printed in the Transactions.

J. Clerk Maxwell

Glenlair 14 August 1869

(10) n is the angular velocity of the radius vector of the point of contact measured from the foot of the perpendicular on the rough plane.

(11) Compare equations (10) of Ferrers' 'Note on Professor Sylvester's representation of the motion of a free rigid body': 5.

(12) Poinsot, *Théorie Nouvelle de la Rotation des Corps*: 130; an 'expression trés-simple de la vitesse angulaire avec la pôle instantané de rotation tourne autour du centre de l'herpolhodie σ; cette vitesse, comme on le voit, est composée d'une partie constante, et d'une partie variable qui est réciproque au carré du rayon vecteur v, et, par conséquent, périodique comme ce rayon.' Poinsot's 'herpolhodie' is the curve traced by the point of contact on the plane when the ellipsoid rolls on the plane; see Volume I: 500n. The 1851 edition of Poinsot's text, here cited (see note (5)), is the edition in Maxwell's library (Cavendish Laboratory, Cambridge).

(13) Adopting Ferrers' symbols in place of Poinsot's. In a paper 'On the angular velocity of the instantaneous axis in space', *Quarterly Journal of Pure and Applied Mathematics*, 7 (1865): 74–5, Ferrers obtained an equation for n with a minus sign between the two terms. The sign is corrected to a plus sign in the published Royal Society paper; compare Ferrers, 'Note on Professor Sylvester's representation of the motion of a free rigid body': 5 (equation (12), there an equation for p).

(14) See Ferrers' addendum, dated February 1870, in the published text of his 'Note on Professor Sylvester's representation of the motion of a free rigid body': 6–7.

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LETTER TO WILLIAM THOMSON

17 AUGUST 1869

From the original in the University Library, Cambridge[1]

Ardhallow
Dunoon
17 Aug 1869

(a)

Dear Thomson

I have not heard of you since you were at Brest[2] and should like to know if you are in these parts. We are to be here for 3 or 4 weeks, and I am sticking my electrostatics and electrokinetics together, and should like a little of your influence to assist me in brooding over the mass.

I have been doing the case of the influence of an electrified body on a conductor at potential zero separated from it by a grating of parallel wires also at potential zero.[3] I have not met with any examination of this case, have you? or have you done it yourself?

I find that if two planes A_1 and A_2 are at distances b_1 and b_2 on opposite sides of a grating and if A_2 is in metallic connexion with the grating and at potential zero and if A_1 is brought to some other potential then the electric density induced on A_2 through the grating is to that which would be induced on it if the grating were removed as 1 to $1+\dfrac{2b_1b_2}{\alpha(b_1+b_2)}$ where α is a linear quantity whose approximate value is

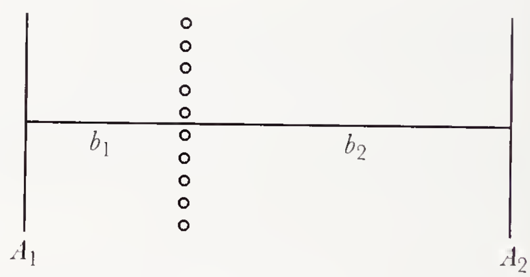

Figure 326,1

$$\alpha = \frac{a}{2\pi}\log_e\frac{a}{2\pi c}$$

where a is the distance between the axes of consecutive wires of the grating and $2\pi c$ is the circumference of one of the wires.[4]

This expression is approximate only when the distance between the wires is considerably greater than the circumference of a wire for it is manifest that

(a) {Thomson} I have made extract of this. T

(1) ULC Add. MSS 7655, II/33.

(2) Thomson had been aboard the *Great Eastern* at Brest in June 1869: see Number 324 note (4).

(3) See Numbers 323 and 324.

(4) See the *Treatise*, **1**: 248–51 (§§203–5).

α ought always to be positive when a is greater than $2c$ and to become zero when $a = 2c$.

I am also trying to reduce to the simplest form the mathematical conception of a dielectric bad conductor which shows electric absorption. I got a differential equation between the electromotive force and the current through the dielectric which may contain any number of differential coefficients of all orders such as

$$A_1 F + A_2 \frac{dF}{dt} + \&c = B_1 \gamma + B_2 \frac{d\gamma}{dt} + \&c.$$

If F is the electromotive force and γ the current

$$F = \left\{ r_1 \left(1 + \alpha_1 \frac{d}{dt}\right)^{-1} + r_2 \left(1 + \alpha_2 \frac{d}{dt}\right)^{-1} + \&c \right\} \gamma$$

where $r_1 + r_2 + r_3 + \&c = r$ the actual specific resistance and $\alpha_1\ \alpha_2$ &c are *times* which in unabsorbing media are all equal to $\frac{r}{4\pi k}$.[5]

How is your reprint of electrical papers coming on.[6] The last I heard of was the electrified cup which I see you can do completely for any kind of influence.[7] I tried the case of an incomplete cylindric tube but found hard integrations in the way.

Can you let me see your *complete* version of the cup. I mean to describe it if I can without bagging it whole.[8]

Yours truly
J. Clerk Maxwell

(5) k is the reciprocal of the specific inductive capacity; see Maxwell's discussion of conduction in dielectrics in the *Treatise*, **1**: 376–80 (§§328–29).

(6) See Number 321 note (21).

(7) See Number 310 note (2).

(8) See the *Treatise*, **1**: 221–5 (§§176–81).

LETTER TO WILLIAM THOMSON

1 OCTOBER 1869

From the original in the University Library, Cambridge[1]

Ardhallow
Dunoon
1st October 1869

Dear Thomson

Many thanks for yours of the 22nd Sept. You see I am again on the Clyde & I propose to be in Glasgow on Tuesday 5th Oct and will call at Whites[2] about 12 or as I hear from you.

In which of your papers is there a discussion of Poissons k.[3] Thalén gives for Neumann's κ 32.32, 31.80, 32.61.[4]

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(1) ULC Add. MSS 7342, M 105. First published in Larmor, 'Origins': 739–40.

(2) See Number 301 (note (2)).

(3) Thomson had discussed Poisson's theory of magnetism in his paper 'On the theory of magnetic induction in crystalline and non-crystalline substances', *Phil. Mag.*, ser. 4, **1** (1851): 177–86 (= *Electrostatics and Magnetism*: 465–80). In his discussion of 'Magnetic permeability, and analogues in electro-static induction, conduction of heat, and fluid motion' (dated March 1872), first published in the reprint of his papers on *Electrostatics and Magnetism*: 482–6, he terms Maxwell's quantity κ, which Maxwell calls 'Neumann's coefficient of Magnetization by Induction' (see the *Treatise*, **2**: 54 (§430), the 'magnetic susceptibility'. Thomson's 'magnetic permeability' is termed the 'Coefficient of Magnetic Induction' (μ) by Maxwell, who writes $\mu = 4\pi\kappa+1$ (following Thomson). While k, 'Poisson's Magnetic Coefficient represents the ratio of the volume of the magnetic elements to the whole volume of the substance'; see the *Treatise*, **2**: 54 (§430). Maxwell writes the relation $k = 4\pi\kappa/(4\pi\kappa+3)$. See Siméon Denis Poisson, 'Mémoire sur la théorie du magnétisme', and 'Second mémoire sur la théorie du magnétisme', *Mémoires de l'Académie Royale des Sciences*, **5** (1826): 247–338, 488–533.

(4) Tobias Robert Thalén, 'Recherches sur les propriétés magnétiques du fer', *Nova Acta Regiae Societatis Scientiarum Upsaliensis*, ser. 3, **4** (1863): esp. 31–43. Thalén obtains these values from three experiments on bars of soft iron. In the *Treatise*, **2**: 54 (§430) Maxwell takes $\kappa = 32$ and finds $k = 134/135$. F. E. Neumann's discussion is in his paper 'Entwickelung der in elliptischen Coordinaten ausgedrückten reciproken Entfernung zweier Punkte in Reihen, welche nach den Laplace'schen $Y^{(n)}$ fortschreiten; und Anwendung dieser Reihen zur Bestimmung des magnetischen Zustandes eines Rotations-Ellipsoids, welcher durch vertheilende Kräfte erregt ist', *Journal für die reine und angewandte Mathematik*, **37** (1848): 21–50, esp. 46–50, on the distribution of the magnetisation of an ellipsoid of revolution under the action of magnetic forces. Maxwell's brief notes on this paper are in ULC Add. MSS 7655, V, c/40. The paper is discussed by Wilhelm Weber, in his 'Bestimmung der rechtwinkeligen Componenten der erdmagnetischen Kraft in Göttingen in dem Zeitraume von 1834–1853', *Abhandlungen der Math. Classe der Königlichen Gesellschaft der Wissenschaften zu Göttingen*, **6** (1856): esp. 20 (= *Wilhelm Weber's Werke*, 6 vols. (Berlin, 1892–4), **2**: 333–73), referring to the 'Neumannsche magnetische Constante', which Maxwell denotes by the symbol κ.

I have written 40 pages on Magnetic Measurements(5) and am beginning Terrestrial Magnetism.(6)

I have put in Joule's method of swinging two magnets together, so as to eliminate induction.(7) I think that the magnets ought to be as heavy as a single silk fibre will safely carry and as short as is consistent with strong permanent magnetization.

I got into a mess with an investigation of the 2 best distances at which to place the deflecting magnet in order to deduce its moment by the eq$^{\mathrm{n}}$

$$\frac{2M}{H} = \frac{D_1 r_1^5 - D_2 r_2^5}{r_1^2 - r_2^2}\,^{(8)}$$

supposing the relation between the probable error in the measurement of D the tangent of deflexion and r the distance to be known.

In fact I got a pair of impossible values for r_1 & r_2. If one distance only is used the best distance is when

$$\frac{\delta D}{D} = \sqrt{3}\frac{\delta r}{r}$$

where $\frac{\delta D}{D}$ is the probable percentage error of D and $\frac{\delta r}{r}$ that of r.(9)

Your bowl investigations are first rate.(10) I must find the induction through a round hole in a plate by means of them. Whether would you have me bag the whole thing for my book, or give results and references with an *account* of the method?(11)

Yours truly
J. Clerk Maxwell

C. Neumanns theory of the transmission of Potentials is altogether unique, the Potential $\frac{mm_1}{r}$ $\left(\text{not the potential function } \frac{m}{r}\right)$ starts from m (with the consciousness of the value of r and m_1 at the instant) and travels along r with uniform velocity not absolute but relative to m till it reaches m_1 which

(5) See the *Treatise*, **2**: 88–119 (§§449–64).

(6) See the *Treatise*, **2**: 120–7 (§§465–74).

(7) J. P. Joule, 'On an apparatus for determining the horizontal magnetic intensity in absolute measure', *Proceedings of the Literary and Philosophical Society of Manchester*, **6** (1867): 129–35. See the *Treatise*, **2**: 105–6 (§457).

(8) M is the magnetic moment of the magnet, H the horizontal component of terrestrial magnetism.

(9) See the *Treatise*, **2**: 97–101 (§§453–4).

(10) See Number 310 note (2).

(11) In the *Treatise*, **1**: 221–5 (§§176–81) Maxwell follows the latter procedure.

receives it after a time t.(12) Truly those who supposed that Neumanns potential travelled like light were greatly mistaken.(13)

(12) Carl Neumann, 'Resultate einer Untersuchung über die Principien der Elektrodynamik', *Nachrichten von der Königl. Gesellschaft der Wissenschaften und der Georg-August-Universität zu Göttingen* (1868): 223–35, esp. 225–6; 'Das emissive Potential ist dasjenige, welches jeder Punct in dem gegebenen Augenblick aussendet, und welches erst einige Zeit später den andern Punct erreicht... [Das] emissive Potential $= \frac{mm_1}{r}$... Das receptive Potential anderseits ist dasjenige, welches jeder Punct in dem gegebenem Augenblick empfängt, welches also schon einige Zeit früher von dem andern Punct ausgesendet wurde. Das dem gegebenem Augenblick entsprechende receptive Potential ist demnach immer identisch mit dem einem früheren Augenblick entsprechenden emissiven Potential.'

(13) For further comments see the *Treatise*, **2**: 435–6 (§863).

LETTER TO WILLIAM THOMSON

5 OCTOBER 1869

From the original in the University Library, Cambridge[1]

Ardhallow
Dunoon
5 Oct 1869

Dear Thomson

I did not go to Glasgow today as you were not to be there. I can go on Thursday Friday or Saturday. We are going home the beginning of next week. There are but few other things I have to do in Glasgow.

In Shurrocks hairbrushing room I examined the endless band in motion. The velocity of propagation of a disturbance in space is in the case of a flexible tube with water going at velocity V when m = mass of tube μ of water in unit of length

$$\text{vel of disturbance} = \frac{\mu}{m+\mu}V \pm \sqrt{\frac{T}{m+\mu} - \frac{m\mu V^2}{m+\mu}}.$$

When the 2nd term is impossible, that is if T the effective tension (tension-pressure) is small compared with V the motion is unstable. When however $m = 0$ as at Shurrocks this cannot be.

When the shape of the tube is in equilibrium $T = T_0 + \mu V^2$ where T_0 is the statical value. Hence the velocity may be written

$$\frac{\mu}{m+\mu}V \pm \sqrt{\frac{T_0}{m+\mu} + \frac{\mu^2 V^2}{(m+\mu)^2}}$$

which shows that the velocities are always in opposite directions but when T_0 is small one of them is very small. Hence the sluggishness of the wave near the bottom of a loop of chain or flexible pipe, part of it darts up the ascending side and part slowly ascends the descending one but faster as it gets away from the bottom.

Can you tell me where you were getting *light* indiarubber cloaks when I was at Largs? last year.

Yours truly
J. Clerk Maxwell

(1) ULC Add. MSS 7342, M 106. First published in Larmor, 'Origins': 740–1.

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OUTLINE OF CONTENTS OF THE *TREATISE ON ELECTRICITY AND MAGNETISM*

circa OCTOBER 1869[1]

From the original in the King's College London Archives[2]

[*TREATISE ON ELECTRICITY AND MAGNETISM*: CONTENTS]

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(1) This date is conjectural. But see Numbers 327 and 333, which indicate progress in writing the *Treatise* roughly corresponding to this partial and preliminary outline of its contents. Other jottings in the notebook (see note (2)) support this conjectural dating.

(2) From Maxwell Notebook (3), King's College London Archives.

(3) The numbers in the right hand margin, indicating section numbers in the draft *Treatise*, were added in pencil. Compare the *Treatise*, **1**: 1–29 (§§1–26).

(4) Compare Part I Chapter I of the *Treatise*, **1**: 30–65 (§§27–62).

(5) Compare Part I Chapters II and III of the *Treatise*, **1**: 66–97 (§§63–94).

(6) It is unclear which theorem Maxwell intends to denote; but see Number 274 esp. note (10).

(7) See Number 274 esp. note (9).

(8) Compare Part I Chapter IV of the *Treatise*, **1**: 98–118 (§§95–102).

(9) Compare Part I Chapter V of the *Treatise*, **1**: 119–34 (§§103–11).

(10) Compare Part I Chapters VI, VII, VIII and IX of the *Treatise*, **1**: 134–80 (§§112–46).

Nuclei of any order & their spherical harmonics and axes. Points of Equilibrium and their spherical harmonics. Electrified sphere. $\sum Y_i Y_j dS$.[(11)] Distribution of points of equilibrium. Instability of electrical equilibrium. 13

Ch.VI[(12)] Theory of Electrical Images.
Spherical Harmonic of order 0. Uninsulated sphere & electrified point. Charged sphere and electrified point or system. Internal spherical surfaces. Spherical Condensers. Theory of two spheres. 30

Ch.VII[(13)] Test of a particular set of surfaces being equipotential. Application to confocal quadrics. 16

Ch.VIII[(14)] Electrical Instruments.
Electrical Machines.[(15)] Electrophorus. Doubler. Revolving Doubler. Varleys multiplier.[(16)] Thomsons water dropping collectors & multipliers.[(17)] Condensers. Electrometers.

Part II Electrokinematics

Ch.I[(18)] On the Electric Current. How it is produced. Why it is called a Current. Its properties. 1 Transference of Electrical state. 2 Chemical action. 3 Magnetic action. 4 Resistance. 5 Production of Heat. Measurement of Currents.

Ch.II[(19)] Ohms Law of Resistance explained.
Resistance of linear conductors in series.
——————————in multiple arc.
Determination of the distribution of currents in any linear system. Minimum of Heat. Reciprocity of EMF & Current. Simple case. Wheatstone Bridge.[(20)] Different arrangements of the bridge. 12

(11) See Number 294 esp. note (20).
(12) Compare Part I Chapter XI of the *Treatise*, **1**: 191–225 (§§155–81).
(13) Compare Part I Chapter X of the *Treatise*, **1**: 181–9 (§§147–54).
(14) Compare Part I Chapter XIII of the *Treatise*, **1**: 254–87 (§§207–29).
(15) See Number 302.
(16) See Number 260 note (17).
(17) See Number 302 note (2).
(18) Compare Part II Chapters I, II and III of the *Treatise*, **1**: 288–306 (§§230–54).
(19) Compare Part II Chapters VI and XI of the *Treatise*, **1**: 329–37, 388–425 (§§273–84, 335–70).
(20) See Number 230.

Results & applications of the measurement of resistance.

Ch.III[21] Theory of conduction in crystallized bodies the equipotential surfaces being plane. General equations of conduction. The rotatory property probably nonexistent.[22]

Ch.IV[23] Theory of conduction in two dimensions. Equipotential Lines and Lines of Flow with their Conjugate Functions.[24] Theory of the transformation of results from one set of curvilinear coordinates to another. Graphic method of finding the resultant of the superposition of two systems. Examples of the solution of problems. Extension to cases in 3 dimensions symmetrical about an axis. 12

Ch.V Theory of Submarine Cables.[25]

Electrolysis,[26] Bourgoin Ann de Ch IV, XV p 47.[27] Millers Chemical Physics 4th edn.[28] Daniell Phil Trans 1839 p 97.[29] D'Almeida Ann de Ch III, 4, 263.[30] Miller Phil Trans 1844, 16.[31] Magnus Pogg cii.[32]

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(21) Compare Part II Chapters VII, VIII and IX of the *Treatise*, **1**: 338–73 (§§285–324).

(22) See Number 301 esp. note (5).

(23) Compare Part I Chapter XII of the *Treatise*, **1**: 226–53 (§§182–206).

(24) See Number 303 esp. note (3).

(25) Compare Part II Chapter X of the *Treatise*, **1**: 374–87 (§§325–34).

(26) Compare Part II Chapters IV and V of the *Treatise*, **1**: 307–28 (§§255–72).

(27) E. Bourgoin, 'Du role de l'eau dans l'électrolyse', *Ann. Chim. Phys.*, ser. 4, **15** (1868): 47–57.

(28) William Allen Miller, *Elements of Chemistry: Theoretical and Practical. Part I. Chemical Physics* (London, $_4$1867).

(29) J. F. Daniell, 'On the electrolysis of secondary compounds', *Phil. Trans.*, **129** (1839): 97–112; and also Daniell, 'Second letter on the electrolysis of secondary compounds', *Phil. Trans.*, **130** (1840): 209–24.

(30) J. Ch. D'Almeida, 'Décomposition par la pile des sels dissous dans l'eau', *Ann. Chim. Phys.*, ser. 3, **51** (1857): 257–90.

(31) J. F. Daniell and W. A. Miller, 'Additional researches on the electrolysis of secondary compounds', *Phil. Trans.*, **134** (1844): 1–19.

(32) Gustav Magnus, 'Elektrolytische Untersuchungen', *Ann. Phys.*, **102** (1857): 1–54.

Part III Magnetics

Ch.I[33] Experiments with magnets. Magnetization.
Every particle a magnet with poles of equal strength.
Induction of magnetism in soft iron and in steel.
Gauss & Webers method for the Determination of the direction of the axis of a magnet and of that of the earths horizontal component.[34]
The dipping needle.
Determination of the strength of a magnet and of the Earths horizontal component. Joules method of eliminating the effects of induction.[35]

Ch.II[36] Mathematical Theory of the distribution of magnetization and of Magnetic Potential.
Gauss' 24 coeffts of Terrestrial Magnetism.[37]

Ch.III[38] Theory of Magnetic Induction.
Para & Diamagnetic bodies & Magnecrystallic phenomena. Weber's Theory.[39]

Ch.IV[40] Theory of the magnetism of Ships and of the correction of the compass by magnets by iron and by tables.

Ch.[41] Earths mag axis Gauss 77°50 N 296°29 E from Greenwich to 77°50 S 116°29′. Moment $= 947.08R^3$.[42]

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(33) Compare Part III Chapters I and VII of the *Treatise*, **2**: 1–20, 88–119 (§§371–94, 449–64).

(34) Carl Friedrich Gauss, 'Ueber ein neues, zunächst zur unmittelbaren Beobachtung der Veränderungen in der Intensität des horizontalen Theiles des Erdmagnetismus bestimmtes Instrument', in *Resultate aus den Beobachtungen des magnetischen Vereins in Jahre 1837*, ed. C. F. Gauss and W. E. Weber (Leipzig, 1838): 1–19 (= Gauss, *Werke*, **5** (Göttingen, 1867): 357–73); translation in *Scientific Memoirs*, ed. R. Taylor, **2** (London, 1841): 252–67. Wilhelm Weber, 'Bemerkungen über die Einrichtung und den Gebrauch des Bifilar-Magnetometers', in *Resultate...in Jahr 1837*: 20–37 (= *Wilhelm Weber's Werke*, 6 vols. (Berlin, 1892–4), **2**: 43–57); trans. in *Scientific Memoirs*, **2**: 268–80. See *Treatise*, **2**: 107–111 (§§459–60).

(35) See Number 327 esp. note (7).

(36) Compare Part II Chapters II and III of the *Treatise*, **2**: 21–43 (§§395–423).

(37) Compare the *Treatise*, **2**: 124 (§469), and see below.

(38) Compare Part II Chapters IV, V and VI of the *Treatise*, **2**: 44–87 (§§424–48).

(39) See Numbers 278 esp. note (8) and 295.

(40) Compare the *Treatise*, **2**: 70–3 (§441).

(41) These notes are taken from Carl Friedrich Gauss, 'Allgemeine Theorie des Erdmagnetismus', in *Resultate...in Jahre 1838*, ed. C. F. Gauss and W. Weber (Leipzig, 1839): 1–57, 146–8 (= Gauss, *Werke*, **5**: 119–93); translation in *Scientific Memoirs*, **2**: 184–251.

(42) Gauss, *Werke*, **5**: 164; here R is the earth's major semi-axis.

$$\cos u = e \quad \sin u = f^{(43)}$$

$$P^{n,m} = \left(e^{n-m} - \frac{(n-m)(n-m-1)}{2(2n-1)} e^{n-m-2} + \&\text{c.}\right) f^m$$

$$P^n = g^{n,0}P^{n,0} + (g^{n,1}\cos\lambda + h^{n,1}\sin\lambda)\,P^{n,1}$$
$$+ (g^{n,2}\cos 2\lambda + h^{n,2}\sin 2\lambda)P^{n,2} + {}^{(44)}$$

$g^{1,0} = +925.782,$	$g^{1,1} = +89.024$	$h^{1,1} = -178.744$
$g^{2,0} = -22.059,$	$g^{2,1} = -144.913$	$h^{2,1} = -6.030$
	$g^{2,2} = +0.493$	$h^{2,2} = -39.010$
$g^{3,0} = -18.868$	$g^{3,1} = +122.936$	$h^{3,1} = +47.794$
$g^{3,2} = -73.193$	$g^{3,3} = 1.396$	$h^{3,2} = -22.766$
$h^{3,3} = -18.750$	$g^{4,0} = -108.855$	$g^{4,1} = -152.589$
$h^{4,1} = +64.112$	$g^{4,2} = -45.791$	$h^{4,2} = +42.573$
$g^{4,3} = +19.774$	$h^{4,3} = -0.178$	$g^{4,4} = 4.27$
		$h^{4,4} = 3.175^{(45)}$

Webers magnet of 142 grammes $\frac{M}{H} = .08765$

metre gramme deflexion 11°24 at .450 when $H = 1.774.^{(46)}$
Limiting magnetic moment of iron = 2100 per milligramme.
Best steel magnets 400 Weber.
Prof A Waltenhofen Sitzungsberichte der K Acad in Wien 1869 No 12.(47)

(43) u is the earth's latitude; e and f are Maxwell's own symbols.

(44) λ is the earth's latitude, g and h the coefficients of the harmonics of P, the potential of the distribution of the earth's magnetism. See Gàuss, *Werke*, **5**: 142.

(45) Gauss' table of the 24 coefficients of the potential of the earth's magnetism: 3 for the first degree, 5 for the second, 7 for the third, and 9 for the fourth. See Gauss, *Werke*, **5**: 150.

(46) For these symbols see Number 327 note (8). The values are from Wilhelm Weber, 'Beschreibung eines kleinen Apparats zur Messung des Erdmagnetismus nach absolutem Maass für Reisende', in *Resultate... in Jahre 1836*, ed. C. F. Gauss and W. Weber (Leipzig, 1837): 63–89 (= Weber, *Werke*, **2**: 20–42), translation in *Scientific Memoirs*, **2**: 65–87, esp. 82–3.

(47) These two values are given by A. von Waltenhofen, 'Über die Grenzen der Magnetisbarkeit des Eisens und des Stahles', *Wiener Berichte*, Abtheilung II, **59** (1869): 770–88, esp. 777, 780.

Part IV Electromagnetics

Ch.I[48] Oersted's discovery of the action of a current on a magnet.[49] Ampère's experiments and mathematical theory.[50] Faraday's experiments on rotation of magnets & currents.[51]

Ch.II[52] Faraday's discovery of the induction of electric currents.[53] Faradays Theory of Lines of Force and of the Electrotonic State.[54]

Ch.III[55] Helmholtz and Thomsons deduction of the induction of currents from their electromagnetic action.[56] Theory of the coefficients of induction of two linear currents. The Induction Coil.[57]

$V=$ potential of a plane area in xy bounded by a curve s density unity.

$$\frac{dV}{dz}=W \quad \frac{dV}{dy}=F \quad \frac{dV}{dx}=-G \quad H=0.^{(58)}$$

(48) Compare Part IV Chapters I and II of the *Treatise*, **2**: 128–61 (§§475–527).

(49) See Number 238 note (17).

(50) See Volume I: 305–6n; and Number 332 note (10).

(51) See Faraday, *Electricity*, **2**: 127–47.

(52) Compare Part IV Chapter III of the *Treatise*, **2**: 162–79 (§§528–45).

(53) See Number 238 note (15).

(54) See Number 238 notes (12) and (13).

(55) Compare Part IV Chapters III (the latter part), IV and VII of the *Treatise*, **2**: 176–83, 206–10 (§§543–52, 578–84).

(56) See Number 238 notes (9), (10) and (11).

(57) Compare Part IV Chapter XVII of the *Treatise*, **2**: 352–7 (§§752–7).

(58) See Maxwell's discussion of plane current-sheets in the *Treatise*, **2**: 264–5 (§657).

LETTER TO THE LONDON MATHEMATICAL SOCIETY ON THE POTENTIAL OF A DISC

NOVEMBER 1869[1]

From the *Proceedings of the London Mathematical Society*[2]

[ON THE POTENTIAL OF A UNIFORM CIRCULAR DISC]

Can the potential of a uniform circular disc at any point be expressed by means of elliptic integrals? Suppose V is the potential of the disc bounded by the circle

$$z = 0, \quad x^2+y^2 = a^2;$$

then
$$\frac{dV}{dx} = 2x\left(\frac{a}{r}\right)^{\frac{1}{2}}\frac{1}{\sqrt{c}}(E-F),$$

where
$$r^2 = x^2+y^2;$$

and if AB be a diameter parallel to r,

$$c = \frac{PB-PA}{PB+PA},$$

and E, F are complete elliptic functions for modulus c;[3]

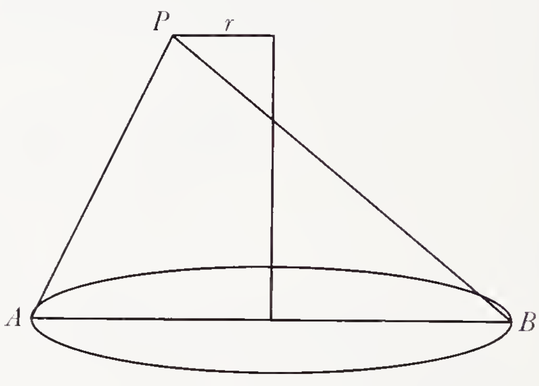

Figure 330,1

also
$$\frac{dV}{dy} = 2y\left(\frac{a}{r}\right)^{\frac{1}{2}}\frac{1}{\sqrt{c}}(E-F).$$

But $\dfrac{dV}{dz} = w$, where w is the solid angle subtended at P by the circle; that is, the area of the spherical ellipse on a sphere of unit radius cut off by the cone whose vertex is P and base the circle. We have expressions for $\dfrac{dV}{dx}$ and $\dfrac{dV}{dy}$. Can $\dfrac{dV}{dz}$ also be expressed by elliptic functions? and if so, can V itself be so expressed?

I am writing out the theory of circular electric currents, in which these

(1) Maxwell's letter of inquiry was read at a meeting of the London Mathematical Society on 11 November 1869.

(2) *Proceedings of the London Mathematical Society*, **3** (1869): 8.

(3) See Number 262 note (3).

quantities occur.(4) The expression $\frac{dV}{dz}$ for an elliptic disc can be found if we know it for a circular one, for the spherical ellipses in the one case are no more complicated than in the other. Can $\frac{dV}{dx}$, or V itself, be found for the elliptic disc?

(4) See the *Treatise*, **2**: 305–7 (§701); and Number 341.

REMARKS ON A PAPER BY GEORGE OLDHAM HANLON ON THE *VENA CONTRACTA*(1)

NOVEMBER 1869(2)

From the *Proceedings of the London Mathematical Society*(3)

REMARKS OF MR J. CLERK-MAXWELL ON MR HANLON'S PAPER ON THE VENA CONTRACTA(4)

It appears from this paper that it has been stated, that when a hole is made in one side of a vessel containing water, so as to allow the water to escape, the pressure on the opposite side of the vessel is thereby increased.(5) This statement Mr. Hanlon shows to be erroneous, and asserts that the immediate effect of opening the hole, and allowing motion to take place, is to diminish the pressure in every part of the vessel.

He then shows, by a calculation of the momentum communicated in a unit of time to the fluid which is projected from an orifice in the side of a vessel, that the force acting on it is measured by $\iint \rho v^2 dS$, where ρ is the density, v the velocity at the Vena Contracta where the pressure vanishes, and dS an element of the section of the stream.

Now at the Vena Contracta $v^2 = 2gz$ approximately,(6) where z is the depth, so that the force of propulsion is $F = 2g\rho \iint z dS$; or if $\bar{z}$ is the depth of the centre of gravity of the section S, $F = 2g\rho\bar{z}S$. But at a distance from the

(1) G. O. Hanlon, 'The vena contracta', *Proceedings of the London Mathematical Society*, **3** (1869): 4–5.

(2) Hanlon's paper was read to the London Mathematical Society on 11 November 1869; see note (3) and *Nature*, **1** (1869): 91.

(3) *Proceedings of the London Mathematical Society*, **3** (1869): 6–8.

(4) On the term *vena contracta* see Joseph Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes, including Letters of other Eminent Men* (London, 1851): 39n, in comment on Newton's letter to Cotes of 27 March 1710/11. According to Edleston the 'term [was] first used by Jurin, *Philosoph. Transact.* Sept.–Oct. 1722, p. 185...to denote the same thing [as Newton]': 'the water...passes through the hole with a converging motion & thereby grows into a smaller stream after it is past the hole...' (Newton to Cotes, letter of 24 March 1710/11).

(5) Hanlon stated that his paper was written in response to a discussion in *The Engineer* on the hydrodynamics of the 'nozzle ship', the gunboat *The Waterwitch* powered by a water turbine (tested in October 1866). See letters on 'Hydro-propulsion', especially by J. A. L. Airey and R. D. Napier, in *The Engineer* (November 1866 to March 1867).

(6) Following Newton, *Principia* (Book II, Prop. 36). The result was standard in the literature: see W. H. Miller, *The Elements of Hydrostatics and Hydrodynamics* (Cambridge, $_4$1850): 51: 'the velocity of the issuing fluid is equal to the velocity acquired by a heavy body in falling'.

hole, where the velocity is small, the pressure is the hydrostatic pressure $p = g\rho z$.

Let us assume (for an instant) that this is the value of the pressure on every part of the surface of the vessel, in which there is a hole whose section is S', and whose centre of gravity is at a depth z'. The resultant of the pressures on the sides will be $F' = g\rho z' S'$, and this must be equal to F, because action and reaction are equal and opposite. This gives

$$2zS = z'S'.$$

There is very little difference between z and z', the depths of the centre of gravity at the hole and at the Vena Contracta. Hence we ought to have $S = \frac{1}{2}S'$, or section of Vena Contraction = .5 of the hole.

I am not aware how far the author is original in this method of treating the subject; but it is certainly very clear and intelligible, without the aid of symbols, which is very important in practical matters. Rankine, however (Steam Engine, p. 96), gives $.6zS'$ as the section of the Vena Contracta for round holes;[(7)] and I have not seen any experimental result as low as $.5S'$.

This arises partly from our having made an erroneous assumption – that the pressure on the sides is the hydrostatic pressure ρgz, whereas it must be diminished by $\frac{1}{2}\rho v^2$ wherever there is a velocity v.

Now v will evidently be greatest near the hole, so that the pressure on the side next the hole will be diminished for this reason, as well as by the removal of the stopper of the hole, and the value of F' will be greater than $g\rho z' S'$, and therefore S will be greater than $.5S'$.

If the fluid is a perfect liquid originally at rest, it will have a velocity-potential ϕ,[(8)] and the pressure is given by the equation

$$p = \rho gz - \rho\frac{\delta\phi}{\delta t} = \rho gz - \rho\frac{d\phi}{dt} - \tfrac{1}{2}\rho V^2,$$

where $\frac{\delta}{\delta t}$ denotes the rate of change in a moving particle, and $\frac{d}{dt}$ denotes rate of change at a point of space. V is the velocity of the fluid.

We must also have at the surface of the vessel $\frac{d\phi}{dv} = 0$, where v is the normal to the surface, and ϕ satisfies Laplace's equation[(9)]

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0.$$

(7) W. J. Macquorn Rankine, *A Manual of the Steam Engine and other Prime Movers* (London/Glasgow, 1859): 96.

(8) See Number 311 note (6).

(9) See Volume I: 261n.

Now consider the case of a body which conducts electricity – a mass of copper of the same form as the vessel; and let electricity be supplied at the upper surface, and carried off at the point corresponding to the hole, so that the current of electricity entering or escaping is everywhere proportional and parallel to the current of water. Then the electric-potential everywhere will be equal to ϕ, and its value is perfectly definite for a given problem, and problems can be constructed for which the solution can be found by our present methods.(10)

Let ψ be the electric-potential for the copper conductor; then, if the form of the vessel and the motions of the surface remain similar, the velocity-potential $\phi = \psi T$, where T is a function of the time.

If we assume $\psi = 0$ at the free surface, then, since ψ increases along every line of motion,(11) the value of ψ will be positive throughout the vessel, and will have its greatest values at the place of the issuing stream.

Let us first consider the *immediate* effect of opening the hole. In this case we may neglect the square of the velocity; and since at the orifice the pressure becomes zero, we have

$$p = 0 = \rho g\bar{z} - \rho\frac{d\bar{\phi}}{dt},$$

where $\bar{\phi}$ is the value of ϕ at the orifice. If $\bar{\psi}$ is the value of ψ at the orifice, then

$$\bar{\phi} = \bar{\psi}g\bar{z}t;$$

and if ψ is the value of ψ at any other point, then

$$\phi = \psi g\bar{z}t,$$

and

$$p = \rho gz - \rho g\bar{z}\frac{\psi}{\bar{\psi}}.$$

Hence the assertion that the pressure is everywhere diminished is correct, and the amount of diminution is everywhere proportional to ψ, the electric-potential of a conducting mass similar to the water in the vessel, of which the upper surface is maintained at potential zero and the orifice at potential unity, the sides being supposed coated with insulating material.

When the motion has become constant, we may neglect $\frac{d\phi}{dt}$, and we have

$$p = \rho gz - \tfrac{1}{2}\rho V^2,$$

where V is the velocity at any point. At the orifice, $V^2 = 2g\bar{z}$. It appears, therefore, that in this case also the pressure at every point is less than the hydrostatic pressure.

(10) On the hydrodynamic analogy for electricity see Number 223, and Volume I: 353–7, 367–9.

(11) See Numbers 223 esp. note (4) and 337.

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LETTER TO WILLIAM THOMSON

16 NOVEMBER 1869

From the original in the University Library, Cambridge(1)

Glenlair
Dalbeattie
16 Nov 1869

Dear Thomson

Mr Tatlock(2) tells me you want to know about the conductivity of liquids for heat. The last thing on the subject is Prof. F. Guthrie On the Thermal Resistance of Liquids (Proc. RS Jan. 21, 1869).(3) He states in his paper (I do not know if it is printed or to be printed)(4) previous results. His experimental methods seem very good. His chief defect is that he never seems to know what he is going to measure. He works at the Royal Institution and has been so impregnated with radiant heat and otherwise Tyndallized(5) that he describes the specific resistance of a liquid to be the ratio of the quantity of heat *arrested* by the liquid to that arrested by an equal thickness of water.

He states his object to be 'to determine the laws according to which heat travels by conduction through liquids' but he goes to work as if he wanted to find their absorption of radiant heat. The actual phenomena he observes are mainly phenomena of conduction.

He finds that heat gets through a millimeter of water in a minute very much better than any other liquid their resistances being(6)

Water	1	All solutions
Glycerine	3.84	of salts
Acetic acid glacial	8.38	increase
Alcohol	9.08	resistance
Oil of Turpentine	11.75	or diminish
Chloroform	12.10	conductivity
	&c.	

Of course mercury is not in this series. From my recollection of the paper the only results previously obtained were proofs that there is such a thing as true conduction of heat by liquids.

(1) ULC Add. MSS 7342, M 107. Previously published in Larmor, 'Origins': 741–2.

(2) John Tatlock, Thomson's assistant and amanuensis; see S. P. Thompson, *The Life of William Thomson Baron Kelvin of Largs*, 2 vols. (London, 1910), **2**: 595.

(3) Frederick Guthrie, 'On the thermal resistance of liquids', *Proc. Roy. Soc.*, **17** (1869): 234–6.

(4) Guthrie's paper was printed in *Phil. Trans.*, **159** (1869): 637–60.

(5) See Numbers 255 and 258.

(6) The values are taken from Guthrie's table of 'specific resistance'.

Perhaps D[r] Matthiessen could give you information. He knows what conduction means and he collects the properties of bodies,[(7)] and he will not tell you what he does not know.

For experiments on steady conduction an iron plate covered with copper on both sides might be useful as a measurer of the flow of heat. The two copper plates to be connected with a galvanometer. The galvanometer having a great resistance compared with the plate, its indications may be trusted to give the mean electromotive force over the plate or the mean flow of heat per square inch. The value of the galvanometer readings must be found by regular calorimetric methods.

The compound plate being a much better conductor than most of the substances to be tried and also probably thinner the differences of temperature in the other bodies may be found by more direct methods of thermometry.

With respect to stress in a medium arising from magnetism

$$\frac{d}{dx}p_{xx}+\frac{d}{dy}p_{yx}+\frac{d}{dz}p_{zx}$$

is the x-force on an element of the medium referred to unit of volume.[(8)] If in that element there is neither electric current nor magnetization $X = 0$. If there is electric currrent the right expression comes out. If there is magnetization the case is more difficult because of the double def[n] of force within a magnet.[(9)] If there is nothing but Ampère's currents[(10)] and if these are recognized as currents then all is easy, and

$$X = \alpha\rho-\beta w+\gamma v^{(11)}$$

(7) See Number 296 notes (8), (9) and (10).

(8) See the *Treatise*, **2**: 256 (§643), and Number 205 esp. note (14).

(9) Thomson's distinction between 'solenoidal' and 'lamellar' distributions of magnetism in his paper 'A mathematical theory of magnetism', *Phil. Trans.*, **141** (1851): 243–85, esp. 269–85 (= *Electrostatics and Magnetism*: 340–404). See the *Treatise*, **2**: 31–6 (§§407–16); Numbers 322 note (13) and 353 note (15); and Volume I: 256n, 257n, 260n, 323n.

(10) Ampère's theory of magnets as consisting of molecules within which electric currents circulate: see Numbers 322 note (13) and 410 esp. notes (19) and (26).

(11) Termed one of the 'Equations of Electromagnetic Force' in the *Treatise*, **2**: 257 (§643).

(12) See Thomson, 'A mathematical theory of magnetism': 250–6; 'an imaginary magnetic matter... may be conceived to represent the polarity of a magnet of any kind' (on 250).

where $\alpha\,\beta\,\gamma$ are components of magnetic force $u\,v\,w$ are components of currents ρ = density of imaginary magnetic matter[12]

$$4\pi u = \frac{d\gamma}{dy} - \frac{d\beta}{dz}^{(13)} \quad \&c \quad 4\pi\rho = \frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz}.^{(14)}$$

Yours truly
J. Clerk Maxwell

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(13) Maxwell's equations (E), the 'Equations of Electric Currents', in the *Treatise*, **2**: 231 (§607).

(14) On this expression see the *Treatise*, **2**: 256 (§643).

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LETTER TO PETER GUTHRIE TAIT

10 DECEMBER 1869

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
10 Dec 1869

Dr T′

I have never attempted to calculate the modifications in the forms of electric stream lines in a conducting plate due to the presence of a magnet of constant strength in as much as I want it but a vain conceit that a magnet has any just title to cause any deviations of the said stream lines from the paths prescribed to them as laid down in the laws of the late Dr G. S. Ohm.[2]

If the magnet can alter the conducting power of the plate either isotropically or in certain directions the stream lines will change. Also if the plate or magnet is free to move, they will move and so produce currents.

But if the magnet does not affect the quality of the plate (See Thomsons Electrodynamic qualities of Metals)[3] then the stream line, me judice, will be unaffected. If they are, glory over me.

If you want to draw the theoretical forms of stream lines and equipotentials in circular or sectorial plates &c I have done a few, & could write out instructions for your youths to do them by means of tracing paper.[4]

You have seen I suppose Kirchhoffs experimental tracing of the equipotential lines on a compound disk one semicircle copper and the other lead.[5]

The electro-kinetic energy of any system of currents is[6]

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(1) ULC Add. MSS 7655, I, b/15.

(2) Georg Simon Ohm, *Die galvanische Kettè, mathematisch bearbeitet* (Berlin, 1827).

(3) William Thomson, 'On the electro-dynamic qualities of metals', *Phil. Trans.*, **146** (1856): 649–751, esp. 736–51 ['Part V'] (= *Math. & Phys. Papers*, **2**: 307–27).

(4) See the *Treatise*, **1**: 239–40 (§§194–5) and Fig. XI appended to the volume; and see Number 340. See the paper by Tait's assistant William Robertson Smith, 'On the flow of electricity in conducting surfaces', *Proc. Roy. Soc. Edinb.*, **7** (1870): 79–99 (read 21 February 1870); and the *Treatise*, **1**: 149 (§123).

(5) Gustav Kirchhoff, 'Ueber den Durchgang eines elektrischen Stromes durch eine Ebene, insbesondere durch eine kreisförmige', *Ann. Phys.*, **64** (1854): 497–514, esp. 509 and Table V Fig. 3. See the *Treatise*, **1**: 367n (§316).

(6) Compare Maxwell's discussion of the theory of electric circuits in the *Treatise*, **2**: 206–8 (§§578–9) where he defines the 'Electrokinetic Energy of the system' as 'that part of the kinetic energy of the system which depends on squares and products of the strengths of the electric currents.' See Number 430.

$T = \frac{1}{2}\{L_1\gamma_1^2 + L_2\gamma_2^2 + \&c + 2M_{12}\gamma_1\gamma_2 + \&c\}$ where γ_1 γ_2 are the currents in various conductors and L M &c depend on the geometrical arrangement and are of the nature of lines. L_1 depends only on the size & shape of the conductor which carries γ_1 and not on its position. M_{12} depends on the size and shape of the system of two circuits carrying γ_1 & γ_2 &c.

The electromagnetic momentum of a current γ_1 is

$$\xi_1 = \frac{dT}{d\gamma_1} = L_1\gamma_1 + M_{12}\gamma_2 + \&c$$

and if the electromotive force in this circuit is η_1

$$\eta_1 = R_1\gamma_1 + \frac{d\xi_1}{dt}$$

where R_1 is the resistance of the circuit.

If there is no motion of circuits or variation of currents ξ_1 is constant and $\eta_1 = R_1\gamma_1$ or the current when steady is not affected by the presence of other steady currents but is determined by Ohm's Law.

Hence if the stream lines in a conducting plate be drawn when there is no magnet or neighbouring current they will be unaffected when the magnet exists.

For the original lines satisfy Ohm and the conditions of supply & demand, and if there is any change the new system must be the old + a system of reentering streams due to the magnet, which as we have seen does not exist. QED.

I am at the 4th of the 4 parts of my book namely Electrodynamics.(7) I have done the historical and theoretical part (of which you have here a tinkling of the symbols) and am now at Galvanometers and galvanometry(8) but am interrupted by Cambridge Examination.(9) I have still to write out ships magnetism(10) and telegraphy.(11)

I have just revised the paper on Frames and reciprocal figures for the R.S.E.(12) Jenkin has saved me much trouble.(13) I had arranged with W. P. Taylor the independent inventor of the method to give an account of the working of it from a draughtsmanlike point of view and I still expect his

(7) See Number 329.

(8) Compare Part IV chapter XV of the *Treatise* (on 'electromagnetic instruments'), **2**: 313–34 (§§707–29).

(9) Maxwell had been appointed Examiner for the Cambridge Mathematical Tripos in 1870; see *The Cambridge University Calendar for the Year 1870* (Cambridge, 1870): 483.

(10) See the *Treatise*, **2**: 70–73 (§441).

(11) See the Treatise, **1**: 374–87, esp. 383–4 (§332).

(12) See Number 334.

(13) See Number 334 esp. notes (5) and (8).

paper. But as soon as I have drawn a few pictures I will send you mine which opens up several prospects for an adventurous mathematician as e.g.(14)

If P_1 P_2 are the principal stresses at any point $x\,y$ of a plane sheet in equilibrium

$$\iint (P_1+P_2)\,dx\,dy \quad \text{and} \quad \iint P_1\,P_2\,dx\,dy$$

depend only on the external forces applied to the edge of the sheet and not on internal strains &c.

Let the resultant force on the edge of the sheet from O to any point P be R (in direction & magnitude) then the 1st integral is the work done on P in going the circuit of the edge, always acted on by R, and the 2nd integral is the area described by R turning on one end as a pivot.

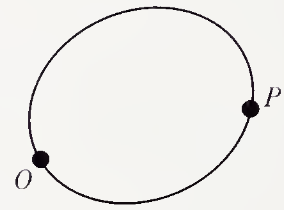

Figure 333,1

Yours
J. C. M.

(14) Compare J. Clerk Maxwell, 'On reciprocal figures, frames and diagrams of forces', *Trans. Roy. Soc. Edinb.*, **26** (1870): 1–40, esp. 27–31 (= *Scientific Papers*, **2**: 192–7).

ABSTRACT OF PAPER 'ON RECIPROCAL FIGURES, FRAMES, AND DIAGRAMS OF FORCES'

[17 DECEMBER 1869][1]

From the *Proceedings of the Royal Society of Edinburgh*[2]

ON RECIPROCAL FIGURES, FRAMES, AND DIAGRAMS OF FORCES.

By J. Clerk Maxwell, Esq., F.R.SS.L. & E.[3]

The reciprocal figures treated of in this paper are plane rectilinear figures, such that every line in one figure is perpendicular to the corresponding line in the other, and lines which meet in a point in one figure correspond to lines which form a closed polygon in the other.

By turning one of the figures round 90°, the corresponding lines become parallel, and are more easily recognised. The practical use of these figures depends on the proposition known as the 'Polygon of Forces'. If we suppose one of the reciprocal figures to represent a system of points acted on by tensions or pressures along the lines of the figure, then, if the forces which act along these lines are represented in magnitude, as they are in direction, by the corresponding lines of the other reciprocal figure, every point of the first figure will be in equilibrium. For the forces which act at that point are parallel and proportional to the sides of a polygon formed by the corresponding lines in the other figure.

In all cases, therefore, in which one of the figures represents a frame, or the skeleton of a structure which is in equilibrium under the action of pressures and tensions in its several pieces, the other figure represents a system of forces which would keep the frame in equilibrium; and, if the known data are sufficient to determine these forces, the reciprocal figure may be drawn so as to represent, on a selected scale, the actual values of all these forces.

In this way a practical method of determining the tensions and pressures in structures has been developed. The 'polygon of forces' has been long known. The application to polygonal frames, with a system of forces acting on the angles, and to several other cases, was made by Professor Rankine in his

(1) The date the paper was received by the Royal Society of Edinburgh. The paper was read on 7 February 1870: see note (2).

(2) *Proc. Roy. Soc. Edinb.*, **7** (1870): 53–6.

(3) Published in *Trans. Roy. Soc. Edinb.*, **26** (1870): 1–40 (= *Scientific Papers*, **2**: 161–207).

Applied Mechanics.[4] Mr W. P. Taylor, a practical draughtsman,[5] has independently worked out more extensive applications of the method. Starting from Professor Rankine's examples, I taught the method to the class of Applied Mechanics in King's College, London,[6] and published a short account of it in the 'Philosophical Magazine' for April 1864.[7] Professor Fleeming Jenkin, in a paper recently presented to the Society,[8] has fully explained the application of the method to the most important cases occurring in practice, and I believe that it has been found to have three important practical advantages. It is easily taught to any person who can use a ruler and scale. It is quite sufficiently accurate for all ordinary calculations, and is much more rapid than the trigonometrical method. When the figure is drawn the whole process remains visible, so that the accuracy of the drawing of any single line can be afterwards tested; and if any mistake has been made, the figure cannot be completed. Hence the verification of the process is much easier than that of a long series of arithmetical operations, including the use of trigonometric tables.

In the present paper I have endeavoured to develope the idea of reciprocal figures, to show its connection with the idea of reciprocal polars as given in pure mathematics,[9] and to extend it to figures in three dimensions, and to cases in which the stresses, instead of being along certain lines only, are distributed continuously throughout the interior of a solid body. In making this extension of the theory of reciprocal figures, I have been led to see the connection of this theory with that of the very important function introduced into the theory of stress in two dimensions by Mr Airy, in his paper 'On the Strains in the Interior of Beams' (Phil. Trans. 1863).[10]

If a plane sheet is in equilibrium under the action of internal stress of any kind, then a quantity, which we shall call Airy's Function of Stress,[11] can always be found, which has the following properties.

(4) W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 137–40.

(5) See Fleeming Jenkin, 'On the practical application of reciprocal figures to the calculation of strains on framework', *Trans. Roy. Soc. Edinb.*, **25** (1869): 441–7, esp. 441.

(6) Number 203.

(7) J. Clerk Maxwell, 'On reciprocal figures and diagrams of forces', *Phil. Mag.*, ser. 4, **27** (1864): 250–61 (= *Scientific Papers*, **1**: 514–25). See also Number 273.

(8) See note (5).

(9) See Number 273 note (2).

(10) George Biddell Airy, 'On the strains in the interior of beams', *Phil. Trans.*, **153** (1863): 49–79.

(11) See Maxwell's discussion of this function, in his referee's report on Airy's paper: Number 205. See also his papers 'On reciprocal figures, frames, and diagrams of forces': 27–31 (= *Scientific Papers*, **2**: 192–7), and 'On reciprocal diagrams in space, and their relation to Airy's function of stress', *Proceedings of the London Mathematical Society*, **2** (1868): 58–60 (= *Scientific Papers*, **2**: 102–4).

At each point of the sheet let a perpendicular be erected proportional to the function of stress at that point, so that the extremities of such perpendiculars lie in a certain surface, which we may call the surface of stress. In the case of a plane frame the surface of stress is a plane-faced polyhedron, of which the frame is the projection. On another plane, parallel to the sheet, let a perpendicular be erected of height unity, and from the extremity of this perpendicular let a line be drawn normal to the tangent plane at a point of the surface of stress, and meeting the plane at a certain point.

Thus, if points be taken in the plane sheet, corresponding points may be found by this process in the other plane, and if both points are supposed to move, two corresponding lines will be drawn, which have the following property:– The resultant of the whole stress exerted by the part of the sheet on the right hand side of the line on the left hand side, is represented in direction and magnitude by the line joining the extremities of the corresponding line in the other figure. In the case of a plane frame, the corresponding figure is the reciprocal diagram described above.

From this property the whole theory of the distribution of stress in equilibrium in two dimensions may be deduced.

In the most general case of three dimensions, we must use three such functions, and the method becomes cumbrous. I have, however, used these functions in forming equations of equilibrium of elastic solids, in which the stresses are considered as the quantities to be determined, instead of the displacements, as in the ordinary form.

These equations are especially useful in the cases in which we wish to determine the stresses in uniform beams. The distribution of stress in such cases is determined, as in all other cases, by the elastic yielding of the material; but if this yielding is small and the beam uniform, the stress at any point will be the same, whatever be the actual value of the elasticity of the substance.

Hence the coefficients of elasticity disappear from the ultimate value of the stresses.

In this way I have obtained values for the stresses in a beam supported in a specified way, which differ only by small quantities from the values obtained by Mr Airy, by a method involving certain assumptions, which were introduced in order to avoid the consideration of elastic yielding.

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DRAFTS RELATING TO PART IV OF THE *TREATISE*

circa LATE 1869[1]

From the originals in the University Library, Cambridge[2]

[1] MAGNETIC POTENTIAL OF A LINEAR ELECTRIC CIRCUIT[3]

The magnetic potential at any point of a field of magnetic force is measured by the work which must be done in order to bring a unit magnetic pole from an infinite distance to that point by a path subject to the condition that it shall not pass through a certain finite surface bounded by the circuit and called the diaphragm.

It may be shown from the principle of the conservation of energy that a potential must exist when the magnetic force is due to a permanent magnet. If we explore a limited portion of the field of force by means of a magnet we cannot distinguish between the case in which the force arises from a permanent magnet and that in which it arises from an electric current, the magnet and the current being understood to be outside the region which we explore. We have therefore good reason to believe that the magnetic force arising from electric currents has, at least in singly connected regions of space through which the current does not pass, a magnetic potential.

In the case of a ⟨plane⟩ ⌊circular⌋ circuit the magnetic potential at all points in the plane of the circle outside the circuit is zero. For if the potential at such a point were P then by turning the whole system 180° about the line joining the given point with the centre we shall find that P is now the potential at the point due to the same circuit with the current flowing in the reverse direction. But since every effect of a current is reversed when the current is reversed, the potential must now be $-P$. Hence, since $P=-P$, the only possible value of P is zero.

Any number of contiguous[a] circuits having equal currents flowing round them in the same direction are in all respects equivalent to the circuit which bounds the whole with a current of the same strength flowing round it.

(a) {Maxwell} conterminous.

(1) This date is conjectural, but the MSS were very likely written after the outline of the contents of the *Treatise* (Number 329, and for dating see note (1)), and at about the same time as Number 333.

(2) ULC Add. MSS 7655, V, c/24, 27(b).

(3) ULC Add. MSS 7655, V, c/24. Compare the *Treatise*, 2: 130–2 (§§480–4).

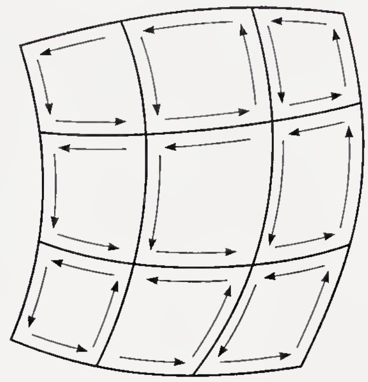

Figure 335,1

For in the linear conductor which forms the common boundary of two contiguous circuits equal currents are flowing in opposite directions and this is in every respect equivalent to no current at all.

Hence the only currents which are not neutralized are those which flow in those parts of the circuits which are not contiguous to other circuits, that is, in the bounding circuit of the whole system.

The effect of a plane circuit at a point whose distance is very great compared with the dimensions of the circuit is proportional to the strength of the current multiplied into the area of the circuit.[4]

For if a number of equal and similar circuits are in the same plane and so near each other that their distances and directions from the given point are sensibly the same the effect of each circuit at the given point will be the same and the effect of the whole system will be proportional to the number of circuits and independent of the mode in which they are arranged.

Hence if the circuits are placed in contact the effect of the circuit which bounds them all will be proportional to its area and independent of its shape.

But we have seen that the effect of a small plane circuit on a distant point depends on its area and not on its shape. Hence the potential due to a small plane area of any form at a distant point in its own plane is zero.

Also since for any plane circuit we may substitute a number of small contiguous circuits in its plane having the same external boundary it follows that the potential due to any plane circuit at any point of the plane outside the circuit is zero.

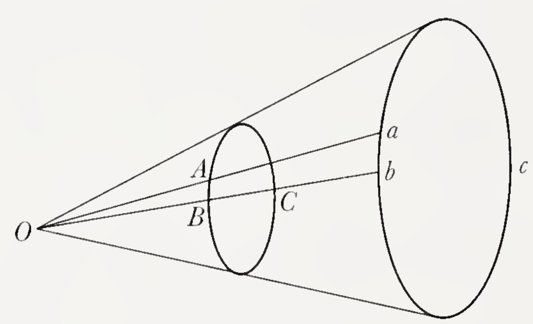

Figure 335,2

If one circuit is the projection of another with respect to the given point the two circuits have the same potential at that point.

For let $A\ B$ be the two neighbouring points on one circuit and $a\ b$ the corresponding points of the other then the points $A\ a\ b\ B$ lie in the same plane with O. Hence the potential due to the circuit $A\ a\ b\ B$ at the point O is zero.

Now consider the circuit $a\ b\ c$ together with all the circuits lying in the conical surface of which O is the vertex and bounded by the circuit $A\ B\ C$ on the one side and $a\ b\ c$ on the other. These circuits are contiguous and the bounding surface of the system is $a\ b\ c$.

(4) See the *Treatise*, **2**: 131 (§482).

The potential at O due to $a\ b\ c$ is therefore equal to the sum of the potentials of all the circuits of the system formed by $A\ B\ C$ and the circuits in the conical surface. But the potential due to those in the conical surface is zero. Hence the potential at O due to $a\ b\ c$ is equal to that due to $A\ B\ C$.

If one of these circuits, say $A\ B\ C$ lies on a spherical surface of radius unity whose centre is O each of the small equal circuits lying on the spherical surface into which it may be divided will be similarly situated with respect to O and the potentials at O due to these circuits will be equal. Hence the potential of the whole circuit at O may be measured by the area of the spherical surface multiplied into the strength of the current.

But the potential due to any circuit is equal to that due to its projection on the spherical surface of radius unity and centre O. Hence the potential at O due to any circuit is equal to the area cut off from the spherical surface of radius unity and centre O by the projection of the circuit with respect to the point O [...].

[2] ON THE CONDUCTION OF ELECTRICITY IN TWO DIMENSIONS(5)

We shall suppose the conduction of electric currents to take place in a thin sheet of conducting matter bounded on both sides by non-conductors. The sheet may be plane or curved and the conductivity may be different at different parts of the sheet or it may be different for different directions at the same point.

Lines of Flow

Let O be any point in the sheet which we may take as origin and let P be any other point and let OP be joined by any curve and let the quantity of electricity which in unit of time crosses the curve OP from left to right be denoted by ϕ.

If any other curve be drawn between O and P and if in the space included between the two curves no electricity be supplied to or removed from the sheet then the quantity of electricity which crosses the second curve must be equal to that which crosses the first in the same time and the value of ϕ will be independent of the form of the curve drawn between O and P, or in other words ϕ is a function of the position of the point P.

Similarly if ϕ' is the value of ϕ corresponding to the point of P' then if a curve PP' be drawn the quantity of electricity which crosses PP' in unit of time from left to right is $\phi' - \phi$.

(5) ULC Add. MSS 7655, V, c/27(b). There are preliminary drafts in V, c/27(a). Compare the *Treatise*, 2: 259–61 (§§647–51).

If a system of lines be drawn on the surface for each of which ϕ is constant and if the consecutive values of ϕ differ by unity (the unit being taken as small as we please) then the system of lines may be called lines of flow and the channels between them unit channels.

If points on the surface are defined by any system of coordinates whose symbols are x and y and if the quantity of electricity which crosses the element dy in unit of time in the direction in which x increases is $u\,dy$ we should have

$$u\,dy = \frac{d\phi}{dy}dy$$

or

$$u = \frac{d\phi}{dy}.$$

If $v\,dx$ is the quantity which crosses dx in the direction in which y increases

$$v = -\frac{d\phi}{dx}.$$

u and v must evidently satisfy the equation of continuity

$$\frac{du}{dx}+\frac{dv}{dy} = 0.$$

u and v may be called the components of the current in the directions x and of y.[(6)]

If at any point of the surface electricity is conveyed to the sheet or removed from it the function ϕ may have an infinite series of values because the line drawn from O to P may pass any number of times round the point either in the positive or the negative direction, and for each circuit the value of ϕ will be increased or diminished by the quantity of electricity flowing from or to the point in unit of time.

The function ϕ may therefore have an infinite series of values but its differential coefficients, which determine the current at any point have determinate and single values.

Equipotential Lines

Let ψ be the potential of the electricity at any point P of the surface and ψ' the value of ψ at any other point P' then if a wire be made to touch the surface at P and P' it will experience an electromotive force $= \psi-\psi'$.

If ψ and ψ' are equal there will be no electromotive force and no current will be formed in the wire so that by determining the points P' at which the

(6) For the hydrodynamical analogy see Volume I: 295–6.

second extremity of the wire must be placed so that there shall be no current we may trace out the equipotential line corresponding to P. If ψ and ψ' are not equal there will be a current which will interfere with the electrical condition of the sheet.

If a system of lines be drawn on the surface for each of which ψ is constant, and if the consecutive values of ψ differ by unity (unity being taken as small as we please), then the system of lines may be called Equipotential Lines and the intervals between them equipotential strata.

If points on the surface are defined by the coordinates x and y then we may define the electromotive force at any point in the direction of x as the electromotive force on the element dx divided by the length of that element. Calling X Y the components of the electromotive force at the point (x, y)

$$X = \frac{d\psi}{dx}$$

$$Y = \frac{d\psi}{dy}.$$

Equations of Conduction

At any point of the surface, the components of the electromotive force and those of the electric current are so related that the one pair may be expressed as linear functions of the other.

The most general equations connecting them may be written

$$X = Au + Bv$$

$$Y = Cu + Dv.$$

If we assume a system of rectangular axes x', y', of which the axis of x' makes an angle of θ with that of x where $\tan 2\theta = \dfrac{B+C}{A-D}$ and if we make

$$E^2 = AD - BC \quad \text{and} \quad F^2 = (A-D)^2 + (B+C)^2 \quad \text{and} \quad k^2 = \frac{A+D+F}{A+D-F}$$

and $\sin\chi = \dfrac{C-B}{2E}$ then if we increase the coordinates x' in the ratio k we shall have in the new system if R' is the resultant of X' and Y' and V' that of u' and v' and if the directions of R' and V' are α' & β'

$$R' = EV' \quad \alpha' = \beta' + \chi$$

or the reduced electromotive force has a constant ratio to the reduced current and makes a constant angle χ with it.

The angle χ affords a measure of a rotatory property in the conduction of the medium. The mathematical character of this property has been pointed out by Professor Stokes in his memoir on the conduction of heat in Crystals[7] and by Prof Sir W Thomson in his memoir on Thermodynamics[8] who has shown how to construct a solid having this rotatory property with respect to the connexion between the distribution of heat and the electric currents produced by it.[9]

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(7) G. G. Stokes, 'On the conduction of heat in crystals', *Camb. & Dubl. Math. J.*, **6** (1851): 215–38 (= *Papers*, **3**: 203–27).

(8) William Thomson, 'On the dynamical theory of heat. Part V. Thermo-electric currents', *Trans. Roy. Soc. Edinb.*, **21** (1854): 123–71, esp. 164–7 (= *Math. & Phys. Papers*, **1**: 232–91).

(9) Compare Maxwell's discussion in 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83, esp. 39–40 (= *Scientific Papers*, **1**: 171–2); and see Volume I: 360.

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FROM A LETTER TO KATHERINE MARY CLERK MAXWELL

3 JANUARY 1870

From Campbell and Garnett, *Life of Maxwell*(1)

There is a tradition in Trinity that when I was here I discovered a method of throwing a cat so as not to light on its feet, and that I used to throw cats out of windows. I had to explain that the proper object of research was to find how quick the cat would turn round, and that the proper method was to let the cat drop on a table or bed from about two inches, and that even then the cat lights on her feet.(2)

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(1) *Life of Maxwell*: 499.

(2) On Maxwell's and Stokes' interest in 'cat-turning' (the motion of the cat being in accordance with the conservation of angular momentum, on which see Volume I: 499–501), see Larmor, *Correspondence*, **1**: 32. The subject was subsequently investigated: see É. J. Marey, 'Des mouvements que certains animaux exécutent pour retomber sur les pieds, lorsqu'ils sont précipités d'un lieu élevé', *Comptes Rendus*, **119** (1894): 714–17; É. Guyou, 'Note relative à la communication de M. Marey', *ibid.*: 717–18; Maurice Lévy, 'Observations sur le principe des aires', *ibid.*: 718–21.

REPORT ON A PAPER BY WILLIAM JOHN MACQUORN RANKINE ON FLUID MOTION[1]

2 MARCH 1870

From the original in the Library of the Royal Society, London[2]

REPORT ON PROF^r RANKINES PAPER ON THE MATHEMATICAL THEORY OF STREAM-LINES[3]

A stream line is defined as the line traced by a particle in a current of fluid.[4] It is therefore to be distinguished from what has been elsewhere defined as a 'line of fluid motion', namely a line which coincides throughout its length with the direction of fluid motion at a given instant.[5] In cases of variable motion the lines of fluid motion are different from the stream lines but each stream line at any instant touches one of the lines of fluid motion at that instant. In cases of steady motion which alone are treated of in this paper the two kinds of lines coincide.[6]

The paper begins with a general discussion of stream lines considered as the

(1) See also Number 223.

(2) Royal Society, *Referees' Reports*, **7**: 55 *bis*.

(3) W. J. M. Rankine, 'On the mathematical theory of stream-lines, especially those with four foci and upwards', *Phil. Trans.*, **161** (1871): 267–306. The paper was received by the Royal Society on 1 January 1870, with a supplement received on 8 January 1870, and read on 10 February 1870; see the abstract in *Proc. Roy. Soc.*, **18** (1870): 207–9.

(4) As defined by Rankine, 'On the mathematical theory of stream-lines': 267. See also W. J. M. Rankine, 'Summary of the properties of certain stream-lines', *Phil. Mag.*, ser. 4, **28** (1864): 282–8; and his 'Supplement to a paper on stream-lines', *ibid.*, **29** (1865): 25–8. The curves which Rankine here terms 'stream-lines' he had formerly denoted by the term 'water-lines'; see W. J. M. Rankine, 'On plane water-lines in two dimensions', *Phil. Trans.*, **154** (1864): 369–91, and Number 223.

(5) Maxwell had used the term 'lines of fluid motion' in this sense (see note (6)) in his paper 'On Faraday's lines of force', *Trans. Camb. Phil. Soc.*, **10** (1856): 27–83, esp. 30–42 (= *Scientific Papers*, **1**: 160–75). See Volume I: 337–50, 357–61. For the distinction (but with different terminology), see G. G. Stokes, 'Remarks on a paper by Professor Challis, "On the analytical condition of the rectilinear motion of fluids"', *Phil. Mag.*, ser. 3, **21** (1842): 297–300, on 297; 'I shall call the path of a particle of a fluid in space a *line of motion*, and a line traced at a given instant from point to point in the direction of the motion a *line of direction*.'

(6) Compare Maxwell, 'On Faraday's line of force': 31 (= *Scientific Papers*, **1**: 160); 'Lines drawn... that their direction always indicates the direction of fluid motion are called *lines of fluid motion*. If the motion of the fluid be... *steady motion*... these curves will represent the paths of individual particles of the fluid.'

intersections of two series of 'stream-line-surfaces'.(7) If ψ and χ are functions which have constant values for each one of these surfaces and if the consecutive surfaces to these are denoted by $\psi + d\psi$ and $\chi + d\chi$ then these four surfaces will cut off a quadrilateral tube of fluid motion(8) the flow across any section of which is constant throughout its length and is therefore a quantity of the form

$$f(\psi, \chi)\, d\psi\, d\chi.$$

Now any function of ψ and χ is also a stream line function. If therefore we take a new function χ' of ψ and χ such that

$$d\chi' = f(\psi, \chi)\, d\chi$$

then the flow through the tube formed by the intersections of the ψ and χ' surfaces will be simply $d\psi\, d\chi'$.

Whatever be the form of the functions ψ and χ' a case of fluid motion such that the flow through any surface S is represented by the surface integral $\iint d\psi\, d\chi$ extended over the surface is consistent with the incompressibility of the fluid(9) though it may be inconsistent with the dynamics of fluid motion.

In the important class of cases in which a 'velocity potential'(10) exists that is in all cases of irrotational motion the stream line surfaces must be

(7) See Rankine, 'On the mathematical theory of stream-lines': 267; 'if the figure of a ship is such that the particles of water glide smoothly over her skin, that figure is a *stream-line* surface'. In a 'Preliminary Report' to the Royal Society's Committee of Papers (Royal Society, *Referees' Reports*, **7**: 55), Stokes criticised this statement: 'Although not expressly stated, it appears to be implied... that the author conceived certain surfaces, but not surfaces in general, to be possible stream-line surfaces... that consequently the investigation of even a very particular class of surfaces which are known to be stream-line surfaces becomes a matter of importance.... Now I contend that the view I have put forward is not correct; that on the contrary the surface of a solid of perfectly arbitrary form... is a stream-line surface... for Nature, as Fresnel remarks, is not embarrassed by difficulties of analysis, and there is nothing to show that a surface, the stream-lines corresponding to which are expressible by an equation of simple form, has an advantage over a surface, the expression of the stream-lines corresponding to which surpasses the power of our analysis.' In a note dated December 1870, appended to this statement in his paper ('On the mathematical theory of stream-lines': 267n), Rankine responded by stating: 'although every surface is a possible stream-line surface, the surface of a ship is not even approximately an actual stream-line surface unless it is such that she does not drag along with her a mass of eddies'.

(8) The term introduced by Maxwell in 'On Faraday's lines of force': 31–5 (= *Scientific Papers*, **1**: 160–5). See Volume I: 338–40. In a note dated June 1871 ('On the mathematical theory of stream-lines': 270n), Rankine referred to Maxwell's term.

(9) Compare Maxwell's non-analytic formulation in 'On Faraday's lines of force': 32–3 (= *Scientific Papers*, **1**: 162–3); and see Volume I: 340.

(10) Helmholtz's term; see his 'Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen', *Journal für die reine und angewandte Mathematik*, **55** (1858): 25–55, esp. 25: and the translation of the paper (by P. G. Tait) 'On the integrals of the hydrodynamical equations, which express vortex-motion', *Phil. Mag.*, ser. 4, **33** (1867): 485–512, on 485. See Number 311 esp. note (6).

perpendicular to the equipotential surfaces[11] the conditions of which are given by the author.

It will be seen from this part of the paper that the general theory of fluid motion is by no means simplified when approached through the theory of stream-line-surfaces. It is in the study of particular cases that these surfaces give us valuable assistance.

The direct problem of finding two systems of stream line surfaces such that the surface of a given ship may be one of them and also such that the relative motion of the water at a distance from the ship is uniform is always capable of a final and single solution, but it generally lies beyond the reach of any known mathematical methods.

Hence we attempt the inverse problem of finding from two given systems of stream line surfaces corresponding to a possible mode of fluid motion what form a ship may have so as to produce this mode of motion in the water.[12]

This problem is comparatively easy and its solution is always capable of expression either in symbols or by diagrams.

By acquiring a knowledge of the different forms of stream-line-surfaces due to different hypotheses as to the nature of the fluid motion and by studying the changes in these forms due to changes in the hypotheses, Prof^r Rankine and those who use his methods hope to be able to design the lines of ships so that the motion of the water near them may be of the kind most favourable to the progress of the ship.

Hence what we have called the inverse problem in hydrodynamics is the leading problem in shipbuilding. The Author then proceeds as in former papers[13] to show how to draw systems of stream lines in two dimensions and also stream surfaces of revolution in three dimensions.

{ With respect to the stream lines in *two* dimensions the most important *theoretical* step has been made by C. Neumann in a paper on the integration of $\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}=0$ Crelle 1861.[14] This depends on the theory of Conjugate Functions and their transformations.[15] }

(11) See Number 223, esp. note (14).

(12) Compare Stokes' comments in his reports on Rankine's paper (notes (7) and (22)).

(13) See Rankine, 'On plane water-lines in two dimensions', and Number 223 esp. note (15); and Rankine, 'Elementary demonstrations of principles relating to stream lines', *The Engineer*, **26** (1868): 285–6.

(14) Carl Neumann, 'Ueber die Integration der partiellen Differential-gleichung: $\frac{\partial^2\phi}{\partial x^2}+\frac{\partial^2\phi}{\partial y^2}=0$', *Journal für die reine und angewandte Mathematik*, **59** (1861): 335–66. See Number 321.

(15) For further discussion see the *Treatise*, **1**: 234–5 (§190); and Number 303 note (3).

I am not aware that any stream lines in 3 dimensions have been investigated except plane stream lines (such as are here given) and the intersections of confocal surfaces of the second order.

In the authors former papers the cases of fluid motion were those compounded of

1st the general motion astern of the whole sea
2nd a motion of divergence from a place in the fore part of the ship
3rd a motion of convergence to a place in the hinder part of the ship.

The ship shapes derived from this hypothesis were named Oogenous Neoids.(16) They were all rather bluff both before and behind and to make them finer other stream lines called Lissoneoids(17) were selected and fastened together in a somewhat discontinuous manner so as to have a sharp angle at both ends.

Now if we wish to construct a case of fluid motion which shall give us a Neoid with sharp ends we must suppose not a single pair of foci of divergence and convergence but a continuous line of foci close to the cutwater, the density along the central line depending on an inverse fractional power of the distance from the point of the cutwater.

From this it will be seen that the foci of most disturbance are very close to the surface of the ship and therefore the suddenness of the disturbance of the water will be greater with an angular end than with one which is rounded.

A result of this kind seems to have been actually obtained by Mr Froude.(18)

This suddenness is avoided by making the internal foci finite in value and at a finite distance from the surface. The Author has used, besides his two original foci, two subsidiary ones before and behind them the effect of which is to give the neoid a longer and a leaner figure near the extremities but to preserve the roundness of the actual extremities.

The shape of the ship is therefore more manageable and by a little more labour which would be willingly undertaken by those who care for shipbuilding any number of foci might be introduced in the places where theory indicates that they would be useful for checking the various wave motions ⟨by⟩ which the power of the engines is wasted. This is pointed out in §19 of the paper.

The author afterwards applies Greens Theorem(19) to the calculation of the

(16) See Number 223 note (17).

(17) See Number 223 note (19).

(18) See William Froude's 'explanations' appended to the 'Report of a Committee ...appointed to report on the state of existing knowledge on the stability, propulsion and sea-going qualities of ships...', *Report of the Twenty-ninth Meeting of the British Association for the Advancement of Science* (London, 1870): 10–47, esp. 43–7 and Plate I.

(19) George Green, *An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism* (Nottingham, 1828): 10; and see *Treatise*, **1**: 108–13 (§100).

energy of the fluid motion. He shows however that for practical purposes the main thing is to prevent the dissipation of energy which is of two kinds.
1st Waves are formed and propagated to a distance carrying off energy depending on their mass and height. This would take place even in a perfect fluid provided the fluid has a free surface.
2nd On account of the viscosity of the fluid rotational motion is set up near the surface and eddies are formed, the energy of which is far greater than that lost by the direct effect of viscosity. These eddies, by Helmholtz' principle,(20) move with the fluid of which they exist, and finally arrange themselves in the wake of the vessel, which is a stream moving behind the vessel, and in the same direction, and such that its central parts move faster than its edges. The author shows that the layer of fluid which is filled with eddies increases in thickness from stem to stern and that $\frac{3}{4}$ of the energy of the wake is in the form of eddies and $\frac{1}{4}$ in that of forward motion. The practical advice to shipbuilders to which the paper leads is
1st to observe the position of the principal waves relatively to an existing ship, and in designing a new one of the same kind to arrange the foci of divergence and convergence so as to check the formation of these waves, or to neutralize them by waves of opposite sign.
2nd To make the variation of velocity of the water in gliding along the water lines as gradual as possible, and thus to reduce to a minimum the formation of eddies.
3rd To deprive, according to Mr Froudes suggestion,(21) the wave as much as possible of its motion so far as this can be done by any arrangement of the propeller. (In screw propellers working in the wake a 'negative slip' has been often observed.)

On the whole I consider the paper a valuable one and worthy of a place in the Transactions, and this chiefly because, in the words of the paper itself, it 'may prove useful in deducing general principles from the data of experiment and observation, and in suggesting plans for further research'.(22)

J. Clerk Maxwell
Glenlair 2nd March 1870

(20) Helmholtz, 'Über Integrale der hydrodynamischen Gleichungen': 33–7; 'On the integrals of the hydrodynamical equations': 491–4.

(21) See Rankine, 'On the mathematical theory of stream-lines': 301.

(22) Rankine, 'On the mathematical theory of stream-lines': 303. In a final report on Rankine's paper, dated 19 January 1871 (Royal Society, *Referees' Reports*, **7**: 56), Stokes wrote: 'I had a little hesitation as to recommending the Committee of papers about this paper. It seems to me that its length is hardly justified by its importance. The first part in which the mathematical reasoning is exact contains little that is new except the following out in detail of

the theoretical motion of a perfect fluid by means of a synthetical solution in which the forms of ships are imitated to a certain extent by curves drawn in accordance with such solution.... The latter part of the paper, into which I have not fully entered, is devoted to a consideration of the effect of waves, i.e. on the resistance. The reasoning in this part of the paper is confessedly probable only and accordingly more or less precarious. To refuse to entertain an investigation on this subject unless it were more exact would be I fear to postpone the question Sine die.... There is no change which I should recommend the Committee to make a condition of printing.'

REPORT ON A PAPER BY WILLIAM JOHN MACQUORN RANKINE ON THE THERMODYNAMIC THEORY OF WAVES

26 MARCH 1870

From the original in the Library of the Royal Society, London(1)

REPORT ON PROF^r RANKINES PAPER 'ON THE THERMODYNAMIC THEORY OF WAVES OF FINITE LONGITUDINAL DISTURBANCES'(2)

by J. Clerk Maxwell

In this paper the author investigates the conditions of the propagation of waves of permanent type,(3) and certain properties of the change of type when these conditions are not fulfilled. The method which he has employed differs from the ordinary methods of hydrodynamics with respect to the variables in terms of which the different quantities are expressed.

The velocity of any point of a rigid body is easily conceived and defined. If we suppose a fluid to consist of individual particles it is just as easy to conceive the velocity of any one of them, and if we also assume that the velocity of a particle, instead of being a function of the time, the form of which for different particles is not capable of expression by any single formula, is a continuous function of the coordinates of the particle, and of the time, then we may form the conception of the velocity of a fluid at any point.

But if we have to deal with the flow of heat or of electricity, or even with the flow of fluids composed of molecules in a state of agitation, we have to adopt a totally different conception of the flux. Velocity is measured by the distance described in unit of time but in the case of heat or electricity we have no means of forming any idea of this distance. We therefore measure the flux by the quantity which crosses unit of area in unit of time, passing from the negative to the positive side of the surface.

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(1) Royal Society, *Referees' Reports*, **7**: 53.

(2) W. J. M. Rankine, 'On the thermodynamic theory of waves of finite longitudinal disturbance', *Phil. Trans.*, **160** (1870): 277–88. The paper was received by the Royal Society on 13 August 1869, with a supplement received on 1 October 1869, and read on 16 December 1869; see *Proc. Roy. Soc.*, **18** (1869): 122, and (abstract) *ibid.*: 80–3.

(3) See Rankine, 'On the thermodynamic theory of waves': 277; 'the word *type* being used to denote the relation between the extent of disturbance at a given instant of a set of particles and their respective undisturbed positions'.

The idea of velocity implies the following a particle in its course. That of flux implies the measurement of the quantity which passes a given surface.

In Prof[r] Rankine's paper, the flux through a plane is called the somatic velocity, and, as in the case of waves the plane is supposed to move with the velocity of the wave, and the motion of the fluid is of less consideration, the flux with its sign reversed is called the somatic velocity *of the plane relative to the fluid*.

If the fluid is continuous, the linear velocity of the fluid relative to the plane is the product of the flux by the bulkiness of the fluid. The bulkiness is the inverse of the density, being the volume of unit of mass. In former papers of the author it is called the *volume*,(4) and I think that the word *rarity*, as the strict inverse of density, might be found as convenient as *bulkiness*, just as it is sometimes convenient to consider slowness instead of velocity & resistance instead of conductivity.

The author then considers the propagation of a plane fronted wave of longitudinal displacement, and selects a cylindrical portion of unit of area for examination. A plane is conceived to move through the fluid with somatic velocity m and the position of any other plane is defined by the mass, μ, of fluid between it and the first plane. Everything is then a function of μ and the time.

When the wave is of permanent type, and the first plane moves with the wave-velocity the pressure density &c will be functions of μ only and if we consider two planes both moving with somatic velocity m they will remain at equal distance always.

The author then considers the momentum of the fluid between these planes. The total mass is constant, though the particles are changing. A quantity m of fluid enters the space with a certain velocity, and an equal quantity of it leaves with a certain other velocity. The excess of momentum can only be produced by difference of pressure before and behind. From this consideration an equation is found by elementary reasoning which is a first integral of the ordinary hydrodynamical equation and leads to a determination of the velocity of a wave of permanent type *assuming such a wave to be possible*.

If m is the somatic velocity of the wave p the pressure and s the velocity at any point then

$$m^2 = -\frac{dp}{ds}$$

(4) See W. J. M. Rankine, 'On the thermo-dynamic theory of steam engines with dry saturated steam, and its application to practice', *Phil. Trans.*, **149** (1859): 177–92, esp. 182.

and the condition of permanent type is that $\frac{dp}{ds}$ must be constant or that the pressure is a linear function of the volume

$$p = P - m^2(s-S).^{(5)}$$

Now in actual bodies the pressure is a function of the volume and temperature and in the above equation $\frac{dp}{ds}$ means the ratio of the increment of pressure to the increment of volume during the actual changes of pressure and volume which the substance undergoes as the wave is propagated through it. In most substances the diagram of volume and pressure gives curves convex to the origin both when the temperature is constant and when there is no communication of heat. For waves of permanent type the line should be straight. Hence in most real cases $\frac{d^2p}{ds^2}$ is positive.

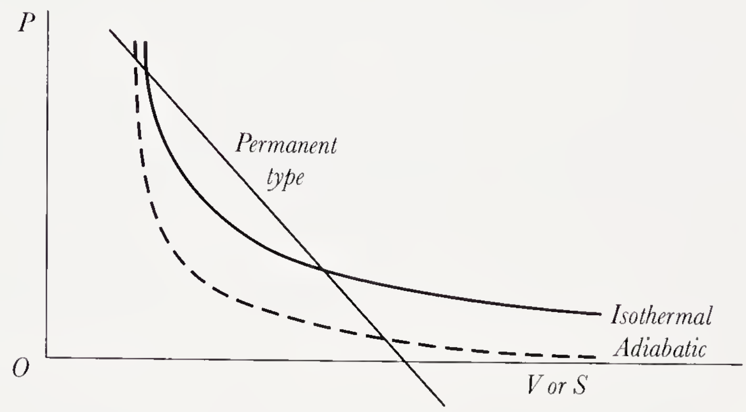

Figure 338,1

The author therefore endeavours to find under what circumstances $\frac{dp}{ds}$ can be made constant during the propagation of the wave by the communication of heat from the neighbouring parts by conduction.

It appears to me that up to this point the paper is characterized by a degree of simplicity, accuracy and distinctness[(6)] which is not so well maintained afterwards. The investigation of the condition of permanent type is very original and the different ideas are expressed so as to be readily appropriated by the reader. The ordinary theoretical elastic string, obeying Hookes Law 'Ut Tensio sic Vis'[(7)] would be, if it could exist, a perfect example of a body transmitting waves of permanent type with constant somatic velocity.

(5) See Rankine, 'On the thermodynamic theory of waves': 278; P is the longitudinal pressure, S the 'bulkiness' of the fluid in the undisturbed state.

(6) Reviewing Rankine's paper in a letter to Stokes of 7 March 1870 (Royal Society, *Referees' Reports*, 7: 51; in Wilson, *Stokes–Kelvin Correspondence*, 2: 343–5), William Thomson agreed that the 'simple elementary method by which he investigates the condition for sustained uniformity of type is in my opinion very valuable.'

(7) See Volume I: 136n, 160.

But if $\dfrac{dp}{ds}$ is to be kept constant during the compression or rarefaction[8] by communication of heat from the neighbouring parts of the substance, the heat must pass from hotter to colder parts and whenever this is the case, energy is dissipated and the wave as it goes on loses its energy whether of motion or of elasticity, leaving behind it an increased temperature in the substance.

I am therefore (under correction) of opinion that if the author would work out his theory of permanence of type secured by conduction of heat, he would find that the type is not really permanent unless the substance is left in a different state after the wave has passed it, in which case there must be a constant expenditure of energy by a piston in the tube in keeping up the wave.

The theory of waves in which there is no conduction of heat is of great importance and seems to be given very well arriving at the ordinary result.[9] Prof^r^ Stokes has shown that in the case of sound-waves the effect of conductivity must be insensible.[10]

The author next considers variation of type.[11]

He considers one plane to move in the undisturbed substance with somatic velocity m and another plane to move in the wave itself with the same somatic velocity so as to include between it and the first plane the constant mass μ.

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(8) In his letter to Stokes of 7 March 1870 Thomson criticised Rankine's assumption 'that a pulse consisting of a sudden rarefaction as well as a pulse of sudden condensation is of permanent type', arguing that 'the permanence of a pulse of sudden rarefaction is of the same character as the permanence... of unstable equilibrium.' In a note dated 1 August 1870, appended to 'On the thermodynamic theory of waves': 278n, Rankine acknowledges Thomson's correction.

(9) See Rankine, 'On the thermodynamic theory of waves': 282–3, where he derives 'Laplace's well-known law of the propagation of sound'.

(10) G. G. Stokes, 'On a difficulty in the theory of sound', *Phil. Mag.*, ser. 3, **33** (1848): 349–56, esp. 353–6, on the generation of a 'surface of discontinuity' (sudden compression followed by gradual dilatation) by sound waves, a 'queer kind of motion' as he described it in a letter to Lord Rayleigh of 5 June 1877 (Larmor, *Correspondence*, **2**: 103). For Thomson's comments on Stokes' paper see his letter of 7 March 1870 (note (6)).

(11) The material on the variation of type was omitted from the published text of Rankine's paper. In a letter to Stokes of 24 May 1870 (Royal Society, *Referees' Reports*, **7**: 54) Rankine wrote: 'Having considered the reports of M^r^ Clerk Maxwell and Sir William Thomson on my paper... I have come to the conclusion that it is advisable to *omit* §§ 14 to 19 inclusive, as relating to problems that require further investigation.' Thomson had made some supplementary comments in a letter to Stokes of 9 May 1870 (Royal Society, *Referees' Reports*, **7**: 52; in Wilson, *Stokes–Kelvin Correspondence*, **2**: 345).

The linear distance between the planes being x, if x remains constant the type is constant but if x increases or diminishes while the somatic velocity m is that of a definite part of the wave there is variation of type.

Here again the author considers the mass m which enters the space between the planes with velocity mS and leaves it with velocity $ms-\frac{dx}{dt}$ and equates the difference of pressure to the difference of momentum. This was perfectly correct as long as the value of x is supposed to remain constant, but it appears to me that the difference of pressure is the effective cause not only of the difference of momentum of the entering and issuing fluid but of the variations of motion within the space considered.

The velocity at any plane (μ) where the rarity is σ is

$$u = (\sigma - S)\, m - \frac{dx}{dt}$$

and the momentum of the fluid between the planes is

$$H = \int u\, d\mu = mx - m^2 S - \int_0^\mu \frac{dx}{dt}\, d\mu.$$

What the pressure $p-P$ does in unit of time is to produce a velocity u in the mass m which leaves the second plane *and also* to increase the momentum of the fluid between the planes by $\frac{dH}{dt}$. Hence the true equation is

$$\begin{aligned} p-P &= mu + \frac{dH}{dt} \\ &= m^2(S-\sigma) + m\frac{dx}{dt} + m\frac{dx}{dt} - \int_0^\mu \frac{d^2x}{dt^2}\, d\mu \\ p-P &= m^2(S-\sigma) + 2m\frac{dx}{dt} - \int_0^\mu \frac{d^2x}{dt^2}\, d\mu. \end{aligned}$$

This equation may also be obtained by transforming the ordinary equation

$$\rho\frac{\partial u}{\partial t} + \frac{dp}{dx} = 0 \text{ (12)}$$

into

$$\frac{d^2x}{dt^2} - 2m\frac{d^2x}{d\mu\, dt} + m^2\frac{d^2x}{d\mu^2} + \frac{dp}{d\mu} = 0$$

(12) See G. G. Stokes, 'On the theories of the internal friction of fluids in motion, and of the equilibrium and motion of elastic solids', *Trans. Camb. Phil. Soc.*, **8** (1845): 287–319, esp. 297 (= *Papers*, **1**: 75–129).

and then increasing with respect to μ from 0 to μ. It differs from equation (42) of Prof Rankines paper by the terms depending on $\frac{dH}{dt}$.

The remarkable result given by the author that the point of maximum density travels with a constant velocity and that the value of this maximum density remains constant is derived from eq^{n} (42) with others. I am not prepared to say whether the accuracy of this result is affected by the want of accuracy of the equation.

I have not studied the investigations of such waves by the authors mentioned in the supplement,(13) so that I cannot pronounce on the originality of this paper but I consider Prof Rankines mode of treatment very original and especially valuable as presenting a succession of distinct physical conceptions to the mind of the reader during the whole investigation.(14) These conceptions are described in language which is clear enough to the author and to those who have studied his writings, but for the sake of others each new concept should be ushered in with a more distinct statement than is sometimes here given. In particular when the independent variables are changed the change should be clearly stated as when μ is introduced at p. 34.

The investigation of waves of permanent types is well worthy of the Philosophical Transactions.

The merit of the thermodynamical investigation of a possible case of permanent type would be greater if the results were more clearly given when the application of the thermodynamical equations is concluded.

If as I suppose there is an error in eq^{n} 42 it should be corrected and the result carried on to the end.

J. Clerk Maxwell

Glenlair, 26th March 1870

(13) Papers by S. D. Poisson, Stokes (see note (10)), G. B. Airy, and Samuel Earnshaw are reviewed in the 'Supplement' to Rankine's 'On the thermodynamic theory of waves': 287–8.

(14) See Maxwell's discussion in Chapter 15 'On the propagation of waves' in his *Theory of Heat* (London, 1871): 203–10 (the 'following method of investigating the conditions of the propagation of waves is due to Prof. Rankine').

LETTER TO WILLIAM THOMSON

14 APRIL 1870

From the original in the University Library, Cambridge[1]

Glenlair
14 April 1870

Dear Thomson[a]

The first table of dimensions that I know is in Fourier Theorie de Chaleur p 157 and he makes frequent use of it. The dimensions are in length, time, and temperature.[2]

If you define the unit of heat as that which raises unit of volume of the substance in its actual state one degree then in the equation

$$\frac{dv}{dt} = -k\nabla^2 v$$

k is the conductivity with this unit of heat.[3]

But to my question. In an infinite solid heated suddenly at the origin and then left to itself the temperature at a distance r after a time t is

$$v = At^{-\frac{3}{2}}e^{-\frac{r^2}{4kt}} \qquad \text{p. } 478.^{(4)}$$

Hence if v_0 is the initial temperature at the point $a\ b\ c$ and v the actual temperature at time t at $x\ y\ z$, distant r from $a\ b\ c$

$$v = \iiint\left(da\, db\, dc\, v_0\, (kt)^{-\frac{3}{2}} e^{-\frac{r^2}{4kt}}\right).^{(5)}$$

This tells us completely what the state of the infinite solid will be at any future time if we know its initial state. The temperature of every point is the mean of the original temperatures of all the points the weight attributed to each point in taking the mean being $e^{-\frac{r^2}{4kt}}$.

(a) {Thomson} When do you go to Cambridge? Thanks for the diffusion of gases.[6]

(1) ULC Add. MSS 7655, II/36.

(2) Joseph Fourier, *Théorie Analytique de la Chaleur* (Paris, 1822): 157 (§161).

(3) See Fourier, *Théorie Analytique de la Chaleur*: 136 (§142); equation (A), 'l'equation générale... qui est celle de la propagation de la chaleur dans l'intérieur de tous les corps solides'. Here v is the temperature at time t. Maxwell writes $k = K/CD$, where in Fourier's equation (A) K is the conductivity of the body for heat, C the heat capacity per unit mass, and D the density of the body. For Maxwell's discussion of this equation see also the *Treatise*, **1**: 384 (§332), and Number 489.

(4) Fourier, *Théorie Analytique de la Chaleur*: 478 (§376).

(5) Fourier, *Théorie Analytique de la Chaleur*: 479.

(6) Chapter 19 of Maxwell's *Theory of Heat* (London, 1871): 253–60.

This all stands to reason.

Now suppose we have observed the actual state of the infinite solid and wish to deduce its previous state at a time $-t$.

Can we adapt Fouriers solution.

In the formula there is the awkward quantity $(kt)^{-\frac{3}{2}}$ which is objectionable when t is $-^{\mathrm{ve}}$. In the method of taking means $e^{\frac{r^2}{4kt}}$ is a very respectable quantity, only it gives the greatest weight to the most distant points and it does this *especially* when t is small,[(b)] that is just before the time of observation, when if ever,[(c)] we ought to be able to deduce the previous state from the state of neighbouring points. I have not found any attempt at this inverse problem in Fourier. Has it been done?[(d)(7)] or shown to lead to insuperable difficulties? and if you do not know about it who does? I do not know any (Joseph) Fourierists.[(8)] He is principally known as having invented Fourier's Theorem the ratio of which to a 3 day problem is an unknown quantity.[(9)]

Of course if you cut up the distribution of temperature into harmonics, you can work back till the harmonic series becomes divergent. If you invent a function to express the present temperature the divergence usually comes on as soon as you put t negative.[(e)] It is only when you write down an harmonic series of set purpose that you can avoid this.[(10)]

It is because you have attended to the historical problem of the conduction of heat that I ask you. Everybody else inclines to the prophetical problem which is much easier.[(f)]

(b) {Thomson} This is the analytical expression of the impossibility to find a physical antecedent of an arbitrary distribution. See an Essay 'De Caloris Motu per Terrae Corpus' read in the Faculty room of the old College in 1846,[(11)] and now to be met through all space (if there is no limit to the greatest velocity of an individual molecule of gas) combined with oxygen.

(c) {Thomson] but this is just what cannot be done in a distribution (such as $v = e^{-\frac{r^2}{t}} t^{-\frac{1}{2}}$ when $t = 0$) which is essentially initial.

(d) {Thomson} See CMJ Notes on Certain Points in the Theory of Heat or some other equally appropriate & suggestive title, about year 1844.[(7)]

(e) {Thomson} Hear hear; see CMJ.

(f) {Thomson} because of the irreversibility of dissipation.[(12)]

(7) In his 'Note on some points in the theory of heat', *Camb. Math. J.*, **4** (1844): 67–72 (= *Math. & Phys. Papers*, **1**: 39–45), Thomson had discussed the problem of negative value assigned 'to the time...[so] the initial state is such as not to be deducible from a previous distribution', leading to a divergent series.

(8) As distinct from disciples of Charles Fourier.

(9) In the Cambridge Mathematical Tripos candidates for Honours sat an examination in which 'the first three days shall be assigned to the more elementary parts of Mathematics and Natural Philosophy'; see the *Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 26.

(10) Maxwell gave a succinct account of the issue in his *Theory of Heat*: 238–45, where he refers to Thomson's 'Note on some points in the theory of heat'; see note (7).

(11) Thomson's Inaugural Dissertation at the University of Glasgow; see S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 187.

(12) See Number 344; and *Theory of Heat*: 245–8.

I am boiling all this down for my chapter on Conduction with pictures of the diffusion of heat and salts and gases.(13)

The thermal view of an(g) harmonic is a distribution of heat which cools down without altering the ratios of the temperatures of the parts.

To prove that every figure has a fundamental harmonic with a series of higher harmonics in order would be probably a stiff business.(h)

These harmonics die away, the highest most rapidly, and at last, however the body is originally heated (provided the fundamental harmonic is not absent altogether) the distribution approximates to that of the fundamental harmonic.

I have arranged the paraffin between two prisms to determine its refractive index but have not got the angle measured till I have a salt wick going.(14)

I am greatly surprised that Joules combination of levers for magnifying magnetic disturbances does any good.(15) I should have thought that if a microscope is to be used at all there should be no magnification except that done by levers of light which do not get shaken or affected by gravity or viscosity.(16)

Yrs

$\dfrac{dp}{dt}$(17)

(g) {Thomson} a

(h) {Thomson} Not so very. It was a favourite (unsolved) propos[n] of Liouville in 1846 Jan. Feb. March.(18)

(13) Chapter 18, 'On the diffusion of heat by conduction', of the *Theory of Heat*: 233–53.

(14) To determine the relation between the dielectric constant and the index of refraction; for further discussion see Maxwell's letter to Thomson of 21 March 1871 (Number 362). See the *Treatise*, **2**: 388–9 (§§788–9) and Volume I: 687n on the significance of this relation for Maxwell's electromagnetic theory of light.

(15) On Joule's dip circle see his paper reported in the *Proceedings of the Literary and Philosophical Society of Manchester*, **8** (1869): 171–3. The axis of the magnetic needle is suspended from two silk filaments, the ends of the thread being supported by the beam of a balance. Nevertheless, Maxwell describes Joule's dip circle in the *Treatise*, where he includes (*Treatise*, **2**: 116, Fig. 18) an elaborated version of the figure which Joule published in his 1869 paper. In a letter to Maxwell (ULC Add. MSS 7655, II/49), which can be dated *c.* 20 June 1871 (from a reference to 'last Saturday June 17') Joule commented that 'Your figure describes the principle of the dip needle exactly'. He went on to describe recent improvements in the design of the instrument, some of which Maxwell incorporated into his account of the dip circle in the *Treatise*, **2**: 115–17 (§463). For Joule's own account (dated 1881) of successive improvements to the dip circle see *The Scientific Papers of James Prescott Joule*, 2 vols. (London, 1884–7), **1**: 577–83.

(16) See Maxwell's proposal for the 'dip needle in the Cambridge Physical Laboratory' in the *Treatise*, **2**: 117 (§463).

(17) Maxwell's thermodynamic *nom de plume* expressed the second law of thermodynamics, taking its form – $dp/dt = \mathrm{JCM}$ – from Tait's expression for the law in his *Sketch of Thermodynamics* (Edinburgh, 1868): 91 (§162): 'Hence the second law of thermodynamics may be expressed in

the form $\frac{dp/dt}{M} = \mathrm{JC}$.' Thus p and t denote pressure and temperature; J the mechanical equivalent of heat ('Joule's equivalent'); M is a coefficient of proportionality, the heat absorbed per unit volume change in an isothermal expansion; and C is a universal function (the 'Carnot function') of the temperature (the reciprocal of the absolute temperature). Tait later explained this usage to Maxwell: see his card of 1 February 1871 (Number 353 note (11)). The subtleties of the signature are fully explained by Martin J. Klein, 'Maxwell, his demon, and the second law of thermodynamics', *American Scientist*, **58** (1970): 84–97, esp. 94–5.

(18) Joseph Liouville, 'Lettres sur diverses questions d'analyse et de physique mathématique concernant l'ellipsoide', *Journal de Mathématiques Pures et Appliquées*, **11** (1846): 217–36, 261–90.

340

LETTER TO JOHN WILLIAM STRUTT

18 MAY 1870

From the original in private possession(1)

Glenlair
Dalbeattie
18 May 1870

Dear M^r Strutt,

I am very much obliged to you for your remarks on the equation of stream-lines in which you are right and I have been wrong hitherto.(2)

I remember making out what it should be long ago and copying it when I wanted it instead of doing it again, and I cannot now say whether I made a deliberate error or went wrong in copying. The effect is the same for all that.

The curves $M =$ const are orthogonal to the curves $V =$ const.(3) and in particular

$$\frac{dM}{da} = 2\pi a \frac{dV}{db}$$

and

$$\frac{dM}{db} = -2\pi a \frac{dV}{da}.$$

Hence

$$\frac{dV}{da} = -\frac{1}{2\pi a}\frac{dM}{db}$$

and

$$\frac{dV}{db} = \frac{1}{2\pi a}\frac{dM}{da}.$$

$$\frac{d^2V}{da\,db} = -\frac{1}{2\pi a}\frac{d^2M}{db^2}$$

$$= \frac{1}{2\pi a}\frac{d^2M}{da^2} - \frac{1}{2\pi a^2}\frac{dM}{da}.$$

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(1) Rayleigh Papers, Terling Place, Terling, Essex.

(2) Compare Number 337 on stream lines. On 10 March 1870 Maxwell's paper 'On the displacement in a case of fluid motion' was read to the London Mathematical Society (see the *Proceedings*, **3** (1870): 82–7 (= *Scientific Papers*, **2**: 208–14)). See Numbers 307 esp. note (9) and 311 for Maxwell's use of the concept of 'stream function' in this paper.

(3) For Helmholtz's discussion see Number 302 note (5). M and V are the velocity potential and stream function, respectively.

Equating these two values of $\dfrac{d^2V}{da\,db}$

$$\frac{d^2M}{da^2}+\frac{d^2M}{db^2}-\frac{1}{a}\frac{dM}{da}=0.$$

To try it, put $\qquad V=\dfrac{1}{r}$

and $\qquad M=\pi\dfrac{b}{r}$

where $\qquad r^2=a^2+b^2,$

or $\qquad V=b$ and $M=\pi a^2$.

I have not got a solution of the case of a plate with a slit in it at one potential and a parallel plate at another but this is my way to get the result.(4)

There are various ways which might do.

For instance we know that if

$$x_1=e^{\rho}\cos\theta \quad\text{and}\quad y_1=e^{\rho}\sin\theta$$

then x and y will be conjugate functions with respect to ρ and θ.(5)

Also if $x_2=e^{-\rho}\cos\theta$ and $y_2=e^{-\rho}\sin\theta$.

Whence adding

$$2x_3=(e^{\rho}+e^{-\rho})\cos\theta \;\&\; 2y_3=(e^{\rho}-e^{-\rho})\sin\theta.$$

The point $(x_3\, y_3)$ is in an ellipse of which the semiaxes are $(e^{\rho}+e^{-\rho})$ and $(e^{\rho}-e^{-\rho})$ and in a hyperbola of which the semiaxes are $2a\cos\theta$ & $2a\sin\theta$.

Now let $x_3=e^{\frac{\xi}{a}}\cos\dfrac{\eta}{a}$ and $y_3=e^{\frac{\xi}{a}}\sin\dfrac{\eta}{a}$ then ξ and η will be conjugate functions of ρ and θ.

In particular when $\theta=0$ $\;y_3=0$ and $\eta=0$ or $n\pi a$ $\;\&\; 2e^{\frac{\xi}{a}}=e^{\rho}+e^{-\rho}$.

When $\theta=\dfrac{\pi}{2}$ $\;x_3=0$ and $\eta=(n+\frac{1}{2})\,\pi a$.

When $\rho=0$ $\;y_3=0$ and $\eta=0$ or $n\pi a$ and $e^{\frac{\xi}{a}}=\cos\theta$.

If we now suppose ρ to be the potential at the point $(\xi\,\eta)$ we shall have $\rho=0$ for the planes $\eta=n\pi$ when ξ is negative but in the same planes when ξ is positive.

$$\frac{\xi}{a}=\log\frac{1}{2}(e^{\rho}+e^{-\rho}).$$

(4) Compare Number 303 on the potential between parallel planes; and the *Treatise*, **1**: 237–48 (§§192–202).

(5) See Number 303 note (3).

Hence the equipotential lines are like this (negative values to the right by mistake) turn figure upside down.(6)

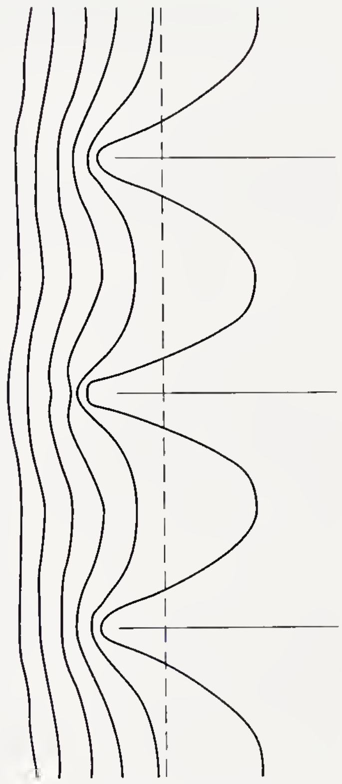

Figure 340,1

At a distance from the ends of the planes when ξ is a large positive quantity

$$\frac{\xi}{a} = \rho - \log_\epsilon 2 \text{ nearly}$$

or

$$\rho = \xi + a \log_\epsilon 2.$$

But if instead of a thing like a curry comb we had a plane then $\rho = \xi$ at a distance.

Hence a thing like a curry comb with a lot of planes cut off by an imaginary plane acts like a plane at a distance $a \log 2$ behind the imaginary plane where the distance between the edges of the planes is πa. The equivalent plane is dotted in the figure.

Now if we have two opposed planes at a distance b their potentials being 0 and V then if σ is the superficial density $4\pi\sigma = \frac{V}{b}$.

If instead of one of the planes we have a set of planes cut off as before the distance between consecutive planes being πa then if b is the distance between the single plane and the edges of the system of planes and σ the mean density on the single plane

$$4\pi\sigma = \frac{V}{b + a\log 2}.$$

Now let A be the area of the plane and let it be opposed to n planes at a distance πa the lengths of the edges being l.

Then $A = n\pi al$ and the whole charge $= \sigma A = \frac{1}{4\pi}\frac{Vn\pi al}{b + a\log 2}$.

Now let all the intervals between the planes be filled up except one.

This will certainly not diminish the charge on the sides of the slit though it may increase it. Hence the whole charge is between

$$\frac{1}{4\pi}\frac{VA}{b} \quad \text{and} \quad \frac{1}{4\pi}\left(\frac{n-1}{n}\frac{VA}{b} + \frac{V\pi al}{b + a\log 2}\right)$$

but probably very nearly the latter even though the slit is not deep.

(6) Compare the *Treatise*, **1**: 238–9 (§ 193) and Fig. XI appended to the volume.

All this supposes b large compared with a. The last expression is if we make

$$\alpha = a \log 2 = (\text{breadth of slit}) \frac{\log 2}{\pi}$$

then the charge on the plate of area A bounded by the slit is

$$\frac{V}{4\pi}\left(\frac{A}{b}+\frac{1}{2}\frac{A'-A}{b+\alpha}\right)$$

where A' is the area of the plate together with that of the slit, that is of the aperture in the guard plate. This is the same as if it had been the mean of the areas A and A' provided b is great compared with α.

If you cause a conductor of any form to spin round a magnet and mirror hung up in the axis of spinning you are safe to make the magnet move in the same direction as the conductor both on account of the Earths magnetic induction and on account of the magnet's own induction. See Arago's experiments[7] and the spinning coil Brit Ass Report 1863.[8]

The equations of inductive coil machines must be very interesting.

I am writing all this out of my head which I had carefully emptied of most of these subjects and I have put away my notes so excuse the signs of working in the dark.

Have you tried whether the sudden starting or stopping of a current in a coil has any the least effect in turning the coil in its own plane as it would be turned if the current were of water in a tube.[9]

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(7) Arago's discovery of the generation of electric currents in a metallic disc rotating in a magnetic field (described in the *Treatise*, **2**: 271–5 (§§668–9)). See François Arago, 'Note concernant les phénomènes magnétiques auxquels le mouvement donne naissance', *Ann. Chim. Phys.*, ser. 2, **32** (1826): 213–23.

(8) See the 'Description of an experimental measurement of electrical resistances, made at King's College' by Maxwell and Jenkin, in the 'Report of the Committee appointed by the British Association on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association for the Advancement of Science; held at Newcastle-upon-Tyne in August and September 1863* (London, 1864): 111–76, esp. 163–76. See Numbers 210, 211, 213, 214, 216, 217, 218, 219.

(9) Maxwell had mentioned an experiment on these lines in his letter to Thomson of 10 December 1861 (Volume I: 698). Compare his more detailed discussion in the *Treatise*, **2**: 200–2 (§574), where he concludes that 'no phenomenon of this kind has yet been observed'; and see also Number 430.

If the coil is hung in a horizontal plane you can easily destroy the earths horizontal magnetism by means of magnets.(10)

I remain
Yours truly
J. CLERK MAXWELL

I shall probably be in London by the end of next week.

(10) Maxwell makes the same point in the *Treatise*, **2**: 200 (§574).

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LETTER TO CECIL JAMES MONRO

6 JULY 1870

From the original in the Greater London Record Office(1)

Glenlair
Dalbeattie
July 6 1870

My dear Monro

I was very glad to see your handwriting again and that it was from your old address.(2) My permanent address is as above and I have no other now. My question to the Math Soc.(3) bore fruit in various forms and you have put it very clearly. It would give my mind too great a wrench just now to go into elliptic integrals but I will do so when I come to revise about circular conductors.(4) The area of a spherical ellipse cannot be expressed in terms of *complete* elliptic integrals of the 1st & 2nd kinds only. It may be expressed in complete integrals one of which is of the 3rd kind, and this last may be

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(1) Greater London Record Office, Acc. 1063/2091. Published (in part) in *Life of Maxwell*: 346–7.

(2) In reply to Monro's letter of 2 June 1870 (GLRO, Acc. 1063/2105); 'I had *Nature* sent me abroad last winter, and I observed in the number for Nov. 18 this question among others propounded in your name to the Mathematical Society, namely whether the solid angle of a cone can be expressed in elliptic integrals. I may have been in some muddle from that time to this, but it seemed to me you must have overlooked a little theorem which I think does the business.... The theorem is that the solid angle of a cone or pyramid is the complement to four right angles of what I will call the total exterior angle, that is, the angle through which an applied plane rolls completely round the figure. This follows at once from the usual form of the value of the area of a spherical polygon: and it has always made me suspect that the solid angle of a cone of the second degree *was* expressible in elliptic integrals. I found in the winter that the expression for the total exterior angle has a great look of an elliptic integral: but I know nothing about elliptic integrals. However, turning over Ellis's Works I find p. 298 that Gudermann had reduced to elliptic integrals the arc of a spherical ellipse; and this settles the matter.... Accordingly on taking it up again I see that the expression for the total exterior angle does easily reduce itself to a linear function of two complete elliptic integrals of the first and third kinds.' Monro is alluding to a passage in R. L. Ellis' 'Report of the recent progress of analysis (theory of the comparison of transcendentals)' in the *Report of the Sixteenth Meeting of the British Association for the Advancement of Science* (London, 1847): 34–90, on 73, reprinted in *The Mathematical and Other Writings of Robert Leslie Ellis, M.A.*, ed. W. Walton (Cambridge, 1863): 238–323, on 298; 'In the fourteenth volume of Crelle's *Journal* (p. 217) M. Gudermann has considered the rectification of the curve called the spherical ellipse, which is one of a class of curves formed by the intersection of a cone of the second order with a sphere. He has shown that its arcs represent an elliptic integral of the third kind.'

(3) Number 330; reported in *Nature*, **1** (1869): 91.

(4) See the *Treatise*, **2**: 299–312 (§§694–706).

expressed in terms of incomplete integrals of the 1st & 2nd kinds. Fortified with this information, which is confirmed by competent authorities and finally by yourself, I can cut the subject short with an easy conscience for I have no scruple about steering clear of tables of double entry, especially when, in all really useful cases convergent series may be used with less trouble, and without any knowledge of elliptic integrals.

On this subject see a short paper on Fluid Displacement in next part of the Math. Soc. Trans. where I give a picture of the stream-lines and the distortion of a transverse line as water flows past a cylinder so.(5)

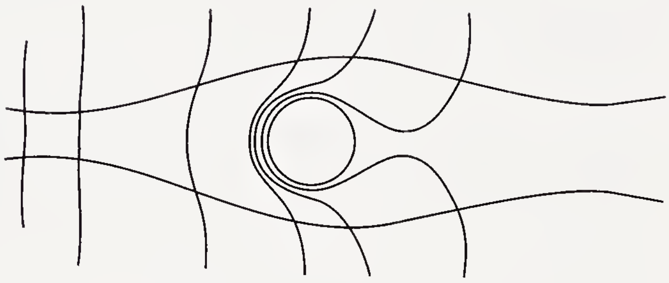

Figure 341,1

Mr. W. Benson, architect 147 Albany Street Regents Park NW told me(6) that you had been writing to Nature(7) and that yours was the only rational statement in a multitudinous correspondence on colours.(8) Mr Benson considers that Aristotle and I have correct views about primary colours. He has written a book with coloured pictures on the science of colour and he shows how to mix colours by means of a prism.(9) He wants to publish an elementary book with easy experiments(10) but gets small encouragement, being supposed an heretic. No other architect in the Architects Society believes him. This is interesting to me as showing the chromatic condition of Architects.

I made a great colour-box in 1862 and worked it in London in 62(11) & 64.(12) I have about 200 equations each year which are reduced but not published. I have set it up here this year and have just got it in working order.

(5) See Fig. 1 of Maxwell's paper 'On the displacement in a case of fluid motion', *Proceedings of the London Mathematical Society*, **3** (1870): 82–7 (= *Scientific Papers*, **2**: 208–14).

(6) William Benson to Maxwell, May 1870 (ULC Add. MSS 7655, II/37).

(7) C. J. Monro, 'Correlation of colour and music', *Nature*, **1** (1870): 362–3.

(8) A correspondence initiated by W. F. Barrett, 'Note on the correlation of colour and music', *Nature*, **1** (1870): 286–7. See also *Nature*, **1** (1870): 314, 335, 384–5, 430–1, 557–8, 651–3, and *ibid.*, **2** (1870): 48.

(9) William Benson, *Principles of the Science of Colour concisely stated to aid and promote their useful application to the decorative arts* (London, 1868). There is a copy in Maxwell's library (Cavendish Laboratory, Cambridge).

(10) See Number 359.

(11) See Maxwell's letter to Monro of 18 February 1862 (Volume I: 711); and see Number 202.

(12) See Number 225.

I expect to get some more material and work up the whole together.[13] In particular I want to find any change or evidence of constancy in the eyes of myself & wife during 8 years. I can exhibit the yellow spot[14] to all who have it and all have it except Col. Strange F.R.S.[15] my late father-in law and my wife, whether they be negroes Jews Parsees, Armenians Russians Italians Germans Frenchmen Poles &c. Professor Pole,[16] for instance, has it as strong as me though he is colour blind. Mathison,[17] also colour blind, being fair, had it less strongly marked.

One J. J. Müller in Pogg. Ann. for March & April 1870, examines compound colours and finds the violet without any tendency to red or the red to blue.[18] He also selects a typical green out of the spectrum.[19]

Many thanks for your formula about complementary colours[20] and for

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(13) See Maxwell's paper 'On colour-vision at different points of the retina' in the *Report of the Fortieth Meeting of the British Association for the Advancement of Science; held at Liverpool in September 1870* (London, 1871), part 2: 40–1 (= *Scientific Papers*, **2**: 230–2).

(14) On the insensitivity of the 'yellow spot' on the retina to greenish-blue light see Volume I: 318, 636.

(15) Lieut. Col. Alexander Strange, FRS 1864; see *Nature*, **13** (1876): 408–9.

(16) William Pole: see Volume I: 632.

(17) William Collings Mathison, Trinity 1834, Tutor 1850–68 (Venn).

(18) J. J. Müller, 'Zur Theorie der Farben', *Ann. Phys.* **139** (1870): 411–31, 593–613, esp. 423–4, where he questions Maxwell's conclusion, in 'On the theory of compound colours, and the relations of the colours of the spectrum', *Phil. Trans.*, **150** (1860): 57–84, on 77–8 (= *Scientific Papers*, **1**: 436), that 'the extreme ends of the spectrum are probably equivalent to mixtures of red and blue'.

(19) Müller, 'Zur Theorie der Farben': 426. Compare Maxwell's choice of 'standard green' in his paper 'On colour vision', *Proceedings of the Royal Institution*, **6** (1870–2): 260–71, esp. 266–7 (= *Scientific Papers*, **2**: 274–5). In a letter to Maxwell of 26 July 1872 (ULC Add. MSS 7655, II/61) J. D. Everett contrasted Maxwell's value with Müller's, and referred disparagingly to Müller's 'mode of experimenting'. This letter, and one of 19 July 1872 (ULC Add. MSS 7655, II/60), were written in connection with Maxwell's review of the proof of the chapter on colour for Everett's translation (with additions) of A. Privat Deschanel, *Elementary Treatise on Natural Philosophy*, 4 parts (London, 1870–2), Part IV: 1004–8.

(20) In his letter of 2 June 1870 (see note (2)) Monro had asked: 'Have you, or has anybody, any further observations than those in your R.S. paper of 1860? The violet region was left so incomplete. At a time when I presumed to meddle with your figures, I hit on what seemed to me a rather curious formula for the relation of complementary colours, namely $(\lambda - L)(L' - \lambda') = M^2$ where λ, λ' are complementary wave-lengths*, and

	L,	L',	M,
for K,	2076,	1842,	77.9
for J,	2132,	1860,	51.2.

* Between 2450 and 2100/, 1825 and 1600.' J and K denote the two observers (Maxwell and his wife Katherine) in Maxwell's paper 'On the theory of compound colours, and the relations of the colours of the spectrum': esp. 69–71, 74–5 (= *Scientific Papers*, **1**: 426–8, 431–2).

your letter in general which I hope may be followed by others. Did you ever get one from me about 4 years ago?

Yours sincerely
J. Clerk Maxwell

REPORT ON A PAPER BY CHARLES BLAND RADCLIFFE[1] ON ANIMAL ELECTRICITY

circa JULY 1870[2]

From the original in the Library of the Royal Society, London[3]

REPORT ON D^r CHARLES BLAND RADCLIFFES PAPER 'RESEARCHES ON ANIMAL ELECTRICITY'[4]

I have in the first place to state, that I have never either made or witnessed any experiments on animal electricity, such as those described in this paper, and that I have not read enough on the subject to be able to form any judgment on the novelty of the doctrines here advanced, or on their consistency with those of other enquirers in the same field.

The experiments, however are so clearly described that I have learnt enough from the paper itself to see something of the state of the subject as it presents itself to the author and to others.[5]

The well known facts that nerves and muscles have certain electrical properties when at rest, and that those are modified when the muscle is in action and that action may be excited by electrical means, are in themselves very interesting from an electrical point of view, and also share in the importance which every phenomenon having a bearing, however remote, on the theory of animal motion must have, both in physics and in physiology.

The first investigation is into the electrical state of a piece of still living nerve or muscle, dissected out of the body and therefore having two sections cut across.

Prof Du Bois Reymond found that if the electrodes of a galvanometer are

(1) MD 1851, FRCP 1858 (Boase).

(2) According to the Royal Society's *Register of Papers Received* Radcliffe's paper was referred to Maxwell on 27 June 1870.

(3) Royal Society, *Referees' Reports*, **7**: 49.

(4) Charles Bland Radcliffe, 'Researches on animal electricity' (Royal Society, AP. 52.10). Part I of Radcliffe's paper was received by the Royal Society on 10 March 1870, Part II on 19 May 1870, both parts being read on 16 June 1870; see the abstract in *Proc. Roy. Soc.*, **19** (1870): 22–8. Radcliffe had previously read a paper on the same subject: see his 'Researches in animal electricity', *Proc. Roy. Soc.*, **17** (1869): 377–91.

(5) Radcliffe's paper (see note (4)) fills 100 ms. pages. In his report on the paper (Royal Society, *Referees' Reports*, 7: 50) William Sharpey noted that 'very many pages of MS are occupied with detailed expositions of well known facts and doctrines in electro-physiology, such as are to be found in most systematic or even elementary treatises'.

put in contact, one with the side of the nerve or muscle and the other with either of its cut extremities a current flows through the coil from the side of the cut extremity, and this goes on for some time, after which the tissue becomes dead.(6)

We shall suppose, though it is not explicitly stated that the electrodes of the galvanometer are of the same metal, say platinum and that the moisture on the ends and on the side of the animal substance is of the same chemical nature. If these conditions are not fulfilled the experiment proves nothing new with respect to animal tissues as the same currents would happen in any moist substance if the fluids or the metals were heterogeneous

but I mention this only to shew that it is important to state the electrical data of an experiment so clearly as to be beyond suspicion and to hint that a little more statement as to the mode of insulating the frogs legs and the galvanometer would be desirable. I shall suppose the method to be that of Du Bois Reymond with electrodes of moist paper.

This experiment clearly shews that, in a nerve or muscle in its normal state, the potential of the outside is positive with respect to that of the inside. This is shown by the current through the galvanometer but Dr Radcliffe has shown it in a more direct manner by means of Sir W. Thomson's quadrant galvanometer.(7)

As far as I can see he has made a decided advance on the statement of Du Bois Reymond with respect to the character of the electrical state of the nerve or muscle by describing it as a state in which each fibre may be compared to a charged Leyden jar (or better a submarine cable) in which the outer coating is positive as regards the inner coating.(8)

This is a much better form of stating the theory than by introducing 'peripolar molecules' but it hardly contains as much as is given by the experiments.(9)

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(6) Emil Du Bois-Reymond, *Untersuchungen über thierische Electricität*, 2 vols. (Berlin, 1848–9). In 'Researches in animal electricity' (Part I, ff. 1–3) Radcliffe attributed the discovery of the 'nerve-current' and the 'muscle-current' to Du Bois-Reymond.

(7) Read: quadrant electrometer, described in Thomson's 'Report on electrometers and electrostatic measurements', *Report of the Thirty-seventh Meeting of the British Association for the Advancement of Science* (London, 1868): 489–512, on 490–7 and Plate 5 (= *Electrostatics and Magnetism*: 260–309).

(8) Radcliffe maintained that '[Du Bois-Reymond's] nerve current and muscle current... are no more than accidental phenomena... the sheaths of the fibres in nerve and muscle... play the parts of *dielectrics*... the sheath of each fibre acting in fact as a Leyden jar'; 'Researches in animal electricity' (Part I, ff. 9, 14).

(9) See Radcliffe, 'Researches in animal electricity' (Part I, f. 7): 'In order to account for the nerve-current and muscle-current Du Bois-Reymond supposes that the fibre of living nerve and

We find certain currents and certain differences of potential. We infer from these the existence of an electromotive force. We seek for the seat of this electromotive force. We find it in the sheath of the nerve or fibre, for the difference of electrical state is primarily a difference of 'within and without' and the observed difference of 'middle and ends' is only secondary.

How then does the sheath act? Dr Radcliffe supposes it to be like the glass of a Leyden jar, that is, a dielectric and he shows that the nerve substance is sufficiently insulating to act in this way.

He does not explain, however, how the nerve gets charged, or how when discharged through the galvanometer, it becomes constantly recharged, so as to keep up a current for a long time.

The existence of the current through the galvanometer is a proof that the circuit is completed through the sheath of the nerve and that there is here an electromotive force from within to without constantly acting in the normal state of the nerve.[(10)]

The result of this if the nerve is cut out and insulated is to keep up an exceedingly slow current through the sheath which when it comes to the outer surface creeps along from the middle towards the ends and completes its circuit in the interior. There is therefore a superficial distribution of electrification, positive at the middle, negative at the ends, and this might be tested by a sufficiently delicate proof plane.[(11)] What is tested by the quadrant electrometer is the difference of potential at different points and this is done without carrying away electricity. The galvanometer tests the very same thing but it cannot do it without carrying off the electricity in a stream.

It may be asked Does the sheath act like a diaphragm between two liquids of different kinds, or in what way is the electromotive force sustained?

I have endeavoured at the risk of introducing errors of my own to make a clear statement of what has been observed, because I think that the author makes use of several expressions which are apt to be misunderstood. 'Free electricity' is one of these and 'Tension' is another. I think 'Tension' in this

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muscle is made up of an infinite number of what he calls *peri-polar molecules*...which are negatively electrified at the two poles which point to the two ends of the fibre, and positively electrified in the equatoreal interpolar belt which is turned toward the side of the fibre'. See Du Bois-Reymond, *Untersuchungen*, **1**: 680–2. In his report (see note (5)) Sharpey noted however that 'many eminent physiologists who also reject Du Bois Reymond's hypothesis admit the validity of the empirical proof of a current'.

(10) Having seen Maxwell's report Sharpey quotes, in agreement, these last two paragraphs, noting: 'I would add that this hypothesis appears to break down altogether when tested by anatomical facts. It might no doubt be contended with some probability *in the case of a nerve-fibre* that the medullary sheath might act as a dielectric, but the...sheath of a muscular fibre seems utterly unsuited to that end'.

(11) See the *Treatise*, **1**: 277–81 (§§223–5).

paper always means potential. It is often used in another sense (which I prefer) as the tendency, at any point of an electrified surface, of the electricity to escape by a discharge.(12) I also am afraid that the wording of the paper implies that a piece of nerve, electrified like a Leyden jar, positive on the outside and negative on the inside can have this state of induced charge heightened or relaxed by raising or lowering. The author does not say how the limbs were supported. If, by electrifying them positively, a current from within outwards was set up, this current might act on the internal parts. But if they had been enclosed in brass tubes and if the brass tubes had been electrified so as to raise or lower their potential with that of all that they contained, then whether this operation were slow or sudden, nothing could be known of it to the frog inside from any electrical effect within.

The author attributes the contractions on making and breaking the circuit to the extra-currents which then occur. Volta supposed the contraction on breaking the circuit to be due to an effect like that of the 'hydraulic ram' but Marianini (Dela Rive II 436) showed that this contraction takes place when the circuit is not broken, but the current diverted from the frog by the interposition of a better conductor.(13) This shows that the simple cessation of the current produces contraction. If the good conductor be now removed, so as to throw the current back through the frog, then there will be a true extra-current in the same direction but of greater intensity than the constant current.

He also discusses Electrotonus.(14) When he has shown that the same phenomena occur when a piece of moist silk &c is substituted for the nerve(15) we may as well discontinue our physiological experiments till we have made enough experiments on dead matter to satisfy ourselves as to the nature of the purely electrical part of the phenomenon.

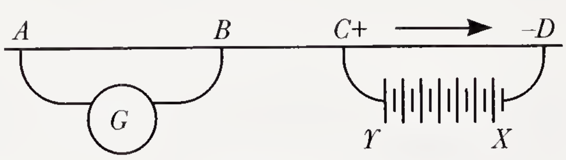

Figure 342,1

Now if by means of a battery XY a current is produced in a part CD of a linear conductor then if the ends are insulated there will be no currents beyond CD, and if A & B are connected

(12) See the *Treatise*, **1**: 47 (§48).

(13) A. De la Rive, *Traité d'Électricité Théorique et Appliquée*, 3 vols. (Paris, 1854–8), **2**: 436. The analogy of the rebounding of a fluid is stated by De la Rive; the paper by Stefano Marianini alluded to is his 'Mémoire sur la secousse...', *Ann. Chim. Phys.*, ser. 2, **40** (1829): 225–56.

(14) See Radcliffe, 'Researches in animal electricity' (Part II, ff. 1–2): 'Under the action of a voltaic current the special electricity and activity of a nerve are strangely modified. The name now given to these strange modifications... is *Electrotonus* – the name which was first applied by their discoverer Prof. Du Bois-Reymond'. See Du Bois-Reymond, *Untersuchungen*, **2**: 289.

(15) Radcliffe, 'Researches in animal electricity' (Part II, f. 7).

by a galvanometer which is insulated there will be no current in the coil. But if A or the further part of the galvanometer coil is not well insulated there will be a derived current in G from B to A, and this is the direction of the current, or the variation of current, observed in electrotonus.

But whatever be the true cause of these phenomena, no part of the phenomenon should be stated as a physiological fact about the structure and functions of nerves as nerves(16) till it is clearly established that it is not due to the ordinary electrical properties of the substance experimented on considered as a body having a certain form and electrical resistance and capable of electrolysis and polarization like any other moist conductor.

Towards the end of his paper the author propounds a theory of muscular contraction. He supposes the sheaths of the muscular fibres when at rest to be charged within with negative and without with positive electricity, the attraction of which produces a transverse pressure on the substance of the sheath and this again produces a longitudinal elongation.

There is no doubt that such a charge would produce an effect of this kind on an elastic hollow cylindrical sheath.

But, in the first place, I understand that the appearance of a muscular fibre is that of a series of disks connected by fibrils like the generating lines of a cylinder and that when the fibre is contracted these fibrils are pinched in between every two disks so as to appear like truncated cones. This is not at all similar to the image raised by Dr Radcliffes description.(17)

In the second place the work done by the muscular contraction is indefinitely greater than the energy of the electrical distribution can ever be, so that the moving power of the muscle does not arise from its electrification any more than the moving power of an artificial explosive torpedo arises from the electric spark which explodes it.

In conclusion. The paper was very interesting to me, though by no means satisfactory in an electrical point of view. The only decided novelty seems to be the use of the electrometer. In a physiological point of view I am not qualified to give any opinion.

J. Clerk Maxwell

(16) Contesting Radcliffe's claim that 'electrotonus' was not due to modifications of the nerve current but to 'a charge of free electricity', Sharpey asserted that Radcliffe gave 'no experiments directly proving its nature'.

(17) Sharpey concurred: 'the objection applies which is derived from the anatomical structure of muscle'.

ON THE CHROMATIC EFFECTS OF POLARISED LIGHT ON DOUBLE REFRACTING CRYSTALS:[1] ADDITION TO A PAPER BY FRANCIS DEAS[2]

circa SUMMER 1870[3]

From the *Transactions of the Royal Society of Edinburgh*[4]

[ADDITION TO A MEMOIR BY FRANCIS DEAS]

In Mr Deas' paper a number of interesting experiments are described, in which, by means of a spectroscopic microscope fitted with polarising and analysing prisms, the true nature of the phenomena observed by Brewster, Biot, and others, in plates of selenite, &c.,[5] is made exceedingly intelligible to the understanding, while, at the same time, the eye is satiated with new forms of splendour.

The subject is one to which the attention of experimenters is not so strongly directed as it was fifty years ago; and therefore it is desirable that the remarkably simple methods of observation here described, and the perfection with which the phenomena may be seen by means of modern instruments, should be more generally known.

In the text, the paper appears purely descriptive, without any theoretical application, and the æsthetic beauty of the phenomena might be assumed to be the object of the experiments. But the carefulness of the selection of the experiments and the faithfulness of the description make me think that the author himself looked at what he saw in the light of the theory of double

(1) Phenomena discussed by Maxwell in 1848–50; see Volume I: 97–100, 125.

(2) Francis Deas, 'On spectra formed by the passage of polarised light through double refracting crystals', *Trans. Roy. Soc. Edinb.*, **26** (1870): 177–85. A lawyer (LLB Edinburgh 1864) Deas was elected FRSE in 1867; see *Proc. Roy. Soc. Edinb.*, **6** (1867): 70.

(3) The date is conjectural: Deas' paper was read to the Royal Society of Edinburgh on 6 June 1870; see *Proc. Roy. Soc. Edinb.*, **7** (1870): 172–3. But see note (8).

(4) J. Clerk Maxwell, 'Addition to the above paper', *Trans. Roy. Soc. Edinb.*, **26** (1870): 185–8.

(5) David Brewster, 'On the laws of polarisation and double refraction in regularly crystallized bodies', *Phil. Trans.*, **108** (1818): 199–273; J. B. Biot, 'Mémoire sur les lois générales de la double réfraction et de la polarisation, dans les corps régulièrement cristallisés', *Mémoires de l'Académie Royale des Sciences*, **3** (1820): 177–384. The phenomena are described by Brewster in his *A Treatise on Optics* [in Lardner's *Cabinet Cyclopaedia*] (London, 1831): 183–254. See also Jed Z. Buchwald, *The Rise of the Wave Theory of Light* (Chicago/London, 1989): 67–107, 254–6, 368–9, 400–3.

refraction and the interference of light. I, therefore, think that a simple statement of the relation of the visible things here described to the results of theory would greatly increase the value of the paper; for in scientific education the identification of what is observed with what is deduced from theory is of more value than either the process of observation or the process of deduction.

This might be done as follows –

Begin with the plane polarized light, the equations of motion of which are

$$x = c\cos nt \quad y = 0.$$

Now let it pass through a plate of crystal of which the axis is inclined α to the axis of x; and let this crystal produce a retardation whose phase is p in the light polarised in the plane of the axis

parallel to axis $\qquad x' = c\cos\alpha\cos(nt+p)$

perpendicular to axis $\qquad y' = c\sin\alpha\cos nt.$

Next, let the light fall on an analyser in a plane inclined β to the axis of the crystal. The analysed light will be

$$x'' = c\cos\alpha\cos\beta\cos(nt+p) + c\sin\alpha\sin\beta\cos nt.$$

The intensity of this light will be

$$c^2\{\cos^2\alpha\cos^2\beta + \sin^2\alpha\sin^2\beta - 2\sin\alpha\cos\alpha\sin\beta\cos\beta\cos p\}$$

or

$$\frac{1}{2}c^2\{1 + \cos 2\alpha\cos 2\beta - \sin 2\alpha\sin 2\beta\cos p\}.$$

We may represent this whole process geometrically as follows –

Let OCO' represent the original polarised light, OCA the angle between the plane of polarisation and the axis of the crystal. The light is resolved into ACA' and DCD'. Now, let a semicircle be drawn with radius OA, and let $OAp = p$ be the phase of retardation; draw pT perpendicular to AO, and draw an ellipse with centre C and

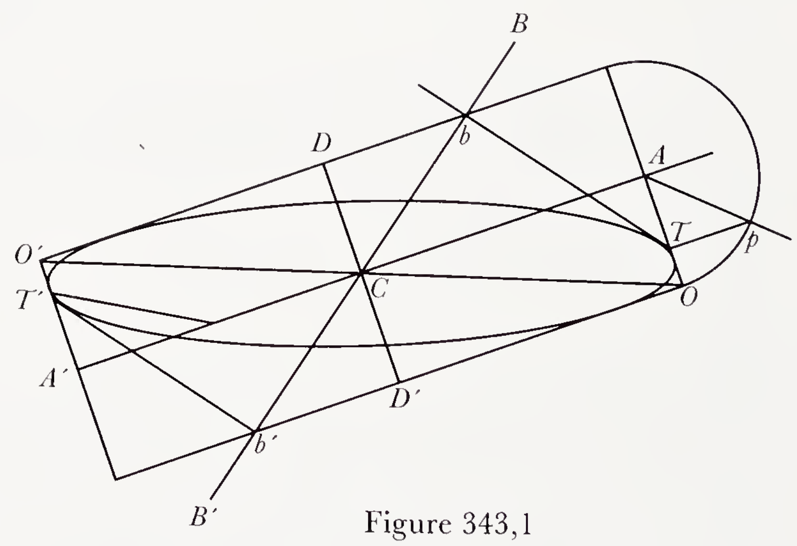

Figure 343,1

touching AO in T and also the other sides of the parallelogram. This ellipse is the path of the light emergent from the crystal. Now let BCB' be the plane of the analyser. Draw Tb $T'b'$ tangents to the ellipse perpendicular to BB', then bCb' represents the amplitude of the emergent light.

The *result* of the process may be made still simpler thus:

Draw $CO = c$, in the plane of polarisation, CA parallel to the axis of the crystal, and CB parallel to the analyser. Draw OA perpendicular to CA, AB to CB, and OD to CB, then $CB = c \cos\alpha \cos\beta$, and $BD = c \sin\alpha \sin\beta$; make $DBP = p$, the phase of retardation, and $BP = BD$. Then CP represents the amplitude of the emergent light.

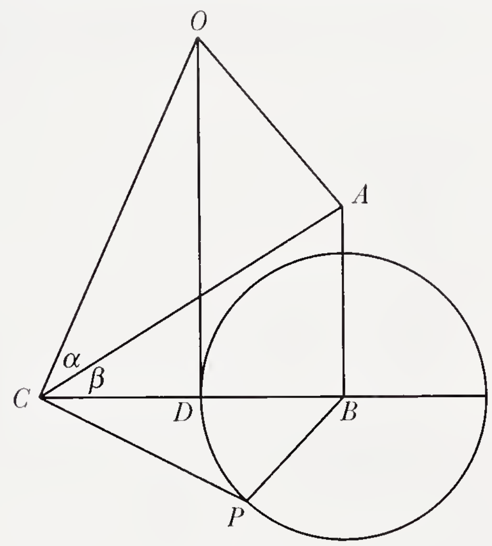

Figure 343,2

The emergent light will be either a maximum or a minimum when $p = 0°$ or $n\pi$.

The minimum will be zero, or blackness, only in the following cases,

1. When $\alpha + \beta = \frac{\pi}{2}$ and $p = 0$ or $2n\pi$.

2. When $\alpha - \beta = \frac{\pi}{2}$ and $p = 2(n+1)\pi$.

3. When $\alpha = 0$ and $\beta = \frac{\pi}{2}$.

4. When $\alpha = \frac{\pi}{2}$ and $\beta = 0$.

To compare our results with the experiments, we observe that for a given thickness of the crystal p is a function of the *kind of light*, so that in passing from one end of the spectrum to the other the value of p increases (or diminishes) in a continuous manner. When the film is thick, p will make several entire revolutions within the spectrum. When it is thin, there will be only one or two, or a fraction of a revolution. Take the case of a thick film, then there will be a certain set of black bands when $\beta = \frac{\pi}{2} - \alpha$. We may call these No. 1. For these $p = 2n\pi$.

When $\beta = \frac{\pi}{2} + \alpha$ there will be another set of black bands, No. 2, intermediate in position to No. 1. For these $p = (2n+1)\pi$.

When $\beta = 0$ or $\frac{\pi}{2}$ the system of bands vanishes.

When $\beta = -\alpha$ the black bands of No. 1 become bright and of maximum intensity.

When $\beta = \alpha$ the black bands of No. 2 become bright and of maximum intensity.

When $\alpha = \frac{\pi}{4}$ all these phenomena are at their greatest distinctness.

In turning the analyser there is simply a dissolution of one system into the other, without motion of the system of bands in the case of a single plate of crystal. But if we place the crystal with its axis inclined 45° to the plane of primitive polarisation, and place above this a film of retardation $\frac{\pi}{2}$ with its axis parallel to the original polarisation, then we have as before for the light emerging from the first crystal,

$$x' = c\frac{1}{\sqrt{2}}\cos(nt+p) \qquad y' = c\frac{1}{\sqrt{2}}\cos nt.$$

Resolving these rays in the direction of the axis of the second film, we have

$$x'' = \frac{1}{2}c(\cos(nt+p)+\cos nt)$$

$$y'' = \frac{1}{2}c(\cos(nt+p)-\cos nt),$$

and since x'' is retarded $\frac{\pi}{2}$ it becomes

$$x'' = \frac{1}{2}c(\sin(nt+p)-\sin nt),$$

y'' remaining the same. We may put these values into the form

$$x'' = c\cos\left(nt+\frac{p}{2}\right)\cos\frac{p}{2}$$

$$y'' = c\cos\left(nt+\frac{p}{2}\right)\sin\frac{p}{2}.$$

This shows that after emerging from the circular polarising film the ray is plane-polarised, that the plane of polarisation inclined $\frac{1}{2}p$ to that of primitive polarisation.

If the emergent light is analysed by a dispersion prism, and a Nicol's prism[(6)] inclined β to the plane of primitive polarisation, there will be black bands (perfectly black) for all colours for which

$$p = 2\beta \quad \text{or} \quad 2\beta + 2n\pi,$$

and as the prism is turned these bands will march forwards in a regular manner across the spectrum.

This very beautiful experiment, in which the phenomena of rotatory polarisation are imitated, is not so well known as it deserves to be. One form of it is due, I believe, to Biot,[(7)] and another to Wheatstone,[(8)] but the arrangement here described is by far the most convenient.

When the second plate is thick, then for some points of the spectrum its retardation is $(2n+\frac{1}{2})\,\pi$. At these points the bands will move forwards when the analyser is turned. At an intermediate set of points the retardation is $(2n-\frac{1}{2})\,\pi$. At these points the bands will appear to move backwards. At intermediate points the retardation is $n\pi$. At these points the bands will not move, but will become deeper or fainter. I suppose this to be the explanation of the experiment described at p. 181, but the arrangement of the films is not very precisely described.[(9)]

The experiments with the rings in crystals are very well described, and must be beautiful,[(10)] but are not so instructive to a beginner as those with the selenite plates. Those, however, who have made out the meaning of the experiments first described have a good right to regale themselves with gorgeous entanglements of colour.

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(6) See Volume I: 117.

(7) J. B. Biot, 'Mémoire sur les rotations que certaines substances impriment aux axes de polarisation des rayons lumineux', *Mémoires de l'Académie Royale des Sciences*, **2** (1819): 41–136.

(8) Charles Wheatstone, 'Experiments on the successive polarization of light, with the description of a new polarizing apparatus', *Proc. Roy. Soc.*, **19** (1871): 381–9 (read 23 March 1871). In a Royal Institution lecture delivered on 3 February 1871, 'On some experiments on successive polarization of light made by Sir C. Wheatstone', *Proceedings of the Royal Institution of Great Britain*, **6** (1870–2): 205–8, on 205, William Spottiswoode remarked that 'The experiments which formed the subject of this discourse were made by Sir Charles Wheatstone some years ago'. Maxwell's reference to Wheatstone is not therefore inconsistent with the suggested date of Summer 1870 for this text.

(9) Deas, 'On spectra formed by the passage of polarised light': 181; 'a circularly polarising film is interposed between the analyser and the film producing the bands'.

(10) Deas, 'On spectra formed by the passage of polarised light': 182–5; and see Volume I: 99–100.

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FRAGMENT OF A DRAFT OF THE 1870 PRESIDENTIAL ADDRESS TO SECTION A OF THE BRITISH ASSOCIATION[1]

circa SUMMER 1870

From the original in the University Library, Cambridge[2]

[ON IRREVERSIBILITY AND ENTROPY]

[...] In the case of the interdiffusion of two different gases, they can be separated again by chemical means,[3] but no natural process can be even thought of which will bring all the individual molecules which are now in the upper part of the room into the upper part again, after they have once been diffused among the lower particles.

This is a case of the irreversible diffusion of material bodies, but the conduction of heat is an example of the diffusion of energy, and it has been pointed out by Sir W. Thomson that this diffusion is not only irreversible, but that it is constantly diminishing that part of the stock of energy which exists in a form capable of being converted into mechanical work.[4] This is Thomsons theory of the irreversible dissipation of energy, and is equivalent to Clausius doctrine of the growth of what he calls Entropy.[5]

The irreversible character of this process is symbolically embodied in Fouriers theory of the conduction of heat, where the formulae themselves indicate a possible solution for all positive values of the time but assume

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(1) Published in *Nature*, **2** (1870): 419–22, and in the *Report of the Fortieth Meeting of the British Association for the Advancement of Science; held at Liverpool in September 1870* (London, 1871), part 2: 1–9 (= *Scientific Papers*, **2**: 215–29).

(2) ULC Add. MSS 7655, V, h/6. Only fragments of the manuscript are extant: only those folios which differ from the published Address are reproduced here.

(3) Thomas Graham, 'On the absorption and dialytic separation of gases by colloid septa', *Phil. Trans.*, **156** (1866): 399–439. See Number 264.

(4) William Thomson, 'On the dynamical theory of heat, with numerical results deduced from Mr Joule's equivalent of a thermal unit and M. Regnault's observations on steam', *Trans. Roy. Soc. Edinb.*, **20** (1851): 261–88; and Thomson, 'On a universal tendency in nature to the dissipation of mechanical energy', *Proc. Roy. Soc. Edinb.*, **3** (1852): 139–42, esp. 140 (= *Math. & Phys. Papers*, **1**: 174–210, 511–14). Maxwell had first raised the issue in his letter to Thomson of 15 May 1855 (Volume I: 307).

(5) In 1870 Maxwell was still unclear about the concept of entropy. See Number 286 and his discussion in his *Theory of Heat* (London, 1871): 186–8; and Number 483 esp. note (20).

critical values when the time is made zero, and become absurd when the time is assumed to be negative.(6)

The idea which these researches impress on the mind when we follow the natural course of time, is that of an ultimate state of uniform diffusion of energy, which however is not actually reached in any finite time.

But if we reverse the process, and inquire into the former state of things by causing the symbol of time to diminish, we are led up to a state of things which cannot be conceived as the result of any previous state of things, and we find that this critical condition actually existed at an epoch, not in the utmost depths of a past eternity, but separated from the present time by a finite interval.

This idea of a beginning is one which the physical researches of recent times have brought home to us more than any observer of the course of scientific thought in past times would have had reason to expect.(7)[...]

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(6) See Number 339, esp. note (7).

(7) Compare the *Theory of Heat*: 245–8. See William Thomson, 'On the secular cooling of the earth', *Trans. Roy. Soc. Edinb.*, **23** (1861): 157–70 (= *Math. & Phys. Papers,* **3**: 295–311), Maxwell's source for these remarks.

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BRITISH ASSOCIATION PAPER ON HILLS AND DALES

[SEPTEMBER 1870]

From the *Report of the British Association for 1870*[1]

ON HILLS AND DALES[2]

After defining level surfaces and contour-lines on the earth's surface, the author showed that the only measure of the height of a mountain which is mathematically consistent with itself is found by considering the work done in ascending the mountain from a standard station.

By considering a level surface, such as that of the sea, which is supposed gradually to rise by the addition of water from the level of the deepest sea-*bottom* to the *tops* of the highest mountains, he showed that at first there is but one wet region round the deepest bottom. Afterwards other wet regions appear at other bottom points of the surface and continually enlarge. For every new wet region there is a bottom; and when two wet regions coalesce into one there is a point where the surface is level, but neither a top nor a bottom, and this may be called a *Bar*. When a wet region, as the water rises, throws out arms and embraces within it a dry region, there is another level point which may be called a *Pass*. The wet region then becomes cyclic. When the water covers the top of the island thus formed the wet region loses its cyclosis again, and at last, when all the tops are covered, the wet region extends over the whole globe. Hence the number of mountain-tops is equal to the number of passes plus one, and the number of bottoms is equal to the number of bars plus one.

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(1) *Report of the Fortieth Meeting of the British Association for the Advancement of Science; held at Liverpool in September 1870* (London, 1871), part 2: 17–18.

(2) On 10 March 1870 Maxwell's paper on 'Topographical geometry' was read (by Robert Tucker) to the London Mathematical Society; see the *Proceedings*, **3** (1870): 82, where it is recorded that Arthur Cayley 'made remarks' on the paper. The published version of the paper (of which the present text is, essentially, an abstract), 'On hills and dales', *Phil. Mag.*, ser. 4, **40** (1870): 421–7 (= *Scientific Papers*, **2**: 233–40), is prefixed by a letter (dated 12 October 1870) from Maxwell to the editors of the *Philosophical Magazine*: 'I find that in the greater part of the substance of the following paper I have been anticipated by Professor Cayley, in a memoir "On Contour and Slope Lines", published in the *Philosophical Magazine* in 1859 (S. 4. Vol. XVIII p. 264). An exact knowledge of the first elements of physical geography, however, is so important, and loose notions on the subject are so prevalent, that I have no hesitation in sending you what you, I hope, will have no scruple in rejecting if you think it superfluous after what has been done by Professor Cayley.'

The author then considered lines of slope which are normal to the contour-lines. In general a line of slope is terminated by a top on the one side and by a bottom on the other. At a pass or a bar, however, there is a singularity. Two lines of slope can be drawn through this stationary point; one of these is terminated by two tops and is a line of watershed, the other is terminated by two bottoms and is a line of watercourse. The watershed intersects the watercourse at right angles.

If we consider all the watersheds which meet at the same mountain-top, each of these will reach a pass or a bar. The watercourses, which also pass through these points, form a closed boundary, which is that of the region occupied by all the lines of slope which meet at the mountain-top. This region round the mountain is called a Hill.

In the same way there is a system of watersheds forming the boundary of a region called a Dale, within which all the lines of slope run to the same bottom.

The whole surface of the earth may be divided into Hills, the number of these being the same as that of their Tops.

By an independent division, the whole surface may be divided into Dales, each Dale having a different Bottom.

Besides this, we may, by superposing these divisions, consider the earth as divided into Slopes, each slope being bounded by two watersheds and two watercourses, and being named from the top and the bottom between which all its lines of slope run.

The number of Slopes is shown to be equal to the total number of Tops, Bottoms, Passes, and Bars minus two.(3)

(3) Compare Number 317.

LETTER TO PETER GUTHRIE TAIT

7 NOVEMBER 1870

From the original in the University Library, Cambridge(1)

Glenlair
Dalbeattie
Nov 7 1870

Dear Tait

$$\nabla = i\frac{d}{dx}+j\frac{d}{dy}+k\frac{d}{dz}.^{(2)}$$

What do you call this? Atled? I want to get a name or names for the result of it on scalar or vector functions of the vector of a point.

Here are some rough hewn names.(3) Will you, like a good Divinity shape their ends properly so as to make them stick.

(1) The result of ∇ applied to a scalar function might be called the slope of the function. Lamé would call it the differential parameter,(4) but the thing itself is a vector, now slope is a vector word, whereas parameter has, to say the least, a scalar sound.

2 If the original function is a vector then ∇ applied to it may give two parts. The scalar part I would call the Convergence of the vector function and the vector part I would call the twist of the vector function.(5)

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(1) ULC Add. MSS 7655, I, b/16. Previously published in Knott, *Life of Tait*: 143–4.

(2) See P. G. Tait, *An Elementary Treatise on Quaternions* (Oxford, 1867): 221, 267; and Tait, 'Formulae connected with small continuous displacements of the particles of a medium', *Proc. Roy. Soc. Edinb.*, **4** (1862): 617–23, esp. 618; i, j, k are unit vectors at right angles to each other, which Tait (*Quaternions*: 46–7) terms 'quadrantal versors'. Maxwell had discussed Hamilton's '*characteristic of operation*' ∇ in his letter to Tait of 11 December 1867 (Number 277). See William Rowan Hamilton, *Lectures on Quaternions: containing a Systematic Statement of a New Mathematical Method* (Dublin, 1853): 610 (§620); and also W. R. Hamilton, 'On quaternions; or on a new system of imaginaries in algebra', *Phil. Mag.*, ser. 3, **31** (1847): 278–93, esp. 291.

(3) See Number 347; and Maxwell's discussion of quaternions in his essay 'On the mathematical classification of physical quantities', *Proceedings of the London Mathematical Society*, **3** (1871): 224–32 (= *Scientific Papers*, **2**: 257–66).

(4) Gabriel Lamé, *Leçons sur les Coordonnées Curvilignes et leurs Divers Applications* (Paris, 1859): 6; and see Number 277 note (18).

(5) Hamilton's terms 'scalar' and 'vector' are used by Tait in his *Quaternions*: 49, where he gives a definition: 'a quaternion, in general, may be decomposed into the sum of two parts, one numerical, the other a vector. Hamilton calls them the SCALAR, and the VECTOR, and denotes them respectively by the letters S and V prefixed to the expression for the quaternion'. See also Number 347.

(Here the word twist has nothing to do with a screw or helix. If the words *turn* or *version* would do they would be better than twist for twist suggests a screw.)

Twirl is free from the screw notion and is sufficiently racy. Perhaps it is too dynamical for pure mathematicians so for Cayleys sake I might say Curl (after the fashion of Scroll).

Hence the effect of ∇ on a scalar function is to give the slope of that scalar and its effect on a vector function is to give the convergence and the twirl of that vector.

The result of ∇^2[(6)] applied to any function may be called the concentration of that function because it indicates the mode in which the value of the function at a given point exceeds (in the Hamiltonian sense) the average value of the function in a little spherical surface drawn round it.

Now if σ be a vector function of ρ and F a scalar function of ρ[(7)]

∇F is the slope of F

$V\nabla.\nabla F$ is the twirl of the slope which is necessarily zero

$S\nabla.\nabla F = \nabla^2 F$ is the convergence of the slope, which is the concentration of F.

Also $S\nabla\sigma$ is the convergence of σ

$V\nabla\sigma$ is the twirl of σ.[(8)]

Now the convergence being a scalar if we operate on it with ∇ we find that it has a slope but no twirl.

The twirl of σ is a vector function which has no convergence but only a twirl.

Hence $\nabla^2\sigma$, the concentration of σ is the slope of the convergence of σ together with the twirl of the twirl of σ & $\therefore$ the sum of two vectors.

What I want is to ascertain from you if there are any better names for these things, or if these names are inconsistent with anything in Quaternions, for I am unlearned in quaternion idioms and may make solecisms.

I want phrases of this kind to make statements in electromagnetism and I do not wish to expose either myself to the contempt of the initiated, or Quaternions to the scorn of the profane.

Yours truly

J. Clerk Maxwell

(6) See Number 277.

(7) On the use of the symbols S and V see note (5).

(8) For these applications of Hamilton's operator ∇ see Tait, *Quaternions*: 267–8. See Maxwell, 'On the mathematical classification of physical quantities': 230–2 (= *Scientific Papers*, **2**: 263–6) and the *Treatise*, **1**: 28–9 (§25) where he develops the argument.

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MANUSCRIPT ON THE APPLICATION OF QUATERNIONS TO ELECTROMAGNETISM

NOVEMBER 1870[1]

From the original in the University Library, Cambridge[2]

ON THE APPLICATION OF THE IDEAS OF THE CALCULUS OF QUATERNIONS TO ELECTROMAGNETIC PHENOMENA[3]

The invention of the Calculus of Quaternions by Hamilton is a step towards the knowledge of quantities related to space which can only be compared for its importance with the invention of triple coordinates by Descartes.[4]

The limited use which has up to the present time been made of Quaternions must be attributed partly to the repugnance of most mature minds to new methods involving the expenditure of thought and partly to the hitherto undeveloped state of the mathematical investigations in which their peculiar power is most apparent.

The ideas of the calculus of quaternions may be distinguished from its operations and methods. I propose at present to shew how these ideas may be applied to our subject without entering on the operations and methods of the Calculus.

The quantities treated of in the calculus of quaternions are of two essentially distinct kinds Scalars and Vectors.[5] A Scalar quantity is one which does not involve direction. The Mass of a body, the quantity of electricity in a body the potential at a point are instances of scalar quantities.

A Vector quantity is one which has direction as well as magnitude and is such that a reversal of its direction reverses its sign. The typical quantity of this kind is the displacement of a point which is represented by a line drawn from its original to its final position.

The velocity of a body, the force acting on it an electric current, the magnetization of a particle of iron are instances of vector quantities.

There are other quantities which have reference to direction which are not vector quantities. Stresses and Strains are examples of these. These quantities

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(1) See Number 348.

(2) ULC Add. MSS 7655, V, c/15.

(3) Compare Maxwell's essay 'On the mathematical classification of physical quantities', *Proceedings of the London Mathematical Society*, **3** (1871): 224–32 (= *Scientific Papers*, **2**: 257–66), read on 9 March 1871, where these quaternion arguments are developed. See also the *Treatise*, **1**: 27–9 (§§25–6).

(4) See also Number 485.

(5) See Number 346 note (5).

may be turned through two right angles without altering their value. They may be called [linear and vector functions of a vector.][(6)]

The symbol of a vector is understood to express its direction as well as its magnitude. This is the fundamental idea of the calculus of quaternions. When the symbol of a vector is put before us, we must conceive a quantity definite in direction and magnitude. The single symbol implies as much as the three components of the quantity do in the Cartesian or any other coordinate system. But though the single symbol implies all this, it does not and cannot express it numerically for three numerical equations are required to express a directed quantity.

The calculus of quaternions is not therefore like the Cartesian geometry a method of applying the science of number to the investigation of space, it is a calculus founded on the independent investigation of space and in which several of the rules relating to operations with numbers are not applicable.

The sum of two vectors is a vector which is the resultant, in the mechanical sense, of the two vectors.

Thus if OA and OB are two vectors

$$OA+OB = OC$$

and

$$OA-OB = BA.$$

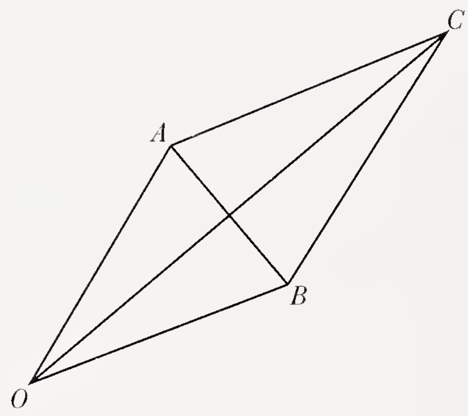

Figure 347,1

The product of two vectors is a quantity consisting of a scalar and a vector part. The scalar part of the product of OA and OB is $OA\,.\,OB\cos AOB$.

The vector part of $OA\,.\,OB$ is a vector perpendicular to the plane of the paper drawn downwards

and is equal to $OA\,.\,OB\sin AOB$

or to the area of the parallelogram formed on OA and OB.

For instance, if OA represents a force and OB the displacement of a body acted on by the force then the work done by the displacement is a quantity which has no direction and is therefore scalar. In fact it is the scalar part of $OA\,.\,OB$ which is written

$$S.(OA\,.\,OB) = OA\,.\,OB\cos AOB.$$

The moment of the force OA acting at B about the origin O is a quantity whose direction is that of the axis through O about which the moment is taken, that is, the perpendicular to the plane of OA and OB drawn upwards.

(6) As in the *Treatise*, **1**: 10 (§12). Space in the MS.

It is therefore a vector quantity and in this case its value is the vector part of the product of OA and OB which is written

$$V.(OA.OB) = OA.OB \sin AOB.$$

The position of a point in space is generally expressed by the vector, ρ, drawn from the origin to that point and any quantity whose value depends on the position of the point is said to be a function of ρ.

Thus the potential of a point is a scalar function of ρ, and the resultant force at a point is a vector function of ρ.

Since scalar quantities are of the same kind as those expressed by the symbols we have been in the habit of using we shall retain the same symbols to express them.

For vector quantities Hamilton uses Greek letters but since in electromagnetism we have a large number of different vector quantities we shall express them by German capitals. All such symbols must be understood to have direction as well as magnitude.

Vector Functions of the Electromagnetic Field[7]

$\mathfrak{A}$ = Electromagnetic Momentum
$\mathfrak{B}$ = Magnetic Force
$\mathfrak{C}$ = Electric Current (Total)
$\mathfrak{D}$ = Electric Displacement
$\mathfrak{E}$ = Electromotive Force
$\mathfrak{F}$ = Mechanical Force
$\mathfrak{G}$ = Velocity
$\mathfrak{H}$ = Magnetization
$\mathfrak{I}$ = Magnetic Induction
$\mathfrak{K}$ = Conduction current

As examples of Addition we have

$$\mathfrak{I} = \mathfrak{B} + 4\pi\mathfrak{H}$$

or the magnetic induction is the resultant of the magnetic force and 4π times the magnetization.[8]

We have also

$$\mathfrak{C} = \mathfrak{K} + \dot{\mathfrak{D}}$$

---

(7) See the *Treatise*, 2: 236–7 (§618) for a list of 'principal vectors', which differs from the present list. In the *Treatise* Maxwell writes $\mathfrak{B}$ for magnetic induction $\mathfrak{H}$ for magnetic force, and $\mathfrak{I}$ for intensity of magnetisation.

(8) See the *Treatise* 2: 238 (§619) where he writes $\mathfrak{B} = \mathfrak{H} + 4\pi\mathfrak{I}$ (see note (7)). He follows this usage in Number 353, see esp. note (15).

or the total electric current is the resultant of the current of conduction and the rate of variation of the electric displacement.[9]

The symbol ∇ placed before a function of the vector ρ denotes that the operation denoted by

$$\nabla = i\frac{d}{dx}+j\frac{d}{dy}+k\frac{d}{dz} \; {}^{*\,(10)}$$

is to be performed on the function, $i\ j\ k$ being three unit vectors at right angles to each other.

We shall first consider the effect of ∇ on f a scalar function of ρ

$$\nabla f = i\frac{df}{dx}+j\frac{df}{dy}+k\frac{df}{dz}.$$

The interpretation of this equation is that ∇f is a vector, the components of which are $\frac{df}{dx}$ in the direction of x, $\frac{df}{dy}$ in that of y and $\frac{df}{dz}$ in that of z.

For instance if f is the electric potential of a point then $-\nabla f$ represents in direction and magnitude the resultant electromotive force at the point due to the variation of the potential from point to point in space. I shall call ∇f the *slope* of the scalar function f.[11] Lamé† uses the term Differential Parameter[12] but neither the term itself nor the mode in which Lamé uses it indicates that the quantity referred to has direction as well as magnitude. I have used the word slope for want of a better to express the vector which indicates at any point the rate of variation of a scalar quantity with the variation of position in space. The slope is measured in the direction in which the scalar increases most rapidly and its length indicates this rate.

Let us next examine the effect of ∇ upon a vector function of ρ, for instance

$$\sigma = i\xi+j\eta+k\zeta.$$

Performing the operation and remembering the Quaternion rules for the multiplication of $i\ j$ and k we find that the result consists of two parts. The scalar part is

$$S\nabla\sigma = -\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right)$$

* See Taits Quaternions §364[10]

† Fonctions Inverses[12]

(9) See the *Treatise*, **2**: 238 (§619).

(10) P. G. Tait, *An Elementary Treatise on Quaternions* (Oxford, 1867): 267–8.

(11) Compare Maxwell's discussion in his essay 'On the mathematical classification of physical quantities': 230–2 (= *Scientific Papers*, **2**: 263–6).

(12) Gabriel Lamé, *Leçons sur les Fonctions Inverses Transcendantes et les Surfaces Isothermes* (Paris, 1857): 2, where he uses the symbol Δ_2 for ∇^2; see Number 277. Lamé had used the expression '*paramètres differentiels*' in his *Leçons sur les Coordonnées Curvilignes*: 6; see Number 346 note (4).

and the vector part is

$$V\nabla\sigma = i\left(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\right)+j\left(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\right)+k\left(\frac{d\eta}{dx}-\frac{d\xi}{dy}\right).$$ [13]

Here ξ η and ζ are the rectangular components of the vector function. If we suppose σ to represent the velocity of a fluid then $S\nabla\sigma$ represents the rate at which the motion of the fluid converges to a given point. I propose therefore to call $S\nabla\sigma$ the *convergence* of the vector σ.

If σ_0 is the value of σ at a given point then if $V\nabla\sigma$ is positive the vector $\sigma-\sigma_0$ near the point will be directed more towards the point than from it hence the propriety of the term convergence.

The vector part of $\nabla\sigma$ or $V\nabla\sigma$ is indicated by its three components. If σ is the velocity of a fluid then $V\nabla\sigma$ is the rotation of the particles of the fluid, the direction of this vector being the axis of rotation drawn in such a direction that the rotation appears to an eye looking in the direction of the axis to be opposite to the hands of a watch.

I propose to call $V\nabla\sigma$ the *curl* of the function σ. It might be called rotation, version, twist or twirl but all these names have motion implied in them so that I prefer the word curl which has not hitherto been used in any other mathematical sense.

At A we have an illustration of convergence without curl, at B of curl without convergence and at C of curl combined with convergence.

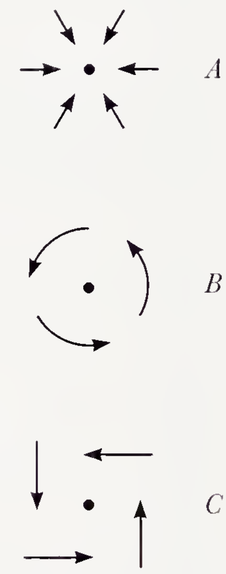

Figure 347,2

If σ represent the velocity of an incompressible liquid, the convergence of σ is zero.

If σ represent the resultant force of any system of attracting points then the curl of σ is zero.

If we now take as the original vector function ∇f or the slope of the scalar function f and perform on it the operation ∇ we shall find

$$\nabla^2 f = S\nabla.\nabla f$$

for

$$V\nabla.\nabla f = 0.$$

This may be shown symbolically by performing the operation ∇ on itself.

(13) Compare Maxwell's statement of these quaternion expressions in the *Treatise*, **1**: 28 (§25), and see Tait, *Quaternions*: 268–9. Compare also Tait, 'On Green's and other allied theorems', *Trans. Roy. Soc. Edinb.*, **26** (1870): 69–84, esp. 79, where the expressions for $S\nabla\sigma$ and $V\nabla\sigma$ are given in the form as stated by Maxwell here.

This result is

$$\nabla^2 = -\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right).^{(14)}$$

Hence ∇^2 is a scalar operation and the result of it on any function is scalar or vector as the original function is scalar or vector. Hence $\nabla^2 f$ is a scalar since f is so and $\nabla^2\sigma$ is a vector since σ is so.

If round any point we draw a small sphere whose radius is r then if q is any function whatever

$$q_0 - \frac{3}{4\pi r^3}\sum q\,dv = \frac{r^2}{10}\nabla^2 q$$

where q_0 is the value of q at the centre and the integration is extended over the sphere. In other words the value of q at the centre exceeds the average value of q in the sphere by $\frac{r^2}{10}\nabla^2 q$.

I propose to call $\nabla^2 q$ the *concentration* of q at a point in space because it indicates the excess of the value of q at that point over its mean value in the immediate neighbourhood of the point.

If q is a scalar function the meaning of its mean value is well known. If q is a vector function its mean value is a vector which must be found by the rules for integrating vector functions and the excess of q_0 above this value is also a vector.

Hence we get the following results.

$$\nabla^2 f = S.\nabla.V\nabla f$$

The concentration of a scalar function is also scalar and it is the convergence of the slope of that scalar.

$$\nabla^2\sigma = V\nabla S\nabla\sigma + V\nabla V\nabla\sigma$$

The concentration of a vector function is a vector and it is the sum of two vectors, one of which is the slope of the convergence of the vector and the other is the curl of the curl of the vector.

We also have

$$V\nabla.\nabla f = 0$$

or the slope of a scalar has no curl

$$S\nabla V\nabla\sigma$$

or the curl of a vector has no convergence.

It may also be shown that every vector function without curl is the slope of

(14) Tait, *Quaternions*: 267; W. R. Hamilton, *Lectures on Quaternions* (Dublin, 1853): 611 (§620). See the *Treatise*, **1**: 29 (§26) where Maxwell notes that ∇^2 is 'an operator occurring in all parts of Physics, which we may refer to as Laplace's Operator'.

some scalar, and that every vector function without convergence is the curl of some vector and that every vector function may be represented as the slope of a certain scalar together with the curl of a certain vector.

LETTER TO PETER GUTHRIE TAIT

14 NOVEMBER 1870

From the original in the University Library, Cambridge[1]

Glenlair
14 Nov 1870

D^r^ T′

I return you Smith's letter.[2] If Cadmus had required to use ∇ and had consulted the Phoenician professors about a name for it there can be no question that Nabla[3] would have been chosen on the א ב ג principle.

It is plain that Hamilton's ∇ derives itself with all its congeners from Leibnitz' d which has become consecrated to differentiation along with D ∂ δ $\mathfrak{z}$ &c and a name derived from its shape is hardly the thing.

With regard to my dabbling in Hamilton I want to leaven my book with Hamiltonian ideas without casting the operations into a Hamiltonian form for which neither I nor, I think, the public are ripe.

Now the value of Hamiltons idea of a Vector is unspeakable and so are those of the addition and multiplication of vectors. I consider the form into which he put these ideas, such as the names of Tensor Versor, Quaternion[4] &c important and useful but subject to the approval of the mathematical world. As for the particular symbols, Roman Greek small or capital erect or inverted these should be preserved only for the sake of consistency.

For instance if you have 7 or 8 kinds of vectors in one problem one might use – say German Capitals to distinguish them from the scalar quantities in the same problem.

The names which I sent you were not for ∇ but the results of ∇. I shall send you presently what I have written, which though it is in the form of a chapter of my book is not to be put in but to assist in leavening the rest.[5] I shall take

(1) ULC Add. MSS 7655, I, b/17. Published in extract in Knott, *Life of Tait*: 144.

(2) William Robertson Smith, Tait's assistant at Edinburgh University 1868, Professor of Hebrew at the Free Church Aberdeen 1870 (*DNB*). See Knott, *Life of Tait*: 143, 171, 291–2.

(3) An Assyrian harp. Hence Maxwell's dedication of his poem, written in Edinburgh in September 1871 at the British Association meeting, to Tait as the 'Chief Musician upon Nabla'. See *Life of Maxwell*: 634–6; and Knott, *Life of Tait*: 172–3, a version which includes the Hebrew superscription added by Robertson Smith to Maxwell's autograph original (ULC Add. MSS 7655, V, 1/3).

(4) See P. G. Tait, *An Elementary Treatise on Quaternions* (Oxford, 1867): 33; 'it appears that a quaternion...may itself be decomposed into two factors...the *stretching* factor...is called the TENSOR...[and the] *turning* factor...is called the VERSOR.'

(5) See Number 347.

the learned Auctor and the grim Tortor into my serious consideration though Tortor has a helical smack which is distasteful to me but poison to T.

As for T″[6] who took his Bain[7] to gnaw in the Alps[8] along with the Farbenlehre he cured his distress by applying to the Sortes Bainales. Send his lecture.[9]

I have had no Nablody from you since the motion of a rigid body[10] and will be thankful for it and the lecture which Smith refers to.[11]

Here is something for Sang.[12]

If a heavy particle move on a smooth sphere radius a so that the difference of height at its highest and lowest point is $2c$ then if a circle of radius a be placed so that its highest and lowest points are at the same levels as those of the path of the particle

namely $$b+c \quad \text{and} \quad b-c$$

and if (as is necessary) the height above the mean point (b) to which the velocity in the sphere is due be

$$\frac{b^2+c^2-a^2}{2b}$$

and that in the circle

$$\frac{b^2-c^2+a^2}{2b}$$

then the bodies if started at the same level will preserve equal heights.

From this I get that if an infinite number of such bodies were moving in a

(6) John Tyndall.

(7) Alexander Bain, *Logic*, 2 vols. (London, 1870).

(8) Tyndall had recently published an essay on 'Climbing in search of the sky', *Fortnightly Review*, **13** (1870): 1–15.

(9) In his lecture on the 'Scientific use of the imagination', presented at the meeting of the British Association at Liverpool on 16 September 1870 (which Maxwell had attended: see Numbers 344 and 345), Tyndall had commented critically on Goethe's *Farbenlehre* and Bain's *Logic*. The lecture was published, and subsequently reprinted in Tyndall's *Essays on the Use and Limit of Imagination in Science* (London, 1870): 13–51 (see esp. 13–14).

(10) P. G. Tait, 'On the rotation of a rigid body about a fixed axis', *Trans. Roy. Soc. Edinb.*, **25** (1869): 261–303, where Tait had employed quaternion notation.

(11) Part of an address Tait had delivered to the University of Edinburgh was published as 'Energy, and Prof. Bain's Logic' in *Nature*, **3** (1870): 89–90. Tait trounced Bain's use of the expression 'convertibility of *force*' in his *Logic*, **2**: 30–1.

(12) Edward Sang, 'On the motion of a heavy body along the circumference of a circle', *Trans. Roy. Soc. Edinb.*, **24** (1867): 59–71; Sang, 'Additional note on the motion of a heavy body along the circumference of a circle', *Trans. Roy. Soc. Edinb.*, **26** (1870): 449–57, read 6 February 1871; see *Proc. Roy. Soc. Edinb.*, **7** (1871): 361–5.

sphere then if a small force came to act on them the centre of gravity of the lot would be neither raised nor lowered.

Yours
J.C.M.

The prop is the same in form for a body of two equal axes of inertia moving about a point fixed in the third.

349

LETTER TO JOHN HUTTON BALFOUR[(1)]

28 NOVEMBER 1870

From a holograph copy by Peter Guthrie Tait in the University Library, Cambridge[(2)]

G[lenlair]
D[albeattie]
(a) 28/11/70

D^{r} Prof Balfour

I do not presume to inform an officer of the Society with respect to its recent awards. I saw that T[ait] had got the K[eith] Prize which is or ought to be known to the public.[(3)] I have not yet got a copy of the reasons for w^{h} it was awarded, so if I coincide with them it does not arise from imitation.

The question seems to be What is T′ good for? Now I think him good 1st for writing a book on Q[uaternions],[(4)] and for being himself a living example of a man who has got the Q mind directly from H[amilton]. I am unable to predict the whole consequences of this fact because besides knowing Q T′ has a most vigorous mind & is well able to express himself especially in writing, and no one can tell whether he may not yet be able to cause the Q ideas to overflow all their math. symbols & to become embodied in ordinary language so as to give their form to the thoughts of all mankind.

I look forward to the time when the idea of the relation of two vectors will be as familiar to the popular mind as the rule of 3 & when the fact that $ij = -ji$[(5)] will be introduced into hustings' speeches as a telling illustration. Why not? We have had arithmetical & geometrical series, and lots of old scraps of Math used in speeches.

Nevertheless I do not recommend some of T′′s math papers to be read as

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(a) {Tait} Balfour having asked Maxwell to write something which could be read at a meeting of R.S.E. when I was to get the Keith medal was mystified as follows.

(1) General Secretary of the Royal Society of Edinburgh; see *Proc. Roy. Soc. Edinb.*, **7** (1870): 231.

(2) ULC Add. MSS 7655, II/38. First published in Knott, *Life of Tait*: 149–50, where Tait's abbreviations are expanded.

(3) The letter was apparently written in connection with David Milne Home's address (as Vice-President of the Royal Society of Edinburgh) on 5 December 1870. See *Proc. Roy. Soc. Edinb.*, **7** (1870): 232–307, esp. 234–5, where Milne Home eulogises Tait for the award of the Society's Keith Prize (as announced on 20 December 1869: see *Proc. Roy. Soc. Edinb.*, **7** (1869): 33), and quotes a letter from William Thomson to Balfour, presumably solicited (along with the Maxwell letter reproduced here) as evidence of Tait's contributions to science.

(4) P. G. Tait, *An Elementary Treatise on Quaternions* (Oxford, 1867).

(5) Hamilton's abandonment of the commutative law of multiplication in quaternion algebra. See Tait, *Quaternions*: 46–7; and on the unit vectors *i* and *j* see Number 346 note (2).

an address to the Soc$^{\text{y}}$, ore rotundo. That on Rotation[6] is very powerful, but the last one on Green's and other allied theorems[7] is really great.

The work of math$^{\text{ns}}$ is of two kinds, one is counting, the other is thinking. Now these two operations help each other very much but in a great many investigations the counting is such long & such hard work, that the math$^{\text{n}}$ girds himself to it as if he had contracted for a heavy job and thinks no more that day. Now T′ is the man to enable him to do it by thinking, a nobler though more expensive occupation, and in a way by w$^{\text{h}}$ he will not make so many mistakes as if he had pages of $=^{\text{ns}}$ to work out.

I have said nothing of his book on Heat,[8] because though it is the clearest thing of the sort, it is not so thoroughly imbued with his personality as his Q. works. In this however I am probably entirely mistaken, so I advise you to ask T′ himself who I have no doubt could hit off the thing much better than any one.

I remain
Yours truly
J. Clerk Maxwell

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(6) P. G. Tait, 'On the rotation of a rigid body about a fixed axis', *Trans. Roy. Soc. Edinb.*, **25** (1869): 261–303, the paper for which Tait was awarded the Keith Prize; see *Proc. Roy. Soc. Edinb.*, **7** (1869): 33.

(7) P. G. Tait, 'On Green's and other allied theorems', *Trans. Roy. Soc. Edinb.*, **26** (1870): 69–84.

(8) P. G. Tait, *Sketch of Thermodynamics* (Edinburgh, 1868).

350

LETTER TO JOHN WILLIAM STRUTT

6 DECEMBER 1870

From the original in private possession(1)

Glenlair
Dec 6 1870

I send you my paper on viscosity of gases.(2) The value for air was tested by new experiments the year after and found not to need correction.(3)

Dear Strutt

If this world is a purely dynamical system and if you accurately reverse the motion of every particle of it at the same instant then all things will happen backwards till the beginning of things the rain drops will collect themselves from the ground and fly up to the clouds &c &c and men will see all their friends passing from the grave to the cradle till we ourselves become the reverse of born, whatever that is. We shall then speak of the impossibility of knowing about the past except by analogies taken from the future & so on.(4)

The possibility of executing this experiment is doubtful but I do not think that it requires such a feat to upset the 2nd law of Thermodynamics.

For if there is any truth in the dynamical theory of gases the different molecules in a gas at uniform temperature are moving with very different velocities. Put such a gas into a vessel with two compartments and make a small hole in the wall about the right size to let one molecule through. Provide a lid or stopper for this hole and appoint a doorkeeper, very intelligent and exceedingly quick, with microscopic eyes but still an essentially finite being.

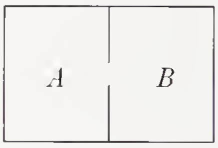

Figure 350,1

Whenever he sees a molecule of great velocity coming against the door

(1) Rayleigh Papers, Terling Place, Terling, Essex. Previously published (in part) in R. J. Strutt, *John William Strutt, Third Baron Rayleigh* (London, 1924): 47–8.

(2) See Number 252.

(3) See Number 260.

(4) For discussion of time-reversal see Number 286 esp. note (12). Compare the discussion by William Thomson, 'The kinetic theory of the dissipation of energy', *Nature*, **9** (1874): 441–4 (= *Math. & Phys. Papers*, **5**: 11–20); if 'the motion of every particle in the universe were precisely reversed at any instant, the course of nature would be simply reversed for ever after... if also the materialistic hypothesis of life were true, living creatures would grow backwards, with conscious knowledge of the future, but no memory of the past, and would become again unborn'. See Thomson's comment on time-reversal appended to Maxwell's letter to Tait of 11 December 1867 (Number 277).

from A into B he is to let it through, but if the molecule happens to be going slow he is to keep the door shut. He is also to let slow molecules pass from B to A but not fast ones. (This may be done if necessary by another doorkeeper at a second door.) Of course he must be quick for the molecules are continually changing both their courses and their velocities.

In this way the temperature of B may be raised and that of A lowered without any expenditure of work, but only by the intelligent action of a mere guiding agent (like a pointsman on a railway with perfectly acting switches who should send the express along one line and the goods along another).

I do not see why even intelligence might not be dispensed with and the thing be made self-acting.(5)

Moral The 2nd law of Thermodynamics has the same degree of truth as the statement that if you throw a tumblerful of water into the sea you cannot get the same tumblerful of water out again.(6)

Many thanks for your two papers.(7) The electromagnetic one has just come in time for me as I am at that part of the subject. Have you seen Helmholtz on the Equations of Motion of Electricity in conductors at rest.(8) It is a very powerful paper.

I have been doing Webers theories of magnetic and diamagnetic induction.(9) There are some mistakes in integration(10) but the theory of moveable magnetic molecules is of great use in explaining phenomena especially all about magnetization demagnetization and remagnetization.

Yours truly

J. Clerk Maxwell

I have improved my book by means of 3 of your suggestions.(11)

1 Wrong sign in an equation about M.(12)

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(5) See Number 277 for the first version of this argument.

(6) See Number 287 for the first statement of this point.

(7) J. W. Strutt, 'On an electromagnetic experiment', *Phil. Mag.*, ser. 4, **39** (1870): 428–35; and Strutt, 'Remarks on a paper by Dr. Sondhauss', *ibid.*, **40** (1870): 211–17.

(8) Hermann Helmholtz, 'Ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper', *Journal für die reine und angewandte Mathematik*, **72** (1870): 57–128.

(9) See the *Treatise*, **2**: 74–87 (§§442–8) and 418–25 (§§832–45); and Numbers 278 and 295.

(10) See the *Treatise*, **2**: 78n (§443); and Number 411 esp. note (4).

(11) In Strutt's paper 'On some electromagnetic phenomena considered in connexion with the dynamical theory', *Phil. Mag.*, ser. 4, **38** (1869): 1–15. Compare the 'Theory of electric circuits' in the *Treatise*, **2**: 206–10 (§§578–84).

(12) The 'coefficient of mutual induction' between two circuits. See Strutt, 'On some electromagnetic phenomena': 6, and the *Treatise*, **2**: 209–10 (§582–4); and see Number 430.

2 ⌊discussion of⌋ Terms of kinetic energy involving products of currents and ordinary velocities.(13)

3 Magnetization as a test of maximum current and as a cause of anomalies in galvanometry.(14)

If therefore any more occur to you and you send me them I shall be thankful.

APPENDIX: FROM THE MANUSCRIPT OF THE *THEORY OF HEAT*

circa LATE 1870(15)

From the original in the University Library, Cambridge(16)

[1] [LIMITATION OF THE SECOND LAW OF THERMODYNAMICS](17)

Before I conclude I wish to direct attention to an aspect of the molecular theory which deserves consideration.

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(13) See Strutt, 'On some electromagnetic phenomena': 6–7, and the *Treatise*, **2**: 206–9 (§§578–81); and see Numbers 333 and 430.

(14) See Strutt, 'On some electromagnetic phenomena': 8–9. On galvanometry, see a card (postmarked 11 January 1871) from William Thomson to Maxwell: 'Adjust R till contact at K makes no change in deflec^{n} of galvan^{r} needle. This determines G, the resistance of galv^{r} coil $a:b::R:G$. Do you know this plan? It is too absurdly obvious yet for years I had never known how to find the resist^{ce} of a galv^{r} coil simply from one self deflection, & without knowing the resistance of the battery. / Interchange battery & galv^{r}, and you have Mance's method for resistance of battery.' (ULC Add. MSS 7655, I, a/6). Henry Mance's paper, on a 'Method of measuring the resistance of a conductor or of a battery, or of a telegraph-line influenced by unknown earth-currents, from a single deflection of a galvanometer of unknown resistance', *Proc. Roy. Soc.* **19** (1871): 248–52, was communicated by Thomson to the Royal Society (recorded as received 12 January 1871). Mance's paper, and Thomson's paper on a 'Modification of Wheatstone's bridge to find the resistance of a galvanometer-coil from a single deflection of its own needle', *Proc. Roy. Soc.*, **19** (1871): 253, repeating the contents of his card to Maxwell, were read to the Royal Society on 19 January 1871. Maxwell gave an account of Thomson's and Mance's methods, for the determination of the resistance of a galvanometer and a battery, in the *Treatise*, **1**: 410–13 (§§356–7).

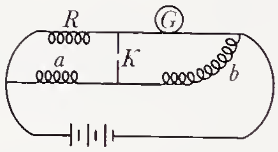

Figure 350,2

(15) It is apparent that Maxwell was well advanced in writing the *Theory of Heat* by April 1870 (see Number 339); that he had abandoned work on the *Treatise* at that time (see Number 341); and that in December 1870 he wrote to J. W. Strutt developing the argument here reproduced from the final folios of the manuscript of the *Theory of Heat*.

(16) ULC Add. MSS 7655, IV, 1 (last 5 ff.).

(17) Compare the *Theory of Heat* (London, 1871): 308–9.

One of the best established facts in thermodynamics is that it is impossible in a system enclosed in an envelope which permits neither change of volume nor passage of heat, in which the temperature and pressure is everywhere the same, to produce any inequality of temperature or of pressure without the expenditure of work. This is the second law of Thermodynamics(18) and it is undoubtedly true as long as we can deal with bodies only in mass and have no power of perceiving or handling the separate molecules of which they are made up. But if we conceive a being whose faculties are so sharpened(19) that he can follow every molecule in its course, such a being whose attributes are still as essentially finite as our own would be able to do what is at present impossible to us.

For we have seen that the molecules of a mass, say, of air at uniform temperature and pressure are moving with velocities by no means uniform though the mean velocity of any great number of them arbitrarily selected is almost exactly uniform.

Now let us suppose that a vessel full of air of uniform temperature and pressure is divided into two portions *A* and *B* by a division in which there is a large hole. Over this hole is placed a sliding plate pierced with a single hole so small that only one molecule can get through at a time.

This sliding plate the mass of which may be supposed excessively small is placed in charge of a being whose senses are so acute that he can see every molecule of the air, at least when it is near the hole and he is instructed that whenever a molecule of division *A* is approaching the hole with more than the mean velocity he is to shift the plate so as to let it pass through to division *B* but when a molecule is coming slowly in the same direction he is to arrange so that it strikes the plate and does not get through the hole.

In the same way he is to allow slow molecules to pass from *B* to *A* and to refuse a passage to fast molecules in the same direction.

The result will be that the mean velocity of the molecules in division *B* will be increased and that of those in division *A* diminished by the mere sliding of the plate without any expenditure of work.

It follows that the second law of thermodynamics is no longer true if we suppose an intelligent and active being able to perform the operations with the sliding piece which we have just described.

This is only one of the instances in which conclusions which we have drawn from our experience of bodies consisting of an immense number of molecules may be found not to be applicable to the more delicate observations and experiments which we may suppose made by one who can perceive and handle the individual molecules which we deal with only in large masses.

(18) See Number 277 notes (9) and (10).

(19) See note (5).

In dealing with masses of matter while we do not perceive the individual molecules we are compelled to adopt what I have described as the statistical method of calculation and to abandon the strict dynamical method in which we follow every motion by the calculus.[20]

It would be interesting to enquire how far those ideas about the nature and methods of science which have been derived from examples of scientific investigation in which the dynamical method is followed are applicable to our actual knowledge of concrete things which as we have seen is of an essentially statistical nature because no one has yet discovered any practical method of tracing the path of a molecule or of identifying it at different times.

I do not think however that the perfect identity which we observe between different portions of the same kind of matter can be explained on the statistical principle of the stability of the averages of large numbers of quantities each of which may differ from the mean.

For if of the molecules of some substance such as hydrogen some were of greater mass than others, we have the means of producing a separation between molecules of different kinds and in this way we should be able to produce two kinds of hydrogen one of which would be somewhat denser than the other. As this cannot be done, we must admit that the equality which we assert to exist between the molecules of hydrogen applies to each individual molecule and not merely to the average of groups of millions of molecules.

[2] [NATURE AND ORIGIN OF MOLECULES][21]

We have thus been led by our study of visible things to a theory that they are made up of a finite number of parts or molecules each of which has a definite mass and possesses other properties. The molecules of the same substance are all exactly alike but different from those of other substances. There is not a regular gradation in the mass of molecules from that of hydrogen which is the least of those known to us to that of [22] but they all fall into a limited number of classes or species the individuals of each species being exactly similar to each other and no intermediate links are found to connect one species with another by a uniform gradation.

We are here reminded of the speculations concerning the relations between the species of living things. Here also we find that the individuals are naturally grouped into species and that intermediate links between the species are wanting. But in each species variations occur and there is a

(20) See the *Theory of Heat*: 288; and Number 478 §6.

(21) Compare the *Theory of Heat*: 310–12.

(22) The published text reads: bismuth.

perpetual generation and destruction of the individuals of which the species consist.

Hence it is possible to frame a theory to account for the present state of things by means of generation variation and discriminative destruction.

In the case of the molecules however each individual is permanent there is no generation or destruction and no variation or rather difference between the individuals of each species.

Hence the kind of speculation with which we have become so familiar under the name of theories of evolution is quite inapplicable to the case of molecules.

It is true that Des Cartes whose inventiveness knew no bounds has given a theory of the evolution of molecules. He supposed that the molecules with which the heavens are nearly filled have received a spherical form from the long continued grinding of their projecting parts so that like marbles in a mill they have 'rubbed each others angles down'.[(23)] The result of this attrition forms the finest kind of molecules with which the interstices of the globular molecules are filled. But besides these he describes another elongated kind of molecules, the *particula striata* which have received their form from their often threading the interstice between three spheres in contact. They have thus acquired three longitudinal ridges and since some of them during their passage are rotating on their axes these ridges are not parallel to the axis but are twisted like threads of a screw. By means of these little screws he most ingeniously explains the phenomenon of magnetism.[(24)]

We cannot understand Des Cartes without bearing in mind that he recognizes no property in matter except extension so that he confounds matter with the space which it occupies. ⟨He is therefore full of error with respect to all kinetic properties depending on mass.⟩[(25)] His opinions therefore about the mutual action of bodies and the parts of bodies whether in motion or at rest are exceedingly strange and in contrast with the perfection of his geometry.

But it is evident that his molecules are very different from ours. His seem to be produced by some general break up of his solid space and to be ground down in the course of ages and though their relative magnitude is in some degree determinate there is nothing to determine the absolute magnitude of any of them.

Our molecules on the other hand are unalterable by any of the processes

(23) R. Descartes, *Principia Philosophiæ* (Amsterdam, 1644): 93; 'quod alicujus corporis anguli sic atterantur' (Pars Tertia, Prop. XLVIII).

(24) Descartes, *Principia Philosophiæ*: 266–78 (Pars Quarta).

(25) Compare Maxwell's comment in Number 377 para 7.

which go on in the present state of things and every individual of each species is of exactly the same magnitude as though they had all been cast in the same mould like bullets and not merely selected and grouped according to their size like small shot.

The individuals of each species also agree in the nature of the light which they emit, that is in their natural periods of vibration. They are therefore like tuning forks all tuned to concert pitch or like watches regulated to solar time.(26)

In speculating on the cause of this equality we are debarred from imagining any cause of equalization on account of the immutability of each individual molecule. It is difficult to conceive of selection and elimination of intermediate varieties for where can these eliminated molecules have gone to if as we have reason to believe the hydrogen &c of the fixed stars is composed of molecules identical in all respect with our own.

The time required to eliminate from the whole of the universe visible to us every molecule whose mass differs from that of our so called elements by processes similar to Grahams method of dialysis(27) which are the only methods we can conceive of at present would exceed the utmost limits ever demanded by evolutionists as many times as these exceed the period of vibration of a molecule.

But if we suppose the molecules to be made at all or if we suppose them to consist of something previously made why should we expect any irregularity to exist among them. If they are as we believe the only material things which still remain in the precise condition in which they first began to exist why should we not rather look for some indication of that spirit of order our scientific confidence in which is never shaken by the difficulty we experience in tracing it in the complex arrangements of visible things, and of which our moral estimation is shown in all our attempts to think and speak the truth and to ascertain the exact principles of distributive justice.

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(26) Compare the concluding remarks of Maxwell's discourse on 'Molecules', *Nature*, **8** (1873): 437–41 (= *Scientific Papers*, **2**: 361–78).

(27) Thomas Graham, 'On the absorption and dialytic separation of gases by colloid septa', *Phil. Trans.*, **156** (1866): 399–439. See Number 264.

LETTER TO GEORGE GABRIEL STOKES

11 JANUARY 1871

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
11 Jan 1871

My dear Stokes

I received the copy of the Adams Prize Essay[2] some time ago, and have been gradually getting into the subject.

Did not you set the theorem about the surface integral

$$\left(\left(\frac{d\gamma}{dy}-\frac{d\beta}{dz}\right)dy\,dz+\ldots+\right)$$

over a surface bounded by the curve s being equal to

$$\left(\alpha\frac{dx}{ds}+\beta\frac{dy}{ds}+\gamma\frac{dz}{ds}\right)ds.^{(3)}$$

I have had some difficulty in tracing the history of this theorem.[4] Can you tell me anything about it.

Yours truly
J. Clerk Maxwell

I hope you saw the eclipse well.[5]

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(1) ULC Add. MSS 7656, M 428. First published in Larmor, *Correspondence*, **2**: 31.

(2) According to the 'Book of Minutes relating to The Adams Prize, kept by the Plumian Professor [James Challis]' (Cambridge Observatory): '1869 Feb. 11, Professor Cayley was appointed Adams Prize adjudicator from the nomination of Downing College, and 1869 Feb. 15, Mr J. C. Maxwell was appointed adjudicator from the nomination of St. Catharine's College. Prize to be adjudged in 1871.' A further note states: '1869 March 15, Adams Prize subject agreed upon in the Lent Term of 1869, to be adjudged in 1871. / A determination of the circumstances under which discontinuity of any kind presents itself in the solution of a problem of maximum or minimum in the Calculus of Variations, and applications to particular instances. / *** It is expected that the discussion of the instances should be exemplified as far as possible geometrically, and that attention be especially directed to cases of real or supposed failure of the Calculus.' Challis subsequently recorded the award of the prize: '1871 April 4, the Adams Prize was adjudged to Mr Todhunter M.A. of St John's College (Subject, *Discontinuity in Calculus of Variations*, proposed in Lent Term of 1869).'

(3) The theorem, known as 'Stokes' theorem', first stated by William Thomson in a letter to Stokes of 2 July 1850 (ULC Add. MSS 7656, K 39; printed in Wilson, *Stokes–Kelvin Correspondence*, **1**: 97), was published by Stokes in his Smith's Prize examination of 1854, where Maxwell was placed equal Smith's Prizeman; see Number 366 note (3) and Volume I: 257–8n. The terms and form in which Maxwell describes the theorem here follow its statement by Thomson and Tait, *Natural Philosophy*: 124 (§190j).

(4) See also Number 366.

(5) The total eclipse of the sun on 22 December 1870; see *Proc. Roy. Soc.*, **19** (1870–1): 123, 290.

352

DRAFT LETTER TO PETER GUTHRIE TAIT[1]

23 JANUARY 1871

From the original in the University Library, Cambridge[2]

Ardhallow
Dunoon
Jan 23 1871

D^r^ T′

Still harping on that Nabla![3]

The present Nablody is on ∇^{-1}.[4]

Let us first consider all space and then particular regions.

We know that the equation $\nabla^2\xi = x$

with the condition $\xi = 0$ at ∞ has no solution

but $$\xi = \frac{1}{4\pi}\iiint\frac{x}{r}\,d(\text{volume}) \qquad \text{(over all space).}$$

This is true whether x be a scalar or a vector and ξ is scalar or vector according as x is.

Hence if σ is a vector function we can express σ as $\sigma = \nabla\tau$ by finding τ from the equation $\nabla^2\tau = \nabla\sigma$.

Now $\nabla\sigma$ is partly scalar $(= m)$ and partly vector $(= \alpha)$. Let P be the potential of m and L M N the constituents of vector potential[5] of α then τ is expressed by an equation of the form

$$\tau = P + iL + jM + kN.$$

See Stokes on Dynamical Theory of Diffraction Camb Trans 1849 or 50.[6]

This can be done only in one way, that is, P, L, M, N are all determinate quantities vanishing at ∞.

Now for the present question.[7]

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(1) A preliminary version of Number 353, but possibly sent to Tait.

(2) ULC Add. MSS 7655, I, b/19.

(3) The operator ∇; see Number 348 note (3).

(4) As discussed by Tait in his paper 'On Green's and other allied theorems', *Trans. Roy. Soc. Edinb.*, **26** (1870): 69–84, esp. 73–4.

(5) See also the *Treatise*, **2**: 27–8, 236 (§§405, 617) on the vector potential.

(6) G. G. Stokes, 'On the dynamical theory of diffraction', *Trans. Camb. Phil. Soc.*, **9** (1849): 1–62, esp. 6–10 (= *Papers*, **2**: 243–327).

(7) As discussed by Tait in his paper 'On some quaternion integrals', *Proc. Roy. Soc. Edinb.*, **7** (1870): 318–20 (read 19 December 1870).

If σ is a vector function and if within the region $\sum$

$$S\nabla\sigma = 0^{(8)}$$

then it is possible to find a vector function τ such that within the region $\sum$, $\sigma = V\nabla\tau$.

The only case in which this is not possible is when $\sum$ is a periphractic region[(9)] enclosing a region $\sum'$, in which $S\nabla\sigma$ is finite for then for a closed surface within $\sum$ but surrounding this region $\sum'$

$$\iint S.\, V\nu\sigma\, ds = M \text{ instead of } 0.$$

This is the reason why it is impossible to express τ as an integral derived from the density as the potential is derived except by some such dodge as the following.

Let $P = \dfrac{1}{r}$ be the potential of a unit at the origin, then if we make

$$F = 0$$

$$G = \frac{d}{dz}\int P\, dx = \frac{xz}{r(y^2+z^2)}$$

$$H = -\frac{d}{dy}\int P\, dx = -\frac{xy}{r(y^2+z^2)}$$

then the vector $\tau = iF+jG+kH$ is such that $V.\nabla\tau = \nabla P$.

Now what is the reason of our having to adopt such an unsymmetrical form to express a thing derived from $P = \dfrac{1}{r}$. Because though $\nabla^2 P = 0$ in the whole periphractic region surrounding the origin, $\iint SU\nu\nabla P\, ds$ is not zero but 4π for any closed surface surrounding the origin but $\iint SU\nu V\nabla\tau^{(10)}$ *must* be zero for every closed surface so that if $V\nabla\tau = \nabla P$ throughout the region $\sum$ that region

(8) The solenoidal condition: see the *Treatise*, **1**: 21, 28 (§20, 25) and Number 396 note (8). See also Numbers 322 note (13) and 353 note (15).

(9) See Maxwell, *Treatise*, **1**: 17 (§18); 'When a region encloses within itself other regions, it is called a Periphractic region.' J. B. Listing had used the term 'Periphraxis' in his 'Der Census räumlicher Complexe', *Abhandlung der Math. Classe der Königlichen Gesellschaft der Wissenshaften zu Göttingen*, **10** (1861): 97–182, esp. 135–6, 168–170, 182, with the same meaning: 'Eigenschaft einer Fläche oder eines Raumes, wenn sie allseitig zusammenhängen und einen Complex oder Complextheil rings umhüllen.' This definition was included by Maxwell in his transcription of the definitions of terms which Listing appended to his paper (ULC Add. MSS 7655, V, c/40).

(10) Read: $\iint SU\nu V\nabla\tau ds$. U is the 'versor': see Number 353 note (9).

cannot be periphractic enclosing the origin. There must be a discontinuity somewhere and we have chosen to make it by boring a hole along the negative part of the axis of x. Along the line it is not true that $V\nabla\tau = \nabla P$.

Of course by integrating F, G, H and taking in all the elements of mass we might concoct a value of τ more or less satisfactory, but it is simply impossible to construct τ so as to be valid for the whole region surrounding a point at which is placed a finite mass. If however there is a region $\sum'$ in which the algebraic sum of the masses is zero, then we may deal with it as with a magnet whose components of magnetization at any point are A, B, C. If we then make

$$F = \iiint \frac{C(y'-y) - B(z'-z)}{r^3}\,dx\,dy\,dz$$

$$G = \iiint \frac{A(z'-z) - C(x'-x)}{r^3}\,dx\,dy\,dz$$

$$H = \iiint \frac{B(x'-x) - A(y'-y)}{r^3}\,dx\,dy\,dz$$

or if

$$\tau = iF + jG + kH$$

$$\mathfrak{I} = iA + jB + kC^{(11)}$$

$$\rho' - \rho = i(x'-x) + j(y'-y) + k(z'-z)$$

$$\tau = \iiint \frac{V.\mathfrak{I}(\rho'-\rho)}{r^3}.$$

We have also

$$P = \iiint \frac{A(x'-x) + B(y'-y) + C(z'-z)}{r^3}$$

and $$V\nabla\tau = \nabla P.$$

If within any closed surface $\nabla^2 P = 0$ then it is always possible and in one way only to find a function P' vanishing at ∞ for which, outside the surface, $\nabla^2 P = 0$ and for which, at the surface

$$SU\nu\nabla P = SU\nu\nabla P'.$$

Then if the surface is a magnetic shell of strength $P - P'$ at every point, the potentials P & P' will be those of the shell.

Also if the surface is a conducting sheet for which $P - P'$ is the stream function the magnetic potentials will still be P and P'.

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(11) Maxwell now uses $\mathfrak{I}$ for the intensity of magnetisation. Compare Number 347 esp. note (7).

353

LETTER TO PETER GUTHRIE TAIT[1]

23 JANUARY 1871

From the original in the University Library, Cambridge[2]

this week only

Ardhallow
Dunoon
Jan 23 1871

D^r^ T′

Still harping on that Nabla?

You will find in Stokes on the Dynamical Theory of Diffraction[3] something of what you want, this at least which I quote from memory.

1 For all space – your eq^n^

$$\nabla\sigma = \nabla^2(\tau + v)$$

where σ is given and τ & v are to vanish at ∞ gives but one solution for τ and for v the first derived by integration from $V\nabla\sigma$ and the second from $S\nabla\sigma$ by the potential method and we then get the result in the form

$$\sigma = V\nabla\tau + \nabla v$$

(because as Helmholtz has shown (Wirbelbewegung) $S\nabla\tau = 0$).[4] All this is as old as 1850 at least. See Stokes.

Now we leave all space and consider a region $\sum$ within which $\nabla^2 P = 0$ and therefore ∇P has no convergence. Now if a vector function has no convergence it ought to be capable of being represented as the curl of a vector function[5] or there ought to be a vector σ such that

$$V\nabla\sigma = \nabla P.$$

The simplest case to begin with is of course the potential due to unit of mass at the origin. Find σ and τ for that case! The difficulty arises from the fact that the region in which $\nabla^2 P = 0$ is here periphractic and surrounds completely the origin where this is not true. If we draw a closed surface

(1) Compare Number 352.

(2) ULC Add. MSS 7655, V, b/18. Published in part in Knott, *Life of Tait*: 145–7.

(3) G. G. Stokes, 'On the dynamical theory of diffraction', *Trans. Camb. Phil. Soc.*, **9** (1849): 1–62, esp. 6–10 (= *Papers*, **2**: 243–327).

(4) Hermann Helmholtz, 'On the integrals of the hydrodynamical equations, which express vortex-motion', *Phil. Mag.*, ser. 4, **33** (1867): 485–512, esp. 495.

(5) On the terms 'convergence' and 'curl' see Numbers 346 and 347.

including the origin then

$$\iint SU\nu\nabla P\,ds = 4\pi$$

whereas $\iint SU\nu V\nabla\sigma^{(6)} = 0$, *necessarily*.(7)

Hence to make it impossible for the region $\sum$ to include the origin we must get rid of periphraxy by drawing a line from the origin to ∞ and defining the region $\sum$ so as not to interfere with this line.

We may then write p for $\dfrac{1}{r}$ and

$$P = -\int_0^\infty S\nabla p\,d\rho = p$$

$$\sigma = \int_0^\infty V\nabla p\,d\rho.$$

If we suppose the line to be in the axis of x this gives

$$\sigma = i(0) + j\frac{xz}{r(y^2+z^2)} - k\frac{xy}{r(y^2+z^2)}$$

an exceedingly ugly form for a thing derived from so symmetrical a beginning.

But this cannot be avoided if the algebraic sum of the masses is finite.

If it is 0 we may treat it as magnetic matter.

If in a region $\sum'$ in which there is magnetization the intensity of magnetization be

$$\mathfrak{I} = iA + jB + kC$$

and if $p = \dfrac{1}{r}$ where r is the distance between $x\,y\,z$ and $x'y'z'$ then

$$P = \iiint \frac{A(x'-x)+B(y'-y)+C(z'-z)}{r^3}\,dx'\,dy'\,dz'$$

$$= \iiint S\mathfrak{I}\nabla p\,d\varsigma'$$

or

$$= -\iiint \frac{1}{r}\left(\frac{dA}{dx}+\frac{dB}{dy}+\frac{dC}{dz}\right)dx'\,dy'\,dz'$$

$$= -\iiint pS\nabla\mathfrak{I}\,d\varsigma'.$$

(6) See Number 352 note (10); and on 'Periphraxis' see Number 352 note (9).

(7) See P. G. Tait, 'On some quaternion integrals', *Proc. Roy. Soc. Edinb.*, **7** (1870): 318–20.

Also $\sigma = iF + jG + kH$

where $$F = \iiint \frac{C(y'-y) - B(z'-z)}{r^3} dx'\, dy'\, dz' \quad \&c$$

or $$\sigma = \iiint V\mathfrak{I}\nabla p\, d\varsigma'.$$

All this occurs in passing from the old theory of magnetism to the electromagnetic.

I have put down a lot of imitations of your jargon mainly that you may check me in any solecism. I think if you are making a new edition of 4^nions^(8) you should give prominence to the rules defining the extent of the application of symbols such as V, S, T, U, K,(9) &c which are consecrated letters, not to be used for profane purposes.

The use of dots and brackets should also be defined so as to know how far the virtue of an operator extends.

Here is another view of your case.(10)

Let S be a region within which $\nabla^2 P = 0$ and let P' be a function vanishing at ∞ and having $\nabla^2 P' = 0$ outside S & such that at the surface

$$SU\nu\nabla P = SU\nu\nabla P'.$$

It is well known that there is only one solution for P'.

Now let $P - P' = 4\pi\phi$ at the surface S.

Then $$P = \iint \frac{\phi}{r^3} SU\nu . r\, ds$$

and $$\sigma = \iint \frac{\phi}{r^3} VU\nu . r\, ds.$$

Another expression for the components of σ is

$$F = \iint \frac{\phi}{r^2} dr\, dx \quad G = \iint \frac{\phi}{r^2} dr\, dy \quad H = \iint \frac{\phi}{r^2} dr\, dz$$

the integrations being taken over the surface S and the independent variables being r and *one* of the coordinates $x\, y$ or z.

What do you make of this?

---

(8) P. G. Tait, *An Elementary Treatise on Quaternions* (Oxford, 1867).

(9) V and S are the 'vector' and 'scalar' parts of a quaternion; T and U the 'tensor' ('stretching' factor) and 'versor' ('turning' factor); and Kq denotes the 'conjugate' of a quaternion q, which 'has the same tensor, plane, and angle, only the angle is taken the reverse way'. See Tait, *Quaternions*: 49, 33, 35; and see Numbers 346 note (5) and 348 note (4).

(10) See Tait, 'On some quaternion integrals': 319–20.

You say that the constituents of τ are potentials with densities $\dfrac{1}{4\pi}\dfrac{dP}{dx}$ &c.

Well then take $P = \dfrac{1}{r}$ & $\dfrac{dP}{dx} = -\dfrac{x}{r^3}$ &c then the constituents of τ will be $\dfrac{1}{8\pi}\dfrac{x}{r}$ &c

or
$$8\pi\tau = i\frac{x}{r} + j\frac{y}{r} + k\frac{z}{r}$$

and
$$\nabla\tau = \frac{1}{4\pi}\frac{1}{r} = P \quad \text{a scalar.}$$

In fact whatever scalar form P be if $\nabla^2\tau = \nabla P \quad \nabla\tau = P$ a pure scalar.(11) Multiply this by $d\rho$ (a pure vector) and you get a pure vector $d\rho\,\nabla\tau = d\rho\,P$. Hence your expression(12)

$$S\int V(d\rho\,\nabla)\,\tau = S\int d\rho\,P = 0$$

because if it is anything it is the integral of a vector multiplied by a scalar and that is a pure vector and the scalar part of it is 0.

I suppose this is nonsense arising from our being barbarians to one another. Will you therefore be so kind as to give me a code by which I may interpret the symbols $V\,d\rho\,\nabla$ that is to say, tell me what these symbols, thus arranged, ask me to do.

Helmholtz as you will see goes in for

$$\left.\begin{array}{c} \nabla^2\Psi = -2\dfrac{dP}{dt} \\ \text{where } P \text{ is the electrostatic potential.} \end{array}\right\}^{(13)}$$

(11) In his reply dated 1 February 1871 Tait wrote: 'D[r] J. C. M. [= dp/dt (T's Thermodyn[cs] §162)] / You say – "of whatever scalar form P be, if $\nabla^2\tau = \nabla P$, then $\nabla\tau = P$." I fear not, and with this falls your inferences. Would it were so simple! But in reality it leads $S.\nabla\tau = 0$. / Try your hand at *this*. If $U\nu$ be the unit normal vector at any point of a non-closed surface, and σ any vector, $\iint V.U.\nu\nabla^2\sigma ds = \int V(\nabla d\rho\nabla)\,\sigma$ / where the first extends over the surface, the second round its edge. When am I to see the MSS of the curls of the slopes &c &c. Y[rs] ' (ULC Add. MSS 7655, I, a/7). For Maxwell's thermodynamic signature dp/dt = J. C. M. see Number 339 note (16). The example Tait gives Maxwell is discussed in his 'On some quaternion integrals': 320, and for Maxwell's reply see Number 356. Tait's signature is a compact monogram giving all three initials P. G. T.; see Knott, *Life of Tait*: 92n.

(12) Tait, 'On some quaternion integrals': 320.

(13) Hermann Helmholtz, 'Ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper', *Journal für die reine und angewandte Mathematik*, **72** (1870): 57–128, esp. 79, where Ψ is the potential of dP/dt. Maxwell made notes on Helmholtz's paper (ULC Add. MSS 7655, V, c/23), noting this expression.

Note – the vector σ as determined above is such that $S\nabla\sigma = 0$ so that we may truly say $\nabla\sigma = \nabla P$.

In electromagnetism P is the magnetic potential and ∇P is the magnetic force outside the magnet or inside it in a hollow tube whose sides are parallel to the magnetization,

$$\nabla\sigma = \nabla P \quad \text{outside but inside}$$
$$\nabla\sigma = \nabla P + 4\pi\mathfrak{I}$$

where $\mathfrak{I}$ is the magnetization.(14) $\nabla\sigma$ is the magnetic force in a crevasse $\perp \mathfrak{I}$.(15)

I have not been able to make much of your τ. I coloured some diagrams of lines of force Blue & red but I must study the Astronomer to define the magnetic tints and softness. Sir W. Hamilton (Edin[h]) was partial to redintegration,(16) an operation you should get a symbol for. Among other scientific expressions I would direct your attention to the salutary influence of Demon-stration and Deter-mination, and to two acids recently studied, Periodic and Gallery Thronic acids. The 1[st] you will find use for. The 2[nd] is for the L[d] High Commissioner.

Yours J. C. M.

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(14) See Number 352 esp. note (11).

(15) The expression above may be written $\mathfrak{B} = \mathfrak{H} + 4\pi\mathfrak{I}$ (see the *Treatise*, **2**: 238 (§619)), and expresses the relation between magnetic induction ($\mathfrak{B}$) and magnetic force ($\mathfrak{H}$); see the *Treatise*, **2**: 22–4 (§§397–400). Maxwell here reformulates Thomson's distinction between 'lamellar' and 'solenoidal' distributions of magnetism (see Number 322 note (13)) into a distinction between the 'flux' of magnetic induction and magnetic force. In his 'A mathematical theory of magnetism', *Phil. Trans.*, **141** (1851): 243–85, esp. 275, 277 (= *Electrostatics and Magnetism*: 340–404) Thomson had represented a solenoidal distribution of magnetism by 'the force at a point in an infinitely small crevass tangential to the lines of magnetization'; while in a lamellar distribution the magnetic force 'at a point in an infinitely small crevass perpendicular to the lines of magnetization' differs from the force in a tangential crevass by a term 'equal to the product of 4π into the intensity of the magnetization'.

(16) Sir William Hamilton, *Lectures on Metaphysics and Logic*, ed. H. L. Mansel and J. Veitch, 4 vols. (Edinburgh/London, 1859–60), **2**: 238; 'the law of Redintegration or Totality.... Those thoughts suggest each other which had previously constituted parts of the same entire or total act of cognition.'

REPORT ON A PAPER BY JOHN WILLIAM STRUTT ON THE THEORY OF RESONANCE

31 JANUARY 1871

From the original in private possession[1]

REPORT ON A PAPER 'ON THE THEORY OF RESONANCE' BY THE HON. J. W. STRUTT[2]

The resonators here considered are supposed to be cavities in a rigid and fixed body communicating with the atmosphere through holes or necks and the resonance is of a period such that the wave length of the sound is considerably greater than four times the greatest dimension of the cavity.

The energy of the system may be separated into the kinetic and the potential energy and in this case the kinetic energy may be regarded as residing in the air close to the hole or in the neck of the resonator while the potential energy resides in the air which fills the whole cavity.

Of course this can never be strictly true for there must be variations in the density of the air in the neck and also motion of the air in the cavity, but it may be shown that the smaller the dimensions of the cavity compared with the wave length of its resonance the more nearly may the air in the neck be regarded as moving without change of density and the air in the cavity be regarded as acting like a spring.

We have therefore two questions to study, the motion of a fluid of constant density passing from a vessel into another or into infinite space through a hole or neck the transverse dimensions of which are small compared with those of either vessel.

In this case it may be shown that the motion in the parts of the vessels distant from the neck is very small and that the whole motion depends on the variation of one variable and may be expressed in terms of the quantity of fluid which, since a given epoch has passed through the neck in the positive direction.

If this quantity be called X then the total strength of the current through the neck at a given instant is $\dot{X}$ and if there is but one neck the kinetic energy of the motion is

$$\tfrac{1}{2}A\dot{X}^2$$

(1) Rayleigh Papers, Terling Place, Terling, Essex.

(2) J. W. Strutt, 'On the theory of resonance', *Phil. Trans.*, **161** (1871): 77–118. The paper was received by the Royal Society on 2 July 1870 and read on 24 November 1870; see *Proc. Roy. Soc.*, **19** (1870): 106–7.

where A is a constant depending on the shape of the neck and the density of the fluid. In the generalized language of dynamics A is the Moment of Inertia of the neck or channel. The calculation of this quantity in different cases is one of the principal theoretical results of this paper.

If X represents the volume of fluid we may write

$$A = \frac{\rho}{c} \quad \text{where } \rho \text{ is the density and } c \text{ is a linear quantity.}^{(3)}$$

The momentum of the motion is $\frac{\rho}{c}\dot{X}$ and the force which produces it is $\frac{\rho}{c}\ddot{X}$ since c and ρ are independent of X. This quantity is simply the difference of hydrostatic pressure in the communicating vessels.

Much use is made in the paper of the analogy between the case of such a tube and that of an electrical conductor.(4) If the conductor is of the same shape as the neck and connects two large conducting bodies of the same substance then if X is the quantity of electricity which passes through the conductor $\dot{X}$ will be the strength of the current and $R\dot{X}^2$ will be the dynamical value of the heat generated by friction in unit of time. Also $R\dot{X}$ will be the difference of potentials of the two bodies. Hence cæteris paribus, c is a quantity proportional to the conductivity of the neck.

In fact in the electrical case c is the side of a cube of the same material as the conductor which has the same resistance.

In the hydrodynamical case it is the side of a cube of the same density as the fluid such that the same difference of hydrostatic pressure on two opposite sides will produce the transfer of matter through a fixed plane.(5)

It is in the calculation of this important quantity that the author in my opinion has shown the greatest ability. It is in the approximate solution of problems of which we cannot obtain an exact solution that the mathematician displays the power of judicious selection and adaptation of means to ends which he seldom is credited with by the outer world. In the present case the author has shown how to calculate two quantities one of which is certainly greater and the other certainly less than the quantity to be found and by a sufficiently careful adjustment of the conditions chosen these two quantities may be made to approximate, from opposite sides, to the true value.

As the analogy of the conductor of electricity is perfect and as the author

(3) The quantity c depends on the form of the necks of resonators; see Strutt, 'On the theory of resonance': 78.

(4) See Strutt, 'On the theory of resonance': 81, where the analogy is based on 'the motion of an incompressible fluid' (Maxwell's analogy: see Volume I: 337–50, 357–61).

(5) Strutt, 'On the theory of resonance': 78, 81.

has mixed the two analogous cases in his sketch I shall state his method in the electrical form.

(1) If the conductivity of any portion of a conductor is increased the conductivity of the whole is increased and if the conductivity of any portion is diminished that of the whole is diminished.

(2) If an infinitely thin sheet of any form drawn in a conductor is made perfectly conducting, the conductivity of the conductor will be increased unless the sheet coincides with an equipotential surface, in which case the conductivity of the conductor will remain the same.

Hence the true conductivity is less than that due to any arrangement of perfectly conducting sheets in the body except that in which the sheets are equipotential surfaces.

(3) If the sheet be made a perfect non-conductor it will diminish the conductivity of the body unless the sheet coincides with a surface of flow in the natural state of the body in which case it will not affect the conductivity.

If therefore we calculate the conductivity on two suppositions in the first of which a series of perfectly conducting surfaces is made to intersect the conductor while in the second the current is constrained by tubes of flow then we know that the first value is greater and the second less than the truth unless the surfaces and tubes are chosen to coincide with the equipotential surfaces and lines of flow.(6)

The nearer the forms of the surfaces are to these forms the nearer will the two values approach the truth and since the truth is a minimum of the first value and a maximum of the second small variations in the forms of the surfaces will not affect it.

If F is the function which is constant for each perfectly conducting surface and V its potential then the distance between two consecutive surfaces is $-\dfrac{dF}{\nabla F}$ where $\nabla = i\dfrac{d}{dx}+j\dfrac{d}{dy}+k\dfrac{d}{dz}$(7) and the total current is

$$C = \iint \nabla F\, dS.\frac{dV}{dF}$$

where dS is an element of the surface $F = \text{const}$ and the integration is extended over the whole of this surface which belongs to the conductor.

Hence $$\frac{V}{C} = \int \frac{dF}{\iint \nabla F\, dS} \quad (8)$$

(6) Strutt, 'On the theory of resonance': 98–110. On his use of extremal conditions see Number 355 note (3).

(7) See Number 346 esp. note (2).

(8) See Strutt, 'On the theory of resonance': 109.

and $\frac{V}{C}$ is the resistance of the conductor $\frac{C}{V}$ being the conductivity. The value of the conductivity thus found is certainly not smaller than the truth.[9]

Next let two systems of surfaces of flow ψ_1 and ψ_2 be drawn in the conductor so as to guide the current in lines not differing much from the true lines of flow. If dC be the current flowing in the tube formed by the interaction of ψ_1, ψ_2 $\psi_1 + d\psi_1$, $\psi_2 + d\psi_2$ the intensity of the current is given in velocity and direction by the quaternion expression

$$\frac{dC}{d\psi_1\, d\psi_2} V(\nabla\psi_1 \nabla\psi_2)$$

(where V denotes the operation of finding the vector of the product which follows).[10]

The resistance of the tube is

$$\frac{1}{d\psi_1\, d\psi_2}\int ds V(\nabla\psi_1 \nabla\psi_2)$$

where ds is an element of the length of the tube.

The conductivity of the conductor formed of the system of tubes is

$$\iint \frac{d\psi_1\, d\psi_2}{\int ds\, V(\nabla\psi_1 \nabla\psi_2)}$$

and this is certainly not greater than the true conductivity.

In the case of figures of revolution we put $\psi_2 = \phi$ the angle measured round the axis and the result becomes that in the paper.

The application of this method to finding quantities respectively greater and less than the conductivity of a conductor in the form of a figure of revolution whose sides are inclined to the axis at an angle which never becomes great is one of the most important results of the paper.[11]

The determination of the correction where the section suddenly becomes very great is also important.

In the calculation of this correction the investigation of the potential on itself, of a disk whose density is $1 + \mu r^2$ where r is the distance from the centre might I think be simplified thus.[12]

Begin by finding the potential at the edge of such a disk of radius a. Take

(9) Compare the *Treatise*, **1**: 353–6 (§306), where Maxwell acknowledges his use of Strutt's method.

(10) See Numbers 346 and 347.

(11) Strutt, 'On the theory of resonance': 108–10.

(12) Strutt added a footnote incorporating Maxwell's discussion of this case; see 'On the theory of resonance': 102–3n.

polar coordinates (ρ, θ) the pole being at the edge, then

$$r^2 = \rho^2 - 2\rho a \cos\theta + a^2$$

and
$$V = \iint (1+\mu)\,(\rho^2 - 2\rho a \cos\theta + a^2)\, d\theta\, dr$$

the limits of r being 0 and $2a\cos\theta$ and those of θ $-\frac{\pi}{2}$ and $+\frac{\pi}{2}$.

We get at once
$$V = 4a + \frac{20}{9}\mu a^3.$$

Now let us cut off a strip of breadth da from the edge of this disk. The mass of this strip is $2\pi a(1+\mu a^2)\, da$.

The work done in carrying this strip off to infinity is

$$2\pi a\, da(1+\mu a^2)\left(4a + \frac{20}{9}\mu a^3\right).$$

If we gradually pare the disk down to nothing and carry all the parings to ∞ we find for the total work by integrating with respect to a from a to 0

$$\frac{8\pi}{3}\left(a^3 + \frac{14}{15}\mu a^5 + \frac{5}{21}\mu^2 a^7\right).$$

The potential of the disk on itself in the sense in which this expression is used in the paper is twice this quantity. I think there is less chance of numerical errors if we follow this method of calculation than in the method of the paper, especially if we wish to take in higher powers of r^2 in the expression for the density, as the path of approximation indicates.

I have spoken of these investigations as if they related to the conductivity for electricity because in the acoustical application even the accurate solution of this question is only an approximation to the truth and because in the present state of experimental science the resistance of a conductor can be determined with far greater accuracy than the pitch of a resonance. By means of mercury a conductor of uniform conductivity may be obtained of any required form and since the forms here discussed terminate in large masses, the electrodes may be made so large that no error of imperfect contact can occur.

In the experiments described, the pitch of the resonance was generally lower than that calculated.[13] Is it possible that any part of this difference is due to a yielding of the walls of the cavity.[14] When the resonator is a thin

(13) Strutt, 'On the theory of resonance': 110–18.

(14) In revising his paper for publication Strutt added a discussion (acknowledging Maxwell's suggestion) of the effect of deficient rigidity of the envelope, finding that this would have no sensible effect. See 'On the theory of resonance': 87–8.

glass flask the motions of the sides may be felt by the hand and the author mentions this fact. Any want of rigidity in the sides will diminish the apparent elasticity and so lower the pitch of the resonance. Of course the viscosity of the air in the neck has the same tendency but its effect is probably small.

The method adopted by the author of producing the sound independently of the resonator is better than the common method of blowing over the orifice and so introducing rapid motion probable change of temperature and certain change of density into the part of the apparatus where we have originally supposed no motion but the ebb and flow of the resonance.

When the exciting sound is produced by the voice, the resonator responds to the proper pitch and produces an effect which reacts on the voice in a more marked way than it is distinguished by the ear. See p. 7 of the experimental part.(15)

I have not read any of the works cited by the author(16) so I take his word for his originality in the acoustics and for what he has found accomplished. I think however that besides the very valuable method of approximation by two limits already mentioned the paper in its physical aspect has merits which render it suitable for the Philosophical Transactions.(17)

J. Clerk Maxwell
31 Jan 1871

P.S. I find that if at p 27(18) instead of assuming a stream function ψ, we use this function merely to split up the conductor into tubes of flow, and then determine the conductivity of the conductor as the sum of the conductivities of the tubes, we obtain for the lower limit of the conductivity(19)

$$\frac{\pi}{\displaystyle\int \left|\frac{1}{y^2}\frac{dy}{dx}\right|^2 dx}\log\left\{\frac{1+\displaystyle\int\left|\frac{1}{y^2}\frac{dy}{dx}\right|^2 dx}{\displaystyle\int \frac{1}{y^2}\,dx}\right\}^{(20)}$$

(15) Strutt, 'On the theory of resonance': 112.

(16) These include Hermann Helmholtz, 'Theorie der Luftschwingungen in Röhren mit offenen Enden', *Journal für die reine und angewandte Mathematik*, **58** (1859): 1–72.

(17) In a letter to Stokes of 20 January 1871 (Royal Society, *Referees' Reports*, **7**: 138) R. B. Clifton stated that: 'Mr Strutt's treatment of these questions appears to me to differ from all similar investigations which have come under my notice, and to be applicable to a more extended class of problems than that of which solutions have hitherto been attempted.'

(18) Strutt, 'On the theory of resonance': 105–6.

(19) Read: upper limit. See Number 355 esp. note (6).

(20) y is the radius of the conductor. See Strutt, 'On the theory of resonance': 105–6.

and this value, owing to there being less constraint, is a little nearer to the truth than the value in the paper, as is easily shown by expansion of the logarithm.[21]

(21) See Number 355.

355

LETTER TO JOHN WILLIAM STRUTT

4 FEBRUARY 1871

From the original in private possession(1)

Ardhallow
Dunoon
Feb 4 1871

Dear Strutt

Your letter was forwarded to me here and as I expected to be at home before this I put off answering till now. I shall be at Glenlair by the 8th, I expect.

Your method of determining two quantities, one of which is certainly greater and the other certainly less than the resistance is a most valuable one and as far as I know it is original.(2) I have translated it into electrical language for my book.(3)

If $$Q_1 = \iiint \frac{1}{\alpha^2}(a^2+b^2+c^2)\,dx\,dy\,dz$$

where $$\frac{da}{dx}+\frac{db}{dy}+\frac{dc}{dz} = 0$$

and where $$la+mb+nc = q$$

is given for every point of a closed surface (of course with the condition $\iint q\,dS = 0$) then Q_1 is an absolute and unique minimum when

$$a = -\alpha^2\frac{dV}{dx} \quad b = -\alpha^2\frac{dV}{dy} \quad c = -\alpha^2\frac{dV}{dz}$$

where V is some function of $x\,y\,z$.(4)

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(1) Rayleigh Papers, Terling Place, Terling, Essex.

(2) See Number 354.

(3) See the *Treatise*, **1**: 115–118 (§102), where he acknowledges Strutt's paper 'On the theory of resonance', *Phil. Trans.*, **161** (1871): 77–118 as suggesting his method. Strutt had based his theoretical argument on the treatment of extremal conditions which determine the bounding surface of fluids in Thomson and Tait, *Natural Philosophy*: 229–30, which was itself based on William Thomson's paper 'Notes on hydrodynamics. V. On the vis-viva of a liquid in motion', *Camb. & Dubl. Math. J.*, **4** (1849): 90–4 (= *Math. & Phys. Papers*, **1**: 107–12), where Thomson had applied energy conditions to the analysis of the motion of an incompressible fluid enclosed within a flexible and extensible envelope. See Number 427 note (3) and also Thomson's paper 'Theorems with reference to the solution of certain partial differential equations', *Camb. & Dubl. Math. J.*, **3** (1848): 84–7 (= *Math. & Phys. Papers*, **1**: 93–6) and the *Treatise*, **1**: 103–7 (§98) for the general statement of the theorem Maxwell terms 'Thomson's theorem', now generally known (following Riemann) as 'Dirichlet's principle'.

(4) Compare Maxwell's discussion of the 'Superior limit of the coefficients of potential' in the

If the system is so arranged that q is a linear function of some quantity C then V will also be a linear function of C and we may write $C = KV$ and $Q_1 = \frac{1}{2}CV = \frac{1}{2}KV^2$.

Now if Q_1' be a value of Q_1 found from a wrong distribution of $a\ b\ c$

$Q_1' > Q_1$ and $K < \dfrac{2Q_1'}{V^2}$.

Again if $$Q_2 = \iiint \alpha^2 \left(\overline{\frac{dV}{dx}}\Big|^2 + \overline{\frac{dV}{dy}}\Big|^2 + \overline{\frac{dV}{dz}}\Big|^2\right) dx\,dy\,dz$$

where V is given $= V_1, V_2$ at certain surfaces S_1, S_2 and at other points of the surface

$$l\frac{dV}{dx} + m\frac{dV}{dy} + n\frac{dV}{dz} = 0$$

Q_2 is a minimum when

$$\frac{d}{dx}\alpha^2\frac{dV}{dx} + \frac{d}{dy}\alpha^2\frac{dV}{dy} + \frac{d}{dz}\alpha^2\frac{dV}{dz} = 0.^{(5)}$$

If $$C = \iint \alpha^2\left(l\frac{dV}{dx} + m\frac{dV}{dy} + n\frac{dV}{dz}\right) dS_1$$

$$Q_2 = \tfrac{1}{2}\sum CV = \tfrac{1}{2}RC^2.$$

Hence if Q_2' is the result of an arbitrary form given to V

$$R < \frac{2Q_2'}{C^2}.$$

But in cases where S_1 and S_2 are the only conducting surfaces $KR = 1$ so that $R > \dfrac{V^2}{2Q_1}$.

Your value $\dfrac{1}{\pi}\displaystyle\int \frac{dx}{y^2}\left\{1 + \frac{1}{2}\overline{\frac{dy}{dx}}\Big|^2\right\}^{(6)}$ may be made smaller thus.

You first cut your conductor into layers by the function ψ[7] and assume that the current in each layer is the same.[8] If instead of this assumption you

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Treatise, **1**: 117 (§102), establishing the unique minimum value of the energy quantity Q within a region bounded by a surface S, the quantities a, b, c being subject to the equation of continuity, and l, m, n being the direction-cosines of the normal.

(5) Compare Maxwell's account of the 'Method of approximating to the values of coefficients of capacity, &c.' in the *Treatise*, **1**: 115 (§102).

(6) See Strutt, 'On the theory of resonance': 106. Strutt's expression for the upper limit of the conductivity; his expression for the lower limit is $\int dx/\pi y^2$, where y is the radius of the conductor.

(7) The stream function.

(8) See Strutt, 'On the theory of resonance': 104.

treat the several layers as independent conductors, having the same difference of potential at the ends you will find for the conductivity of the system

$$\frac{\pi}{\displaystyle\int \frac{1}{y^2}\left|\frac{dy}{dx}\right|^2 dx}\log_\epsilon\left(\frac{1+\displaystyle\int\frac{1}{y^2}\left|\frac{dy}{dx}\right|^2 dx}{\displaystyle\int\frac{1}{y^2}dx}\right)$$

As there is less constraint here than in your supposition the conductivity is greater and the resistance less, but the resistance is still greater than the true resistance when there is no constraint.(9)

With respect to colour boxes.

I have had so much trouble with bisulphide of carbon owing to its sensitiveness to change of temperature that I will have no more to do with it.(10)

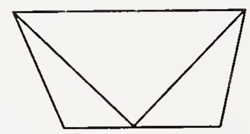

Figure 355,1

M^r Huggins showed me beautiful compound prisms by Grubb(11) which give a large field and great dispersion. If I have time I mean to try something of this kind.

The system of reflexion is very convenient with respect to compactness. It is very necessary however to be able to clean the surfaces well as the surface from which the light finally emerges is exposed to the full light which enters the box. A large lens in front of or close behind the slits would be an improvement for the reason you state. It may be made out of thick plate glass $6\frac{1}{2}$ inches aperture for 15/ at least that was the charge for one which answered me very well though not achromatic.

The only thing to be attended to about the lens near the prisms is the pair of reflected images which must be arranged so as not to be in that part of the field where the two kinds of light are in contact. If they are inconvenient you can stop them out by cutting out a card properly and setting it up in the box.

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(9) See also Number 354. In his reply of 14 February 1871 (see Number 358 note (6)) Strutt acknowledged: 'I believe the new limit you give for the resistance of a conductor of rev^n is different from any in my paper & closer, but it is nearly a year since my paper was written so that I hardly remember.'

(10) For Maxwell's use of a carbon bisulphide prism in his 1862 colour box see Volume I: 711. These prisms were used for spectroscopy, being of great dispersive power. In his paper 'Description of a train of eleven sulphide-of-carbon prisms arranged for spectrum analysis', *Proc. Roy. Soc.*, **13** (1864): 183–5, J. P. Gassiot states a difference of refractive index for extreme spectral rays as 0.077 for carbon bisulphide as compared with 0.026 for flint glass (values cited from Brewster's *Optics*). In his reply of 14 February 1871 Strutt wrote: 'Thanks for your hints about colour boxes w^h will be very useful.... I had myself experienced the inconvenience of bisulphide of carbon.'

(11) The Dublin opticians and instrument-makers Thomas Grubb & Son; see Larmor, *Correspondence*, **1**: 203–7.

If you are quite sure of the angle of your broken mirror, you can silver one of those very obtuse prisms which are used for interference.

Figure 355,2

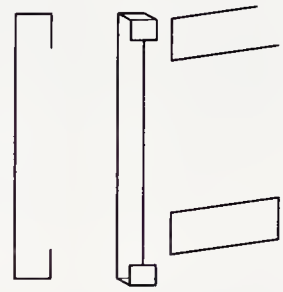

Figure 355,3

Silver is better than quicksilver and is not liable to go wrong. I have not tried silver as an exposed surface but only the surface of contact of silver & glass. My original slits were pieces of brass made to slide on a frame with shutters of card-board and silk hinges behind. Of course they were often not perpendicular to the scale which was divided both above and below. My present slits are bounded by pieces of sheet brass like [Fig. 355, 4]. There are six, which slide in a broad channel in the frame. The jaws of the slits are all in one plane. I made it in card in 1853 but I despaired of getting anyone to make it in brass till 1863 when Miller who came to Becker from White of Glasgow made it very well. It was a troublesome piece of work. The adjustments can be made outside the box. For a reflecting box I would not recommend it only I should prefer the plane slips made so as not to break your nails when you try to slide them.

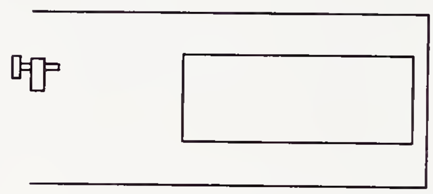

Figure 355,4

Can you compare the resonance of a hollow india rubber ball with a similar ball of glass. The want of rigidity must lower the pitch.

I have been trying large tin foot warmers and flasks with thin sides but I have nothing to compare them with except a piano which is certainly flat and the thick sided things I have tried give an uncertain answer except when made to speak themselves.[12]

Yours truly
J. Clerk Maxwell

(12) In his reply of 14 February 1871 (see note (9)) Strutt responded: 'Do you refer to resonators communicating with external air by holes, or necks? I could calculate the pitch of the *inertia* if the elastic case could be neglected so that a simple relation would hold between the volume & pressure inside. / A moderator globe makes a good resonator. A piece of rubber tubing (French is best & if of suitable diam will stay in the ear without being held) forms communication between ear & interior. Covering one hole lowers pitch about a fifth. I can determine by resonance the note of almost anything down to a jam pot to about a $\frac{1}{4}$ of a semitone, but that requires practice.'

POSTCARD TO PETER GUTHRIE TAIT

14 FEBRUARY 1871

From the original in the University Library, Cambridge[1]

[Glenlair
Dalbeattie]

D^r^ T′[2] You must explain the laws of the operators V & S. I suppose $VS = 0 = SV$, $VV = 1$, $SS = 1$, ∇ is a mere operator like $\frac{d}{dx}$, of dimensions -1 in length. What is $\int V.(V d\rho \nabla)\sigma$ compared with $\int V(d\rho.\nabla\sigma)$?

Have you been introduced to Virial and Ergal? Ergal is an old Friend = Potential of a system on itself or $\sum m_1 m_2 \phi_{12}$. Virial is

$$\sum m_1 m_2 r_{12} \frac{d\phi_{12}}{dr_{12}}.^{(3)}$$

He appears in $\frac{dp}{dt}$ on Reciprocal Figures &c p13 near bottom.[4]

Clausius is now working along with these eminent artistes at the 2^nd^ law of $\Theta\Delta^{cs}$,[5] but as far as I see they have not yet furnished him with the dynamical

(1) ULC Add. MSS 7655, I, b/20.

(2) In reply to Tait's postcard of 1 February 1871: see Number 353 note (11).

(3) The concepts of 'virial' and 'ergal' were introduced by Rudolf Clausius in his paper 'Ueber einen auf die Wärme anwendbaren mechanischen Satz', *Ann. Phys.*, **141** (1870): 124–30; (trans.) 'On a mechanical theorem applicable to heat', *Phil. Mag.*, ser. 4, **40** (1870): 122–7. 'Virial...from the Latin word *vis* (force)' is the mean value of the magnitude $\frac{1}{2}\sum r\phi(r)$, where $\phi(r)$ is the force between two mass points at a distance r. Clausius terms the 'magnitude whose differential represents the negative value of the work, from the Greek word ἔργον (work), the *ergal* of the system'. He concludes that: '(1) the sum of the *vis viva* and the ergal is constant. (2) the mean *vis viva* is equal to the virial.'

(4) On Maxwell's signature dp/dt see Number 339 note (17). He is referring to his paper 'On reciprocal figures, frames, and diagrams of forces', *Trans. Roy. Soc. Edinb.*, **26** (1870): 1–40, esp. 13 (= *Scientific Papers*, **2**: 175–6); '*Theorem*. In any system of points in equilibrium in a plane under the action of repulsions and attractions, the sum of the products of each attraction multiplied by the distance of the points between which it acts, is equal to the sum of the products of the repulsion multiplied each by the distance of the points between which it acts.'

(5) Rudolf Clausius, 'Ueber die Zurückführung des zweiten Hauptsatzes der mechanischen Wärmetheorie auf allgemeine mechanische Principien', *Ann. Phys.*, **142** (1871): 433–61; first published in *Sitzungsberichte der Niederrheinischen Gesellschaft für Natur- und Heilkunde* (1870): 167–89.

condition of the equilibrium of temperature. This is got by the celebrated principle of Assumption and Resumption.

Pray read line 3 on opposite side after the Lion & Unicorn.(6)

Yrs $\dfrac{dp}{dt}$(7)

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(6) Printed on the post card under the Royal crest is the legend: 'The address only to be written on this side'. Addressing his card to Maxwell at Glenlair, Tait had added: 'Forward if necessary'. The post office had thereupon stamped 'Contrary to regulations' on the card. Tait misunderstood Maxwell's chiding: see note (7).

(7) In his reply of 17 February 1871 (ULC Add. MSS 7655, I, a/8) Tait wrote: 'D[r] dp/dt. In verity $VS = 0 = SV$, but also of a truth $VV = \underline{V}$ and not $= 1$; also $SS = \underline{S}$. / $\int V(V.d\rho\nabla)\,\sigma = \int(d\rho\,\mathrm{S}.\nabla\sigma - \nabla S\sigma d\rho)$ but our friend / $\int V(d\rho\nabla\sigma) = \int(d\rho\,\mathrm{S}.\nabla\sigma - \nabla S\sigma d\rho + S(d\rho\nabla)\,\sigma)$. In each case ∇ applies to σ only not to $d\rho$. / Is not 'Argal' used in the preparations for the 'Burial' scene of the fiancée of the Prince of Denmark. Perhaps C. has designs extending even further than Schleswig. But whatever they are they are bosh. The true thing was given by Newton, though he didn't know it. Yours T'. P.S. 3[rd] line after Unicorn was University. I fail utterly to take the joke.' On 'argal' (a corruption of 'ergo') see Shakespeare, *Hamlet*, Act V, 1: *l*. 21 (*OED*). Tait alludes to the Prussian annexation of the Danish duchies of Schleswig and Holstein.

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DRAFT LETTER TO EDWARD WILLIAM BLORE(1)

15 FEBRUARY 1871

From the original in the University Library, Cambridge(2)

Glenlair
Dalbeattie
Feb 15 1871

My dear Blore(3)

Though I feel much interest in the proposed chair of Experimental Physics I had no intention of applying for it when I got your letter, and I have none now unless I come to see that I can do some good by it.

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(1) Trinity 1848, Fellow 1853 (Venn).

(2) ULC Add. MSS 7655, II/39. Published (in part) in *Life of Maxwell*: 350.

(3) In a letter of 13 February 1871, addressed from Trinity College, Cambridge (ULC Add. MSS 7655, II/38A), Blore wrote: 'My dear Maxwell / Our Professorship of Experimental Physics is now founded & though the Salary is not magnificent (£500 a year) yet there is a general wish in the University that this branch of Science should be supported in a way creditable to the University. The Duke of Devonshire has undertaken the expense of the building & Apparatus, & it remains for us that we should find the Professor. Many residents of influence are desirous that you should occupy the post hoping that in your hands this University would hold a leading place in this department. It has, I believe, been ascertained that Sir W. Thomson would not accept the Professorship. I mention this in case you should wish to avoid the possibility of coming into the field against him. Should you be willing to stand, as I hope, I & many others will exert our influence to secure your election. / Believe me / Yours very truly / E. W. Blore'.

The post was advertised on 14 February 1871: 'The Vice-Chancellor hereby gives notice that the election of a Professor of Experimental Physics will take place in the Senate House on Wednesday, March 8, at One o'Clock in the Afternoon. / The Electors are the persons whose names are on the Electoral Roll of the University. / The Vice-Chancellor and Proctors will receive the votes from One to half-past Two o'Clock, when the Vice-Chancellor will declare the Election.' (ULC, CUR, 39, 33 (13); and *Cambridge University Reporter* (15 February 1871): 188).

Stokes (the Lucasian Professor) wrote to Maxwell on 16 February 1871: 'My dear Maxwell, / The election of the new physical professor is fixed for March 8. Are you coming forward? If, as I hope, you are, I think you would most likely be elected. If you are not it would be desirable to let your friends know at once that good men may not be prevented from coming forward by thinking that you are doing so. / Yours sincerely / G. G. Stokes / Pray let it be known without delay be your decision what it may. / Sir William Thomson will not come forward.' (ULC Add. MSS 7655, II/40).

In a letter of 14 February 1871 J. W. Strutt wrote to Maxwell (see Number 358 note (6)): 'When I came here last Friday I found every one talking about the new professorship and hoping that you would come. Thomson it seems has definitely declined, and there is a danger that some resident may get promises unless a proper candidate is seen in the field. There is no one here in the least fit for the post. What is wanted by most who know anything about it is not so much a lecturer as a mathematician who has actual experience in experimenting, & who might direct the

Can you tell me anything of the nature and duties of the Professor.[4]

Is he a University Professor or does he derive his origin from Trinity or any other College or from the Chancellor or other munificent person.

Who appoints him and is the appointment for life or during pleasure?

For how many terms a year is he expected to reside & lecture?

Are the pupils to have facilities for doing experimental work, that is to say is there to be a room where things may be kept as they are from one day to another and not be required to be cleared off at the end of the days work?

I suppose that the supply of pupils is neither encouraged nor checked by any University regulation.

Who are the other candidates?

I am sorry Sir W. Thomson has declined to stand.[5] He has had practical

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energies of the younger Fellows & bachelors into a proper channel. There must be many who w^d^ be willing to work under a competent man & who while learning themselves w^d^ materially assist him. There w^d^ I am told be every disposition on the part of authorities to help the new Professor. I hope you may be induced to come; if not I can't imagine who it is to be. / Do not trouble yourself to answer me about this, as I believe others have written to you about it.'

(4) In a letter of 18 February 1871 (ULC Add. MSS 7655, II/41) Stokes wrote: 'My dear Maxwell / The duties of the Professor of Experimental Physics to be appointed on March 9 are to reside 18 weeks at least in Term time in each academical year; to give at least one course of lectures in each of two terms and not fewer than 40 lectures in the year. His scheme of lectures to be subject to the approval of the board of Mathematical Studies. He is subject to the regulations of the statute for Sir Tho^s^ Adams's professorship and certain other professorships in common but this contains details which would not influence your choice. / I only returned from London in time for a late dinner so I take no time in writing to you. / The attendants at the lecture would probably consist mainly / 1. of those who take some branch of physics for one of their subjects i.e. the subjects which they "take in" for the math^l^ tripos when the new regulations come into force. / 2. Of Poll men who take one of these branches of physics for the subject of their "special" exam^n^. / 3. Of Natural Sciences Tripos men. / One of the first duties of the new Professor [...]'. The second sheet of the letter is missing: but see Stokes' letter of 14 March 1871 (Number 358 note (11)) for his account of the duties of the professor. The terms of the professorship as stated by Stokes had been published as the 'Regulations for the Professorship of Experimental Physics', *Cambridge University Reporter* (8 February 1871): 175.

(5) For the circumstances see S. P. Thompson, *The Life of William Thomson Baron Kelvin of Largs*, 2 vols. (London, 1910), 1: 558–66. Maxwell decided to stand for election to the professorship within the week. On 23 February 1871 Stokes wrote again: 'My dear Maxwell / I am glad you have decided to come forward. / As the election rests with the electoral roll, a rather numerous body, I think it would be well that you should print a circular concerning your intention to come forward and direct it to be sent to the members of the electoral roll. If it be generally known that you stand I think it probable there will be no other candidate, but the body is rather numerous to inform otherwise than by a circular. If you have written to many perhaps they will make it sufficiently known. I will help when I return to Cambridge. / Yours sincerely / G. G. Stokes' (ULC Add. MSS 7655, II/42). On 24 February 1871 Blore issued a notice on

experience in teaching experimental work, and his experimental corps have turned out very good work.

I have no experience of this kind and I have seen very little of the somewhat similar arrangements of a class of real practical chemistry. The class of physical investigations which might be undertaken with the help of men of Cambridge education and which would be creditable to the University demand, in general, a considerable amount of dull labour which may or may not be attractive to the pupils.

Maxwell's behalf (ULC, CUR, 39, 33 (14); and *Cambridge University Reporter* (1 March 1871): 219). Maxwell was duly elected on 8 March: see the *Reporter* (15 March 1871): 247. In a letter to Stokes of 3 March 1871 (ULC Add. MSS 7656, K 173; printed in Wilson, *Stokes–Kelvin Correspondence*, **2**: 352–4), William Thomson wrote: 'I am very glad Maxwell is standing for the Professorship'.

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LETTER TO JOHN WILLIAM STRUTT

15 MARCH 1871

From the original in private possession(1)

Glenlair
Dalbeattie
15 March 1871

I hear Monro has been corresponding with you(2) wh: is a good thing. Benson has published a *little* book called Manual of Colour (Chapman & Hall).(3) It is I think an advance on the big book.(4)

Dear Strutt

I am to lecture on colours at the Royal Institution on the 24th.(5) If you care to go and are not a member I have tickets to spare. I have been so busy with the lecture that I have fallen back with correspondence. I have yours of S. Valentine's Day.(6)

Prisms of 60° would do very well.(7) I happened to have a pair of prisms of 45° when I made a reflecting box. The only disadvantages of a large angle are greater loss of light by reflexion and narrower field of view for the same length of side of prism.

I have received your paper on sky blue.(8) For experiments of the kind mentioned there (which you do not describe)(9) I should think the simplest

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(1) Rayleigh Papers, Terling Place, Terling, Essex.

(2) On colour vision, as Monro informed Maxwell in his letter of 3 March 1871 (see Number 359 note (2)). Monro wrote to Strutt on 5 and 27 February 1871 (Rayleigh Papers, Terling); and see J. W. Strutt, 'Some experiments on colour', *Nature*, **3** (1871): 234–7.

(3) As he informed Maxwell in a letter of 13 March 1871 (ULC Add. MSS 7655, II/43). See William Benson, *Manual of the Science of Colour* (London, 1871). See Maxwell's comments in 'On colour vision', *Proceedings of the Royal Institution*, **6** (1872): 260–71, esp. 264 (= *Scientific Papers*, **2**: 272).

(4) See Number 341 note (9).

(5) See Number 360 notes (1) and (2).

(6) J. W. Strutt to Maxwell, 14 February 1871 (typed copy in the Rayleigh Papers, Terling Place; printed in R. J. Strutt, *John William Strutt, Third Baron Rayleigh* (London, 1924): 48–9), written in reply to Number 355: see Number 355 notes (9), (10) and (12), and Number 357 note (3).

(7) In response to Strutt's query in his letter of 14 February 1871: see Number 355 note (10).

(8) Strutt enclosed a copy of his paper, published in the February 1871 number of the *Philosophical Magazine*, with his letter of 14 February 1871; see J. W. Strutt, 'On the light from the sky, its polarization and colour', *Phil. Mag.*, ser. 4, **41** (1871): 107–20. See Number 359.

(9) Strutt, 'On the light from the sky': 113–14. Strutt described his apparatus by which a 'comparison was made between sky light and that of the sun diffused through white paper', in

method would be to use the sky blue light to form a spectrum to be observed in the ordinary way and to allow the light to be compared with it (say sun light from white paper) to form a spectrum through the same slit or a continuation of it of different breadth and to polarize the white light in a plane to be varied at pleasure.

You observe both spectra through a fixed Nicols prism as analyzer and turn the polariser till you get equality of light at a particular point of the spectrum.

In this way you could analyse the blue colour of both portions of sky light that polarized in the plane of the Sun and that polarized in the perp. plane.

Of course you place your analyzer so  with its long diameter parallel to the edges of your prisms, so as to destroy as little transmitted light as you can.

All this depends very much on the question whether you can more easily get a slit with a variable breadth capable of exact measurement or a beam of constant light polarized in a variable plane.

My eyes are almost blinded by the snow and the sun on it so that I write at random.

Many thanks for your good wishes with respect to the new professorship.(10) I always looked forward to it with much interest tempered with some anxiety when it was merely to be erected in the University. I now take your good wishes as personal to myself and my anxiety has developed into responsibility.

I hope you will be in Cambridge occasionally for it will need a good deal of effort to make Exp. Physics bite into our University system which is so continuous and complete without it.(11)

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a supplement to his paper 'On the light from the sky, its polarization and colour', *Phil. Mag.*, ser. 4, **41** (1871): 274–9, esp. 278–9.

(10) See Strutt's letter of 14 February 1871: see Number 357 note (3).

(11) Following Maxwell's election on 8 March (see Number 357 note (5)), Stokes wrote to him on 14 March 1871 (ULC Add. MSS 7655, II/44): 'My dear Maxwell, / The principal duty, I take it, of the new professor in the first instance will be to give his advice as to the construction of the proposed physical laboratory & museum, and as to the expenditure of the sum which the University will lay out in establishing a collection of physical instruments. I take for granted that no one will expect you to lecture next Easter Term. I don't recollect whether the residence required is distributed – so much in the Mich[s] term & so much in the Lent and Easter terms – or merely left personal, so much in the year, but I can ascertain when I return to Cambridge. / The Syndicate appointed to consider the site &c &c of the proposed physical laboratory has only just been appointed. I should suppose it would be generally felt that you ought to be added to it. / I am afraid you will find a good deal of difficulty in getting a house in Cambridge. The supply is hardly equal to the demand. / Yours sincerely / G. G. Stokes / I return to Cambridge this evening.' Maxwell was appointed to the Syndicate on 16 March 1871 (ULC, CUR, 39, 33 (16)).

To wrench the mind from symbols and even from experiments on paper to concrete apparatus is very trying at first, though it is quite possible to get fascinated with a course of observation as soon as we have forgotten all about the scientific part of it.

If we succeed too well, and corrupt the minds of youth, till they observe vibrations and deflexions and become Senior Op.s instead of Wranglers,(12) we may bring the whole University and all the parents about our ears.

Yours truly
J. Clerk Maxwell

(12) Candidates for honours in the Mathematical Tripos were graded (in descending order) as Wranglers, Senior Optimes and Junior Optimes.

LETTER TO CECIL JAMES MONRO

15 MARCH 1871

From the original in the Greater London Record Office(1)

Glenlair
Dalbeattie
15 March 1871

Dear Monro(2)

I have been so busy writing a sermon on Colour and Tyndalizing my imagination up to the lecture point(3) that along with other business I have had no leisure to write to any one.

I think a good deal may be learned from the *names* of colours, not about colours, of course, but about names and I think it is remarkable that the rhematic instinct has been so much more active, at least in modern times, on the less refrangible side of primary green ($\lambda = 510 \times 10^{-9}$ mètre).(4)

I am not up in ancient colours, but my recollection of the interpretations of the lexicographers is of considerable confusion of hues between red and yellow and rather more discrimination on the blue side. Qu. If this is true, has the red sensation become better developed since those days? Benson has a new book Chapman & Hall 1871 called 'Manual of Colour'.(5)

I think it is a great improvement on the Quarto,(6) both in size and quality. It is the size of this paper I write on.

I have not asked you if you wish to go to sermon on Colour for I do not

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(1) Greater London Record Office, Acc. 1063/2092. Previously published in *Life of Maxwell*: 379–81.

(2) In reply to Monro's letters of 3 March 1871 (Greater London Record Office, Acc. 1063/2106, 2109b, 2109c; reproduced in part in *Life of Maxwell*: 376–9), and 9 March 1871 (Greater London Record Office, Acc. 1063/2107).

(3) For his Royal Institution lecture 'On colour vision' on 24 March 1871: see Number 360 notes (1) and (2).

(4) In response to Monro's remark, in his letter of 3 March (see note (2)) on the 'insane trick of reasoning about colours *as identified by their names*'. See Monro's reply in his letter of 21 March 1871 (Number 363 notes (3) and (4)). The word 'rhematic' is defined by the *OED* as meaning 'pertaining to the formation of words', where Maxwell's usage (as quoted in the *Life of Maxwell*) is the second example quoted. The first usage cited is by Friedrich Max Müller in his essay (of 1856) on 'Comparative mythology' in his *Chips from a German Workshop*, **2** (London, 1867): 9; '[The] period to which we must assign the first beginnings of a free and simply agglutinative grammar...we call it the Rhematic Period.' In the essay on 'Comparative mythology' Max Müller outlined his theory of the origin of mythology in words originally descriptive of the sun. Maxwell was familiar with Max Müller's solar theory (see Number 449).

(5) See Number 358 note (3).

(6) See Number 341 note (9).

think the R. I. a good place to go to of nights even for strong men. I have however some tickets to spare.

The peculiarity of our space[(7)] is that of its three dimensions none is before or after another. As is x so is y and so is z.

If you have 4 dimensions this becomes a puzzle for first if three of them are in our space then which three. Also if we lived in space of m dimensions but were only capable of thinking n of them then 1[st] which n? 2[nd] If so things would happen requiring the rest to explain them and so we should either be stultified or made wiser.

I am quite sure that the kind of continuity which has 4 dimensions all coequal is not to be discovered by merely generalizing Cartesian space equations. (I dont mean by Cartesian space that which Spinosa worked from Extension the one essential property of matter, and Quiet the best glue to stick bodies together.)[(8)]

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(7) In his letter of 3 March (see note (2)) Monro had asked: 'Could you tell me where I could find anything about geometry, of any number of dimensions more than 3, in a form intelligible to moderate capacities? Don't refer me to Helmholtz's article in the *Academy*. What stumps me is this. Take four. Is $ijkl$ to be $= -1$ or not? If yes, operate upon l; the $ijk = l$, therefore ijk is not $= -1$ (for whatever l may be, it is not the scalar -1), therefore space of three dimensions in space of four dimensions is not *our* space of three dimensions, which they who talk, as Riemann and, more hypothetically, Helmholtz do, of our moving (in four dimensions) into regions where our space (of three dimensions) is *curved* – necessarily assume it to be. On the other hand if $ijkl$ is not $= -1$, but $= -l$, (so that space of three dimensions in space of four dimensions may be our space of three dimensions, & ijk may be $= -1$,) then in the first place, space of three dimensions does not stand to space of four in the same relation in which space of two stands to space of three, and secondly in particular, the dimension l stands in a relation to the other three peculiar to itself. This cannot be intended by those who speak of geometry of four dimensions. / In either case there is no reason that I can see for calling it "geometry". The formal relations are not analogous; the metaphor does not hold. But *something* is meant by calling it geometry; – Riemann expressly distinguishes between the properties necessary to a multiplicity of three variables and the additional properties which belong to space. I don't know whether you will throw over Riemann; but Cayley, as I see by a recent *Nature*, speaks of geometry of m dimensions as a conception useful to aid the imagination. Very good; to this it may be no objection that space of more than three dimensions is out of all experience; but it is an objection, if the conception is not merely inexperiencible but self contradiction. Such I make it out to be as above, by an argument which at any rate imposes on myself.' (This part of Monro's letter is not reproduced in the *Life of Maxwell*: see note (2)). See Arthur Cayley, 'On abstract geometry', *Proc. Roy. Soc.*, **18** (1869): 122–3 (= *Nature*, **1** (1870): 294). Monro was referring to Helmholtz's paper on 'The axioms of geometry', *The Academy*, **1** (1870): 128–31. For Maxwell's comments on Riemann's non-Euclidean geometry see his letter to Tait of 11 November 1874 (to be published in Volume III; reproduced in facsimile in P. M. Harman, *Energy, Force and Matter* (Cambridge, 1982: 96).

(8) In a letter of 10 September 1871 (Greater London Record Office, Acc. 1063/2109a; reproduced in large part in *Life of Maxwell*: 382–3), Monro responded: 'Looking at your old

I think it was Jacob Steiner who considered the final cause of space to be the suggestion of new forms of continuity.(9)

I hope you will continue to trail clouds of glory after you and tropical air and be as it were a climate to yourself. I am glad to see you occasionally in Nature.(10) I shall be in London for a few days next week, address Athenæum Club.

I think Strutt on sky blue is very good.(11) It settles Clausius vesicular theory.(12)

for putting all his words together
'tis 3 blue beans in 1 blue bladder.

Mat. Prior(13)

The exp. phys at Cambridge is not built yet but we are going to try.(14)

The desideratum is to set a Don and a Freshman to observe & register (say) the vibrations of a magnet together, or the Don to turn a winch & the Freshman to observe and govern him.

Yours sincerely
J. Clerk Maxwell

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letter again, I don't quite see the force of either of your objections to space of more than three dimensions. First you ask, if we can think some of the dimensions and not others, then *which*? Surely one might answer that depends, – depends namely on your circumstances, on circumstances which in your circumstances you cannot expect to judge of.... So, now, the missing dimension or dimensions, if any, might be determined by circumstances which we could not tell unless we knew all about the said dimension or dimensions. In my *ijkl* argument I fancied I had found actual contradiction in the laws of combination, but I was guilty of the old mistake of using symbols without making up my mind what they meant.'

(9) Possibly suggested by the preliminary discussion in *Jacob Steiner's Vorlesungen über synthetische Geometrie. Zweiter Teil. Die Theorie der Kegelschnitte, gestützt auf projektivische Eigenschaften*, ed. Heinrich Schröter (Leipzig, 1867): 1–3.

(10) See C. J. Monro, 'On the colour yellow', *Nature*, **3** (1871): 246.

(11) J. W. Strutt, 'On the light from the sky, its polarization and colour', *Phil. Mag.*, ser. 4, **41** (1871): 107–20, esp. 111; 'When light is scattered by particles which are very small compared with any of the wave-lengths, the ratio of the amplitudes of the vibrations of the scattered and incident light varies inversely as the square of the wave-length, and the intensity of the lights themselves as the inverse fourth power.' See Number 358.

(12) Rudolf Clausius, 'Ueber die Lichtzerstreung in der Atmosphäre und ueber die Intensität des durch die Atmosphäre reflectirten Sonnenlichts', *Ann. Phys.*, **72** (1847): 294–314; Clausius, 'Ueber die Natur derjenigen Bestandtheile der Erdatmosphäre, durch welche die Lichtreflexion in derselben bewirkt wird', *ibid.*, **76** (1849): 161–88; Clausius, 'Ueber die Blaue Farbe des Himmels und die Morgen – und Abendröthe', *ibid.*: 188–95. In the subsequently published supplement to his paper 'On the light from the sky, its polarization and colour', *Phil. Mag.*, ser. 4, **41** (1871): 274–9, Strutt critically reviewed Clausius' theory that the light of the sky was due to reflection from water bubbles.

(13) Matthew Prior, 'Alma', Canto I, *ll.* 28–9 (the 'Cambridge wits' on Aristotle).

(14) In his letter of 9 March 1871 (see note (2)) Monro had congratulated Maxwell on his appointment: 'I am very glad to see they have elected you at Cambridge.'

I shall be happy to propose you as a member of the Math Society if you agree. £1 per ann I think but I compound. I will see about the Proceedings.(15)

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(15) In his letter of 9 March 1871 (see note (2)) Monro had asked: 'Can the outer world obtain the *Proceedings of the London Mathematical Society*? and where? & what does it cost?'. In a letter of 21 March 1871 (Greater London Record Office, Acc. 1063/2108) Monro replied: 'I think I may as well ask you to propose me, as you offer to do, as a member of the Mathematical Society: – and I hereby do ask you.' Monro was proposed for election on 13 April and elected on 11 May 1871; see *Proceedings*, **3** (1871): 233, 266.

DRAFTS ON COLOUR BLINDNESS[1]

circa MARCH 1871[2]

From the originals in the University Library, Cambridge[3]

[COLOUR BLINDNESS]

[1][4] But the most valuable evidence which we have as to the true nature of colour vision is furnished by the colour blind.

I have not hitherto said anything about the evidence as to colour vision given us by a very important class of witnesses – the colour blind. One of the most celebrated cases of this peculiarity of vision is that of Dr Dalton, the founder of the Atomic Theory in Chemistry. The scarlet gown, the symbol of the dignity of Doctor when conferred on him by the University of Oxford appeared to the Quaker philosopher as a sober drab. Sir John Herschel in a letter written to Dalton in 1832[5] was the first to explain the true nature of colour blindness. It arises simply from the absence of one of the three primary colour sensations. In all cases actually observed it is the sensation which we call red which is absent. The other two sensations which we call green and blue appear to be the same to the colour blind as to those who have perfect vision. Hence Sir John Herschel proposed to call the vision of such persons dichromic as dependent on two primary colours, whereas ordinary vision is trichromic, depending on three primary colours.[6] Dichromic vision is also called Daltonism from the most celebrated instance of its occurrence[7] but, the late Dr George Wilson very properly protested against such an unworthy use of Daltons name as if his most memorable characteristic had been a defect of vision. Daltonism can only mean an adherence to Dalton's atomic theory not an inability to distinguish colours.[8]

A Daltonian must be one who can see what Dalton pointed out to us about the constitution of bodies, not one who like Dalton cannot perceive Red.

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(1) See J. Clerk Maxwell, 'On colour vision', *Proceedings of the Royal Institution of Great Britain*, **6** (1872): 260–71 (= *Scientific Papers*, **2**: 267–79).

(2) The lecture was read on 24 March 1871: see note (1) and Numbers 358 and 359.

(3) ULC Add. MSS 7655, V, b/12.

(4) Compare Maxwell, 'On colour vision': 269–70 (= *Scientific Papers*, **2**: 277–8).

(5) John Herschel to Dalton, 20 May 1833, in W. C. Henry, *Memoirs of the Life and Scientific Researches of John Dalton* (London, 1854): 25–7.

(6) See Herschel's letter to Dalton in Henry, *John Dalton*: 26; and compare Volume I: 654–6.

(7) See Volume I: 245n.

(8) George Wilson, *Researches on Colour-Blindness* (Edinburgh, 1855): 161; 'Daltonism *should* signify the Doctrine of Indivisible Chemical Atoms; and a Daltonian, a believer in such.'

[2] [...Colour blind people can perceive the extreme end of the spectrum which we call red, though it appears much][9] darker to the colour blind than to us but it is not invisible to them. Hence we are obliged to deduce the nature of the missing sensation by calculation as we cannot find any part of the spectrum which represents it.

It appears from calculation that the sensation which we must henceforth consider as the true primary red is a colour not unlike the extreme red of the spectrum but deeper in hue.

All the colours between the red end of the spectrum and primary green require the same quantity of blue to render them equivalent to the standard white as seen by the colour blind. Hence all these colours are to their eyes identical in hue, the only difference between the appearance of the different parts of this portion of the spectrum being in brightness.

If, then, we call Primary Red the sensation which we have in addition to those which are experienced by the colour blind all the colours between primary green and the extreme red of the spectrum are compounded of primary red and primary green in various proportions. The extreme red of the spectrum is itself capable of exciting the green sensation but in a very small degree.

It is probable therefore that by gazing for some time at a green light and then suddenly turning our eyes to the red of the spectrum beyond *C* we shall experience a sensation which is almost exactly the primary red.[10]

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(9) As in Maxwell, 'On colour vision': 269 (= *Scientific Papers*, 2: 277).

(10) Compare Maxwell's discussion of colour blindness in 1860; see Volume I: 649–56.

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FROM A LETTER TO KATHERINE MARY CLERK MAXWELL

20 MARCH 1871

From Campbell and Garnett, *Life of Maxwell*(1)

[London]
20 March 1871

There are two parties about the professorship. One wants popular lectures, and the other cares more for experimental work. I think there should be a gradation – popular lectures and rough experiments for the masses; real experiments for real students; and laborious experiments for first-rate men like Trotter(2) and Stuart(3) and Strutt.(4)

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(1) *Life of Maxwell*: 381.

(2) Coutts Trotter, Trinity 1855, Fellow 1861, College Lecturer in Physics 1869–84; his career was devoted to University administration (Venn). See his letter to Maxwell of 20 April [1871] (ULC Add. MSS 7655, II/46) on R. B. Clifton's Oxford physical laboratory, and on the Syndicate appointed to oversee the construction of the Cambridge laboratory (see Number 358 note (11)). Trotter informed Maxwell: 'I have just returned from Oxford. Clifton's building seems to me as far as I can judge very convenient.... There is no doubt much to be said for natural selection but will not the struggle for existence between the men who want their rooms darkened and the men who want their rooms light the men who want to move about magnets and the men who want to observe galvanometers be unduly severe. Anent architects... I hope it will not be a great swell from London, there is I take it no one who is likely to have the faintest idea of what is wanted from a physical laboratory and the only chance of a convenient building seems to be the getting of some one who will not be above taking hints as to the arrangements....'.

(3) James Stuart, Trinity 1862, Professor of Mechanism 1875 (Venn).

(4) See Number 358.

LETTER TO WILLIAM THOMSON

21 MARCH 1871

From the original in the University Library, Cambridge(1)

Athenæum
21 March 1871

Dear Thomson(a)

Thanks for your card. I must put Tait right. His modesty is such that he speaks as a mere mouthpiece of Hamilton when the inspiration, if any, is of the kind which allows the almost immodified manifestation of the personality of the writer.(2)

I am going to Cambridge on Saturday to discuss the Adams prize subjects and I believe you are to set some.(3) Can you inspire me a little if you are not to be there yourself so as to get a physical subject this time.(4) We had the

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(a) {Thomson} Please return to Sir W. Thomson The College Glasgow.(5)

(1) ULC Add. MSS 7655, II/45. Previously published in A. T. Fuller, 'James Clerk Maxwell's Cambridge manuscripts: extracts relating to control and stability – VI', *International Journal of Control*, **43** (1986): 1135–68, esp. 1164–5.

(2) See also Number 349.

(3) The 'Book of Minutes relating to the Adams Prize, kept by the Plumian Professor [James Challis]' records that: 'At the congregation on Thursday March 16th, 1871, Sir William Thomson, LL.D., of St. Peter's College, on the nomination of St. Peter's College, and Professor J. Clerk Maxwell of Trinity College, on the nomination of Trinity College were appointed examiners for the Adams Prize to be adjudged in 1873.' (Cambridge Observatory Archive).

(4) The 'Book of Minutes' (see note (3)) records: '1871 March 27, the following Adams Prize subject, to be adjudged in 1873 was agreed upon: / *A Dissertation on the effect of the Tides in altering the length of the day.* / ⁂ it is required that the amount of alteration be connected, at least by approximate considerations, with quantities known by observation or experiment, and that an attempt be made to calculate it strictly either by employing directly the general equations of Hydrodynamics, or by assuming the height and time of Tide all over the earth to be known. The dissertation may include a discussion of the evidence of a retardation of the earth's rotation derived from astronomical theory and observation. / (The meeting was held at Pembroke College Lodge, and was attended by Profrs. Challis & Maxwell. Profr. Sir William Thomson was not present, but had signified his assent to the choice of subject.)' A subsequent undated note records that: 'No exercises were sent in for the Prize to be adjudged in 1873.' The determination of the tidal retardation of the earth's rotation had been discussed by Thomson in his 1866 Rede Lecture at Cambridge; see his paper 'On the observations and calculations required to find the tidal retardation of the earth's rotation', *Phil. Mag.*, ser. 4, **31** (1866): 533–7 (= *Math. & Phys. Papers*, **3**: 337–41). Thomson was currently a member of the British Association Committee on tidal observations; see S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 581; **2**: 611, 619.

(5) Thomson probably forwarded Maxwell's letter to Challis for consideration at the meeting

Calculus of Variations last time.(6) One essay is in 5 vols quarto, not unintelligible and very elaborate but has no examples of the kind of discontinuity which first occurred to me. eg Suppose the velocity of travelling up a road is some power of the cosine of the inclination, make the quickest road to the top of a hemispherical hill.(7) Here the result (a pure diff calc affair) is a road of constant slope till the slope of the hill is equal to that slope and then an arc of a great arch to the top. The reason is that the three roots of an equation coalesce and the line of slope from being a line of maximum slowness of ascent becomes a line of maximum speed.

I believe you have a question about stability. Here was the form in which I put it before but if we can fuse any good points together I think it will be carried.

In the motion of a connected system a particular case is defined by the eq$^{\text{ns}}$

$$F_1 = \text{const} \quad F_2 = \text{const} \quad \&\text{c}$$

F_1 F_2 being functions of the variable coordinates. If this motion receives a slight instantaneous disturbance determine the character and periodicity (if periodic) of the subsequent motion discussing the properties of dynamical stability and the relations of kinetic foci by means of well chosen examples of progressive complexity.

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of the Adams Prize examiners on Monday 27 March: see note (4). The parts of the letter not concerned with Adams Prize problems have a line drawn through them by Thomson.

(6) Maxwell was currently serving as an examiner for the Adams Prize for 1871; see his letter to Stokes of 11 January 1871 (Number 351, esp. note (2)).

(7) There is a draft of the problem in a notebook (ULC Add. MSS 7655, V, k/9, f. 21$^{\text{v}}$). This draft may be dated (see Number 300 esp. note (3)) probably to summer 1868: 'If the velocity of a carriage is proportional to the n$^{\text{th}}$ power of the cosine of the inclination of the road determine the angle of quickest ascent and show that the road of quickest ascent up a hemispherical hill consists of an epicycloid and an arc of a great circle. If $\tan\alpha = \sqrt{\frac{1}{n}}$ the epicycloid is formed by a great circle rolling on a circle rad α.' Maxwell subsequently set the problem for the 1873 Mathematical Tripos: see Number 436. In his discussion of the problem in his paper 'On a problem in the calculus of variations in which the solution is discontinuous', *Proc. Camb. Phil. Soc.*, **2** (1873): 294–5 (= *Scientific Papers*, **2**: 310), he states that the problem 'was set as an example of discontinuity introduced into a problem in a way somewhat different from any of those discussed in Mr Todhunter's essay.... In the problem now before us there is no discontinuity in the statement'. He is alluding to Isaac Todhunter, *Researches in the Calculus of Variations, principally on the Theory of Discontinuous Solutions: An Essay to which the Adams Prize was awarded in the University of Cambridge in 1871* (London 1871).

2

An exposition of the doctrine of the distribution of errors in fallible observations special attention being paid to the application of the method to determine the proper choice of the form and dimensions of apparatus and other variable magnitudes in an experiment in order to deduce from the results the least fallible value of the quantity to be determined.

3

A statement of the principles of the Calculus of Quaternions in which every operation is explained in connexion with the physical meaning of some of its applications.

4

A discussion of the cases in which theorems applicable to the space within a region bounded by a single surface of simple continuity require modification when applied to a region bounded by more than one surface or by one or more cyclic surfaces.

An exposition (consisting of explanations &c) of Maupertuis principle of Least Action and of Hamiltons principle of Varying action with examples.

Will you if you have time also give me some notion about what you think is wanted for material accommodation for exp. physics at Cambridge.[8]

Lecture room *taken for granted.*
Place to stow away apparatus d^{o}.
Large room with tables &c for beginners at experiments, gas & water laid on &c.
A smaller place or places for advanced experimenters to work at experiments which require to be left for days or weeks standing.
A place on the ground floor with solid foundations for things requiring to be steady.
Access to the roof for atmospheric electricity.
A place with good ventilation to set up Groves or Bunsens batteries[9] without sending fumes into the apparatus.

(8) See also Number 364.

(9) See the *Treatise*, **1**: 326–8 (§272); and Number 289 note (12) on Grove's cell. Bunsen's cell was like Grove's, with the platinum plate replaced by porous carbon.

A good Clock in a quiet place founded on masonry, electric connexion from this to other clocks to be used in the expts and from these connexion to machines for making sparks, marks on paper, &c.

A well constructed oven, heated by gas to get up a uniform high temperature in large things.

A gas engine (if we can get it) to drive apparatus, if not, the University crew in good training in four relays of two, or two of four according to the nature of the exp[t].

We should get from the B.A. some of their apparatus for the Standard committee. In particular the spinning coil[(10)] and the great electrodynamometer.[(11)] If not we must have a large coil made and measured with the utmost precision to be used as a standard to compare others with by putting the latter inside and concentric and dividing the current till there is a null deflexion.

I shall discuss the small apparatus with you another time.

Will you send me in a week to Glenlair (after 1[st] April) half an ounce of the paraffin of which the sp ind capacity is 1.977[(12)] that I may find its index of refraction.[(13)] Observe 1.96 is the square of 1.4.[(14)] I suppose it is too opaque

(10) Used for experiments on standards of electrical resistance: see Number 210 note (2).

(11) Described in the *Treatise*, **2**: 330–1 (§725); and see Number 416 esp. note (3).

(12) The value for the dielectric constant of paraffin obtained by J. C. Gibson and T. Barclay, 'Measurement of specific inductive capacity of dielectrics, in the Physical Laboratory of the University of Glasgow', *Phil. Trans.*, **161** (1871): 573–83. The paper was received on 23 November 1870 and read on 2 February 1871.

(13) Maxwell mentioned this experiment – intended to test his prediction from his electromagnetic theory of light that 'the dielectric capacity of a transparent medium should be equal to the square of its index of refraction' (*Treatise*, **2**: 388 (§788)) – in his letter to Thomson of 14 April 1870 (Number 339). He had first stated this relation in his letter to Faraday of 19 October 1861 (Volume I: 686–7), and in 'On physical lines of force. Part III', *Phil. Mag.*, ser. 4, **23** (1862): 12–24, on 22–3 (= *Scientific Papers*, **1**: 500–1).

(14) In the *Treatise*, **2**: 388 (§789) he cites an experimental value of 1.422 for the index of refraction of paraffin, indicating only an approximate agreement with theory. 'At the same time, I think that the agreement of the numbers is such that if no greater discrepancy were found between the numbers derived from the optical and electrical properties of a considerable number of substances, we should be warranted in concluding that the square root of K, though it may not be the complete expression for the index of refraction, is at least the most important term in it.' Ludwig Boltzmann subsequently provided evidence for Maxwell's theoretical argument; see Boltzmann, 'Über die Verschiedenheit der Dielekricitätsconstante des krystallisirten Schwefels nach verschiedenen Richtungen', *Wiener Berichte*, **70**, Abtheilung II (1874): 342–66. See Volume I: 687n.

to make a prism of but with sodium light I expect to find its index by sticking it on the side of a prism and finding when total reflexion begins.

I am to lecture on colour on Friday at the R.I. & to rehearse tomorrow.(15)

Yours truly

J. Clerk Maxwell

(15) Maxwell delivered his Royal Institution lecture 'On colour vision' on Friday, 24 March 1871; see Number 360 notes (1) and (2).

FROM A LETTER TO KATHERINE MARY CLERK MAXWELL

22 MARCH 1871

From Campbell and Garnett, *Life of Maxwell*[1]

Athenæum
22 March 1871

I also got a first-rate letter from Monro about colour, and the Arab words for it (I suppose he studied them in Algeria).[2] They call horses of a smutty yellow colour 'green'. The 'pale' horse in Revelation is generally transcribed green elsewhere, the word being applied to grass, etc.[3] But the three green things in the Arabic dictionary are 'gold, wine, and meat', which is a very hard saying.[4]

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(1) *Life of Maxwell*: 382.

(2) C. J. Monro to Maxwell, 21 March 1871 (Greater London Record Office, Acc. 1063/2108; the main part of the letter is reproduced in *Life of Maxwell*: 381–2).

(3) Monro wrote: 'You know the 'pale' horse of the Apocalypse (vi. 8) well that is *χλωρός* which is usually 'green' you know.... The Arabic for green ... is akhdár ... But *χλωρός* and akhdár too are certainly the colour of chlorophyll'.

(4) Monro wrote: 'according to dictionaries "the three greens" in Arabic are "gold wine and meat" ...'. Compare A. I. Sabra's note on terms for 'green' in the Arabic and Latin versions of Alhazen's *Optics*. See *The Optics of Ibn Al-Haytham. Books I–III. On Direct Vision*, trans. A. I. Sabra, 2 vols. (London, 1989), **2**: 40–4, esp. 41; '*akhdar/viridis* = green. Three shades of green are mentioned: *akhdar zar'ī/viridis segetalis* = fresh green, apparently the colour of fresh vegetation ... *akhdar zinjārī/viridis myrti* (*sic*) = rust-green; *akhdar fustuqī/viridis levistici* = pistachio-green.'

364

PLANS FOR THE PHYSICAL LABORATORY

circa MARCH 1871[1]

From the original in the Cavendish Laboratory[2]

PHYSICAL LABORATORY

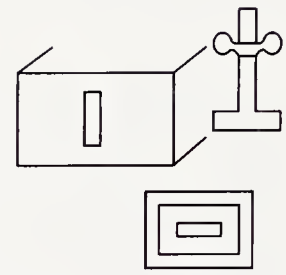

Figure 364,1

Cast iron hollow bricks built into the walls at intervals.
Every third joist of the ceiling 3 inches deeper so as to project beyond the plaster.
No hot water pipes in the end of the building where the magnetic room is.
Windows of lecture room to the lane only.
Shutters with pasteboard pannels painted black to draw all at once.
Loft above the lecture room for diagrams &c.
All gas & water pipes to be exposed everywhere.
All cocks to be on vertical portions of the pipe.
Flues from the principal chimneys to be carried to the battery closets, the lecture table &c. Gas and water and electrodes in the lecture table to be below the level and to be covered with square brass traps.

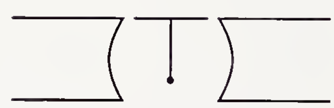

Figure 364,2

⌊dry electrical room
long loft in back with 2 traps.
Clarkes Potentiometer[3]
Microfarad.[4]⌋
Comparator for comparing lengths.
Pair of microscopes sliding on a bar.
Dividing instrument.
Spherometer.

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(1) See Number 365.

(2) Maxwell notebook, Museum of the Cavendish Laboratory, Cambridge (photocopy in ULC Add. MSS 7655, V, n/1).

(3) See Number 415 esp. note (5).

(4) 10^{-13} absolute units of capacity; see Fleeming Jenkin, *Electricity and Magnetism* (London, 1873): 159; and see also Numbers 415 and 420 esp. note (8).

Cathetometer.[5]

Theodolite.

Balance large and small standard.

Rough balances and  in every room.

Weights British & metrical.

Astronomical Clock with electric connexions.

Common clocks in connexion in every room.

Experimental clocks in connexion.

Frame for swinging pendulums.

Vacuum case for d°.

Helmholtzs chronographic pendulum.[6]

Foucaults pendulum[7] in Tower.

Jenkin's Governor.[8]

Chronograph?

(5) An instrument for measuring differences in level. In his note book (see note (2)) Maxwell jotted down a reference to a cathetometer described by Quincke; see G. Quincke, 'Ueber die Capillaritätsconstanten des Quecksilbers', *Ann. Phys.*, **105** (1858): 1–48, esp. 12–18 and Plate I.

(6) See Hermann Helmholtz, 'On the methods of measuring very small portions of time, and their application to physiological purposes', *Phil. Mag.*, ser. 4, **6** (1853): 313–25, esp. 315–17.

(7) Léon Foucault, 'Sur une nouvelle démonstration expérimentale du mouvement de la terre, fondée sur la fixité du plan de rotation', *Comptes Rendus*, **35** (1852): 421–4; Foucault, 'Sur les phénomènes d'orientation des corps tournant entraînés, par un axe fixe à la surface de la terre', *ibid.*: 424–7.

(8) See Number 219 esp. note (8).

POSTCARD TO WILLIAM THOMSON

30 MARCH 1871

From the original in the National Library of Scotland, Edinburgh[1]

[Glenlair]

What time between the 4th & 11 or outside the latter limit would do to see you in Glasgow about the Laboratory. I will send you a plan along with a paper on the size of molecules by Loschschmidt of Vienna.[2] Consider especially whether a water turbine to drive a magneto electric engine would be preferable to Grove.[a][3] I mean to have a water engine. I am to look after you and Clifton.[4] Where is Helmholtz? Berlin or Heidelberg.[5]
(Address Glenlair)

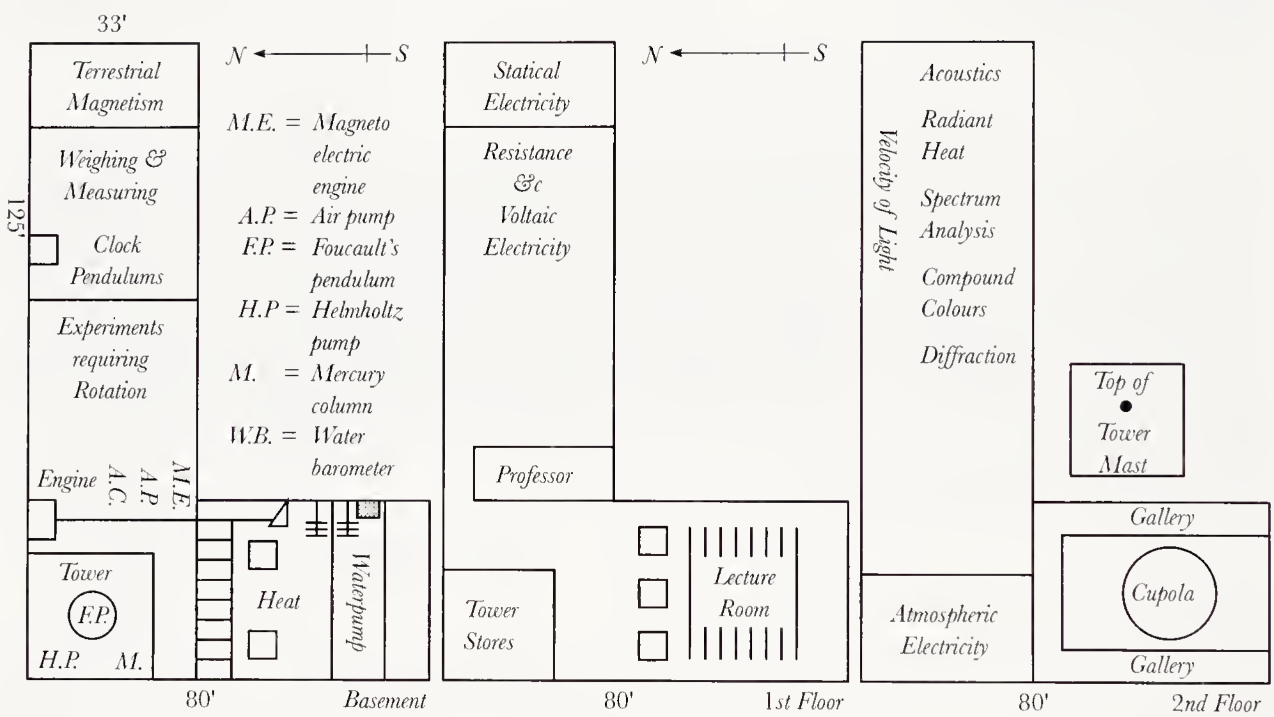

Figure 365,1

(a) {Thomson} might be so / but certainly not preferable to a Porous-cell-less Daniel[6]

(1) National Library of Scotland, MS. 1004 f. 40. Published in facsimile in C. W. F. Everitt, 'Maxwell's scientific creativity', in *Springs of Scientific Creativity: Essays on Founders of Modern Science*, ed. R. Aris, H. T. Davis and R. H. Stuewer (Minneapolis, 1983): 89.

(2) Joseph Loschmidt, 'Zur Grösse der Luftmolecüle', *Wiener Berichte*, **52**, Abtheilung II (1865): 395–413. On Thomson's interest see Number 377 esp. note (34).

(3) A Grove battery: see Number 289 note (12).

(4) Robert Bellamy Clifton, Professor of Experimental Philosophy at Oxford (Venn). A copy of the plans of the Oxford Physical Laboratory is preserved in ULC Add. MSS 7655, V, j/1.

(5) Helmholtz had accepted the professorship of physics at Berlin in December 1870; see Leo

Koenigsberger, *Hermann von Helmholtz*, 3 vols. (Braunschweig, 1902–3), **2**: 186. In answer to Maxwell's query Tait responded in a card of 31 March 1871 (ULC Add. MSS 7655, I, a/9): 'D^{r} *dp/dt*. / H^{2}'s address is now Königinn Augusta Str. 45 Berlin. / Did you get the proofs of your book and a letter I sent to the Athenaeum? Y^{rs} T′.' Tait refers to the proofs of Maxwell's *Theory of Heat* (London, 1871). See Numbers 366, 367, 372, 373 and 374.

(6) See Thomson's paper 'On a constant form of Daniell's battery', *Proc. Roy. Soc.*, **19** (1871): 253–9, read 19 January 1871. Maxwell describes Thomson's form of Daniell's battery in the *Treatise*, **1**: 327–8 (§272).

POSTCARD TO PETER GUTHRIE TAIT

4 APRIL 1871

From the original in the Cavendish Laboratory(1)

[Glenlair]

D^r^ T′. Proofs &c come to hand.(2) Corrections thankfully received and honour given to whom honour is due. But the history of

$$\iint\left\{l\left(\frac{dZ}{dy}-\frac{dY}{dz}\right)+m\left(\frac{dX}{dz}-\frac{dZ}{dx}\right)+n\left(\frac{dY}{dx}-\frac{dX}{dy}\right)\right\}dS=\int\left(X\frac{dx}{ds}+Y\frac{dy}{ds}+Z\frac{dz}{ds}\right)ds$$

ascends (at least) to Stokes Smiths Prize paper 1854 and it was then not altogether new to yours truly.(3) Do you know its previous history? Poisson?(4) on light???(5)

Any suggestions about physical laboratories thankfully received. Present notion is – Avoid smooth plastered walls & ceilings where no wood can be found for the thread of your screw, but introduce freely wooden pillasters and beams in ceiling not plastered over.

(1) Museum of the Cavendish Laboratory, Cambridge (photocopy in ULC Add. MSS 7655, I, b/21).

(2) Proofs of the *Theory of Heat*: see Number 365 note (5).

(3) The terms and form in which Maxwell writes 'Stokes' theorem' follow Stokes' statement of the theorem in his Smith's Prize examination of February 1854, where Maxwell was placed equal Smith's Prizeman; see *The Cambridge University Calendar for the Year 1854* (Cambridge, 1854): 415 (= Stokes, *Papers*, **5**: 320). See also Maxwell's similar discussion of the theorem in the *Treatise*, **1**: 25–7 (§24); and compare Number 351.

(4) Theorems relating surface and volume integrals are stated by S. D. Poisson, 'Mémoire sur la théorie du magnétisme', *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, **5** (1826): 247–338, esp. 294–6; in his 'Mémoire sur la théorie du magnétisme en mouvement', *ibid.*, **6** (1827): 441–570, esp. 455–8; and in his 'Mémoire sur l'équilibre et les mouvements des corps élastiques', *ibid.*, **8** (1829): 357–570, 623–7. See J. J. Cross, 'Integral theorems in Cambridge mathematical physics, 1830–55', in *Wranglers and Physicists*, ed. P. M. Harman (Manchester, 1985): 112–48, esp. 118–21.

(5) In his reply of 5 April 1871 (ULC Add. MSS 7655, I, a/10) Tait wrote: '17 Drummond place, Edin^h^/O dp/dt, See above, and don't send cards meant for me to T. As to $\iint SU\nu\nabla\sigma\, ds = \int S\sigma\, d\rho$ I really thought it due to T and first published by the Archiepiscopal pair. I am glad you took my remonstrance about *my* thunder in good part as I felt some difficulty in making it – but it was for ∇ alone that I took to Q originally, and it has always been my endeavour to work it out. The paper of w^h^ I sent you a Proc. Abstract will contribute an immense deal more to its development. As to Laboratories, come here on y^r^ way from Glasgow, and you will see my poor make-shifts, and learn my *address* practically. T′.' Following Tait's statement of Stokes' theorem in quaternion form, see Maxwell's similar statement of the theorem in the *Treatise*, **1**: 28 (§25). The 'Archiepiscopal pair' were Thomson and Tait, whose names were those of the Archbishops

What about magneto electric engines as against Grove's battery?(6) There is to be a spinning room on ground floor for stirring water, spinning coils &c with driving gear & governor, and chronoscope.(7)

Yrs $\frac{dp}{dt}$

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of York and Canterbury (William Thomson and Archibald Campbell Tait). On Stokes' theorem see Thomson and Tait, *Natural Philosophy*: 124, and Number 351 note (3). For Tait's 'thunder' see his cards of 1 and 17 February 1871 (Numbers 353 note (11) and 356 note (7)). The paper to which Tait alludes is his 'On some quaternion integrals', *Proc. Roy. Soc. Edinb.*, **7** (1870): 318–20; and see Numbers 352, 353 and 356 for Maxwell's discussion, and Number 362 for his comments to Thomson.

(6) See Number 365.

(7) See Number 364.

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POSTCARD TO PETER GUTHRIE TAIT

3 MAY 1871

From the original in the University Library, Cambridge(1)

[Cambridge]

O T′. Proofs sent May^{2nd}. Pray return to Athenæum quam prox. Have you had pp. 32–48 from T yet. I want your views on it too. If you agree with pp 1–32 I shall have them printed off finally.(2) I have been at the Clarendon & they are to go a head.(3) They need to! I had no time to get to see your fixings(4) so I must conceive them in my mind till August. Clifton has had terrible work and has done it well.(5) Now he is a Plumber, now a Scene shifter and Property man, now a Bricklayer &c through all the trades mentioned by the learned Martinus Scriblerus.(6)

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(1) ULC Add. MSS 7655, I, b/22.

(2) Proofs of the *Theory of Heat*: see Number 365 note (5).

(3) On 10 May 1871 Maxwell signed a contract with the Delegates of the Clarendon Press, Oxford (copy in ULC Add. MSS 8812/159) for the publication of the *Treatise*. He had already discussed publication with Bartholomew Price, the Secretary to the Delegates. In a letter of 4 January 1871 (ULC Add. MSS 7656, P 659) Price had stated that: 'I am pleased to hear that you are so far on with the work that we may begin printing. Our rule here is, to obtain an order from the Delegates for that purpose; and when I apply for it, as I shall do in a few days, I shall be expected to lay before them as full details as possible. Will you therefore now give me an estimate, as near as you can, of the number of pages of the book'.

(4) Tait's Edinburgh laboratory: see Number 366 note (5).

(5) See Number 365 note (4). Maxwell sketched the plan of R. B. Clifton's 'Oxford Physical Laboratory' in his notebook (Museum of the Cavendish Laboratory, Cambridge; photocopy in ULC Add. MSS 7655, V, n/1).

(6) The 'Memoirs of Martinus Scriblerus', a satirical work written mainly by John Arbuthnot, and published in the second volume of Pope's prose works in 1741.

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POSTCARD TO PETER GUTHRIE TAIT

8 MAY 1871

From the original in the University Library, Cambridge(1)

[London]

O T′. I am desolated! I am like the Ninevites! Which is my right hand? Am I perverted?(2) a mere man in a mirror, walking in a vain show? What saith the Master of Quaternions? i to the South j to the West and k to the Heavens above. See Lectures §65 κ, τ, λ.(3) Lay hold of one of these and turn screw wise and you rotate +. To this agree the words of my text. But what say T and T′ §234. They are perverted. If a man at Dublin finds a watch, he lays it on the ground with its face up, and its hands go round from S to W and he says This is + rotation about an axis looking upwards. If the watch goes to Edin^h^ or Glasgow T′ or T carefully lays it down on its face, and after observing the gold case he utters the remarkable aid to memory contained in §234 of the book.(4) Please put me out of suspense by a note to 15 Upper Baker Street, N.W. I must get hold of the Math. Society and get a consensus of the craft.(5)

Tell me order of integration too.(6)

Yrs $\dfrac{dp}{dt}$

a mere chest of drawers

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(1) ULC Add. MSS 7655, I, b/23.

(2) See Numbers 370 and 371. 'Perversion' also had the meaning: 'change to error in religious belief' (*OED*).

(3) William Rowan Hamilton, *Lectures on Quaternions* (Dublin, 1853): 59 (§65); 'let i, j, k, denote three straight lines *equally long*, but differently directed; let it be also supposed that these different directions are *rectangular* each to each; and...let us conceive that these directions of i, j, k, are respectively *southward*, *westward*, and *upward* (in the present or in some other part of the northern hemisphere of the earth); so that i and j are both horizontal, but k is a vertical line.'

(4) Thomson and Tait's discussion of the direction of a couple in *Natural Philosophy*: 173 (§234); 'Hold a watch with its centre at the point of reference, and with its plane parallel to the plane of the couple. Then, according as the motion of the hands is contrary to, or along with the direction in which the couple tends to turn, draw the axis of the couple through the face or through the back of the watch. It will be found that a couple is completely represented by its axis'.

(5) See Number 370.

(6) In his reply dated 9 May 1871 (ULC Add. MSS 7655, I, a/11) Tait wrote: 'O dp/dt, the system $i = \mathrm{S}, j = \mathrm{W}, k = \mathrm{Z}$ is that of H, and was of course adopted by me in Q^{ns} so as to avoid perplexing readers of H & T′. If a new ed^{n} be ever called for, I shall take $i = \mathrm{E}, j = \mathrm{N}, k = \mathrm{Z}$ w^{h} is T & T′ §234. The reason for the latter is that, *from our northern latitudes*, unscrewing represents Earth's rotation & $\mathrm{rev}^{\mathrm{n}}$ Sun's rotation & planets' $\mathrm{rev}^{\mathrm{n}}$ &c and is therefore the *natural physical* + rotation. T′ told T at the time that §234 was confusing. T answered, "all the better, it

will fix it in the reader's mind" !!! T′ did n't see it, but consented. As to $\int$, I have always done this $\int_x\int_y\int_z Q\,dx\,dy\,dz$; – but I know that many say that it is better to do $\int_z\int_y\int_x Q\,dx\,dy\,dz$. This is more matter of taste than of agreement with nature. That trout indeed is "wondrous bad" enough to drive the eater mad, For 'tis but concentrated flea – Flea – smaller animalculae. These feed on spores & deadly germs, With which their stomachs come to terms; Think Edinburgh Pharisee, How kindly such will take to thee! Y[rs] $\frac{dp}{dt}$.' On Tait's *Elementary Treatise on Quaternions* see Number 370 note (3); and on 'Pharisee' see Number 371 note (8).

POSTCARD TO PETER GUTHRIE TAIT

11 MAY 1871

From the original in the University Library, Cambridge[1]

[London]

O T′![2] Are not Vitreous and Boreal $+^{\text{ve}}$ Resinous and Austral $-$ve. Is not from Copper to Zinc through the wire $+^{\text{ve}}$ and from the Arctic to the Antarctic regions on the earths surface $+^{\text{ve}}$ ie the line NS on a compass needle indicates the $+$ direction of magnetic force. But if a current runs round a horizontal circuit withershins, E N W S it becomes $=$ a magnetic shell austral face up, boreal down and a needle within stands $\begin{array}{|c|}\hline N\\ S\\ \hline\end{array}$. Hence if we draw x to E and y to N we must draw z down. If we point in succession to $x\,y$ and z we describe a cone anticlock wise. If we move along any axis and revolve positively we describe a left handed screw. The Master of the Quaternions[3] and the Censor of Space[4] are at one on this point. I am going to consult the Math. Soc. tonight.[5] Hirst's[6] notion was that of T & T′. If I am convinced I will be converted and remodel all my book and all my contrivances for remembering these directions. But it will take one or two of us to settle the hash of the M of Q. and the C of S. The new P.R.S[7] must be involved by all that is red and all that is blue. If I find a watch I will pin it to the North star and it will indicate which way the world goes round to all northerners.[8]

15 Upper Baker Street NW $\dfrac{dp}{dt}$

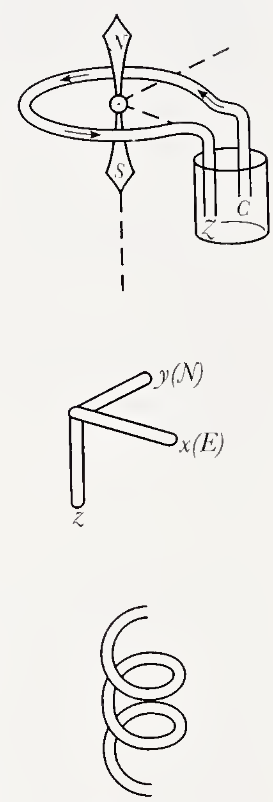


Figure 369,1

(1) ULC Add. MSS 7655, I, b/24.

(2) In reply to Tait's card of 9 May 1871 (Number 368 note (6)).

(3) William Rowan Hamilton: see Number 368.

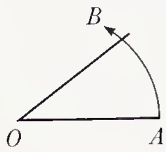
(4) Johann Benedict Listing: see Number 370, a pun on his 'Census räumlicher Complexe' (1861).

(5) See Number 370.

(6) Thomas Archer Hirst, Professor of Mathematics at University College London until 1870, who had a special interest in geometry (*DNB*); see Number 370.

(7) George Biddell Airy was elected President of the Royal Society on 30 November 1871; see *Proc. Roy. Soc.*, **20** (1871): 57.

(8) In his reply of 13 May 1871 (ULC Add. MSS 7655, I, a/12) Tait wrote: 'O *dp*/*dt*. What saith Trigonometry (& Geometry of Two dimensions) to the point at issue? – Surely



(and 

). You do not fancy the third axis to be running away from you? Eh? Catton has started a *mixture* of picoline, carbolic acid &^{c} whose composition (not molecular arrangement) is that of wool, and with it he manures (guano-wise) the backs of his 70,000 sheep! We call it a farm-a-cutical process. ΛΩΦΡ. I have got a dodge for making a battery which can give but $\frac{1}{20}$ inch of voltaic arc give an inch of the same, and a spectroscope of perfectly unlimited dispersion made of *one* piece of glass. Also conical refractions of both kinds made a *class* experiment for 200 gaping spectators. Who is the Censor of Space? Census räumlicher Complexe? $\mathrm{Y^{rs}}$ ϛ. P.S. For further information consult $\mathrm{M^{rs}}$ T′ and $\mathrm{M^{rs}}$ A. C-B at the Langham next week early.' Tait is referring to the wife of Alexander Crum Brown (see Number 478 note (12)); and to the chemist Alfred Catton.

370

QUESTION TO THE LONDON MATHEMATICAL SOCIETY ON SPATIAL RELATIONS

11 MAY 1871

From the *Proceedings of the London Mathematical Society*(1)

Prof. Clerk Maxwell asked for information from the members as to the convention established among Mathematicians, with respect to the relation between the positive direction of motion along any axis, and the positive direction of rotation round it. In Sir W. R. Hamilton's lectures on Quaternions, the coordinate axes are drawn x to South, y to West, and z upwards.(2) The same system is adopted in Prof. Tait's Quaternions,(3) and in Listing's 'Vorstudien zur Topologie'.(4) The positive directions of translation and of rotation are thus connected in a left-handed screw, or the tendril of the hop.

On the other hand, in Thomson's and Tait's Natural Philosophy, §234, the relations are defined with reference to a watch,(5) and lead to the opposite system, symbolized by an ordinary or right-handed screw, or the tendril of the vine.(6) If the actual rotation of the earth from West to East be taken

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(1) *Proceedings of the London Mathematical Society*, **3** (1871): 279–80.

(2) W. R. Hamilton, *Lectures on Quaternions* (Dublin, 1853): 59; see Number 368 note (3).

(3) P. G. Tait, *An Elementary Treatise on Quaternions* (Oxford, 1867): 43.

(4) Johann Benedict Listing, 'Vorstudien zur Topologie', in *Göttinger Studien. 1847. Erste Abtheilung: Mathematische und naturwissenschaftliche Abhandlungen* (Göttingen, 1847): 811–75, esp. 818.

(5) See Number 368 note (4).

(6) In the *Treatise*, **1**: 24n (§23) Maxwell notes that: 'Professor W. H. Miller has suggested to me that as the tendrils of the vine are right-handed screws and those of the hop left-handed, the two systems of relations in space might be called those of the vine and the hop respectively.' Maxwell had presumably consulted William Hallowes Miller, whose *A Treatise on Crystallography* (Cambridge, 1839) is cited by Listing in his 'Vorstudien zur Topologie': 875 with reference to a brief discussion of crystal symmetry. In his discussion of 'Helikoide oder Wendellinie', specifically of 'rechtswendig oder dexiotrop' and 'linkswendig oder laeotrop', Listing referred to the direction of tendrils in botany; see his 'Vorstudien zur Topologie': 838–50. In a report of the discussion, 'Right-handed v. left-handed', *Journal of Botany, British and Foreign*, **9** (1871): 216, Robert Tucker (Secretary of the London Mathematical Society) remarked that: '[Maxwell] refers to Linnæus ('Philosophia Botanica', 1757, p. 39), where, speaking of the trunk, he says, "Caulis...spiraliter ascendens...*Sinistrorsum* (secundum solem vulgo: Humulus, Lonicera, Tamus. *Dextrorsum*)contra motum solis vulgo: Convolvulus, Phaseolus" etc. Mr. Maxwell states also that De Candolle was the first botanist who, in 1827, has decided otherwise, and that many botanists have been led astray and perverted by him.' These comments were not included in the published report in the London Mathematical Society's *Proceedings*. The Linnæus text here cited is quoted (though with some differences in transcription) by Listing, 'Vorstudien zur Topologie':

positive, the direction of the earth's axis from South to North is positive in this system. In pure mathematics little inconvenience is felt from this want of uniformity; but in astronomy, electro-magnetics, and all physical sciences, it is of the greatest importance that one or other system should be specified and persevered in. The relation between the one system and the other is the same as that between an object and its reflected image, and the operation of passing from one to the other has been called by Listing *Perversion*.(7)

Sir W. Thomson and Dr. Hirst(8) stated the arguments in favour of the right-handed system, derived from the motion of the earth and planets and the convention that North is to be reckoned positive, and also from the practice of Mathematicians, in drawing x to the right-hand and y upwards on the plane of the black board, and z towards the spectator. No arguments in favour of the opposite system being given, the right-handed system, symbolized by a corkscrew or the tendril of the vine, was adopted by the Society.

APPENDIX: NOTE ON SPATIAL RELATIONS

11 MAY 1871

From the original in the Cavendish Laboratory(9)

System of the Vine adopted May 11, 1871, by the L.M.S.
Vitreous electricity is +.
Austral magnetism is +.

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845. These differences suggest that Maxwell had also consulted Linnæus' *Philosophia Botanica* (Stockholm, 1751/Vienna, 1755): 39. It would seem likely that Listing's paper was also the source from which the reference to the alternative directional convention, which had been proposed by A. P. de Candolle in his *Organographie Végétale* (Paris, 1827): 156, was drawn; see Listing, 'Vorstudien zur Topologie': 848. For discussion of Maxwell's reported comment on De Candolle see H. F. Hance, 'Right-handed v. left-handed', *Journal of Botany*, **9** (1871): 333–4. In the *Treatise*, **1**: 24n (§23) Maxwell notes that: 'The system of the vine, which we adopt, is that of Linnæus, and of screw-makers in all civilized countries except Japan. De Candolle was the first who called the hop-tendril right-handed, and in this he is followed by Listing, and by most writers on the rotatory polarization of light'.

(7) Listing, 'Vorstudien zur Topologie': 830; 'so dürfte es zweckmässig sein, den Fall einer halben Umdrehung, wodurch sich zwei Dimensionen zugleich umkehren, eine *Inversion* oder *Umkehrung* schlechthin, den Fall einer einzigen Dimensionsumkehrung aber eine *Perversion* oder Verkehrung zu nennen, und im dritten Falle also, wo alle drei Dimensionen umgekehrt sind, den Körper als *verkehrt und umgekehrt zugleich* anzusehen.' See the *Treatise*, **1**: 24 (§23).

(8) See Number 369 note (6).

(9) Notebook, Museum of the Cavendish Laboratory, Cambridge (photocopy in ULC Add. MSS 7655, V, n/1).

The end of a magnet which points N is +.
Terrestrial magnetic force is towards the N.
Magnetization is from the S end to the N end of magnet.
⌊internal⌋ Magnetization of the Earth is from N to South.
Electric current. Copper → wire → Zinc → acid →[10]
System of Hop in Hamiltons Lectures §65–71.

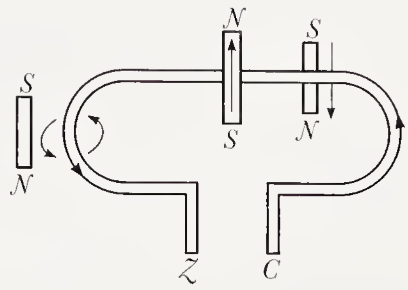

Figure 370,1

(10) Compare the figure in 'On physical lines of force. Part II', *Phil. Mag.*, ser. 4, **21** (1861), Plate V, Fig. 1 (= *Scientific Papers*, **1**: Plate VIII, Fig. 1, facing 488); and see Figure 385, 1.

371

POSTCARD TO PETER GUTHRIE TAIT

12 MAY 1871

From the original in the University Library, Cambridge[1]

[London]

$\boxed{\text{art 23}}$[2] In this treatise, the motions of translation along any axis and of rotation round that axis will be assumed to be of the same sign when they are related to each other in the same way as the motions of translation and rotation of a right handed screw. For instance if the actual motion of rotation of the earth from W to E is taken + the direction from the S pole to the N. pole will be taken +.

If a man walks in the positive direction, the + rotation is upwards on the left and downwards on the right.

This is the right handed definition of directions and is adopted in this treatise and in T & T′.[3] The opposite system is adopted in H and T′s Qs[4] and in Listing.[5] If we confound the one with the other, every figure will become *perverted* (a phrase of L denoting an effect similar to that of reflexion in a mirror.[6] | O T′ I am perverted by T & the L.M.S last night.[7] Will this do?[8] Tell me that I may print.[9]

Yrs $\dfrac{dp}{dt}$

(1) ULC Add. MSS 7655, I, b/25.

(2) See the *Treatise*, **1**: 24 (§23).

(3) See Number 368 note (4).

(4) See Numbers 368 note (3) and 370 note (3).

(5) See Number 370 note (4).

(6) See Number 370 esp. note (7).

(7) See Number 370.

(8) Referring to his card of 13 May 1871 (Number 369 note (8)) in his reply of 14 May 1871 (ULC Add. MSS 7655, I, a/13), Tait there wrote: 'O *dp/dt*, Though I sent you a P.C. last night yrs recd this morning induces me to send another. You are now O.K., and only for the sake of criticism do I permit myself to add the following. The words "this treatise" occur *twice*; "the (linear) direction from the S to N pole". "If a man walk (forwards) in the"..."a wheel (City Arab style) to right is +". / Happy to receive, along with so illustrious a pervert, the whole L.M.S. What a haul! Surely it will now dominate the world of math. & phys. / Have to lecture R.S.E. (is F.R.S.E. = Pharisee?) on Monday on Spectrum Analysis. *Two* carts to take batteries &c down to their rooms. Awful bore. Yrs $\mathcal{G}$'. On 15 May 1871 Tait delivered an 'Address on spectrum analysis' to the Royal Society of Edinburgh; see *Proc. Roy. Soc. Edinb.*, **7** (1871): 455–61.

(9) Maxwell took note of Tait's emendations in revising this draft of *Treatise* §23.

372

POSTCARD TO PETER GUTHRIE TAIT

25 MAY 1871

From the original in the University Library, Cambridge[1]

[London]

O T′. Can you tell me the tale of the Florentine thermometers, one of which is in your Apparatus room having glass beads for degrees? Who made them? at what date? Were any ancient observations made with them which have been translated into modern degrees since the discovery of the instrument. When were they lost? Who discovered them again & when? Who wished they had been discovered? Who gave one to the Edin^h^ Nat Phil. Is there anything in print about it? Information sent to $\frac{dp}{dt}$ will receive due attention.[2]

Again! Who is the author of the theorem

$\iint S.\nabla\frac{1}{\rho}Uvds = 4\pi$ or 0 according as a closed surface encloses the origin or not. It is Gauss or Stokes? I mean in its Cartesian form.[3]

To conclude. Is $Xdx+Ydy+Zdz$ in certain cases, a *complete*, *exact total* or what else? differential. Which is the correct word.[4]

Lastly I thank you and praise you for turning me from the system of the hop to that of the vine. I have perverted the whole of electromagnetics to suit.[5] When you send me my proofs I will send you correct cards of the book. But tell me about the thermometers at once if you can.

$\frac{dp}{dt}$

(1) ULC Add. MSS 7655, I, b/26.

(2) For the *Theory of Heat*. See Number 374.

(3) On Gauss' theorem see Number 374, esp. note (5).

(4) Compare *Treatise*, **1**: 14 (§16), where Maxwell discusses line-integrals, and uses the word 'exact' differential.

(5) See Numbers 370 and 371.

373

POSTCARD TO PETER GUTHRIE TAIT

27 MAY 1871

From the original in the University Library, Cambridge[1]

[London]

O T′. It was you who taught me to revere the name of Jacques Alexandre César Charles.[2] Since then I have endeavored to discover his excellence. He invented the Charlière[3] and went up in it. He also wrote many papers one of these is Essai sur les moyens d'etablir entre les thermomètres une comparabilité &c 1787. Ac. Sc.[4] This is the only thermal paper of his but there is nothing in it about the laws of expansion of gases, but many other excellent obs[ns]. I cannot find in Verdet anything about him, at least in the book I sought in,[5] so Charles must look to those laurels with which, according to the late Lord Byron, he rhymes.[6]

Well done conical refraction!![7] Carnots Geometry of Position[8] is like

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(1) ULC Add. MSS 7655, I, b/27.

(2) See Maxwell's letter to Tait of July 1868 (Number 296 esp. note (19)); and see also Number 382 note (9). See the *Theory of Heat* (London, 1871): 29–30n.

(3) A hydrogen balloon, in which Charles ascended on 1 December 1783.

(4) J. A. C. Charles, 'Essai sur les moyens d'établir entre les thermomètres une comparabilité, sinon exacte, au moins plus approchée que celle qu'on a obtenue jusqu'a présent', *Mémoires de l'Académie Royale des Sciences* (année 1787): 567–82, a reference Maxwell recorded in a notebook (King's College London Archives, Maxwell Papers, Notebook (3)).

(5) Possibly: Émile Verdet, *Cours de Physique professé à l'École Polytechnique*, 2 vols. (Paris, 1868–9), of which there is a copy in Maxwell's library (Cavendish Laboratory, Cambridge). In his reply of 1 June 1871 Tait repeated the source he had cited in his *Sketch of Thermodynamics* for his discussion of 'Charles' law': 'O dp/dt, I sent you today your proofs with a good deal of irreverent pencil scoring thereon. Good *may* perhaps come of it, but I was toothachy & inclined to be severe. / See Note E to Verdet's "Leçons de Chimie et de Physique" apropos of Cha*w*les. / Give me a good empirical formula for temperature along a bar when things have reached a steady state. Forbes takes $\log t = \log A + \frac{Bx}{1-Cx}$, but I find not this nor $t = A\epsilon^{px} + B\epsilon^{qx}$ at all good. Y[rs] ♈.' (ULC Add. MSS 7655, I, a/14). See 'Note E' to É. Verdet, *Théorie Mécanique de la Chaleur*, 2 vols. (Paris, 1868–72), **1**: cviii–cix, on the 'loi de Charles'. For Forbes' formula see Part II of his 'Experimental inquiry into the laws of the conduction of heat in bars, and into the conducting power of wrought iron', *Trans. Roy. Soc. Edinb.*, **24** (1865): 73–110, on 83–4. For further discussion of 'Charles' law' see Tait's card of 7 June 1871 (Number 375 note (2)).

(6) See Number 296 esp. note (21).

(7) See Tait's card of 13 May 1871 (Number 369 note (8)).

(8) L. N. M. Carnot, *Géométrie de Position* (Paris, 1803).

Chasles Superior ditto.[9] Mine is that which Gauss calls Geometria Situs as opposed to G. Magnitudinis.[10]

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(9) Michel Chasles, *Traité de Géométrie Supérieure* (Paris, 1852).

(10) Maxwell is alluding to Gauss' comments on 'Geometria Situs' and 'Geometria Magnitudinis' in a note published in his *Werke*, **5**: 605 (see Number 276 note (8)). The distinction is between topology and projective geometry.

374

POSTCARD TO PETER GUTHRIE TAIT

27 MAY 1871

From the original in the University Library, Cambridge[1]

[London]

O T′. Many thanks for your information &c.[2] It has been the means of giving me the chance of getting Babbages Florentine Thermometer for Cambridge.[3] NB. Libri says that Père Raineri / Rinieri / Reinerius / Renieri / Reinieri made obs^{ns} 5 times a day for 16 years with these thermometers at the convent degli Angeli. He died 1647. Hence thermometers existed in 1631. They were made by Giuseppe Moriani called 'Il Gonfia' Freezing pt. = 13°.5. Accademia del Cimento began 18 June 1657 & was suppressed in 1667. Therm found by Antinori 1829 and described by Guglielmo Brutus Icilius Timoleon Libri Carucci dalla Sommaja Ann de Ch XLV (1830).[4]

The theorem I wished a father for is that in T+T′ §492.[5] I have yet to

(1) ULC Add. MSS 7655, I, b/28.

(2) See Number 372 for Maxwell's query about the 'Florentine thermometers'.

(3) In his first annual report on the 'Cavendish Laboratory', published in the *Cambridge University Reporter* (27 April 1875): 352–4, on 354, Maxwell recorded that a 'Thermometer found by Antinori in the repositories of the Accademia del Cimento' in Florence had been presented to the Laboratory 'by the late C. Babbage F.R.S.' In his reply to Maxwell's query, Tait may have drawn attention to Charles Babbage in connection with the Florentine thermometer. In his *Reflections on the Decline of Science in England* (London, 1830): 182, Babbage had referred to a Florentine thermometer and a paper on it by Libri (see note (4)), to which Maxwell here makes reference. A similar thermometer had been presented to the Royal Society on 3 February 1870: 'Among the Presents received was a Thermometer, presented by Mr Augustus de Morgan, which had been made in Florence in the seventeenth century. It was one of a collection discovered in the Museo Fisico of Florence in 1829, which had belonged to the Accademia del Cimento' (*Proc. Roy. Soc.*, **18** (1870): 183–4). In a manuscript jotting (Notebook, Museum of the Cavendish Laboratory, Cambridge; copy in ULC Add. MSS 7655, V, n/1) Maxwell noted that the thermometer had been 'Presented to R.S. by De Morgan Feb. 3 1870'.

(4) G. Libri, 'Mémoire sur la détermination de l'échelle du thermomètre de l'Académie del Cimento', *Ann. Chim. Phys.*, ser. 2, **45** (1830): 354–61; and compare Maxwell, *Theory of Heat* (London, 1871): 34.

(5) See Number 372, and Thomson and Tait, *Natural Philosophy*: 372 (§492); 'Let S be any closed surface, and let O be a point, either external or internal, where a mass m, of matter is collected. Let N be the component of the attraction of m in the direction of the normal drawn inwards from any point P, of S. Then, if $d\sigma$ denotes an element of S, and $\iint$ integration over the whole of it, $\iint N d\sigma = 4\pi m$, or $= 0$, according as O is internal or external.' The theorem is stated

learn the rudiments of Q^{ns}. I send you pp 1–17 to keep & pp. 65–80 to annotate.(6) I shall be at 15 Upper Baker Street till we have settled with the D. of Devonshire about our plans.(7) I am brewing details still but the principal features are now in rough plans. I mean to try and settle the Ohm again.

$$\frac{dp}{dt}$$

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by C. F. Gauss in his 'Allgemeine Lehrsätze in Beziehung auf die im verkehrten Verhältnisse des Quadrats der Entfernung wirkenden Anziehungs-und Abstossungs-Kräfte', in *Resultate aus den Beobachtungen des magnetischen Vereins in Jahre 1839*, ed. C. F. Gauss and W. Weber (Leipzig, 1840): 1–51, see esp. §23; reprinted in Gauss, *Werke*, **5** (Göttingen, 1867): 197–242, esp. 225–6 for the theorem; and trans. in *Scientific Memoirs*, ed. R. Taylor, **3** (London, 1843): 153–96, esp. 180–81.

(6) Of the *Theory of Heat*.

(7) According to a letter of 1 June 1871 from the Duke of Devonshire to the Vice-Chancellor of Cambridge University, John Power (ULC, V. C. Corr. I. 2/5), Maxwell, together with the architect William Milner Fawcett and the Vice-Chancellor, was to meet the Duke at Devonshire House, London on 8 June.

POSTCARD TO PETER GUTHRIE TAIT

3 JUNE 1871

From the original in the University Library, Cambridge(1)

[London]

$$V = 2\pi\sigma\frac{a}{p}\Big(\Psi(a+p)-\Psi(a-p)\Big).$$

V const. for all values of p less than a. To find Ψ.

Put $\Psi(r) = Cr+\chi(r)$ then if $V = 4\pi\sigma aC$, $\chi(a+p) = \chi(a-p)$.

Since this is true for various values of a as well as of p, $(a+p)$ is independent of $(a-p)$ and $\chi(r)$ must be zero or a constant.

Hence
$$\Psi(r) = Cr+B$$
$$\Psi'(r) = C = rf(r)$$

and potential $= ef(r) = e\dfrac{C}{r}$ $\therefore C =$ unity & $V = 4\pi\sigma a$.

There are no infinite series here. On the other hand there are no excursions into the region of homogeneous solid spheres.(2)

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(1) ULC Add. MSS 7655, I, b/29.

(2) Tait's reply of 5 June 1871 (ULC Add. MSS 7655, I, a/15) establishes the context, and reference to his paper 'On an expression for the potential of a surface-distribution, and on the operator $T\nabla = \sqrt{(d/dx)^2+(d/dy)^2+(d/dz)^2}$', *Proc. Roy. Soc. Edinb.*, **7** (1871): 503–6 (read 20 May 1871): 'O dp/dt Neat but *not* correct, as you will see by what follows. Let fr/r be potential function, e distance of point from centre of shell a, r the dist^{ce} of an element $(2\pi a^2\rho\,\delta a\sin\theta\,\delta\theta)$ from the point. Then $r\,dr = ae\sin\theta\,d\theta$ & potential $V = \dfrac{2\pi\rho a\,\delta a}{e}\displaystyle\int_{a-e}^{a+e} f(r)\,dr$./MULTIPLY BY e & DIFF^{te} WITH RESPECT TO IT./Then $V = 2\pi\rho a\,\delta a\,\{f(a+e)+f(a-e)\}$. Diff^{te} again $0 = f'(a-e)-f'(a-e)$. When, as a & e are independent, $f'r = C, fr/r = C+C'/r$. Your method gives the C' only, and is therefore obviously *incomplete*, to say the least. But what can one expect from a follower of Pratt, and a dealer in infinite series? There is no difficulty in extending the solid sphere affair to this case. Y^{rs} ℘. M^{c}Villain has refused to publish T′ on Thermody^{cs}. It remains for you to render it *essential* in Cambs.' In a subsequent card of 7 June 1871 (ULC Add. MSS 7655, I, a/16) Tait made further reference to the issue, and responded to a missing card of Maxwell's on 'Charles' law' (on which see Number 373 esp. note (5)): 'O dp/dt, I rel*ay* simply on Verdet's Note E, but your quotation bears me out, and I think it desirable to give G.L. merely equivalent volumes (w^{h} is high claim, but) which, x'cpt where you and C are concerned don't much appear in D.T.H. Many thanks for the paper from L.M.S. with its—iteration of T′'s services. Surely, after such ramming, the charge must have been got home! I gave the R.S.E. at its last meeting a quat. proof of the $1/T\rho$ potential for constancy inside *any* closed surface *with proper distribution of matter thereon*. The method also shows what the proper &^{c} *is*. Here is a theorem w^{h} I beg you to ponder & report on. If $\Theta\theta = V.\eta\phi\eta$ (Θ scalar, θ & η vectors) then $H\eta = V.\theta\phi\theta$. This is easily proved. Now suppose η be

15 U B St till Θursday,[3] then Glenlair

$$\frac{dp}{dt}$$

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determined by $\frac{d\eta}{dt} = V.\eta\phi\eta$, is θ given by $\frac{d\theta}{dt} = V.\theta\phi\theta$? I never heard of the Dark Blue, but would be obliged to you for a perusal of the Sylvestrian Ode. Y^{rs}♈. / T went to Lisbon in the L.R. a fortnight ago. Dew in London on Friday.' In his *Theory of Heat* (London, 1871): 295–7 Maxwell gave an account of Gay-Lussac's 'law of volumes', that 'in the case of gases the volumes of the combining quantities of different gases always stand in a simple ratio to each other', and of the 'law of the equal dilatation of gases discovered by Charles'. Gay-Lussac had questioned Charles' claims (but in this was not followed by Verdet, Tait and Maxwell) in his 'Recherches sur la dilatation des gaz et des vapeurs', *Annales de Chimie*, **43** (1802): 137–75, esp. 157–8. In a notebook entry (Cavendish Laboratory, Cambridge; copy in ULC Add. MSS 7655, V, n/1) Maxwell did however transcribe Gay-Lussac's comments denying Charles' claims: 'Il me parait donc qu'on ne peut conclure de ces expériences la vraie dilatation des gaz'. The 'L.M.S.' paper Tait alludes to is Maxwell's 'On the mathematical classification of physical quantities', *Proceedings of the London Mathematical Society*, **3** (1871): 224–32, esp. 230–32 (= *Scientific Papers*, **2**: 263–6) on Tait's work on quaternions.

(3) It had been arranged that Maxwell should meet the Duke of Devonshire in London on Thursday 8 June 1871; see Number 374 note (7).

POSTCARD TO PETER GUTHRIE TAIT

14 JUNE 1871

From the original in the University Library, Cambridge[1]

Glenlair 14 June 1871

O T′. I send you sheet 2 to nabble at.[2] I expect a clean copy soon but I have no other at present. I also send you sheet 6 which I have not been able to correct yet, and I hope to do so in a day or two, but I am not able yet, having been at 106 °F on Sunday & in bed since. Dont be afraid of my card for I have no spots and am perfectly well today. Case of febricula, $MgSO_4$ &c. Probably from working 9 days a week in London. I think you should make a supplementary book on Quaternions explaining the true principles of dots and brackets and defining the limits of the sway of symbols as the Spaniards define the end of an interrogation or we that of a quotation. Behold the Sylvestrian Sonnet.[3]

Tasso to Eleonora

Calm, pure & mirroring the blue above
  To whom comminglingly my life's streams flow,
  Making that one which many seemed but now,
Thou art the sum and ocean of my love!
What though my soul rebellious pulses prove:
  These are the gnats that o'er the surface play,

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(1) ULC Add. MSS 7655, I, b/30.

(2) Proofs of the *Treatise*; see Number 367. This card was written in reply to Tait's cards of 7 June 1871 (Number 375 note (2)), and of 13 June 1871 (ULC Add. MSS 7655, I, a/17) where he wrote: 'O *dp*/*dt*, I wait impatiently for a spare copy of your proof-sheet N° 2; upon which I wish to try my ∇-Math. during the vacation. The first sight I got of it led me to the theorem (of awful import) $\epsilon^{-S.\sigma\nabla}f(\rho) = f(\rho+\sigma)$. / Also $\iiint(\epsilon^{-S\sigma\nabla}-1)Q\,d\varsigma = -\iiint\left(S\sigma\nabla-\frac{1}{1.2}(S\sigma\nabla)^2+\ldots\right)Q\,d\varsigma$. / Suppose the integration extended through a sphere, centre ρ & rad. r, the right side becomes $= 0-\frac{vr^2}{10}\nabla^2Q+\&^{c}$ where v is vol. of sphere. This gives your *concentration* theorem. But there are still better fish in this new sea, several of which have already risen. Y$^{\rm rs}$ T′.' Tait discussed these quaternion theorems in his paper 'On some quaternion transformations', *Proc. Roy. Soc. Edinb.*, **7** (1871): 501–3 (read 20 May 1871). On Maxwell's 'concentration theorem' see Number 347, a result published in his essay 'On the mathematical classification of physical quantities', *Proceedings of the London Mathematical Society*, **3** (1871): 224–32, esp. 231 (= *Scientific Papers*, **2**: 264).

(3) In response to Tait's request for J. J. Sylvester's poem in his card of 7 June 1871 (Number 375 note (2)).

  The fleeting colours painted on the spray;
They cannot in its depths the ocean move.
In the Elysium of thy love I dwell,
  And at its lucid fountain in thine eyes
  Immortal longings of the soul allay,
  Vainly thy pride's dissembling lips devise
How best the dear conclusion to repel,
  The silent message of those orbs unsay.

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MANUSCRIPT ON THE HISTORY OF THE KINETIC THEORY OF GASES: NOTES FOR WILLIAM THOMSON(1)

circa SUMMER 1871(2)

From the original in the University Library, Glasgow(3)

KINETIC THEORY OF GASES(4)

1 Democritus see Lucretius. 2 Lucretius. α Bodies are composed of a finite number of indivisible but invisible parts. β These parts are in constant motion even when the motion of the body in mass is not perceived. γ the direction of this motion is *downward* and sensibly but not mathematically uniform. This is a strong point with Lucretius and the weak point of his theory. δ irregularity of the deflexions of the atoms introduced to account for free will &c. This is very important in T. L. Carus.

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(1) In a post card (post mark: 'Edinburgh June 17, 71'), William Thomson wrote with reference to his forthcoming Presidential Address to the British Association; see the *Report of the Forty-first Meeting of the British Association for the Advancement of Science; held at Edinburgh in August 1871* (London, 1872): lxxxiv–cv, esp. xciii–xcv for his account of gas theory. 'O *dp.dt.* Where is the Address you undertook to write? It should be in the printers hands soon, so please don't delay longer. Send it to Athenaeum Club London posting not later than next Wednesday. It must be *complete* (for press) on Dynamical Theory of gases, Diffusion & all who worked meritoriously on it, whether experimentally or theoretically and *their merits carefully weighed*. This last is de rigeur. Include also everything that occurs to you. Don't omit H^2, on Gauss & Ampère &c. (Neumaniac anticipatory potentials may in pity be spared, but)? Weber's formulae for mutual force? Be explicit, and don't be misled by anything I have said above. If anything else occurs to you don't omit it. I am in London Wed. Thurs. & Frid. and sleep on board LR 5 nights of every week. So don't omit to keep me informed. T′ joins me in the hope that the $MgSO_4$ sufficed.' Tait appended: 'After the above lucid statement of everything, I have nothing to add. My question was, If together they give a parabola, will they separately. ϛ' (ULC Add. MSS 7655, I, a/19). Tait's question referred to his card of 14 June 1871 on thermo-electricity: see Number 378 note (11).

(2) See note (1).

(3) Glasgow University Library, Kelvin Papers, M 23. First published by H. T. Bernstein, 'J. Clerk Maxwell on the kinetic theory of gases', *Isis*, **54** (1963): 206–15, on 210–15.

(4) In 1860 and again in 1867 Maxwell referred to his theory as 'the dynamical theory of gases'; see notes (16) and (30). For this usage see especially his letter to Stokes of 18 December 1866 (Number 266). Compare the definitions of 'dynamics' and 'kinetics' in Thomson and Tait's *Natural Philosophy*: vi; 'we employ the term *Dynamics* in its true sense as the science which treats of the action of *force*, whether it maintains relative rest, or produces acceleration of relative motion. The two corresponding divisions of Dynamics are thus conveniently entitled *Statics* and *Kinetics*.'

Quare in seminibus quoque idem fateare necesse est
Esse aliam præter plagas et pondera causam
Motibus, unde hæc est nobis innata potestas:
De nihilo quoniam fieri nil posse videmus,
Lib II, 284 Pondus enim prohibet, ne plagis omnia fiant,
Externa quasi vi: sed ne mens ipsa necessum
Intestinum habeat cunctis in rebus agendis;
Et devicta quasi cogatur ferre patique:
Id facit exiguum clinamen principiorum
Nec regione loci certa, nec tempore certo.[5]

3 Catena of upholders of intestine motion in hot bodies. Bacon Newton Boyle Cavendish &c.

4 Dan. Bernoulli, not very definite but stated the theory of pressure produced by impact.[6]

5 Lesage of Geneva wrote an essay Lucrèce Newtonien[7] deducing gravity from the impact of ultramundane corpuscles going *in all directions* and maintaining that if Lucretius had possessed half the mathematical skill of this contemporary Euclid of Alexandria he would have carried physical science far beyond the stage to which Newton advanced it. Lesage himself would have made a more important contribution to science, if, before calculating the results of the impact of his corpuscles, he had studied the few sentences in which Newton demonstrates the true laws of impact.

6 Pierre Prevost of Geneva (author of theory of Exchanges) published another treatise of Lesage and one of his own[8] in which he ascribes the pressure of gases to the impact of their molecules against the sides of the vessel, but introduces the ultramundane corpuscles to maintain the motion of gaseous molecules.

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(5) *Titi Lucreti Cari De Rerum Natura Libri Sex*, ed. and trans. H. A. J. Munro, 2 vols. (Cambridge, $_2$1866), **1**: 93–4 (Book II, 284–93); 'Wherefore in seeds too you must admit the same, admit that besides blows and weights there is another cause of motions, from which this power of free action has been begotten in us, since we can see that nothing can come from nothing. For weight forbids that all things be done by blows through as it were an outward force; but that the mind itself does not feel an internal necessity in all its actions and is not as it were overmastered and compelled to bear and put up with this, is caused by a minute swerving of first-beginnings at no fixed part of space and no fixed time.' (*ibid.*, **2**: 35). On the swerve of Lucretian atoms see also Numbers 257 esp. note (10), and 439 esp. note (19).

(6) Daniel Bernoulli, *Hydrodynamica, sive de Viribus et Motibus Fluidorum Commentarii* (Strasbourg, 1738): 200–2.

(7) G. L. Le Sage, 'Lucrèce Newtonien', *Nouveaux Mémoires de l'Académie des Sciences et Belles-Lettres de Berlin*, (1782): 404–32; and in Pierre Prevost, *Notice de la Vie et des Écrits de George-Louis Lesage de Genève* (Geneva, 1805): 561–604.

(8) *Deux Traités de Physique Mécanique* publiés par Pierre Prevost (Geneva/Paris, 1818).

7 Herapath in his Mathematical Physics 1847 gives still more extensive applications of the theory – to gases flowing out of small holes, diffusing through each other &c. I think the notion of temperature being as the square of the velocity is his but he makes the 'true temperature' the square root of what we call absolute temperature. This is a mere definition. He also gives −480 °F or −491 °F as the 'point of absolute cold'.(9)

It is remarkable that Lesage and Herapath should have independently fallen into similar errors about the impact of bodies,(10) these errors being I believe unknown in the text books of their day. The only source from which these errors might have been derived is the Principia of Descartes.(11)

8 Joule in 1848 calculated with great exactness the velocity of the molecules of hydrogen and subjected the theory to the test of experiment.(12)

9 In 1856 Dr Krönig directed attention to the kinetic theory of gases and showed how the gaseous laws may be deduced from the impact of perfectly elastic molecules.(13) His conceptions of the arrangements and motions of the molecules are deficient in generality.

10 The great development of the theory is due to Clausius.(14)

α The arrangement of the molecules at any instant is perfectly general.

β The impacts of the molecules against each other are taken fully into account.

γ The relation between their diameter, the number in a given space and the mean length of path is determined.

δ Mathematical methods are introduced for dealing *statistically* with immense numbers of molecules by arranging them in groups according to their directions velocities &c.

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(9) John Herapath, *Mathematical Physics; or the Mathematical Principles of Natural Philosophy: with a Development of the Causes of Heat, Gaseous Elasticity, Gravitation, and other Great Phenomena of Nature*, 2 vols. (London, 1847), **1**: 243–5, 282–5, and 249.

(10) Herapath, *Mathematical Physics*, **1**: 8–11.

(11) René Descartes, *Principia Philosophiæ* (Amsterdam, 1644), Pars Secunda.

(12) James Prescott Joule, 'Some remarks on heat, and the constitution of elastic fluids', *Memoirs of the Literary and Philosophical Society of Manchester*, **9** (1851): 107–14 (read 3 October 1848); and in *Phil. Mag.*, ser. 4, **14** (1857): 211–16.

(13) August Krönig, 'Grundzuge einer Theorie der Gase', *Ann. Phys.*, **99** (1856): 315–22.

(14) Rudolf Clausius, 'Ueber die Art der Bewegung welche wir Wärme nennen', *Ann. Phys.*, **100** (1857): 353–80; (trans.) 'On the kind of motion which we call heat', *Phil. Mag.*, ser. 4, **14** (1857): 108–27; and Clausius, 'Ueber die mittlere Länge der Wege, welche bei der Molecularbewegung gasförmiger Körper von den einzelnen Molecülen zurückgelegt werden; nebst einigen anderen Bemerkungen über die mechanische Wärmetheorie', *Ann. Phys.*, **105** (1858): 239–58; (trans.) 'On the mean length of the paths described by the separate molecules of gaseous bodies on the occurrence of molecular motion: together with some other remarks upon the mechanical theory of heat', *Phil. Mag.*, ser. 4, **17** (1859): 81–91.

ϵ The slowness of diffusion is accounted for, and steps taken towards a complete theory.

ζ Theory of evaporation and maximum density of vapours.

η Theory of the change of partners among the molecules of compound bodies and the theory of electrolytic conduction under the smallest electromotive force, &c &c.(15)

θ Internal energy of molecules.

11 Maxwell 1860.(16) α Clausius assumed the velocities of the molecules equal. (This is no essential part of his theory but may be regarded as a trial assumption.) Maxwell showed that the velocities range through all values, being distributed according to the same law which prevails in the distribution of errors of observation and in general in all cases in which a general uniformity exists in the mass amidst apparent irregularity in individual cases.

β When there are two or more kinds of molecules acting on one another by impact the average *vis viva* ⌊kinetic energy⌋ of a molecule is the same whatever its mass. Hence follows the dynamical interpretation of

1 Gay Lussacs law of equivalent volumes of gases(17)

2 Dulong & Petits law of specific heats of gases.(18)

I claim N° 1 but am willing to distribute as regards N° 2.

γ Theory of Internal Friction of gases and calculation of the mean length of path of the molecules from Stokes theory(19) of Bailys pendulum expts.(20)

δ Development of Clausius theory of diffusion with errors and failures and a deduction of length of path from Grahams expts.(21)

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(15) R. Clausius, 'Ueber die Electricitätsleitung in Elektrolyten', *Ann. Phys.*, **101** (1857): 338–60; (trans.) 'On the conduction of electricity in electrolytes', *Phil. Mag.*, ser. 4, **15** (1858): 94–109. See Number 478.

(16) J. C. Maxwell, 'Illustrations of the dynamical theory of gases', *Phil. Mag.*, ser. 4, **19** (1860): 19–32; *ibid.*, **20** (1860): 21–37 (= *Scientific Papers*, **1**: 377–409).

(17) On 'Avogadro's hypothesis' and the law of equivalent volumes see Number 259 notes (13) and (14).

(18) A. T. Petit and P. L. Dulong, 'Sur quelques points importants de la théorie de la chaleur', *Ann. Chim. Phys.*, ser. 2, **10** (1819): 395–413, esp. 405. For Maxwell's discussion of 'the law of Dulong and Petit' that 'the specific heat is inversely as the specific gravity', see his lecture to the Chemical Society of 18 February 1875, 'On the dynamical evidence of the molecular constitution of bodies', *Nature*, **11** (1875): 357–9, 374–7, esp. 375 (= *Scientific Papers*, **2**: 432).

(19) G. G. Stokes, 'On the effect of the internal friction of fluids on the motion of pendulums', *Trans. Camb. Phil. Soc.*, **9**, part 2 (1851): [8]–[106] (= *Papers*, **3**: 1–136).

(20) Francis Baily, 'On the correction of a pendulum for the reduction to a vacuum: together with remarks on some anomalies observed in pendulum experiments', *Phil. Trans.*, **122** (1832): 399–492.

(21) Thomas Graham, 'A short account of experimental researches on the diffusion of gases

ε Theory of conduction of heat in gases, obvious, probably due to ever so many people, but comparison of conductivity of air and lead (erroneous) is my own.(22)

12 Clausius made objection N° 1 to an integration founded on his theory of uniform velocity of molecules.(23) (This is the first commitment of Clausius to such a theory.) As he was sure to be converted & I was lazy I said 0.(24) Objections N° 2 &c to theory of diffusion and conduction were well founded and in his paper on Conduction Clausius greatly advanced the methods of treatment,(25) and caused me to go through the subject still in the old style but improved. (Not published)(26)

13 Oscar Emil Meyer made extensive experiments on internal friction(27) and in 1865 made a more extensive theory of friction of gases,(28) still on Maxwells framework.

14 Maxwell in 1865 made experiments on viscosity of gases proving that it is independent of the pressure and proportional to absolute temperature, and that the ratios of the viscosity of air, carbonic acid and hydrogen agree with those given by Graham.(29)

In 1866 he published a revised theory of gases in which the molecules are

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through each other, and their separation by mechanical means', *Quarterley Journal of Science*, **20** (1829): 74–83. Graham's experimental data on gaseous diffusion was cited by Herapath, *Mathematical Physics*, **2**: 24–5.

(22) Maxwell had compared the conductivities of air and copper in 'Illustrations of the dynamical theory of gases. Part II', *Phil. Mag.*, **20** (1860): 32–3 (= *Scientific Papers*, **1**: 404–5), using an incorrect value for the conductivity of copper given by W. J. M. Rankine, *A Manual of the Steam Engine and other Prime Movers* (London/Glasgow, 1859): 259, which did not correctly reduce the value to English measure, and using a number which relates to one hour as the unit of time as though it was calculated for one second. He gave a corrected calculation, acknowledging Clausius' result – in his 'Ueber die Wärmeleitung gasförmiger Körper', *Ann. Phys.*, **115** (1862): 1–56, esp. 54 – that 'lead should conduct heat 1400 times better than air', in his paper 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88, on 88 (= *Scientific Papers*, **2**: 77).

(23) Rudolf Clausius, 'On the dynamical theory of gases', *Phil. Mag.*, ser. 4, **19** (1860): 434–6.

(24) See Number 207 §4 and note (13), and Number 284.

(25) Rudolf Clausius, 'Ueber die Wärmeleitung gasförmiger Körper'; (trans.) 'On the conduction of heat by gases', *Phil. Mag.*, ser. 4, **23** (1862): 417–35, 512–34.

(26) Number 207.

(27) O. E. Meyer, 'Ueber die Reibung der Flüssigkeiten', *Ann. Phys.*, **113** (1861): 55–86, 193–228, 383–425.

(28) O. E. Meyer, 'Ueber die innere Reibung der Gase', *Ann. Phys.*, **125** (1865): 177–209, 401–20, 564–99.

(29) J. Clerk Maxwell, 'On the viscosity or internal friction of air and other gases', *Phil. Trans.*, **156** (1866): 249–68 (= *Scientific Papers*, **2**: 1–25). See Number 252.

not regarded as hard elastic spheres but as acting on one another at various distances[(30)] so as to produce an effect similar to that of a repulsive force varying inversely as the square[(31)] of the distance. Mathematical methods altered and systematized.

15 Prof. J Loschmidt of Vienna 12 Oct 1865 communicated to the Imp. Acad of Vienna a speculation on the size of the molecules of air deduced from Clausius relation between the mean path, the diameter and the number in unit of volume combined with an estimate of the volume occupied by the molecules themselves from a consideration of the volumes of various substances in the liquid state.

Diameter of a molecule of air one millionth of a millimetre.[(32)]

16 Stoney in 1868 independently made an estimate of the same kind founded on the same data and leading to a similar result.[(33)]

17 W. Thomsons stereoscopic view of the same thing from several different directions.[(34)]

18 Loschmidt 1870 describes expts on diffusion of pairs of gases much more accurate than those of Graham.[(35)]

19 Gustav Hansemann of Eupen publishes 1871 'Die Atome und ihre Bewegungen, ein Versuch Zur Verallgeimeinerung der Krönig-Clausius'schen Theorie der Gase' pp. 191 and ranging from elastic spheres to the formation of the Tast-Geschmacks-und Geruchs-Organen and general theory of life intellect and intellectual progress.[(36)] But I only got this as a gift from the author a week ago and I have not looked it in the mouth yet.

I hope to see & hear you at Edinburgh.[(37)] The printers are rather slow

(30) Maxwell, 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88 (= *Scientific Papers*, **2**: 26–78). See Number 263.

(31) Read: fifth power.

(32) Joseph Loschmidt, 'Zur Grösse der Luftmolecüle', *Wiener Berichte*, **52**, Abtheilung II (1865): 395–413.

(33) George Johnstone Stoney, 'The internal motions of gases compared with the motions of waves of light', *Phil. Mag.*, ser. 4, **36** (1868): 132–41.

(34) William Thomson, 'The size of atoms', *Nature*, **1** (1870): 551–3 (= *Math. & Phys. Papers*, **5**: 289–96). See also Thomson's reference to establishing 'a definite limit for the sizes of atoms' in a letter to James Prescott Joule published in extract in *Proceedings of the Literary and Philosophical Society of Manchester*, **2** (1862): 176–8 (= *Electrostatics and Magnetism*: 317–18); and Maxwell's letter to Thomson of 17 December 1861 (Volume I: 702).

(35) Joseph Loschmidt, 'Experimental-Untersuchungen über die Diffusion von Gasen ohne poröse Scheidewände' *Wiener Berichte*, **61**, Abtheilung II (1870): 367–80; *ibid.*, **62**, Abtheilung II (1870): 468–78. See Number 470.

(36) Gustav Hansemann, *Die Atome und ihre Bewegungen. Ein Versuch zur Verallgemeinerung der Krönig-Clausius'schen Theorie der Gase* (Cologne/Leipzig, 1871).

(37) At the meeting of the British Association for the Advancement of Science; see note (1).

about Electricity and I have given up sending you proofs till you have served as an Ass.[38] Tait has been very useful about it. You should let the world know that the true source of mathematical methods applicable to physics is to be found in the Proceedings of the Edinburgh F.R.S.E.'s.

The volume- surface- and line-integrals of vectors and quaternions and their properties as in the course of being worked out by T′ is worth all that is going on in other seats of learning.[39]

Have you got anything about Sir B. Brodie[40] or do you leave that to the Chemists?[41] They have no right to it. Did you get my letters and proofs at the Athenæum? I want to know if I may publish Ts theorem as I have printed it.[42]

Yours $\dfrac{dp}{dt}$

(38) See note (1).

(39) Especially: P. G. Tait, 'On Green's and other allied theorems', *Trans. Roy. Soc. Edinb.*, **26** (1870): 69–84.

(40) Benjamin Collins Brodie, 'The calculus of chemical operations; being a method for the investigation, by means of symbols, of the laws of the distribution of weight in chemical change. Part I. On the construction of chemical symbols', *Phil. Trans.*, **156** (1866): 781–859.

(41) See Number 270.

(42) On 'Thomson's theorem' see the *Treatise*, **1**: 103–7 (§98) and Number 355 note (3).

LETTER TO CHARLES WILLIAM SIEMENS[1]

23 JUNE 1871

From a photocopy of the original in the University Library, Cambridge[2]

Glenlair
Dalbeattie
23 June 1871

My dear Sir

After our conversation in May I put a very short statement of your method of measuring temperatures by electric resistance[3] in the little book on heat which I am doing for Longman.[4] Of course there is no room there for matter belonging properly to electricity. But I wish to give a better account of your researches on resistance at high temperatures in my book on electricity in the chapter on the properties of metals as regards resistance.[5] I have read the abstract of your Bakerian Lecture[6] but I suppose the complete paper will not be out for some time, and I was unfortunately not in London when it was read.

It would be a great benefit to my book and to me if you could give me the values which you have obtained for the coefficients of your formula

$$R = \alpha T^{\frac{1}{2}} + \beta T + \gamma$$

in the case of platinum or any other metal.[7] The existence of the term in $T^{\frac{1}{2}}$ if it is established as a real fact and not merely as an expression of the slower rate of increase of resistance at high temperatures is of great importance. One result is that T cannot be negative or R would be impossible so that the position of absolute zero may be deduced by this method.

I think that if part of the resistance is proportional to the velocity of the molecules we may conclude that it depends on this velocity. But I find that in the theory of gases conduction of heat, and diffusion go on faster at high

(1) Wilhelm Siemens (brother of Werner Siemens), engineer, FRS 1862 (*DNB*).

(2) Courtesy of the English Electric Company: copy in ULC Add. MSS 7655, III, a. First published in *A Collection of Letters to Sir Charles William Siemens 1823–1883* (London, 1953): 23–4.

(3) As described in his Bakerian Lecture read to the Royal Society on 27 April 1871; see Charles William Siemens, 'On the increase of electrical resistance in conductors with rise of temperature, and its application to the measure of ordinary and furnace temperatures; also on a simple method of measuring electrical resistances', *Proc. Roy. Soc.*, **19** (1871): 443–5.

(4) See Maxwell's description of Siemens' work in his *Theory of Heat* (London, 1871): 52–4.

(5) See the *Treatise*, **1**: 416–17 (§360).

(6) In *Proc. Roy. Soc.*; see note (3).

(7) Maxwell gives values for platinum, copper and iron in the *Treatise* §360.

temperatures[8] and we know that the resistance of electrolytes and of most dielectrics diminishes as the temperature rises so that in these bodies I would expect the term in $T^{\frac{1}{2}}$ to appear in the formula for conductivity, not in that for resistance. The metals we know are different and their conductivity both for heat and for electricity diminishes as the temperature rises but I know of no satisfactory explanation of this.

Can you tell me if gas coke increases or diminishes in resistance as the temperature rises. Hockin told me that Selenium increases like the metals.[9] Are there any other non metallic bodies which do so?

Have you seen Tait's results about the electromotive force of thermoelectric circuits.[10] He finds the formula

$$E = A(T_1 - T_2)\{T_0 - \tfrac{1}{2}(T_1 + T_2)\}$$

is true for several combinations

T_1 = temperature of hot junction

T_2 = cold

T_0 = temperature at which the metals are neutral to one another

A a constant for the pair of metals.[11]

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(8) See Number 263.

(9) See a letter from Charles Hockin to Maxwell of 11 March 1870 (ULC Add. MSS 7655, II/34): 'For selenium in the crystalline form the lowest resistance I have obtained is 600 units per metre cube at 100 °C altering exactly one per cent for each degree (increasing with temperature)'. See the *Treatise*, **1**: 418 (§362) where Maxwell gives this value. Hockin also includes data for gutta percha: 'equivalent to 3.53×10^{12} units for the resistance of a piece of gutta percha a metre every way in dimensions... I reckon it to increase 20 times in resistance between 24° & 0 °C'; data which Maxwell records in the *Treatise*, **1**: 423 (§368). Hockin also reported that he had 'had an opportunity of verifying the fact that I had heard before that it is not until some hours after the gutta percha has taken its temperature that the resistance alters to its final value. This is very curious'. Maxwell acknowledges this observation in *Treatise* §368.

(10) P. G. Tait, 'On thermo-electricity', *Proc. Roy. Soc. Edinb.*, **7** (1870): 308–11.

(11) See the *Treatise*, **1**: 305–6 (§254). In a card of 14 June 1871 (ULC Add. MSS 7655, I, a/18) Tait addressed the following query to Maxwell: 'O dp/dt You are aware that I think I have proved that in a Thermo-electric circuit the electromotive force $E = A(t-t_1)\left(t_0 - \frac{t+t_1}{2}\right)$ t & t_1 are absolute temp^res of hot & cold junction, and t_0 the neutral point. Would you consider it a complete proof of the truth of this, if I drop thermometers altogether (the air ther^r being horribly unwieldy) and, plotting from *two separate* circuits with junctions at the same temperature find the curve representing the relation between E & E' to be also (in every case and however highly the heating is carried) a parabola, as it should be if the above equation be correct? An early answer requested. Y^rs $\mathfrak{T}'$.' Tait amplified his query in a note appended to Thomson's card of 17 June 1871 (Number 377 note (1)), and gave further clarification in a card of 20 June

Of course this formula tells us nothing about absolute zero because the sum of the coefficients of the temperatures in such factors are zero. I mention this because along with your result about resistance it helps to clear up the electric properties of metals, and perhaps you do not see the Edinburgh Proceedings.

Yours very truly
J. CLERK MAXWELL

I should be glad of any suggestions on pp 51–53 of the proof I send you.(12)

1871 (ULC Add. MSS 7655, I, a/20): 'O *dp/dt* I fear my hasty scribble on T's post-card was not very intelligible. Behold then the process:– $(\xi-\eta)^2 = 4(f-f')(f'\xi-f\eta)$ may be written $\sqrt{f^2-\xi}-\sqrt{f'^2-\eta} = \pm(f-f')$ ξ & η depend each on temp^{re}, *not* on one another. Hence if we write $\sqrt{f^2-\xi} = \pm(\tau \pm f)$ we have also $\sqrt{f'^2-\eta} = \pm(\tau \pm f')$. These are $\xi = \tau(2f-\tau)$ & $\eta = \tau(2f'-\tau)$. Hence, if ξ gives a parabola in terms of absolute temp^{re}, τ must be a linear function of abs. temp. N'est ce pas? Respondez-Vite! ?' Tait discussed the issue in his 'Relation between corresponding ordinates of two parabolas', *Proc. Roy. Soc. Edinb.*, **7** (1871): 499–500 (read 20 May 1871).

(12) See note (4).

379

LETTER TO JOHN WILLIAM STRUTT

8 AND 10 JULY 1871

From the original in private possession(1)

Glenlair
Dalbeattie
8 July 1871
10th July. Papers received(2)

Dear Strutt

I have received your letter dated 'Fourth of July Day'. I wish you all joy in the new state of things, and I hope that on the 19th the number of individuals existing will be so far diminished and the number of dualities increased.(3)

With regard to the laboratory, the word denotes a place to work at experiments and connotes a place full of articles not wanted at present and liable to noxious fumes. Hence, especially if you use nitric acid, a corner should be consecrated to it and a pipe or flue constructed to carry the fumes up the nearest chimney.

If you have more than one window and all on one wall, the space between

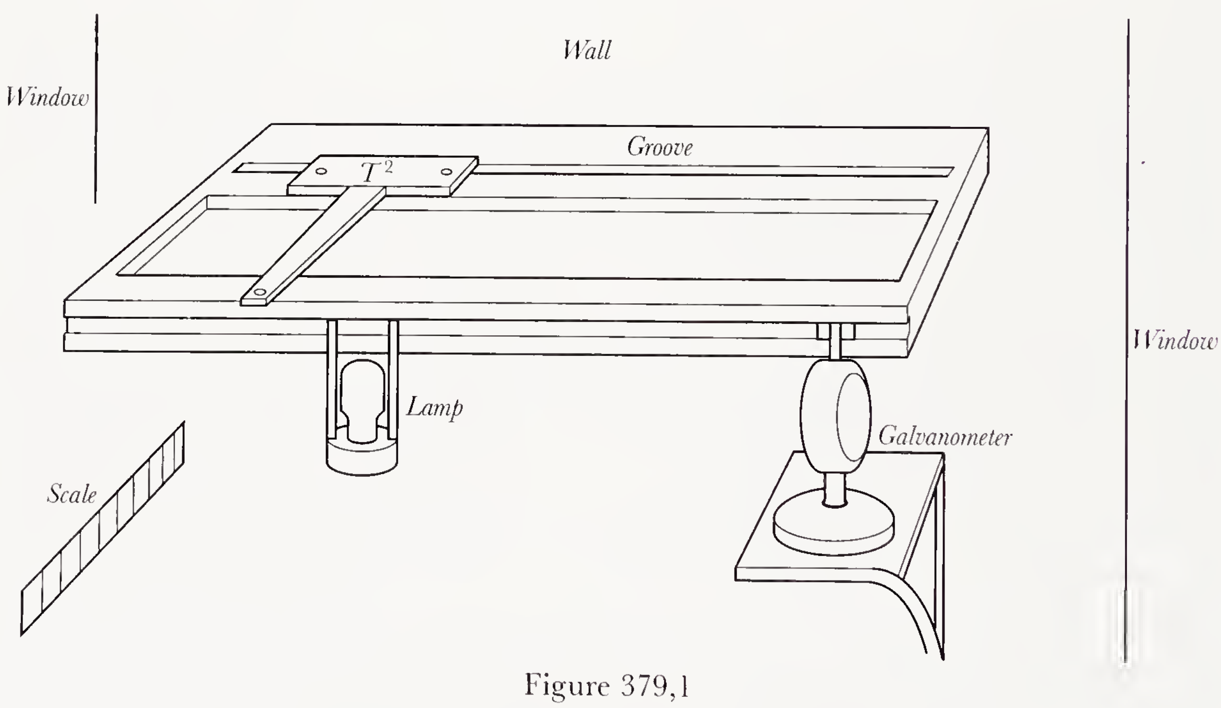


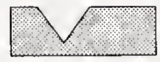
Figure 379,1

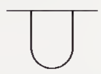
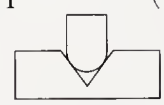
(1) Rayleigh Papers, Terling Place, Terling, Essex.

(2) See note (5).

(3) The occasion of Strutt's marriage to Evelyn Balfour; see R. J. Strutt, *John William Strutt, Third Baron Rayleigh* (London, 1924): 57.

two windows is probably the best to erect a reflecting galvanometer à la Thomson.(4)

About $7\frac{1}{2}$ feet (according to your height) from the ground fasten a frame of wood the length of the flat part of the wall and say 2 feet broad to hang things from. On one of the long sides is a groove of section . The other side is planed flat.

To support anything so as to be free only to move in one direction parallel to itself place it on a stand (a T square) having 3 legs with hemispherical (not conical) feet.  Two of these feet slide in the groove thus  on the principle of the round peg in the triangular hole. The other leg is a little shorter and slides on the flat board. This is Thomson's (and a good) plan for securing one degree of freedom. No carpenter will believe this till he is converted.

In front of the wooden frame hang a curtain rod and have 2 dark or black curtains which may be drawn to meet. They need not hang down more than 3 or 4 feet. Thus you have a darkish but easily accessible tent.

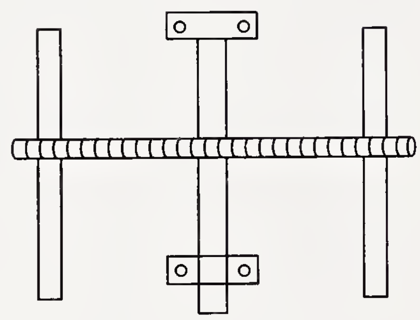


Figure 379,2

You hang the lamp a little above your head fix the galvanometer on a bracket at a height so that you can see it well and your scale at the proper place, which will be at a good height to read when standing in the tent. To adjust the height of the scale fasten it at right angles to a vertical bar which slides *easily* in two mortise holes. Place a vertical bar one on each side of the scale so as to bend the scale slightly concave. The friction of this arrangement will keep the scale from sliding down, and the absence of carpenters tight fittings will make it easily adjustable. A concave scale is a decided advantage especially (of course) one concentric with the mirror.

It is difficult to imagine exactly your condition. I have put down what happened to be in my head. For Optics you want shutters and I think shop shutters the best type (old style of shop do.) with a bar at the top of the window to push them under and a ledge below to rest them on. For a table I prefer a few tressels (masons horses) and a plank or two pretty thick with a selection of screw nails with self entering points, rather of the long and thin

(4) On Thomson's mirror galvanometer, see S. P. Thompson, *The Life of William Thomson Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 347–9, 355. See also Number 399 note (7), and G. Green and J. T. Lloyd, *Kelvin's Instruments and the Kelvin Museum* (Glasgow, 1970): 30–1.

kind. Have the frames of your lenses &c with wooden bottoms with holes to screw them on the planks.

I have not got your papers yet.(5) I do not consider that I am done with you yet as regards the Phys. Lab.(6) I am glad to see Clifford going in for a chair in London instead of poking in rotten trees for South American beetles and fevers.(7) I have drawn the lines of force and equipotential surface for a single and a double tangent galvanometer.(8)

Yours truly
J. Clerk Maxwell

APPENDIX: FROM A LETTER OF REFERENCE FOR WILLIAM KINGDON CLIFFORD

circa JULY 1871

From Clifford, *Lectures and Essays*(9)

The peculiarity of Mr. Clifford's researches, which in my opinion points him out as the right man for a chair of mathematical science, is that they tend not to the elaboration of abstruse theorems by ingenious calculations, but to the elucidation of scientific ideas by the concentration upon them of clear and steady thought. The pupils of such a teacher not only obtain clearer views of the subject taught, but are encouraged to cultivate in themselves that power of thought which is so liable to be neglected amidst the appliances of education.

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(5) Possibly: J. W. Strutt, 'On the light from the sky, its polarization and colour', *Phil. Mag.*, ser. 4, **41** (1871): 274–9; Strutt, 'On the scattering of light by small particles', *ibid.*: 447–54 (published in the April and June numbers of the *Philosophical Magazine*).

(6) See Number 358.

(7) W. K. Clifford had been a member of the Eclipse Expedition in 1870; and in 1871 was appointed Professor of Applied Mathematics at University College, London (Venn; *DNB*).

(8) See Figs. XVIII and XIX in the *Treatise*, **2**; and the *Treatise*, **2**: 307, 318–19 (§§702, 713).

(9) Quoted by Frederick Pollock in his 'Introduction' to *Lectures and Essays by the Late William Kingdon Clifford*, ed. Frederick Pollock and Leslie Stephen, 2 vols. (London, 1879), **1**: 14.

POSTCARD TO PETER GUTHRIE TAIT

13 JULY 1871

From the original in the University Library, Cambridge[1]

[Glenlair]

O T′[2] Total ignorance of H[3] and imperfect remembrance of T′ in Trans RSE[4] caused $\frac{dp}{dt}$ to suppose that H in his optical studies had made the statement in the form of a germ which T′ hatched.[5] I now perceive that T′ sat on his own egg, but as his cackle about it was very subdued compared with some other incubators, I was not aware of its origin when I spoke to B.A.[6] When I examined hastily H on Rays I expected to find far more than was there. But the good of H is not in what he has done but in the work (not near half done) which he makes other people do. But to understand him, you should look him up, and go through all kinds of sciences, then you go back to him and he tells you a wrinkle. I have done lines of force and = pot^{ls} of double tangent galv^{rs} in a diagram, showing the large uniform field. Is T still in London?[a]

(a) {Tait} Send back to T′.

(1) ULC Add. MSS 7655, I, b/31. Previously published in Knott, *Life of Tait*: 99–100.

(2) Written in reply to Tait's postcard of 9 July 1871 (ULC Add. MSS 7655, I, a/21): 'ȳ O *dp/dt* did'st though say, to the B.A. last year that "H discovered that to every brachistochrone problem there corresponds one of free motion"? Pray give me the reference, for I thought T′ had some little credit in the business, & your remark would annihilate it in very great measure.☞'.

(3) William Rowan Hamilton, 'Theory of systems of rays', *Transactions of the Royal Irish Academy*, **15** (1827): 69–174; *ibid.*, **16** part 1 (1830): 2–62; *ibid.*, **16** part 2 (1831): 93–125; *ibid.*, **17** (1832): 1–144.

(4) P. G. Tait, 'On the application of Hamilton's characteristic function to special cases of constraint', *Trans. Roy. Soc. Edinb.*, **24** (1865): 147–66.

(5) Tait and Maxwell had however discussed the subject in 1865: see Maxwell's letter to Tait of 17 June 1865 (Number 249, esp. note (2)).

(6) Published in the *Report of the Fortieth Meeting of the British Association for the Advancement of Science; held at Liverpool in September 1870* (London, 1871), part 2: 1–9, on 8 (= *Scientific Papers*, **2**: 228).

LETTER TO JAMES THOMSON

13 JULY 1871

From the original in the Library of The Queen's University, Belfast[1]

Glenlair
Dalbeattie
13 July 1871

Dear Sir

In a book on heat which I am in the midst of,[2] I have given a short account of D^r^ Andrews researches,[3] about which we had some conversation at Glasgow. I have since heard his lecture at the Royal Institution[4] and seen a little of the phenomena.

My account of the facts and theories is therefore derived partly from Andrews and partly from you and is considerably modified in the process of boiling down. I should like to hear from you if the proof I send gives a fair account of what Andrews and you have done and more particularly if you have told me anything in confidence that you have not yet published mark it out.[5] I hope however that you will publish some of what you told me[6] for the speculation seemed of the fertile kind.

Sir William's relations between capillarity, curvature and pressure of

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(1) James Thomson Papers, MS. 13/22a, The Queen's University of Belfast Library.

(2) J. Clerk Maxwell, *Theory of Heat* (London, 1871). In a letter to Maxwell of 20 June 1871 (ULC Add. MSS 7655, II/48), the publisher, William Longman, wrote: 'I am glad to hear from my friend Goodeve that your Treatise on Heat is, in his opinion, a perfect model of what such a book ought to be. He is so much pleased with it that he is most anxious that you should make up your mind to contribute further to the series by committing yourself to undertake the writing of the Preliminary Discourse, and he advised me to take the opportunity of your enjoying a pleasant comparative holiday in the country to disturb it by suggesting your devoting a part of it to this noble object. I should be very glad to find that you considered this a timely suggestion and were not greatly disgusted my disturbing your tranquillity.' On Thomas Minchin Goodeve see Volume I: 29, 30, 662n. Maxwell did not contribute further to the Longman series of 'Textbooks on Science'.

(3) Andrews' discovery of the continuity of the liquid and gaseous states of matter; and of the critical point of temperature above which these states could not be distinguished. See Thomas Andrews, 'On the continuity of the gaseous and liquid states of matter', *Phil. Trans.*, **159** (1869): 575–90.

(4) Thomas Andrews, On the gaseous and liquid states of matter', *Proceedings of the Royal Institution of Great Britain*, **6** (1872): 356–64; read 2 June 1871.

(5) For Thomson's response see his letter of 21 July 1871 (Number 382 note (2)).

(6) See James Thomson, 'Considerations on the abrupt changes at boiling or condensing in reference to the continuity of the fluid state of matter', *Proc. Roy. Soc.*, **20** (1871): 1–8.

vapour[7] seem to me to be connected with the retardation of boiling and of condensation.[8]

The next difficulty is What determines the true boiling temperature of the steam which is found to be so constant?[9]

If I do not hear from you in 10 days I shall suppose you are not at home and use my own discretion.

Remember me kindly to Mrs Thomson and believe me

Yours very truly
J. CLERK MAXWELL

Shall you be at Edinburgh at the B.A.?[10]

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(7) William Thomson, 'On the equilibrium of vapour at a curved surface of liquid', *Proc. Roy. Soc. Edinb.*, **7** (1870): 63–8.

(8) Compare Maxwell, *Theory of Heat*: 267–8.

(9) See Thomson's letter of 21 July 1871 (Number 382 note (2)); and Maxwell, *Theory of Heat*: 124–6.

(10) See Number 382 note (5).

LETTER TO JAMES THOMSON

24 JULY 1871

From the original in the Library of The Queen's University, Belfast[1]

Glenlair
Dalbeattie
24 July 1871

My dear Thomson

Many thanks for your letters corrections and papers.[2] Pray give my

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(1) James Thomson Papers, MS 13/22c, The Queen's University of Belfast Library.

(2) In reply to Maxwell's letter of 13 July 1871 (Number 381) Thomson wrote on 21 July 1871 (copy in the Thomson Papers, MS 13/22b): 'My dear Maxwell / I have been a little longer of writing to you than I hoped, having been away from home for two long days on a survey & much engaged at home on another day or two. / In the proof sheets which you sent me & which I now return, there are a few sentences which I think would require a little amendment as I think a few of them scarcely attribute to D^r Andrews the credit which is due to him for the revolution he has brought about in people's views in general on the relations of the gaseous & liquid states of matter. / I think in justice to him as well as to make the historical allusions in your book correct in reference to this matter it ought to be stated that it was D^r Andrews who showed that the liquid & gaseous states are continuous & that to him is due the true explanation of Caynard de la Tour's experiment. In your proofs I have inserted a few proposed alterations as suggestions for your consideration, but of course with the intention that you should do whatever you will think right & suitable yourself. / Then as to the passages relating to my suggestion (pages 122 & 123), I think you will find on consideration that what you have written will not hold good altogether. I think it is *not* possible for the substance at the pressure indicated by B to pass into the gaseous state & that if the liquid is in contact with its vapour at this pressure it is *really found that the liquid will not* begin to pass into the gaseous state. On the contrary I think under the circumstances stated it will all go down to the liquid state. / Again:– If there be any drops of liquid in the vessel when the pressure is that belonging to D & H, I think condensation will *not* now begin but on the contrary all the liquid will evaporate into the gaseous state. Then in respect to the question in your letter where you say "The next difficulty is:– *What determines the true boiling temperature of the steam which is found to be so constant?*" I will answer rather a corresponding question which is quite to the same effect but suits better to the diagram before us in which the vertical ordinates represent pressures, and the horizontal ones represent volumes:– *What determines the true boiling pressures of the steam which is found to be so constant for any given temperature?* I reply:– There is just one intermediate point of pressure between the pressure at F and the pressure at D, at which the liquid and its gas can be present together in contact with one another; and that is the boiling, or rather evaporating or condensing pressure, for the temperature to which the curve belongs. In using the name boiling pressure here we must understand not the very uncertain and variable pressure at which bubbles would form themselves in a continuous liquid when the boiling takes

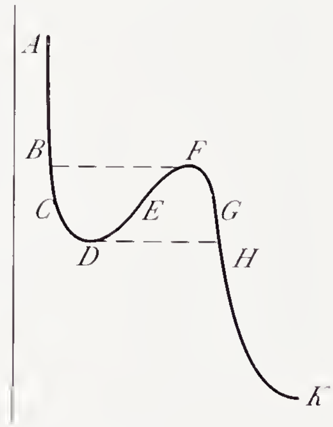

Figure 382,1

thanks to D^r^ Andrews for his lecture.(3) I have not had time to digest all you have sent. May I keep your copy of the R.S. paper(4) till I see you in Edinburgh?(5) You are certainly right that there is a definite pressure at which evaporation takes place at a given temperature (putting aside capillary phenomena). But I never could see what that pressure must be with reference to your continuous curve.(6) I think, however, that the following method determines it.

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place with bumping; but the absolutely definite pressure at which the liquid and its gas can be present together in contact with each other when either evaporation or condensation may be going on, or no change either way may be taking place. / I think there is no "*lingering*" in the liquid nor in the gaseous condition, when the two conditions of the same substance are present together, and the pressure is being altered while the temperature is fixed (or equally when the temperature is altered while pressure is fixed). / I enclose to you a manuscript copy of my paper recently sent to the Royal Society on this subject & which I presume will very soon be published in the *Proceedings*. I suppose it will be published before your book will come out; & I think if you w[d] wish to refer to it as a published paper permission may easily be obtained to do so or information as to the date or page at which it will appear in the Proceedings may easily be obtained by applying to the Secretaries of the Royal Society either Prof. Stokes Lensfield Cottage Cambridge or to Walter White Esq. Royal Society Burlington House London. You might write if you wish. / I sent to you by this post a report of D[r] Andrews's lecture at the Royal Institution at which I think you say you were present; & also I send you a copy of "Nature" containing an article abridged from an essay by me giving an account of D[r] Andrews's researches & conclusions. / M[rs] Thomson and I hope to be at the Brit. Assoc. and hope to see you there & with kind regards I am / Yours truly / James Thomson.'

Thomson's Royal Society paper (see note (4)) was received on 4 July 1871, and read on 16 November 1871. The experiments by Charles Cagniard-Latour referred to were reported in his 'Exposé de quelques résultats obtenus par l'action combinée de la chaleur et de la compression sur certains liquides tels que l'eau, l'alcool', *Ann. Chim. Phys.*, ser. 2, **21** (1822): 127–32, 178–82; and his 'Sur les effets qu'on obtient par l'application simultanée de la chaleur et de la compression à certains liquides', *ibid.*, **22** (1823): 410–15. These experiments had established the basis for Andrews' research; see his 'On the continuity of the gaseous and liquid states of matter', *Phil. Trans.*, **159** (1869): 575–90, esp. 575. This paper, the Bakerian Lecture for 1869, was described in an essay by James Thomson, 'The continuity of the gaseous and liquid states of matter', *Nature*, **2** (1870): 278–81.

(3) Thomas Andrews, 'On the gaseous and liquid states of matter', *Proceedings of the Royal Institution of Great Britain*, **6** (1872): 356–64.

(4) James Thomson, 'Considerations on the abrupt changes at boiling or condensing in reference to the continuity of the fluid state of matter', *Proc. Roy. Soc.*, **20** (1871): 1–8.

(5) At the meeting of the British Association where Thomson read a paper (summarising his ideas) 'Speculations on the continuity of the fluid state of matter, and on relations between the gaseous, the liquid, and the solid states', *Report of the Forty-first Meeting of the British Association for the Advancement of Science; held at Edinburgh in August 1871* (London, 1872), part 2: 30–3.

(6) See Thomson's figure (Figure 382,1) in his letter of 21 July 1871. Andrews had obtained curves that included a straight segment parallel to the axis of volume; see his 'On the continuity of the gaseous and liquid states of matter': 583. In his 'Considerations on the abrupt changes of

For given value of p the pressure and θ the absolute temperature let there be three values of v, the volume

$$\left.\begin{array}{l} v_1 \text{ the liquid volume} \\ v_2 \text{ the unstable volume} \\ v_3 \text{ the gaseous volume} \end{array}\right\} \text{ for unit of mass}$$

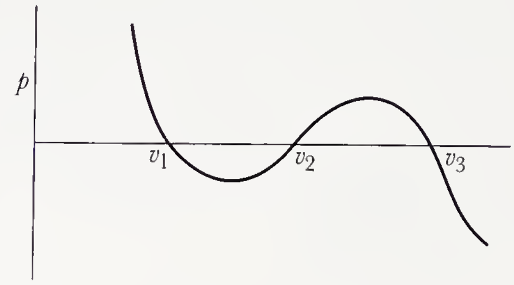

Figure 382,2

Let x be the mass of the liquid

y ———————— gas

$x+y = \text{const.}$

Let ϕ_1 ϕ_2 ϕ_3 be Rankines thermo-dynamic function[7] for the 3 states.

In general, if v and ϕ be made to vary

the work done by the fluid is $p\,dv$

the heat absorbed measured dynamically $\theta\,d\phi$

Energy developed or emitted $p\,dv-\theta\,d\phi$.

Hence if any variation takes place in the mixed mass of liquid and gas

$$\text{Energy emitted} = x(p_1\,dv_1-\theta_1\,d\phi_1)+y(p_3\,dv_3-\theta_3\,d\phi_3).$$

If the variation arises from a change of pressure dp while the temperature θ remains the same then p & θ are the same throughout the expression

$$\text{and energy} = dp\,.\,x\left(p\frac{dv_1}{dp}-\theta\frac{d\phi_1}{dp}\right)+y\left(p\frac{dv_3}{dp}-\theta\frac{d\phi_3}{dp}\right)$$

θ being constant in the differentiation.

Next let x vary then $dx+dy = 0$ and the energy emitted is

$$dp\,dx\left\{p\frac{dv_1}{dp}-\theta\frac{d\phi_1}{dp}-\left(p\frac{dv_3}{dp}-\theta\frac{d\phi_3}{dp}\right)\right\}.$$

Now x will tend to increase or condensation will occur when this quantity is positive and to diminish, indicating evaporation when it is negative. Hence

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boiling or condensing': 2, Thomson argued for 'theoretical continuity', as represented by the isothermal curve in Figure 382,1, which shows a maximum and minimum. Compare Maxwell's discussion in his *Theory of Heat* (London, 1871): 124–6, where he reproduces Thomson's figure: 'the isothermal curves for temperatures below the critical temperature are only apparently, and not really, discontinuous'.

(7) W. J. M. Rankine, 'On the geometrical representation of the expansive action of heat, and the theory of thermo-dynamic engines', *Phil. Trans.*, **144** (1854): 115–75, esp. 126. As Maxwell subsequently grasped (see Number 483, esp. note (18)), 'The entropy of Clausius... is only Rankine's Thermodynamic function'.

the equilibrium of vapour and liquid occurs when

$$p\frac{dv}{dp}-\theta\frac{d\phi}{dp} \tag{A}$$

is the same for the liquid and the gaseous states.

But by thermodynamics $\frac{d\phi}{dp}=-\frac{dv}{d\theta}$(8)

so that we may write the expression

$$p\frac{dv}{dp}_{(\theta\,\mathrm{const})}+\theta\frac{dv}{d\theta}_{(p\,\mathrm{const})}. \tag{B}$$

Here $-\frac{dv}{dp}_{(\theta\,\mathrm{const})}$ denotes the compressibility of unit of mass at constant temperature and $\frac{dv}{d\theta}_p$ the dilatability of unit of mass at constant pressure. The expression (B) must be the same for the liquid & the gas at the point of equilibrium.

Take the case of steam. Suppose it obeys Boyle & Charles(9) in the gaseous state

then $\frac{pv}{\theta}$ is constant and $p\frac{dv}{dp}+v\frac{dv}{d\theta}=0$.

In the liquid state therefore $p\frac{dv}{dp}+\theta\frac{dv}{d\theta}=0$.

In fact both these terms are very small and of opposite signs, but the second is the largest in most cases. Hence for the vapour $p\frac{dv}{dp}+\theta\frac{dv}{d\theta}$ is positive. We know that the compressibility is greater than that given by Boyle's law. Hence, à fortiori the dilatation at constant pressure must be greater than that given by Charles' law. In fact the dilatation of superheated vapour is much greater than that of permanent gases.

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(8) Maxwell's '*First Thermodynamic Relation*', $\frac{dv}{d\theta}_{(p\,\mathrm{const})}=-\frac{d\phi}{dp}_{(\theta\,\mathrm{const})}$; see his *Theory of Heat*: 165, 167n.

(9) See Maxwell's statement in his *Theory of Heat*: 29–30; the 'law of Charles... the volume of a gas under constant pressure expands when raised from the freezing to the boiling temperature by the same fraction of itself, whatever be the nature of the gas.' See Numbers 373 esp. note (5) and 375 note (2).

We may also write the expression (B)

$$\frac{dv}{dp}_{(\theta\,\mathrm{const})}\left(p-\theta\frac{dp}{d\theta}_{(v\,\mathrm{const})}\right)$$

or still more simply $\dfrac{dE}{dp}_{(\theta\,\mathrm{const})}$.

This condition determines the value of p at which the liquid & its vapour can coexist

so that the condition is

$$\frac{d}{dp}(E_1-E_3) = 0$$

or E_3-E_1 is a maximum for the given value of the temperature where E_3 and E_1 denote the intrinsic energy of unit of mass of the gas and the liquid at the same temperature and pressure.(10)

I have made use of writing to you in order to get this matter into shape as I am very busy at present about electricity so I was by no means clear about the matter when I began writing. I now see that E_1 the intrinsic energy is the best thing to look at. In a perfect gas it is constant for the same temperature whatever be the pressure, and is proportional to the temperature. In an incompressible liquid it is independent of the pressure and is proportional to the temperature and the specific heat.

In real liquids the part depending on the temperature is still commonly greater than that depending on pressure.

I think that this leads to a method of determining, from a complete knowledge of the continuous isothermal curve and the consecutive isothermal curves, the points of those curves which correspond to the state of equilibrium between the liquid and its vapour, and shows that I was wrong in supposing that there would be anything indefinite about it.

Have you stated anything about this in the paper you are going to print?

I remain

Yours very truly

J. Clerk Maxwell

(10) Maxwell states this conclusion in his *Theory of Heat*: 125. This establishes the condition determining the pressure at which gas and liquid can co-exist at equilibrium, the condition which determines the position of the line *CG* in Thomson's figure (Figure 382,1). Maxwell subsequently revised this conclusion, in his lecture 'On the dynamical evidence of the molecular constitution of bodies', *Journal of the Chemical Society*, **13** (1875): 493–508, esp. 496–7 (= *Scientific Papers*, **2**: 424–6).

NOTE FOR WILLIAM THOMSON AND PETER GUTHRIE TAIT[1]

LATE AUGUST 1871[2]

From the original in the University Library, Edinburgh[3]

[Glenlair]

Art 391 of $\dfrac{dp}{dt}$ boiled down[4]

$$\begin{array}{lll} \sum x'dm = lM & \sum y'dm = mM & \sum z'dm = nM \\ \sum x'^2dm = A & \sum y'^2dm = B & \sum z'^2dm = C \\ \sum y'z'dm = P & \sum z'x'dm = Q & \sum x'y'dm = R. \end{array}$$

Expansion of V in spherical harmonix

$$\sum U_0\, dm = 0$$

$$\sum U_1\, dm = M\frac{lx+my+nz}{r^3}$$

$$\sum U_2\, dm = \frac{x^2(2A-B-C)-y^2(2B-C-A)+z^2(2C-A-B)+6(Pyz+Qzx+Rxy)}{2r^5}$$

make the axis of x in the direction of $l\ m\ n$ then
Place the origin where

$$x = \frac{2A-B-C}{4M} \quad y = \frac{R}{M} \quad z = \frac{Q}{M}.$$

(1) This note was occasioned by a letter from Thomson to Tait of 21 August 1871 (Edinburgh University Library, Dc.2.76[16] folios 12–16), and is written on the *verso* of Thomson's letter which Tait forwarded to Maxwell. Thomson wrote: 'D^r T' I have at last got to the reprint of electrical papers, and in doing Math. Th. of Magnetism §§8–13 have been forced to bring to an issue the long impending question what is *the* magnetic axis of a magnet and what is its proper centre. Maxwell is (?) sure (?) to have it in his book [here Maxwell appends: "See Art 391 to which please refer. *dp/dt*"] so this is a race and you as Sec RSE are bound to see fair play.' Thomson then gives a statement (which is slightly revised in the text, dated September 1871, published in his *Electrostatics and Magnetism*: 367–70) of his theory of the magnetic axis of a magnet. After giving an account of this theory in his letter to Tait Thomson adds: 'I shall wait to see if the bait takes'. Maxwell responded by giving an abstract of his own version of the problem, intended, as Thomson had surmised, for the *Treatise*.

(2) See note (1).

(3) Edinburgh University Library, Dc.2.76[16] folio 16 *verso*.

(4) Compare the *Treatise*, **2**: 17–19 (§§391–2), where the argument has been slightly revised.

Then turn about the axis of x till P vanishes and the third term of V becomes

$$\frac{(y^2-z^2)\,(3B-C)}{4r^5}.$$

The spherical surface integral of the square of the third term is a minimum when the centre is the centre of the magnet thus found.

The axis of x drawn *through this centre* is The axis of the magnet and those of y & z as drawn above are the secondary axes.

$A\ B\ C$ correspond to moments of inertia
$P\ Q\ R$ to products of inertia
but $A\ B\ C$ are not necessarily $+^{\text{ve}}$.

The total mass $= 0$ and the centre of gravity is at ∞ in the direction of the axis.

O T′. Send back my proofs and let the work proceed.(5)

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(5) Proof sheets of §§145–150 of the *Treatise* (on spherical harmonics) are postmarked (returned to Oxford) 1 September 1871 (ULC Add. MSS 7655, IV/2).

POSTCARD TO PETER GUTHRIE TAIT

5 SEPTEMBER 1871

From the original in the University Library, Cambridge[1]

[Glenlair]

O T′. A complete set of the figs I to XII[2] given to T′ at N. P.[3] Class room. Only one set in my possession at present. Spherical Harmonics first written in 1867[4] but worked up from T & T′ when that work appeared[5] and since. Have you a short and good way to find $\iint (\vartheta_i^{(s)})^2\, dS$?[6] If so make it known at 1ce[7] that I may bag it lawfully as T′ 4nion path to harmonic analysis.

$$\frac{\text{K.N.}}{\text{T}\cdot} = \frac{\text{P.}^{\text{epper}}\text{D.}^{\text{not}}}{\text{no means}}$$

$\dfrac{dp}{dt}$

(1) ULC Add. MSS 7655, I, b/32. Previously published (in part) in Knott, *Life of Tait*: 100.

(2) For the *Treatise*.

(3) Natural Philosophy.

(4) See Maxwell's letter to Tait of 11 December 1867 (Number 277).

(5) Thomson and Tait, *Natural Philosophy*: 140–60.

(6) The quantity $\vartheta_i^{(s)}$ is a function of the spherical coordinate, θ, where i and s denote the poles of the harmonic function, and dS is the element of the surface. See Thomson and Tait, *Natural Philosophy*: 149; and for Maxwell's solution see Number 387.

(7) Maxwell's further work on spherical harmonics for the *Treatise* in the autumn of 1871 (see Numbers 387 and 388) may have been encouraged by papers read by W. K. Clifford and William Thomson at the British Association meeting in Edinburgh in August 1871, to which he makes reference in the *Treatise*, **1**: 171 (§138). See W. K. Clifford, 'On a canonical form of spherical harmonics', and W. Thomson, 'On the general canonical form of a spherical harmonic of the n^{th} order', in the *Report of the Forty-first Meeting of the British Association* (London, 1872), part 2: 10, 25–6.

NOTE FOR FLEEMING JENKIN ON THE THEORY OF ELECTRIC CIRCUITS

circa SEPTEMBER 1871[1]

From the original in the University Library, Cambridge[2]

[ELECTRICAL CIRCUITS AND LINES OF FORCE]

The mutual mechanical action of electric currents was discovered by Ampère,[3] who determined the mathematical form of the law of this action by an investigation which is one of the most brilliant illustrations of scientific method.[4] We shall find it, however more convenient to employ the method of Faraday, in which the action is defined with reference to the lines of magnetic force.

Lines of Force[5]

Every electric current is a closed circuit. The action of this current on magnets or other currents in its neighbourhood is identical with that of a magnetic shell, uniformly magnetized normal to its surface and bounded by the circuit formed by the current.

The lines of magnetic force due to the current are also closed curves each of which passes through the electric circuit and returns outside the circuit thus.[6]

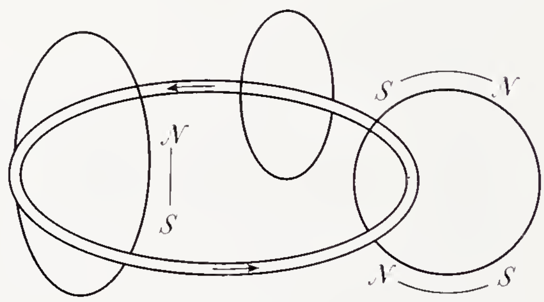

Figure 385,1

When a considerable length of the circuit is straight, the lines of magnetic force near it are circles and the amount of the force is

$$2\frac{C}{r}$$

where C is the current and r the distance from it.

(1) This date is conjectural. The text may well relate to the annotation on the proof of Jenkin's *Electricity and Magnetism* (London, 1873) published as an appendix *infra*: see note (10). The argument itself suggests a date after June 1871: see note (8).

(2) ULC Add. MSS 7655, II/239 (from the Jenkin papers).

(3) A. M. Ampère, 'Mémoire sur la théorie mathématique des phénomènes électro-dynamiques uniquement déduite de l'éxpérience', *Mémoires de l'Académie Royale des Sciences*, **6** (1827): 175–388.

(4) Compare Maxwell's remarks in the *Treatise*, **2**: 162 (§528), where he describes Ampère as 'the "Newton of electricity"'.

(5) Compare the *Treatise*, **2**: 140–1 (§§493–5).

(6) On Figure 385,1 see Number 370 esp. note (10).

The forms of the lines of magnetic force are more complicated in other cases but they can be drawn when the current is circular.

Action on a moveable portion of a conductor carrying a current[7]

Any moveable portion of a conductor carrying an electric current (which may be the same current as the acting current) is acted on by an electromagnetic force which tends to move it in a direction depending on two things, its own direction and that of the magnetic force.

Draw from any point two straight lines, the first representing in magnitude and direction the strength of the current in the moveable wire, while the second represents in direction and magnitude the magnetic force at the place. The electromagnetic force on unit length of the wire is perpendicular to the plane of these two lines and is represented in magnitude by the parallelogram which they contain.

To determine which way this force acts place a corkscrew perpendicular to this parallelogram and turn it from the direction of the electric current to the direction in which the end marked *N* of a compass needle would point. The screw will then move in the direction of the force.[8]

From this it is easy to see that if a circuit has a long straight portion like a telegraph wire it will attract a conductor parallel to itself if the current in the conductor is in the same direction as in the telegraph wire and will repel it if the two currents are in opposite directions.

All these actions are summed up in Faraday's law that any moveable part of the second circuit tends to move so as to increase the number of the lines of magnetic force due to the first circuit which are embraced by the second.[9]

(a)

(a) {Jenkin} J. Clerk Maxwell.

(7) Compare the *Treatise*, **2**: 135–6 (§489).

(8) See Numbers 370 and 371 for Maxwell's adoption of the analogy of a corkscrew: 'A common corkscrew may be used as a material symbol of the same relation' (*Treatise*, **1**: 24n (§23)). Compare Jenkin's discussion of the corkscrew rule in his *Electricity and Magnetism*: 148.

(9) See the *Treatise*, **2**: 175 (§541).

APPENDIX: ANNOTATION ON PROOF OF JENKIN'S *ELECTRICITY AND MAGNETISM*(10)

circa SEPTEMBER 1871(11)

From the original in the University Library, Cambridge(12)

GENERAL ANNOTATION

Electrification a condition of bodies to be described. This may be measured. The quantity on which it depends is called Electricity. When electrified conductors are connected by a wire a transfer of electrification takes place. That which determines the direction of the transfer is the relative Potential of the two conductors. Define equal, higher & lower as applied to Potential. You may afterwards define the exact *measurement* of Potential by the work done on the test charge, but get the notion of Potential in before § 7(13) or you are in danger of beginning with loose and even erroneous expressions. It is quite as easy for a beginner to understand 'potential' as to understand 'electrical state' which is vague or 'electrification' which is wrong.(14)

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(10) Fleeming Jenkin's text *Electricity and Magnetism* was published by Longmans in 1873. This first set of proofs (pp. 1–48) is dated 22 August 1871 by the printer. Jenkin wrote to Maxwell on 28 October 1871 (ULC Add. MSS 7655, II/51) acknowledging receipt of the annotated proofs, sending a second set (pp. 49–64) for comment, and returning the first set for reference. See also Number 450.

(11) See note (10).

(12) ULC Add. MSS 7655, V, c/41.

(13) In § 7 of his *Electricity and Magnetism*: 7–8, Jenkin discusses the production of positive and negative electricity from rubbed glass and resin, respectively.

(14) Maxwell does however use the term 'electrification' in the *Treatise*, **1**: 30–2 (§§ 27–9).

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LETTER TO PETER GUTHRIE TAIT

19 OCTOBER 1871

From the original in the University Library, Cambridge[1]

Ardhallow
Dunoon
19/10/71

O T′

I send you prooves.[2] As I have not engraved copies of all the figures I enclose some of the originals.

T (W & J)[3] & H^2[4] are becalmed in the Hebrides. I got a post card from Gair lock Ross shire which enticed me to Greenock at 4 in the morning on Friday. I enjoyed my own society there till 4 in the afternoon of Saturday. I thought I saw L. R.[5] creeping up by Gourock last night (Monday).

Can you tell me anything about John Hunter M.A. Prof^r &c.[6]

I only know him as the man who charges charcoal with bad smells.[7] Would he be a good demonstrator at Cambridge? I have no doubt that a man who could occlude a fishy fume in a burnt stick could also floor a demon which I suppose to be the essential part of the office. But I doubt if our laboratory would be sufficiently absorbent to occlude so volatile a spirit, for he seems to have improved the shining hour in every laboratory under heaven.

I have written to Cambridge to know my powers of appointing an exorcist and what form of ordination is required.

Hitherto I consider that we would be the better of a man who has seen many professors and known their manners rather than one who represents simply the continuity of University life.

Yrs

$\frac{dp}{dt}$

---

(1) ULC Add. MSS 7655, I, b/33.

(2) Proof sheets of §§150–56 of the *Treatise* are stamped 28 September 1871 by the Clarendon Press (ULC Add. MSS 7655, IV/2)

(3) William and James Thomson.

(4) Helmholtz.

(5) William Thomson's yacht the *Lalla Rookh*.

(6) John Hunter, formerly assistant to Thomas Andrews at Queen's College, Belfast, had been professor of mathematics and natural philosophy at King's College, Windsor, Nova Scotia, 1870–71, but had resigned due to ill-health and returned to Britain in autumn 1871. See *Proc. Roy. Soc. Edinb.*, **8** (1875): 322–4.

(7) John Hunter, 'On the absorption of vapours by charcoal', *Journal of the Chemical Society*, ser. 2, **3** (1865): 285–90; *ibid.*, **5** (1867): 160–4; *ibid.*, **6** (1868): 186–92; *ibid.*, **8** (1870): 73–4.

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POSTCARD TO PETER GUTHRIE TAIT

23 OCTOBER 1871

From the original in the University Library, Cambridge[1]

[London]

O T′! R.U. AT 'OME? $\iint$ Spharc$^2 dS$ was done in the most general form in 1867.[2] I have now bagged ξ & η from T & T′[3] and done the numerical value of $\iint (Y_i^{(s)})^2\, dS$[4] in 4 lines, thus verifying T + T′'s value of $\iint (\vartheta_i^{(s)})^2\, dS$.[5]

Your plan[6] seems indep^t of T + T′ or of me. Publish!

I am busy supplying the physical necessities of scientific life. Address 11 Scroope Terrace, Cambridge. Prooves have got as far as grooves, corrugated plates, gratings and guard-rings.[7] If you have time for criticism they shall be sent.

$$\iint (Y_i^{(s)})^2\, dS = \frac{8\pi a^2}{2i+1}\frac{\lfloor i+s \lfloor i-s}{2^{2s}\lfloor i \lfloor i}\;^{(8)}$$

except when $s = 0$ when $\iint (Q_i)^2 dS = \dfrac{4\pi a^2}{2i+1}.$ [9]

Hence $\displaystyle\int_{-1}^{+1} (\vartheta_i^{(s)})^2\, d\mu = \frac{2}{2i+1}\frac{2^{2s}\lfloor i-s \lfloor s \lfloor s}{\lfloor i+s}$ without exception.[10]

Yrs $\dfrac{dp}{dt}$

(1) ULC Add. MSS 7655, I, b/34. Previously published (in part) in Knott, *Life of Tait*: 100, and in facsimile in C. W. F. Everitt, *James Clerk Maxwell* (New York, 1975): 26.

(2) See Number 384.

(3) In their discussion of spherical harmonics (*Natural Philosophy*: 148) Thomson and Tait had used imaginary coordinates ξ and η, where $\xi = x + \sqrt{-1}\,y$ and $\eta = x - \sqrt{-1}\,y$. See the *Treatise*, **1**: 164 (§132).

(4) $Y_i^{(s)}$ is the surface harmonic, i and s denoting its axes, dS the element of surface.

(5) Thomson and Tait, *Natural Philosophy*: 149; and see Number 384.

(6) P. G. Tait, 'Note on spherical harmonics', *Proc. Roy. Soc. Edinb.*, **7** (1871): 589–96. See Number 388.

(7) See the *Treatise*, **1**: 240–53 (§§196–206).

(8) The value of the surface integral (taken over the surface S of a sphere of radius a) of the square of a surface harmonic $Y_i^{(s)}$ of the symmetrical system, where $i-s$ poles are placed at one point and the remaining s poles at equal distance round one half of the equator. See the *Treatise*, **1**: 163, 174 (§§132, 141). Maxwell uses the symbol $\lfloor\;$ for factorial: see Numbers 389 and 390.

(9) When all the poles are concentrated at the pole of the sphere, the harmonic becomes a zonal harmonic (Q_i) for which $s = 0$. See the *Treatise*, **1**: 163 (§132).

(10) See the *Treatise*, **1**: 173–4 (§141), where μ denotes $\cos\theta$, where θ is the spherical coordinate.

LETTER TO PETER GUTHRIE TAIT

2 NOVEMBER 1871

From the original in the University Library, Cambridge[1]

11 Scroope Terrace
Cambridge
2 Nov 1871

O T′

Your notes have ravished me. An interest in $\Sigma\phi\alpha\rho\xi$ being revived[2] this is exactly what is wanted for a quantitative or computative discussion of the symmetrical system considered as depending only on certain symbols i and s.[3]

It seems to have little or nothing to do with your 4 nionic reduction which is of course indep^t of a selected axis.

My method is also indep^t of a selected axis but does not seem equivalent to your 4 nion reduction which goes by steps.[4]

Murphy is not at all bad in his way and affords a very good specimen of a Caius man working a calculation.[5]

How is it that $\Sigma\phi\alpha\rho\xi$ can be worked only at Caius? See Murphy Green[6] O'Brien[7] Pratt.[8] When I examined here the only men who could do figure of the earth were mild Caius men. All the rest were Prattists if anything.

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(1) ULC Add. MSS 7655, I, b/35. Previously published (in part) in Knott, *Life of Tait*: 100–1.

(2) See Tait's paper, 'Note on spherical harmonics', *Proc. Roy. Soc. Edinb.*, **7** (1871): 589–96; here $\Sigma\phi\alpha\rho\xi$ = 'Spharx'.

(3) See Number 387 esp. note (8).

(4) See Tait's reply of 10 November 1871 (Number 389 note (14)).

(5) Robert Murphy, Caius 1825, third wrangler 1829, Fellow 1829 (Venn). For Murphy's discussion of Laplace coefficients see his *Elementary Principles of the Theories of Electricity, Heat and Molecular Actions. Part I. On Electricity* (Cambridge, 1833): 3–24 ('Preliminary propositions').

(6) George Green, Caius 1832, fourth wrangler 1837, Fellow 1839 (Venn). Green made extensive use of the Laplace coefficients; see especially his 'Mathematical investigations concerning the laws of the equilibrium of fluids analogous to the electric fluid, with other similar researches', *Trans. Camb. Phil. Soc.*, 5 (1833): 1–63 (= *The Mathematical Papers of the Late George Green*, ed. N. M. Ferrers (London, 1871): 119–83), on which see Number 310 esp. note (3).

(7) Matthew O'Brien, Caius 1834, third wrangler 1838, Fellow 1840 (Venn). See his text *Mathematical Tracts. Part I. On Laplace's Coefficients, the Figure of the Earth, the Motion of a Rigid Body about its Center of Gravity, and Precession and Nutation* (Cambridge, 1840).

(8) John Henry Pratt, Caius 1829, third wrangler 1833, Fellow 1836 (Venn). See his text *The Mathematical Tracts. Part I. On Laplace's Coefficients, the Figure of the Earth, the Motion of a Rigid Body Architecture, but chiefly to the Theory of Universal Gravitation* (Cambridge, $_2$1845): 159–75.

I think a very little mortar would make a desirable edifice out of your article.

In selecting the absolute value of the constant coefft of a harmonic we may go on one of several principles. Mine is to differentiate $\frac{1}{r}$ i times with respect to i directions which may be coincident or not,(9) and then divide by $\underline{|i}$ and multiply by r^{i+1}.(10)

If they coincide we get Q_i.(11)

If $i-s$(12) coincide with the axis of symmetry and s are at intervals of $\frac{\pi}{s}$ round the equator we get a symmetrical system containing $s\phi$.

$$\text{This is } Y_i^{(s)} = 2^x \frac{\underline{|i-s}}{2^s\underline{|i}} \sin^s\theta \frac{d^sQ_i}{d\mu^s}\cos(s\phi+\alpha)$$

except when $s = 0$ when $2^x = 1$.(13)

$$\text{Your } \Theta_i^{(s)}\text{(14) is } \sin^s\theta\frac{d^sQ_i}{d\mu}\cos(s\phi+\alpha).$$

That of T+T′(15) is yours multiplied by $\dfrac{2^i\underline{|i}\,\underline{|i-s}}{\underline{|2i}}$

and has the coefft of μ^{i-s} equal to 1.

T+T′'s $\dfrac{\vartheta_i^{(s)}}{\sin^s\theta}$ is $= 1$ when $\mu = 1$.(16)

$$\vartheta_i^{(s)} = \text{your } \Theta_i^s \times 2^s\frac{\underline{|i-s}\,\underline{|s}}{\underline{|i+s}}.^{(17)}$$

The great thing is to avoid confusion. I rather think your value is the best to impress on the mind.

It lies between it and $\vartheta_i^{(s)}$ which has a certain claim.

The diggings in Σφαρξ are very rich and a judicious man might get up a capital book for Cambridge, in which the wranglers would lade themselves with thick clay till they become blind to the concrete.

(9) See Number 281 and the *Treatise*, **1**: 162 (§131).

(10) See the *Treatise*, **1**: 160 (§130). Maxwell uses the symbol $\underline{|}$ for factorial: see Numbers 389 and 390.

(11) See Number 387 note (9).

(12) See Number 387 note (8).

(13) See the *Treatise*, **1**: 164 (§132); for the spherical coordinates θ and ϕ and for μ and $Y_i^{(s)}$ see Number 387 notes (4) and (10).

(14) In his 'Note on spherical harmonics'.

(15) Thomson and Tait, *Natural Philosophy*: 149.

(16) For $\vartheta_i^{(s)}$ see Number 384 note (6).

(17) See Number 387.

But try and do the 4^{nions}. The unbelievers are rampant. They say 'show me something done by 4^{nions} which has not been done by old plans. At the best it must rank with abbreviated notation'.

You should reply to this, no doubt you will. But the virtue of the 4^{nions} lies not so much as yet in solving hard questions as in enabling us to see the meaning of the question and of its solution, instead of setting up the question in $x\,y\,z$, sending it to the analytical engine and when the solution is sent home translating it back from $x\,y\,z$ so that it may appear as A, B, C to the vulgar.

There appears to be a desire for thermodynamics in these regions more than I expected, but there are some very good men to be found.

You will observe a tendency to bosch in this letter which pray xqs as I have been reading an ill assorted lot of books till I cannot correct prooves.

Yours truly

$\dfrac{dp}{dt}$

In (6)[18] divide by $(1-\mu^2)^s$ and then differentiate the equ$^{\text{n}}$ after (13) with respect to μ and you get a result the same as if you put $s+1$ for s and $\theta^{(s+1)}$ for $\dfrac{d\theta^{(s)}}{d\mu}$. Hence if $\theta^{(s+1)} = C\dfrac{d\theta^{(s)}}{d\mu}$, this is a solution. But $\theta^{(0)} = Q_i$. Q.E.D.

Eq$^{\text{n}}$ to begin with.

$$\left\{i(i+1)-s(s+1)\right\}\theta_i^{(s)}-2(s+1)\,\mu\frac{d\theta_i^{(s)}}{d\mu}+(1-\mu^2)\frac{d^2\theta_i^{(s)}}{d\mu^2} = 0.$$

(18) Of Tait's paper 'Note on spherical harmonics': 593.

POSTCARD TO PETER GUTHRIE TAIT

7 NOVEMBER 1871

From the original in the University Library, Cambridge[1]

[Cambridge]

Laplace is a very clever fellow. Liv III, chap. II.[2]

O T′ Weber has reason.[3] His force has a potential[4] which involves the square of the relative velocity.[5] Hence in any cyclic operation no work is spent or gained. So Conservation is conserved. But Helmholtz has shown (Crelle = ns of electric motion)[6] that it is possible (by Webers Law) to produce in a material particle carrying electricity an infinite velocity in a finite space and finite time and it appears from the formula that forthwith it is hurled with this ∞ velocity into a region where by the formula the velocity is $\sqrt{-1}$.[7]

---

(1) ULC Add. MSS 7655, I, b/36. Previously published in facsimile in P. M. Harman, *Energy, Force, and Matter* (Cambridge, 1982): 96. See Plate XV.

(2) Laplace's discussion of the Laplace coefficients in Book III chapter 2 ('Développement en série, des attractions des sphéroides quelconques') of his *Traité de Mécanique Céleste*, 5 vols. (Paris, An VII [1799]–1825), **2**: 25–49 (= *Oeuvres Complètes de Laplace*, 14 vols. (Paris, 1878–1912), **2**: 24–52).

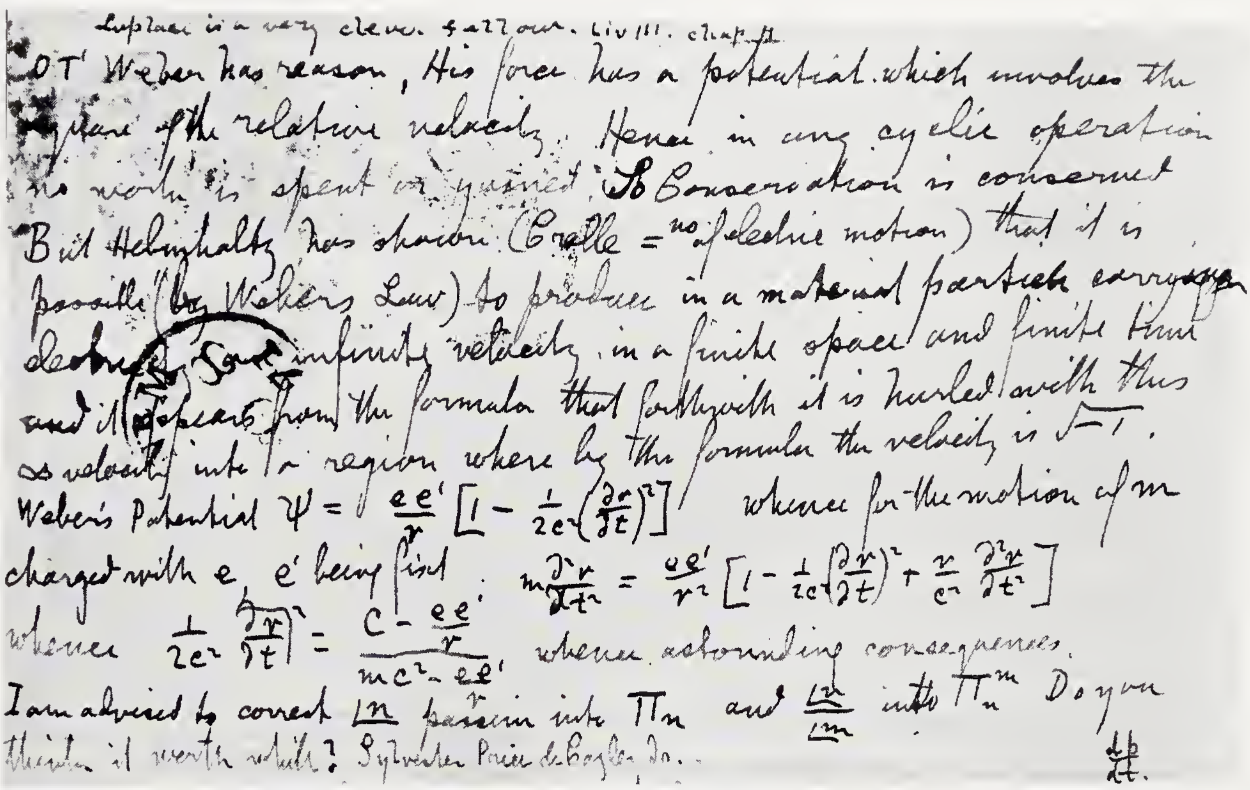
(3) Maxwell now recognises that Helmholtz's argument in *Über die Erhaltung der Kraft*, which he had himself supported (see Number 284 note (13)), that Weber's electrodynamic force law was inconsistent with energy conservation, is invalid. Compare his discussion in the *Treatise*, **2**: 429–30 (§§852–3) where he explains Weber's argument.

(4) As Weber had established in 1848. See Wilhelm Weber, 'Elektrodynamische Maassbestimmungen', *Ann. Phys.*, **73** (1848): 193–240, esp. 229 (= *Wilhelm Weber's Werke*, 6 vols. (Berlin, 1892–4), **3**: 215–54).

(5) Wilhelm Weber, 'Ueber einen einfachen Ausspruch des allgemeinen Grundgesetzes der elektrischen Wirkung', *Ann. Phys.*, **136** (1869): 485–9 (= *Werke*, **4**: 243–6). Weber obtains the formula $\frac{ee'}{r}\left[1-\frac{1}{c^2}\left(\frac{dr}{dt}\right)^2\right]$ for the potential between two moving particles of charge e, e' at a distance r from each other, where c is a limiting velocity of the electric masses.

(6) Hermann Helmholtz, 'Ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper', *Journal fur die reine und angewandte Mathematik*, **72** (1870): 57–128, esp. 63–4.

(7) Helmholtz states: 'Aber es widerspricht in so fern, als zwei elektrische Theilchen, die sich nach diesem Gesetzte bewegen und mit endlicher Geschwindigkeit beginnen, in endlicher Entfernung von einander unendliche lebendige Kraft erreichen und also eine unendlich grosse Arbeit leisten können' ('Ueber die Bewegungsgleichungen der Elektricität': 63). Maxwell states this conclusion in the *Treatise*, **2**: 430 (§854).



Laplace is a very clever fellow. Liv III. chap. ...

O T' Weber has reason, His force has a potential which involves the square of the relative velocity. Hence in any cyclic operation no work is spent or gained. So Conservation is conserved.

But Helmholtz has shown (Crelle = no of electric motion) that it is possible (by Weber's Law) to produce in a material particle carrying electricity infinite velocity in a finite space and finite time and it appears from the formula that forthwith it is hurled with this ∞ velocity into a region where by the formula the velocity is $\sqrt{-1}$.

Weber's Potential $\Psi = \frac{ee'}{r}\left[1 - \frac{1}{2c^2}\left(\frac{\partial r}{\partial t}\right)^2\right]$ whence for the motion of m charged with e, e' being fixt $m\frac{\partial^2 r}{\partial t^2} = \frac{ee'}{r^2}\left[1 - \frac{1}{2c^2}\left(\frac{\partial r}{\partial t}\right)^2 + \frac{r}{c^2}\frac{\partial^2 r}{\partial t^2}\right]$

whence $\frac{1}{2c^2}\left(\frac{\partial r}{\partial t}\right)^2 = \frac{C - \frac{ee'}{r}}{mc^2 - \frac{ee'}{r}}$, whence astounding consequences.

I am advised to convert $\underline{|n}$ passim into Πn and $\frac{\underline{|n}}{\underline{|m}}$ into Π_n^m Do you think it worth while? Sylvester Price de Bogley &c.

$\frac{dp}{dt}$.

Plate XV. Maxwell's postcard to Peter Guthrie Tait of 7 November 1871 (Number 389).

Weber's Potential $\psi = \frac{ee'}{r}\left[1-\frac{1}{2c^2}\left(\frac{\partial r}{\partial t}\right)^2\right]^{(8)}$ whence for the motion of m charged with e, e' being fixt

$$m\frac{\partial^2 r}{\partial t^2} = \frac{ee'}{r^2}\left[1-\frac{1}{2c^2}\left(\frac{\partial r}{\partial t}\right)^2+\frac{r}{c^2}\frac{\partial^2 r}{\partial t^2}\right]^{(9)}$$

whence $\left.\frac{1}{2c^2}\frac{\partial r}{\partial t}\right|^2 = \frac{C-\frac{ee'}{r}^{(10)}}{mc^2-\frac{ee'}{r}}$ whence astounding consequences.[11]

I am advised to correct $\underline{|n}$ passim into Π_n and $\frac{\underline{|n}}{\underline{|m}}$ into Π_n^m.[12] Do you think it worth while? Sylvester Price[13] & Cayley do.[14]

$\frac{dp}{dt}$

(8) But compare Weber's formula (see note (5)), as correctly cited by Helmholtz, 'Ueber die Bewegungsgleichungen der Elektricität': 64n.

(9) In his 'Ueber die Bewegungsgleichungen der Elektricität': 63 Helmholtz gives Weber's electrodynamic force law in the form $m\frac{d^2r}{dt^2} = \frac{ee'}{r^2}\left[1-\frac{1}{c^2}\left(\frac{dr}{dt}\right)^2+\frac{2r}{c^2}\frac{d^2r}{dt^2}\right]$, where m is the mass of the electric particle e, again correctly following Weber's own statement of the law in his paper 'Elektrodynamische Maassbestimmungen, insbesondere Widerstandsmessungen', *Abhandlungen der Königlichen Sächsischen Gesellschaft der Wissenschaften, math.-phys. Klasse*, **1** (1852): 199–381, on 268 (= *Werke*, **3**: 301–471).

(10) Following Helmholtz, 'Ueber die Bewegungsgleichungen der Elektricität': 64. Multiplying the force law by dr/dt and integrating, Helmholtz obtains $\frac{m}{2}\left(\frac{dr}{dt}\right)^2 = C-\frac{ee'}{r}+\frac{ee'}{rc^2}\left(\frac{dr}{dt}\right)^2$ (where C is the constant of integration), and hence $\frac{1}{c^2}\left(\frac{dr}{dt}\right)^2 = \frac{C-(ee'/r)}{\frac{1}{2}mc^2-(ee'/r)}$.

(11) Helmholtz concludes: 'Ist $ee'/r > \frac{1}{2}mc^2 > C$ so ist $(dr/dt)^2$ positiv und grösser als c^2, also dr/dt reell. Ist letzteres selbst positiv, so wird r wachsen, bis $ee'/r = \frac{1}{2}mc^2$, dann wird dr/dt unendlich gross. Dasselbe wird geschehen, wenn in Anfange $C > \frac{1}{2}mc^2 > ee'/r$ und dr/dt negativ ist.' ('Ueber die Bewegungsgleichungen der Elektricität': 64).

(12) Tait had used the symbol $\underline{|n}$ for n-factorial, writing '$1.2.3.\ldots n = \underline{|n}$' in his paper 'On the law of the frequency of error', *Trans. Roy. Soc. Edinb.*, **24** (1865): 139–45, on 141. For further discussion of these symbols see Number 390, esp. notes (3) and (4).

(13) Bartholomew Price: see Number 367 note (3).

(14) In his reply (to Numbers 388 and 389) dated 10 November 1871 (ULC Add. MSS 7655, I, a/22), Tait made further reference to his 'Note on spherical harmonics', presented to the Royal Society of Edinburgh on 18 December 1871 (see Number 388 note (2)): 'O dp/dt ʽηρ αρ

$\mu\omega\rho\ \Sigma\phi\alpha\rho\xi$. Let $\sqrt{1+2\mu h+h^2} = 1+hy$, $h/\sqrt{\quad} = h\,dy/d\mu$. But

$$y = \mu + h\frac{1-y^2}{2} = \mu + h\frac{1-\mu^2}{2} + \frac{h}{1.2}\frac{d}{d\mu}\left(\frac{1-\mu^2}{2}\right)^2 + \dots,$$

$$\therefore Q_i = (-)^i\left(\frac{d}{d\mu}\right)^i\left(\frac{1-\mu^2}{2}\right)^i.$$

This is *not* Potato? The complete integral of $i(i+1)\,q_i + \frac{d}{d\mu}\left(\overline{1-\mu^2}\frac{dq_i}{d\mu}\right) = 0$ is $q_i = CQ_i\int\frac{d\mu}{(1-\mu^2)\,Q_i^2}$; and if Q_i be expressed in terms of q_i, we have $S_i = P_i\,Q_i$ where $\frac{d^2P_i}{dq_i^2} + Q_i^4\frac{d^2P_i}{d\phi^2} = 0$. This at once *suggests* $P_i = \Sigma_0^i A_s\Theta_i^{(s)}\cos(s\phi+\alpha_s)$. The 4[ns] are going on, but the essential asymmetry bothers them as they are not naturally lopsided. $V_i = r^{i+1}S\alpha_1\nabla S\alpha_2\nabla S\alpha_3\nabla\dots S\alpha_i\nabla\frac{C}{r}$ where the operator may be written $S.(\alpha_1\nabla\alpha_2\nabla\dots\alpha_i\nabla)$ – a *very* curious result. At any rate $\iint(V_i)^2\,ds = \frac{2^{i+2}\pi}{\lfloor 2i+1}a^{2i+2}\sum_0^{2_i}({}_sV_0^2)$!!!

By all means make $\lfloor i = \Pi_i$, if Cayley & Sylvester wish it – Price is the opposite of priceless in this matter. Why not take Euler's formula $\lfloor n = \Gamma(n+1)$? ☞.'

390

POSTCARD TO WILLIAM THOMSON

7 NOVEMBER 1871

From the original in the University Library, Cambridge[1]

[Cambridge]

Pray return sheet M (p 161–176) with remarks if possible.[2][a][b] I am requested to extirpate the Johnian symbol $\underline{|i}$[3] and to adopt the Gaussian Π_i.[4] I had used $\underline{|i}$ as a mere abbreviation for 1. 2...i,[5] not as a definite integral which has this value when i is integral. I send you slip as far as 64 which annotate at your convenience & return. You will see the Electrometers there.[6]

Laplace[7] has a clear view of the Biaxial harmonic.[8] T′ has an excellent discussion[9] of Q_i[10] and $\vartheta_i^{(s)}$[11] and their relations deduced from their definitions and not from their expansions as Murphy does.[12] Murphy is very clever, but not easily appreciated by the beginner. We are beginning θermodynamics.[13]

$\dfrac{dp}{dt}$

(a) {Thomson} (done) T Nov 9/71

(b) {Thomson} I thought it very good if it was it you sent me. Can I do more just now in the matter?

(1) ULC Add. MSS 7655, II/52. Previously published (in part) in Knott, *Life of Tait*: 102.

(2) *Treatise* §§130–44.

(3) In referring thus to the symbol $\underline{|i}$ for the i-factorial Maxwell probably has Isaac Todhunter (St John's 1844, senior wrangler 1848, Fellow 1849 (Venn)) in mind. On his use of the symbol '$\underline{|n}$...for the product 1.2,...n' see Todhunter, *A History of the Mathematical Theory of Probability* (Cambridge/London, 1865): ix.

(4) Carl Friedrich Gauss, 'Disquisitiones generales circa seriem infinitam', *Commentationes Societas Regiae Scientiarum Gottingensis Recentiores*, 2 (1812): on 26 (= Gauss, *Werke*, 3 (Göttingen, 1866): 125–62, on 146); where Gauss writes '$\Pi z = 1.2.3....z$'.

(5) See Numbers 387 and 388, a usage he retained in the *Treatise*.

(6) See the *Treatise*, **1**: 263–80 (§§214–25).

(7) See Number 389 note (2).

(8) A term used by Thomson and Tait, *Natural Philosophy*: 157.

(9) See Number 388 note (2).

(10) See Number 387 note (9).

(11) See Number 384 note (6).

(12) See Number 388 esp. note (5).

(13) In his Cambridge lectures on 'Heat': see *Cambridge University Reporter* (October 18, 1871): 16.

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LETTER TO ROBERT DUNDAS CAY

23 NOVEMBER 1871

From the original in the Library of Peterhouse, Cambridge(1)

11 Scroope Terrace
Cambridge
23 Nov 1871

My dear Uncle Robert

I enclose Receipt for £56.17/ .(2)

We are sorry that you have had such a severe attack of Bronchitis but we hope you are now getting better.

We have been very busy getting settled here and are not quite done yet with the settling. We have had severe cold weather which has been very trying for my wife. Our most important business is attending solemn feasts from 7 to 10 p.m. graced by the presence of not less than two Masters of Colleges and ornamented with a selection of Doctors of Divinity and filled up with Masters of Arts and their wives. To arrange these dignitaries a diligent study of the Calendar is required. In due time we shall have to draw up a scheme of their positions round our table so as to avoid any errors of precedence.

The minor amusements of lecturing &c go on without disturbance. Next term building will commence.(3)

With kind regards from self & wife.

Your aff^{t} nephew
J. CLERK MAXWELL

(1) Peterhouse, Maxwell MSS (32).

(2) See Volume I: 682n.

(3) See Number 397.

REPORTED COMMENTS ON THE STRAINS OF AN IRON STRUCTURE

5 DECEMBER 1871[(1)]

From the *Proceedings of the Institution of Civil Engineers*[(2)]

[DISCUSSION OF A PAPER BY WILLIAM BELL][(3)]

Professor J. Clerk Maxwell observed, through the Secretary,[(4)] that if the Author of the Paper could prove that his method of measuring the strains on comparatively small portions, as 50 inches,[(5)] of an iron structure was practicable, it would be a most valuable means of testing the accuracy of engineering calculations; and a careful examination of the marks before and after the erection, and again a year or two afterwards, would enable an opinion to be formed, as to the security of the structure itself, and as to the behaviour of iron under long-continued strains.[(6)] Experiments were confined so exclusively to longitudinal stress, that instances of yielding to different kinds of combined stress, such as occurred in structures, must be very useful. For example, there was no evidence to show how the value of the greatest safe vertical pressure or tension would be modified if one, or two, horizontal pressures or tensions coexisted with it. Probably two horizontal pressures would increase the power of supporting a vertical pressure. A thorough discussion of the experiments of M. Tresca on lead might give some information as to that material, which, however, was not much used by Engineers. Nevertheless, a perusal of Tresca's 'Sur l'écoulement des corps solides'*[(7)] might be interesting to practical men, as it bore on the theory of punching and wire-drawing.

* *Vide* 'Mémoire sur l'écoulement des corps solides soumis à des fortes pressions. Par H. Tresca. Comptes rendus hebdomadaires des Séances de l'Académie des Sciences, 1864'. Tome lix, p. 754 *et seq.*[(7)]

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(1) See note (3).

(2) *Minutes of Proceedings of the Institution of Civil Engineers; with Abstracts of the Discussions*, **33** (1871): 130–1.

(3) William Bell, 'On the stresses of rigid arches, continuous beams, and curved structures', *Minutes of Proceedings of the Institution of Civil Engineers*, **33** (1871): 58–126, read 5 December 1871.

(4) James Forrest.

(5) Bell, 'On the stresses of rigid arches': 124–6.

(6) In response, Bell observed that 'therc were as yet no experiments to show the minimum distance between the marks, which would give reliable information as to the state of strain on the metal...'; *ibid.*: 158–9.

(7) H. Tresca, 'Mémoire sur l'écoulement des corps solides soumis à des fortes pressions', *Comptes Rendus*, **59** (1864): 754–8.

POSTCARD TO PETER GUTHRIE TAIT

7 DECEMBER 1871

From the original in the University Library, Cambridge[1]

[Cambridge]

Find $\int_0^\pi \int_0^{2\pi} \int_{9\mathrm{Dec}1871}^{1\mathrm{Feb}1872} \left\{ Q_0 + 3Q_1 + \ldots + (2i+1)Q_i + \&c \right\} \frac{1}{4\pi} \frac{d}{d\sigma} \frac{dp}{dt} \cos\lambda \, d\tau \, dl \, d\lambda$

where the pole of the zonal harmonics is at $\lambda = 55°.1'$N, $l = 3°.38'$W.[2]

Compare T's equ$^{\text{ns}}$ derived from 2$^{\text{nd}}$ law of $\Theta\Delta^{\text{cs}}$[3] with Edlunds expts on Peltier effect Phil Mag 1871.[4]

Peltier effect (ratios)[5]		Thermoelectric E.M.F. for 10 °C diff.[6]
Bismuth–Copper	141.3	78.47 – Why so small?[7]
Argentan[8]–copper	15.57	24.17
Platinum–copper	5.37	8.30
Copper–iron	17.83	24.93

(1) ULC Add. MSS 7655, I, b/37.

(2) The latitude and longitude of Glenlair: see Number 292.

(3) William Thomson, 'On the dynamical theory of heat. Part V. Thermo-electric currents', *Trans. Roy. Soc. Edinb.*, **21** (1854): 123–71, esp. 133–45 (= *Math. & Phys. Papers*, **1**: 232–91). See also Number 322.

(4) E. Edlund, 'On the electromotive force on the contact of different metals', *Phil. Mag.*, ser. 4, **41** (1871): 18–29.

(5) As given by Edlund, 'On the electromotive force on the contact of different metals': 25.

(6) As given by Edlund, 'On the electromotive force on the contact of different metals': 28.

(7) Thomson responded to Maxwell's query in a card dated 4 January 1872 (ULC Add. MSS 7655, I, a/24): 'T 2 dp/dt / the el. m. f.$^{\text{ce}}$ of a thermo-elect circuit belongs to the whole circuit not to either junction alone. See waterpipe analogy Proc RSE 1851 (if the Secretaries have correctly reported the whole proceedings of the meeting). The more we understand of the potential, pressure, &$^{\text{c}}$ of electricity in metals the more perfect is the analogy. You are welcome to the thermo O$^{\text{t}}$ copper zinc air I have done with it years ago till I come back to it again. Oil of turpentine (as dry as you can get) and melted paraffin may be substituted for the air. I am doing so not very successfully now, and did so many times & years back. You said that Clausius made proportionality of current to external EMF in electrolysis which was what I objected to. / I have no doubt of the diffusion theory of currents through electrolytes in infin. EMF. /25 years ago I did not

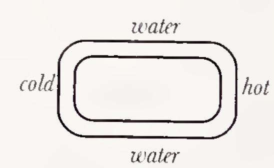

Figure 393,1. If mean temp$^{\text{re}}$ > 4°

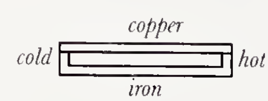

Figure 393,2. If mean temp$^{\text{re}}$ > 289°

Motto for an abuse of vortex atoms
δἶνος βασιλεύει τον Δί᾿ εξεληλακως.(9)

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know that $\iint f(x,y)\,dx\,dy$ is absurd. / The current is against hands of a watch. / Can you come on Frid. or Sat. or if not, when?' The paper Thomson refers to is his paper 'On a mechanical theory of thermo-electric currents', *Proc. Roy. Soc. Edinb.*, **3** (1851): 91–8 (= *Math. & Phys. Papers*, **1**: 316–23), where there is no mention of the analogy to which he alludes. Thomson had published the analogy in his 'On the dynamical theory of heat. Part V': 147. See also Number 428 esp. note (10).

(8) An alloy of nickel, copper and zinc (German silver); see Henry Watts, *A Dictionary of Chemistry*, 5 vols. (London, 1863–9), **1**: 356, and **2**: 51.

(9) Aristophanes, *The Clouds*, 1471. See *The Clouds of Aristophanes* [trans. B. B. Rogers] (Oxford, 1852): 125; 'Young Vortex reigns, and he has turned out Zeus'.

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POSTCARD TO PETER GUTHRIE TAIT

12 DECEMBER 1871

From the original in the University Library, Cambridge[1]

[Glenlair]

O T′. The neutral pt[2] as depending on sp.h of E^y[3] is a T′ thing and should be expressed in terms of T′. Do any such terms exist besides Lab. notes P.RSE 1870–71 p308?[4] If so, send them. I have of course got the relation[5]

$$T_{ab} = \frac{k_a T_a - k_b T_b}{k_a - k_b}$$

where k_a k_b are the sp. h. in 2 metals[6] and T_a T_b the neutral pts with respect to a standard metal (Standard metal should have $k = 0$ if possible (which it is)). Pray send what may be described as the very words of T′.[7]

$\frac{dp}{dt}$

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(1) ULC Add. MSS 7655, I, b/38.

(2) The temperature at which pairs of metals in a thermo-electric circuit are neutral to each other. See William Thomson, 'On the dynamical theory of heat. Part V. Thermo-electric currents', *Trans. Roy. Soc. Edinb.*, **21** (1854): 123–71, esp. 145.

(3) Thomson's term 'specific heat of electricity'; see his 'Thermo-electric currents': 146.

(4) P. G. Tait, 'On thermo-electricity', *Proc. Roy. Soc. Edinb.*, **7** (1870): 308–11.

(5) But see Number 396.

(6) But see Number 401.

(7) See Number 396 esp. note (2).

DRAFT OF PAPER 'ON THE GEOMETRICAL MEAN DISTANCE OF TWO FIGURES ON A PLANE'[1]

circa DECEMBER 1871[2]

From the original in the University Library, Cambridge[3]

ON THE GEOMETRICAL MEAN DISTANCE OF TWO PLANE FIGURES[4]

The following method of treating the mutual induction of two conductors is useful in the case of coils.

Let there be two conductors such that the transverse section of the two conductors is a figure of the same form at all parts of the length of the conductors and let the dimensions of this double section be small compared with the radius of curvature of the axis of either conductor. It is required to find the distance at which two linear conductors must be placed from each other having the same form of axis as the actual conductors so that the coefficient of the mutual induction of the linear conductors may be equal to that of the two given conductors.

The coefficient of mutual induction of two linear conductors which are everywhere equidistant depends on the logarithm of their distance.[5] Hence what we have to find is the arithmetical mean of the logarithms of the distances between every pair of filaments one in each conductor and this is the logarithm of the distance required.

In other words the distance required is the geometrical mean of the distances between every pair of filaments, one in each conductor.

Hence, when the form and relative position of the transverse sections of the two conductors are given, we have only to solve the geometrical problem of finding the geometrical mean of the distances between pairs of points one in each of two plane figures, the points being understood to be uniformly distributed in each figure.

The following are some of the results of this method of finding what we may call the geometrical mean distance.

(1) *Trans. Roy. Soc. Edinb.*, **26** (1872): 729–33 (= *Scientific Papers*, **2**: 280–5), read 15 January 1872 (see *Proc. Roy. Soc. Edinb.*, **7** (1872): 613). Proofs are in ULC Add. MSS 7655, V, c/17.

(2) See Maxwell's letter to P. G. Tait of 21 December 1871 (Number 396).

(3) ULC Add. MSS 7655, V, c/16.

(4) See also the *Treatise*, **2**: 294–8 (§§691–3).

(5) See the *Treatise*, **2**: 289 (§685).

Let the section of the first conductor be a thin circular ring and that of the other a point, then the geometrical mean distance of the point from the ring is equal to its distance from the centre of the ring when the point is outside the ring, and equal to the radius of the ring when the point is inside the ring.

If the sections of both conductors are thin circular rings then if each ring is outside the other the mean distance is the distance of their centres, but if the one is within the other their mean distance is the radius of the greater.

If the sections of both conductors are circular areas, or rings bounded by concentric circles and if each is outside the other then their mean distance is the distance of their centres.

If however the one is wholly within the other and if a_1 and a_2 are the outer and inner radii of the outer ring, and R the mean distance required then

$$\log R = \frac{a_1^2 \log a_1 - a_2^2 \log a_2}{a_1^2 - a_2^2} - \frac{1}{2}.$$

It appears from this that R is independent of the form of the section of the conductor which is within the tubular conductor.

It is not necessary that the two figures should be different in order to determine their mean distance for we may find the geometrical mean of the distance between all pairs of points of the same figure.

In the case of a circular area of radius a this mean distance is R where $\log R = \log a - \frac{1}{4}$.

In the case of a ring bounded by two concentric circles of radii a_1 and a_2, a_1 being the greater

$$\log R = \log a_1 - \frac{a_2^4}{(a_1^2 - a_2^2)^2} \log \frac{a_1}{a_2} + \frac{1}{4} \frac{3a_2^2 - a_1^2}{a_1^2 - a_2^2}.$$

When the ring is very thin R becomes equal to a_1.

If the section of the first conductor is the line AB and that of the second the point O such that OA is perpendicular to AB and $AOB = \theta$

$$\log R = \log OB + \frac{\theta}{\tan \theta} - 1.$$

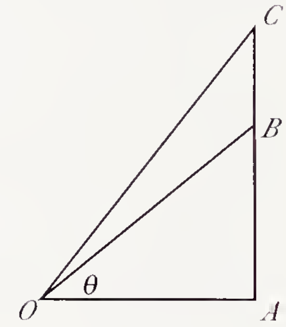

Figure 395,1

The mean distance of BC from O is R where

$$\log R = \frac{AC \log OC - AB \log OB + \widehat{BOC} \,.\, OA}{BC} - 1.$$

If R is the geometrical mean of the distances of every pair of points on a straight line from each other the length of the line being a

$$\log R = \log a - \frac{3}{2}.$$

If R is the geometrical mean of the distances of every pair of points one in each of two lines AB and CD lying in the same straight line then

$$2AB.CD\log R = AD^2\log AD + BC^2\log BC - AC^2\log AC - BD^2\log BD - 3AB.CD.$$

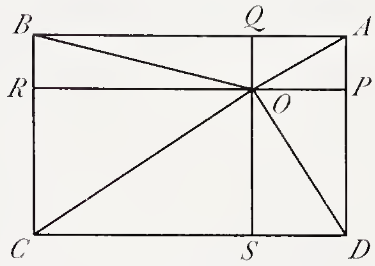

Figure 395,2

If R is the geometrical mean of the distances of every point of the rectangle $ABCD$ from the point O

$$\begin{aligned}2AB.AD\log R = {} & 2OP.OQ\log OA + 2OQ.OR\log OB \\ & + 2OR.OS\log OC + 2OS.OP\log OD \\ & + \widehat{BOA}.OP^2 + \widehat{AOB}.OQ^2 + \widehat{BOC}.OR^2 \\ & + \widehat{COD}.OS^2 - 3AB.AD.\end{aligned}$$

If the point O is one of the angles of a rectangle whose sides are a and b and diagonal $r = \sqrt{a^2+b^2}$

$$\log R = \log r + \frac{1}{2}\frac{a}{b}\tan^{-1}\frac{b}{a} + \frac{1}{2}\frac{b}{a}\tan^{-1}\frac{a}{b} - \frac{3}{2}.$$

When $a = b$ $\quad \log R = \log a + \frac{1}{2}\log 2 + \frac{\pi}{2} - \frac{3}{2}$

$$= \log a + 0.4273699.$$

If R is the geometrical mean of the distances of every pair of points of this rectangle

$$\log R = \log r - \frac{1}{6}\frac{a^2}{b^2}\log\frac{r}{a} - \frac{1}{6}\frac{b^2}{a^2}\log\frac{r}{b}$$

$$+\frac{2}{3}\frac{a}{b}\tan^{-1}\frac{b}{a} + \frac{2}{3}\frac{b}{a}\tan^{-1}\frac{a}{b} - \frac{25}{12}.$$

When $a = b$ $\quad \log R = \log a + \frac{1}{3}\log 2 + \frac{\pi}{3} - \frac{25}{12}$

$$= \log a - 0.8050866.$$

In terms of logarithms to base 10 we have, when

$b = a$		$\log_{10}\frac{R}{a} = \bar{1}.6503553$	$\frac{R}{a} = 0.44705$	$\log_\epsilon\frac{R}{a} = -0.8050866$
$b = 2a$		$\log_{10}\frac{R}{a} = \bar{1}.92648$	$\frac{R}{a} = 0.84427$	$\log_\epsilon\frac{R}{a} = -0.39954$
$b = 3a$		$\log_{10}\frac{R}{a} = \bar{1}.95163$	$\frac{R}{a} = 0.89461$	$\log_\epsilon\frac{R}{a} = -0.11138$
$b = 4a$		$\log_{10}\frac{R}{a} = \bar{1}.96183$	$\frac{R}{a} = 0.91587$	$\log_\epsilon\frac{R}{a} = -0.08789$
$b = 3x$	$a = 2x$	$\log_{10}\frac{R}{x} = 0.05194$	$\frac{R}{x} = 1.1271$	$\log_\epsilon\frac{R}{x} = +0.11960$
$b = 4x$	$a = 3x$	$\log_{10}\frac{R}{x} = 0.19443$	$\frac{R}{x} = 1.5647$	$\log_\epsilon\frac{R}{x} = +0.44770$

If two squares have a side common to both the geometrical mean distance of pairs of points one in each square is $0.99401a$ where a is the side of the square.

If the two squares are placed so as to have one angle in contact and their diagonals in a straight line, the geometrical mean distance is $1.0011d$ where d is the diagonal.

Hence for squares at greater distances we may consider the geometrical mean distance as the distance between their centres of gravity without introducing more than one thousandth part of error.

LETTER TO PETER GUTHRIE TAIT

21 DECEMBER 1871

From the original in the University Library, Cambridge[1]

Glenlair
21 Dec 1871

O. T.

Am I right in supposing the following to be a graphic representation of your Θ.H. theory.[2]

Let k_a k_b k_c be sp. h. of η in metals $a\,b\,c$.[3] From pure thermoelectric experiments we can determine the difference $k_a - k_b$ in absolute measure, and from Thomsons much more precarious direct experiments[4] we may make a shot at the ratio $k_a : k_b$ say in iron & copper in which k_a and k_b are of opposite signs and one of them is small. Hence the absolute values can be ascertained roughly and the differences very approximately.

In this way draw Thomson's figure[5] as on last page. (If you reject Thomson absolute determinations of sp h the figure simply gets a little loose in the joints.)

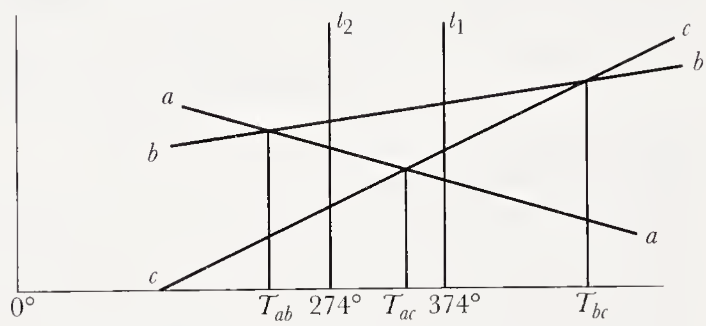

Figure 396,1

Then the Π_{ab}eltier effect at temperature t is the parallelogram whose base is the intercept of the ordinate between the lines a and b and whose height is up to temperature 0°.

The electromotive force for junctions at t_1 and t_2 is the area intercepted between the ordinates t_1 & t_2 and the lines aa and bb. (Remember signs if t_1 & t_2 are on opposite sides of t_{ab}.)

(1) ULC Add. MSS 7655, I, b/39. Published in extract in Knott, *Life of Tait*: 150.

(2) See P. G. Tait, 'On thermo-electricity', *Proc. Roy. Soc. Edinb.*, **7** (1871): 597–602, read 18 December 1871.

(3) See Number 394 esp. note (3).

(4) William Thomson, 'On the electro-dynamic qualities of metals', *Phil. Trans.*, **146** (1856): 649–751, esp. 649–709 (= *Math. & Phys. Papers*, **2**: 189–327, esp. 189–266).

(5) Thomson, 'On the electro-dynamic qualities of metals': 708; and also Thomson, 'On the dynamical theory of heat. Part V. Thermo-electric currents', *Trans. Roy. Soc. Edinb.*, **21** (1854): 123–71, esp. 146 (= *Math. & Phys. Papers*, **1**: 232–91).

Also by property of straight lines

$$(k_b - k_c)\,T_{bc} + (k_c - k_a)\,T_{ca} + (k_a - k_b)\,T_{ab} = 0$$

a relation between the neutral pts.[6]

Would the R.S.E. care for a short statement of the importance of a knowledge of the Geometrical Mean Distance of Two Figures in the same plane (which may be identical)?[7] Of course this means the geometric mean of all the distances between points in the figures, the points being scattered with uniform density.

The Harmonic mean distance of any two bodies is of course still more important in itself but it happens not to be wanted so much.

The Geometric ditto is the thing for coils of wire &c &c.

The Harmonic for ordinary attractions.

If so I will send you it before the 15th Jan.

Pray let me know soon about the thermoelectricity as it is your thing and I am improving the statement of it as per your note.

Impress on T that $\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2} = -\nabla^2$ and not $+\nabla^2$ as he vainly asserts is now commonly believed among us.

Also how much better and easier he would have done his solenoidal and lamellar business[8] if in addition to what we know is in him he had had, say 20 years ago, Q_s^n to hunt for Cartesians instead of vice versâ. The one is a flaming sword which turns every way; the other is a ram pushing westward and northward and (downward?).

What we want a Council to determine is the true doctrine of brackets and dots and the limits of the jurisdiction of operators.

$$\frac{\partial p}{\partial \theta}$$

Thanks for prooves. I send you more just received.

(6) See the *Treatise*, **1**: 306 (§254) for a statement of the argument.

(7) See Number 395 esp. note (1).

(8) On Thomson's discussion of 'solenoidal' and 'lamellar' distributions of magnetism see Numbers 322 note (13) and 353 note (15). Maxwell has in mind his discussion of the effect of ∇ on a vector function σ, terming the scalar part $S\nabla\sigma$ *convergence* and the vector part $V\nabla\sigma$ *curl* (see Number 347 and the *Treatise*, **1**: 28 (§25)). $S\nabla\sigma = 0$ and $V\nabla\sigma = 0$ are solenoidal and lamellar distributions, respectively.

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LETTER TO WILLIAM MILNER FAWCETT(1)

I JANUARY 1872

From the original in private possession(2)

Glenlair
Dalbeattie
1 Jan 1872

My dear Sir

Professor Clifton seems to find his white wall useful to him.(3) I do not know myself what the expense of plastering would be. I think a moveable white screen would be sufficient as most of the work will be done by diagrams or on a black board.

I quite agree with you that it is better to have the ceiling of wood instead of plaster.

I have been settling the positions of the stone blocks in the lower floor and of the tables above and also of the holes through the floors for suspension of instruments below. I shall send you plans of these things(4) as soon as I have got them finished. Wishing you a happy New Year

I am
Yours very truly
J. CLERK MAXWELL

---

(1) Jesus 1855; the architect of the Cavendish Laboratory (Venn), the likely addressee. See Number 374 note (6).

(2) Collection of Sydney Ross, donated to the James Clerk Maxwell Foundation, Edinburgh.

(3) In the Oxford laboratory: see Number 365 note (4).

(4) Compare Maxwell's sketch in his card to Thomson of 30 March 1871 (Number 365).

POSTCARD TO PETER GUTHRIE TAIT

I JANUARY 1872

From the original in the University Library, Cambridge[1]

[Glenlair]

O T′ Art thou a Sec. of the F.R.S.E's and knowest not Vol XXIV pt I N° VII, p59[2] where you will see your problem[3] solved. You may state N° 2 thus. Two equal circles have the same horizontal lowest tangent (or if you like stand at different inclinations on the same horizontal plane one vertical, the other inclined) particles are projected at the same instant from the lowest point of each with velocities due to the highest point of *the other*. They will always remain in the same horizontal plane.

The other prop. that the line joining two particles whizzing in the same ⊙ with the same energy touches a circle with directrix for radical axis, was original to me in 1853 or so.[4] It is involved in the in and circum scribed polygon business.[5] See Cayley[6] & Fox Talbot.[7] It was copied from the C & D.J. of P and A M. into a French book of mathematical varieties[8] which I have but cant find today.

I shoot you the ultramundane corpuscles[9] with the co^{m}p^{s} of the season.[10]

$\frac{dp}{dt}$

(1) ULC Add. MSS 7655, I, b/40.

(2) Edward Sang, 'On the motion of a heavy body along the circumference of a circle', *Trans. Roy. Soc. Edinb.*, **24** (1867): 59–71. See also Sang, 'Additional note on the motion of a body along the circumference of a circle', *ibid.*, **26** (1871): 449–57.

(3) See P. G. Tait, 'Note on pendulum motion', *Proc. Roy. Soc. Edinb.*, **7** (1872): 608–11, read 15 January 1872.

(4) 'Problems' set in the *Camb. & Dubl. Math. J.*, **8** (1853): 188; and see [J. C. Maxwell,] 'Solutions to problems', *ibid.*, **9** (1854): 7–11 (= *Scientific Papers*, **1**: 74–9). Drafts are reproduced in Volume I: 230–6, where see 230n on the radical axis of two circles.

(5) See the preliminary draft of Maxwell's solution to Problem II (Volume I: 235–6).

(6) Arthur Cayley, 'On the problem of the in-and-circumscribed triangle', *Phil. Trans.*, **161** (1871): 369–412.

(7) W. H. Fox Talbot, 'Researches on Malfatti's problem', *Trans. Roy. Soc. Edinb.*, **24** (1865): 127–38.

(8) Michel Jullien, *Problèmes de Mécanique Rationelle, disposés pour servir d'applications aux principes enseignés dans les cours*, 2 vols. (Paris, 1855), **1**: 349–52, where the source is given as *Camb. & Dublin Math. J.*, February 1854, p. 7. See Tait's reply (note (10)).

(9) See William Thomson, 'On the ultramundane corpuscles of Le Sage', *Proc. Roy. Soc. Edinb.*, **7** (1871): 577–89, read 18 December 1871; and see Number 377.

(10) In his reply of 2 January 1872 (ULC Add. MSS 7655, I, a/23) Tait writes: 'O *dp/dt*, It was *precisely* Trans. R.S.E. XXIV, 59, w$^{\text{h}}$ caused me to look up old papers & Tait & Steele. *Jullien* is the book you mean & I have the same. I don't know whether I got y.$^{\text{r}}$ prop.$^{\text{n}}$ out of it, or out of U, or out of C. & D.M.J. but it has been given (as Ex. of Constr.$^{\text{d}}$ M.$^{\text{n}}$) in all the Editions of T. & S. In the 4th it will have y.$^{\text{r}}$ name attached. I wrote a little Note for R.S.E. to point out how Sang's *three* long papers c.$^{\text{d}}$ be condensed into a few lines. Sh.$^{\text{d}}$ I publish? – Prob. Given three pts. on a ⊙ find how to place it so that a particle (starting from rest at one) shall pass to the second in $\frac{1}{3}$$^{\text{rd}}$ of the time it takes to pass to the third. I am suffering from Vacation with *one*, and with *two*, Cs. T'. / Has anyone shown that the Retina *wakes* (from sleep) sooner to the lowest of its three principal vibrations than to the others? If not, *I* have.' Tait refers to an example given in P. G. Tait and W. J. Steele, *A Treatise on the Dynamics of a Particle, with Numerous Examples* (London, 1856): 185–6.

POSTCARD TO PETER GUTHRIE TAIT

circa 4 JANUARY 1872

From the original in the University Library, Cambridge[1]

[Glenlair]

O T. Of course the readers of Alençon[2] in R.S.E. will be greatly obliged to you if you will give them an inspissated version and if they find it wondrous short it will not hold them long.[3] But above all publish your observation of the rosy dawn of vision.[4] For the red is the first sensation to evanesce with faintness of light, and the slowest in being awakened by the sudden appearance of light.[5] (The 1st observation I believe by seeing, the 2nd only as yet by reading.) Any physiological fact about colour vision is of great importance, and especially if not tacked to any theory.

Do you know of any one who has *worked out* the currents in a spinning plate in presence of a magnet? Kirchhoff is the only man beside T who knows how. I have been getting it out in a simple and applicable form much neater than I expected it wd come.[6] Has T got safe for Great Britain.[7] Is the Vaccation Personal or Filial?[8]

$\frac{dp}{dt}$

---

(1) ULC Add. MSS 7655, I, b/41.

(2) Makers of needlepoint lace.

(3) See Tait's discussion of his 'Note on pendulum motion' in his postcard of 2 January 1872 (see Number 398 notes (3) and (10)).

(4) See Tait's observation reported in his postcard of 2 January 1872.

(5) In his 'Note on a singular property of the retina', *Proc. Roy. Soc. Edinb.*, **7** (1872): 605–7, read 15 January 1872, Tait cites Maxwell's comment.

(6) By the principle of images: see Number 400 and the *Treatise*, **2**: 271–5 (§§668–9) on the 'Theory of Arago's rotating disk'.

(7) An allusion to a patent case in December 1871, in which Thomson successfully appealed for the prolongation of his 1858 patent on his mirror galvanometer; see S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), **2**: 619–21; and also Number 379 note (4). Maxwell wrote a poem 'A lecture on Thomson's mirror galvanometer', published in *Nature*, **6** (16 May 1872): 46 (= Thompson, *Life of Thomson*: 349n).

(8) In his reply of 6 January 1872 (ULC Add. MSS 7655, I, a/25) Tait writes: 'O dp/dt Whence get you your (seemingly) constant supply of Mr Lowe's more liberal sized cards? Ex fumo. As to the rotating disc I never tried it – still I think I could do so if necessary – but it is not so, as Jochmann (I think) has done it *very dodgily*, partly in Crelle, partly in Pogg. (the latter in Phil. Mag. about 4 years ago). I'll look them up for you if you like. The Retina goes in on Monday week. So does your Geometric Means. Can you come and read it yourself? The Vaccation is unfortunately Personal, and my left arm is so stiff that I trust to gravity & pressure to hold this card while I scribble thereon. What say you to $\phi(\nabla)$? ♈.'

Robert Lowe was Chancellor of the Exchequer in Gladstone's Liberal administration. In his budget of 1870 he had lowered postage; in 1871 he proposed a tax on matches. Tait's 'ex fumo' (from smoke) puns Lowe's 'Ex luce lucellum' (out of light a little gain) (*DNB*). On Jochmann's papers see Number 404 note (4). Tait alludes to his paper 'On the operator $\phi(\nabla)$', *Proc. Roy. Soc. Edinb.*, **7** (1872): 607–8, read 15 January 1872.

LETTER TO GEORGE GABRIEL STOKES

8 JANUARY 1872

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
8 Jan 1872

Dear Professor Stokes

I send you a paper for the Royal Society in which a peculiar kind of image of an electromagnetic system is shown to be formed by a plane sheet of conducting matter.[2]

If a man on board ship were to drop into the sea every second a piece of lead and a piece of cork attached to each other by a string of such a length that, in sinking, the cork is always one second in the rear of the lead, the series of plummets and corks would form a kind of trail in the rear of the vessel which is always sinking with uniform velocity while it retains its shape as a whole.

This gives a mental image of the trail of images of an electromagnet in a conducting sheet. The plummets are positive images and the corks negative images.

N.B. The paper is not offered for the Transactions, as it is to be put into a slightly different form in a separate book.[3]

Yours very truly
J. Clerk Maxwell

---

(1) ULC Add. MSS 7655, M 429. First published in Larmor, *Correspondence*, **2**: 31.

(2) J. Clerk Maxwell, 'On the induction of electric currents in an infinite plane sheet of uniform conductivity', *Proc. Roy. Soc.*, **20** (1872): 160–8 (= *Scientific Papers*, **2**: 286–96). See Number 404 on Maxwell's addition of the supplementary note to the paper.

(3) See the *Treatise*, **2**: 262–75 (§§654–69).

POSTCARD TO PETER GUTHRIE TAIT

19 JANUARY 1872

From the original in the University Library, Cambridge(1)

[Glenlair]

O T′. Tell me, ex cathedrâ, this, if α & β are vectors and $\gamma = V.\alpha\beta$ is not γ related to α and β in the same (right or left handed) way as z is related to x and y in the system of axes chosen? Or the reverse? Also is not the English of the above β multiplied by α?(2) I wish to be correct. You may if you please assume $S.\alpha\beta = 0$ and then $\alpha\ \beta\ \gamma$ may be made to fit $i\ j\ k$(3) or $x\ y\ z$ by a proper turning of the system as a whole.

Also are you satisfied with T′ on thermoelectricity?(4) I see I have stated wrongly that k is sp h of η(5) whereas kt is the korrect $\theta\iota\nu\gamma$.(6) I wish to get you to press quam prox.(7) I am putting $\mathfrak{C}$ for velocity instead of $\dot{\rho}$(8) to please Bismarck?

$\frac{dp}{dt}$

---

(1) ULC Add. MSS 7655, I, b/42.

(2) See P. G. Tait, An *Elementary Treatise on Quaternions* (Oxford, 1867): 54–5, on the scalar and vector products of the multiplication of two vectors. Tait writes $S\alpha\beta = S\beta\alpha$ and $V\alpha\beta = -V\beta\alpha$, noting that 'the only difference is in the *sign* of the vector parts'.

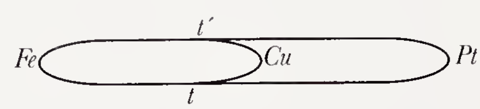
(3) Hamilton's system of three mutually perpendicular unit vectors as lines of reference; see Tait, *Quaternions*: 9.

(4) P. G. Tait, 'On thermo-electricity', *Proc. Roy. Soc. Edinb.*, **7** (1871): 597–602. See Number 396.

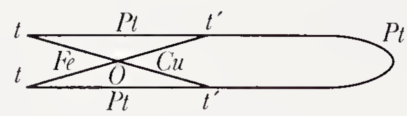
(5) See Numbers 394 and 396.

(6) See the *Treatise*, **1**: 306 (§254) following Tait, 'On thermo-electricity': 599, where k is a constant for a metal and t the absolute temperature.

(7) In a card dated 25 January 1872 (ULC Add. MSS 7655, I, a/26) Tait wrote: 'O dp/dt I asked you to work out, for corroboration of my results the following case.



Pray do so soon – and add to the flavour by doing also this – *no* contact at *O*.



They are all devised with the view of bringing neutral points (otherwise inacceptible) within the range of mercury thermometers. If you can devise any better arrangement I shall be most grateful to you for communicating it. $\mathfrak{T}$. / Lindsay out of all danger but useless to me for a month or 6 weeks to come.' James Lindsay was Tait's laboratory technician (see Knott, *Life of Tait*: 66, 73–4).

(8) See the *Treatise*, **2**: 236 (§617), where Maxwell uses the symbol $\mathfrak{C}$ for 'the (total) electric current'.

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POSTCARD TO WILLIAM THOMSON

8 FEBRUARY 1872

From the original in the University Library, Cambridge[1]

[Cambridge]

[2]p433 $l3$ from bottom, 'infinitely mutual'[3] What are degrees of mutuality?
p448 $l5$ 'irrational'[4] p462 $l14$ for 1867 put 1847[5]
p462 $l17$ ∂, 467 footnote en $\wedge$ /p471 $l14$ sensibility? or susceptibility[6]
p471 $l23$ and 24 read, from the substance in that direction[7]
p478 $l16$ A B and C (see equations), bad arrangement of dashes[8]

Should be[a] $\begin{matrix} A & B' & C'' \\ A'' & B & C' \\ A' & B'' & C \end{matrix}$ when $\begin{matrix} B' = A'' \\ C' = B'' \\ A = B'' \end{matrix}$[b]

line 28 made to turn round a fixed axis[9]

(a) {Thomson} I defined this

(b) {Thomson} $A\ B''\ C\,|\,A' = C$

(1) ULC Add. MSS 7655, II/55.

(2) Maxwell is here correcting proofs of Thomson's *Reprint of Papers on Electrostatics and Magnetism* (London, 1872).

(3) Compare Thomson, *Electrostatics and Magnetism*: 433; 'infinite smaller parts from infinite mutual distances'.

(4) Compare Thomson, *Electrostatics and Magnetism*: 448; 'irrotationally'.

(5) Possibly: Thomson, *Electrostatics and Magnetism*: 453; a reference to a letter to Liouville of 12 September 1847.

(6) Compare Thomson, *Electrostatics and Magnetism*: 472; 'inductive susceptibility'.

(7) See William Thomson, 'On the theory of magnetic induction in crystalline and non-crystalline substances', *Phil. Mag.*, ser. 4, **1** (1851): 177–86, esp. 181 (= *Electrostatics and Magnetism*: 472); 'If the sphere be of isotropic [read: 'non-crystalline' in the 1851 original] substance, the lines of its magnetization are in the same direction as the lines of force in the field into which it is introduced'.

(8) The equations for the magnetisation of a sphere of a homogeneous magnetisable substance in a field of force R, l, m, n being the direction cosines of the force and α, β, γ the components of induced magnetisation: $\alpha = (Al + B'm + C''n)\,R$, $\beta = (A''l + Bm + C'n)\,R$, $\gamma = (A'l + B''m + Cn)\,R$ (= *Electrostatics and Magnetism*: 479). These equations are modified and corrected from those in the original paper; see Thomson, 'On the theory of magnetic induction': 186. A, B, C are coefficients depending solely on the substance.

(9) Thomson had slightly modified the text of his 1851 paper 'On the theory of magnetic induction' to read: 'made to turn round an axis fixed perpendicular to the lines of force...' (*Electrostatics and Magnetism*: 480). For the original text see Number 278 note (10).

Observe how my invincible ignorance of certain modes of thought has caused Clausius to disagree with me (in a digestive sense) so that I failed in my attempts to boil him down and he does not occupy the place in my book on heat to which his other virtues entitle him.[10] If he can get himself assimilated now I shall appear in a state of disgregation.[11] Ergal lusting against Virial, and Virial against Ergal.[12] Any Prooves for $\frac{dp}{dt}$?

(10) See Rudolf Clausius, 'A contribution to the history of the mechanical theory of heat', *Phil. Mag.*, ser. 4, **43** (1872): 106–15 (trans. in the February 1872 number of the *Phil. Mag.* from Clausius's 'Zur Geschichte der mechanischen Wärmetheorie', *Ann. Phys.*, **145** (1872): 132–46), where he complained that Maxwell had intentionally suppressed his name from the *Theory of Heat* (London, 1871).

(11) On Clausius' concept of 'Disgregation', a measure of the arrangement of the molecules in a body, see his paper 'Ueber die Anwendung des Satzes von der Aequivalenz der Verwandlungen auf die innere Arbeit', *Ann. Phys.*, **116** (1862): 73–112, esp. 79. See M. J. Klein, 'Gibbs on Clausius', *Historical Studies in the Physical Sciences*, **1** (1969): 127–49, esp. 135–42.

(12) On 'virial' and 'ergal' see Number 356 note (3).

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POSTCARD TO PETER GUTHRIE TAIT

12 FEBRUARY 1872

From the original in the University Library, Cambridge[1]

11 Scroope Terrace
Cambridge
12 Feb 1872

O T′ What makes you address to Glenlair? I have no time, strength or fury to smash. As for C. though I imbibed my $\Theta\Delta^{cs}$ from other sources, I know that he is a prime source and have in my work for Longman been unconsciously acted on by the motive not to speak about what I dont know.[2] In my spare moments, I mean to take such draughts of Clausiustical Ergon[3] as to place me in that state of disgregation in which one becomes conscious of the increase of the general sum of Entropy.[4] Meanwhile till

Ergal & Virial[5] from their thrones be cast
And end their strife with suicidal yell.

Electromagnetic Trails[6] are to be served up (on toast) by Stokes at R S on Thursday.[7] Note on Felici and Jochmann.[8]

I remain y^{rs} $\frac{dp}{dt}$

---

(1) ULC Add. MSS 7655, I, b/43.

(2) See Number 402.

(3) On Clausius' term 'ergon (work)' see Number 356 note (3).

(4) Clausius had defended his concepts of 'disgregation' (see Number 402 note (11)) and 'entropy' (see Number 483 note (22)) in his paper 'A contribution to the history of the mechanical theory of heat'. *Phil. Mag.*, ser. 4, **43** (1872): 106–15, esp. 114.

(5) See Number 356 note (3).

(6) For Maxwell's explanation of the induction of electric currents in a conducting plane sheet, in terms of a moving 'train or trail of images', see Numbers 400 and 405.

(7) See Number 404.

(8) Tait had drawn Maxwell's attention to Jochmann in his card of 6 January 1872 (see Number 399 note (8)), and Maxwell may have been responding here to another card from Tait (undated and without a postmark, but very likely written in January 1872): 'O *dp/dt* I don't know how your prooves got here – since they were inserted in my letter box *without any* cover or address. No matter what Jochmann may have done *he did n't do* what you mention – so send us it in MSS. for the R.S.E. meeting on the 29th. So far as I see at present it is splendid. Also come yourself and expound it – especially as we should all like to smoke you at the Club &^{c} &^{c}. This is the first letter I have written (with the use of two hands) for a week .' (ULC Add. MSS 7655, I, a/55). There was a meeting of the Royal Society of Edinburgh on 29 January 1872 (see *Proc. Roy. Soc. Edinb.*, **7** (1872): 615).

404

LETTER TO GEORGE GABRIEL STOKES

12 FEBRUARY 1872

From the original in the University Library, Cambridge[1]

11 Scroope Terrace
Cambridge
12 February 1872

My dear Stokes

I send you a note on my paper on induction in a plate,[2] making mention of the researches of Felici[3] and Jochmann,[4] which I could not refer to in the country.

The mutual induction of the induced currents must not be neglected when the relative velocity of the magnet and plate is comparable with V,[5] which, for a copper plate 1 mm thick is about 25 metres per second and for a thicker plate is less, so that the secondary phenomena described in the paper ought to occur, as indeed they do with a thick plate and a high speed i.e.[6]

Primary phenomenon	Tangential Dragging
Secondary	Repulsion from disk
Tertiary	Attraction towards axis.

Yours truly
J. Clerk Maxwell

(1) ULC Add. MSS 7656, M 430. First published in Larmor, *Correspondence*, **2**: 32.

(2) A supplementary note to his paper 'On the induction of electric currents in an infinite plane sheet of uniform conductivity', *Proc. Roy. Soc.*, **20** (1872): 160–8, esp. 167–8 (= *Scientific Papers*, **2**: 286–96, esp. 295–6). On his original submission of the paper to the Royal Society see his letter to Stokes of 8 January 1872 (Number 400).

(3) R. Felici, 'Saggio di una applicazione del calcolo alle correnti indotte'dal magnetismo in movimento', *Annali di Scienze, Matematiche, e Fisiche*, **4** (1853): 173–83; and Felici, 'Sulla teoria matematica dell'induzione elettro-dinamica', *ibid.*, **5** (1854): 35–58.

(4) E. Jochmann, 'Ueber die durch einen Magnet in einen rotirenden Stromleiter inducirten elektrischen Ströme', *Journal für die reine und angewandte Mathematik*, **63** (1864): 158–78, 329–31; (trans.) 'On the electric currents induced by a magnet in a rotating conductor', *Phil. Mag.*, ser. 4, **27** (1864): 506–28, and 'On induction in a rotating conductor', *ibid.*, **28** (1864): 347–9; Jochmann, 'Ueber die durch Magnetpole in rotirenden körperlichen Leitern inducirten elektrischen Ströme', *Ann. Phys.*, **122** (1864): 214–37.

(5) The velocity of the moving 'train or trail of images'; see Number 405.

(6) See Maxwell's discussion of the theory of Arago's rotating metallic disc in the *Treatise*, **2**: 275 (§669).

405

ABSTRACT OF PAPER ON ARAGO'S ROTATING DISC[1]

circa 15 FEBRUARY 1872[2]

From *Nature* (29 February 1872)[3]

ON THE INDUCTION OF ELECTRIC CURRENTS IN AN INFINITE PLANE SHEET OF UNIFORMLY CONDUCTING MATTER

The currents are supposed to be induced in the sheet by the variation in position or intensity of any system of magnets or electromagnets.

When any system of currents is excited in the sheet, and then left to itself, it gradually decays, on account of the resistance of the sheet. At any point on the positive side of the sheet, the electromagnetic action is precisely the same as if the sheet, with its currents, retaining their original intensity, had been carried away in the negative direction with a constant velocity R, where R is the value, in electromagnetic measure, of the resistance of a rectangular portion of the sheet, of length l and breadth 2π. This velocity, for a sheet of copper of best quality of one millimetre thickness, is about twenty-five metres per second, and is, therefore, in general comparable with the velocities attainable in experiments with rotating apparatus.

When an electromagnet is suddenly excited on the positive side of the sheet, a system of currents is induced in the sheet, the effect of which on any point on the negative side is, *at the first instant*, such as exactly to neutralise the effect of the magnet itself. The effect of the decay of this system of currents is therefore equivalent to that of an image of the magnet, equal and opposite to the real magnet, from the position of the real magnet, in the direction of the normal drawn away from the sheet, with the constant velocity R.

When any change occurs in an electromagnetic system, whether by its motion or by the variation of its intensity, we may conceive the change to take place by the superposition of an imaginary system upon the original system; the imaginary system being equivalent to the difference between the original and the final state of the system.

The currents excited in the sheet by this change will gradually decay, and

(1) This abstract of Maxwell's Royal Society paper incorporates the supplementary note on the experiments of Felici and Jochmann; see Number 404.

(2) The date the paper was read to the Royal Society; see Number 404 note (2).

(3) *Nature*, 5 (1872): 354.

their effect will be equivalent to that of the imaginary system carried away from the sheet with the constant velocity R.

When a magnet or electro-magnet moves or varies in any continuous manner, a succession of imaginary magnetic systems like those already described is formed, and each, as it is formed, begins to move away from the sheet with the constant velocity R. In this way a train or trail of images, is formed, moves off, parallel to itself, away from the sheet, as the smoke of a steamer ascends in still air from the moving funnel.

When the sheet itself is in motion, the currents, relatively to the sheet, are the same as if the sheet had been at rest, and the magnets had moved with the same relative velocity. The only difference is, that whereas when the sheet is at rest no difference of electric potential is produced in different parts of the sheet, differences of potential, which may be detected by fixed electrodes are produced in the moving sheet.

The problem of Arago's whirling disc has been investigated by MM. Felici[(4)] and Jochmann.[(5)] Neither of these writers, however, has solved the problem so as to take into account the mutual induction of the currents in the disc. This is the principal step made in this paper, and it is expressed in terms of the theory of images, by which Sir W. Thomson solved so many problems in Statical Electricity.[(6)] In the case of the whirling disc, the trail of images has the form of a helix, moving away from the disc with velocity R, while it revolves about the axis along with the disc. Besides the dragging action which the disc exerts on the magnetic pole in the tangential direction, parallel to the motion of the disc, the theory also indicates a repulsive action directed away from the disc, and an attraction towards the axis of the disc, provided the pole is not placed very near the edge of the disc, a case not included in the investigation. These phenomena were observed experimentally by Arago, *Ann. de Chimie*, 1826.[(7)]

(4) See Number 404 note (3).
(5) See Number 404 note (4).
(6) See Number 301 note (10).
(7) François Arago, 'Note concernant les phénomènes magnétiques auxquels le mouvement donne naissance', *Ann. Chim. Phys.*, ser. 2, **32** (1826): 213–23.

LETTER TO WILLIAM HUGGINS

2 MAY 1872

From Campbell and Garnett, *Life of Maxwell* (2nd edn)[1]

11 Scroope Terrace
Cambridge
2 May 1872

My dear Sir

Toby and I enclose our photographs with our best regards to you and Kepler.[2] I had intended to be in London to-morrow,[3] but I am busy here. I hope the air-pump has recovered its cohesion. There seemed to be a solution of continuity between the mercury and the glass.

Yours very truly
J.C.M.

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(1) *Life of Maxwell* (2nd edn): 293.

(2) Toby and Kepler were dogs. For Huggins' recollection of Kepler, who would bark an answer to numerical questions, see Larmor, *Correspondence*, **1**: 104.

(3) Probably to attend a meeting of the Royal Society. Huggins had submitted a paper 'On the spectrum of the great nebula in Orion, and on the motions of some stars towards or from the earth', but the paper was not read until 13 June, following receipt of a supplement; see *Proc. Roy. Soc.*, **20** (1872): 379–94.

407

FRAGMENT OF A LETTER TO PETER GUTHRIE TAIT

circa EARLY MAY 1872[1]

From the original in the University Library, Cambridge[2]

[Cambridge]

If the surfaces $\lambda_1\ \lambda_2\ \lambda_3$ divide space into cubes and if

$$\overline{\frac{ds}{d\lambda_1}}\Big|^2 = \overline{\frac{dx}{d\lambda_1}}\Big|^2 + \overline{\frac{dy}{d\lambda_1}}\Big|^2 + \overline{\frac{dz}{d\lambda_1}}\Big|^2 \ \&\mathrm{c}$$

then $\dfrac{ds_1}{d\lambda_1} = \dfrac{ds_2}{d\lambda_2} = \dfrac{ds_3}{d\lambda_3} = p$ (a function of xyz or of $\lambda_1\ \lambda_2\ \lambda_3$).

But if R_{12} is the radius of curvature of s_1 in the plane of s_2 (for orthogonal surfaces)

$$\frac{1}{R_{12}} = \frac{\dfrac{d^2s_1}{d\lambda_1\,d\lambda_2}}{\dfrac{ds_1}{d\lambda_1}\dfrac{ds_2}{d\lambda_2}} = \frac{\dfrac{dp}{d\lambda_2}}{p^2} = \frac{1}{R_{32}}.$$

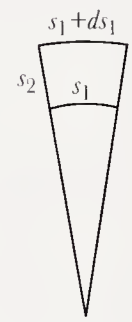

Figure 407,1

Hence the radii of curvature of the sections of the surface λ_2 made by the surfaces λ_1 and λ_3 are equal. But by Dupin's Theorem[3] these are principal curvatures. Hence the principal curvatures of the surface are equal at every point and the surface is a sphere. Now 3 sets of orthogonal spheres cannot be constructed except by making each set have a point of common contact. Prove this as you like. This is not published in any place as yet by $\dfrac{dp}{dt}$.[4]

It is neater and perhaps wiser to compose a nablody on this theme which is well suited for this species of composition.

Send any corrections of prooves to Cambridge, as a lot is going to press presently.

(1) See Numbers 408 and 409, to which this fragment is a preliminary.

(2) ULC Add. MSS 7655, I, b/104.

(3) The theorem that three families of orthogonal surfaces intersect in the lines of curvature. See Charles Dupin, *Développements de Géométrie* (Paris, 1813): 239–40.

(4) See his paper 'On the condition that, in the transformation of any figure by curvilinear co-ordinates in three dimensions, every angle in the new figure shall be equal to the corresponding angle in the original figure', *Proceedings of the London Mathematical Society*, **4** (1872): 117–19 (= *Scientific Papers*, **2**: 296–300), read 9 May 1872.

POSTCARD TO PETER GUTHRIE TAIT

9 MAY 1872

From the original in the University Library, Cambridge[1]

[Cambridge]

O T′. The collops of space[2] are served up to the Math Society this evening.[3] I am just going to look at your method of *drawing* a scientific brock before making him like the frightful porcupig. I have been studying in Bertrands edition of Lagrange the portentous equation

$$\frac{dT}{dq_1} = -\frac{dT}{dq_1}$$

not even with a magnifying glass can I distinguish between the two members, of which one is afflicted with $-$.[4] By a moderate use of suffixes it comes all right. T & T′ use d and ∂[5] which I doubt but if you say $T_{\dot{q}} = T_{p\dot{q}} = T_p$[6] then $T_p = 2T_{p\dot{q}} - T_{\dot{q}}$ or diff.ing

$$\sum\left(\frac{dT_p}{dp}\delta p\right) + \sum\left(\frac{dT_p}{dq}\delta q\right) = \sum \dot{q}\delta p + \left[\sum(p\delta\dot{q}) - \sum\left(\frac{dT_{\dot{q}}}{d\dot{q}}\delta\dot{q}\right)\right] - \sum\left(\frac{dT_{\dot{q}}}{dq}\delta q\right)$$

the terms within [ ] cut out and then we get

$$\frac{dT_p}{dp} = \dot{q} \text{ and } \frac{dT_p}{dq} = -\frac{dT_{\dot{q}}}{dq} \text{ which is trew.}^{(7)}$$

$$\frac{dp}{dt}\left(= Q - \frac{dT}{dq}\right)^{(8)}$$

(1) ULC Add. MSS 7655, I, b/44.

(2) Collop: a slice of meat (*OED*); see Number 407.

(3) See Number 407 note (4).

(4) J. L. Lagrange, *Mécanique Analytique*, ed. Joseph Bertrand, 2 vols (Paris, $_3$1853–5), **1**: 413, in Bertrand's note 'Sur les équations différentielles des problèmes de mécanique' (**1**: 409–22). T is the kinetic energy, q the variable.

(5) Thomson and Tait, *Natural Philosophy*: 217–19.

(6) $T_{\dot{q}}$, $T_{p\dot{q}}$, T_p denote the kinetic energy expressed in terms of the velocities and the variables, the momenta and the velocities, and the variables and the momenta, respectively. See the *Treatise*, **2**: 188–91 (§§560–63).

(7) See the *Treatise*, **2**: 191–2 (§564) and Number 419.

(8) Bertrand's equation (A) in his note 'Sur les équations différentielles des problèmes de mécanique': 413.

409

POSTCARD TO PETER GUTHRIE TAIT

14 MAY 1872

From the original in the University Library, Cambridge[1]

[Cambridge]

O T′ Collops[2] occurred to me (in private) in April 1869. Presented to Math. Soc. & read 9th May 1872.[3] Spottiswoode[4] refers me to Methodes de Transformation en Géometrie &c par M. J. N. Haton de la Goupillière – Journal de l'Ecole Poly. XXV cahier 42 §VII.[5] I have no time this week to look him up. The *occasion* of my collops was Lamé Coordonnées Curvilignes[6] where collops are not xcept in posse. I see you are well entered with vermin when you can draw obrok. Why do you spell distortion with a t. We shall have Tortion next which would be torsure to look at and extorsion on the font of T.

$\frac{dp}{dt}$

(1) ULC Add. MSS 7655, I, b/45.

(2) See Number 408 note (2).

(3) See Number 407 note (4).

(4) William Spottiswoode, President of the London Mathematical Society, who chaired the meeting on 9 May 1872; see the *Proceedings*, **4** (1872): 111.

(5) J. N. Haton de la Goupillière, 'Méthodes de transformation en géométrie et en physique mathématique', *Journal de l'École Impériale Polytechnique*, **25**, cahier 42 (1867): 153–204, esp. 188–96.

(6) Gabriel Lamé, *Leçons sur les Coordonnées Curvilignes et leurs Divers Applications* (Paris, 1859).

REPORT ON A PAPER BY GEORGE BIDDELL AIRY ON THE MAGNETIC PROPERTIES OF IRON AND STEEL

17 MAY 1872

From the original in the Library of the Royal Society, London(1)

REPORT ON A PAPER ENTITLED 'EXPERIMENTS ON THE DIRECTIVE POWER OF LARGE STEEL MAGNETS, OF BARS OF MAGNETIZED SOFT IRON, AND OF GALVANIC COILS, IN THEIR ACTION ON EXTERNAL SMALL MAGNETS'. BY GEORGE BIDDELL AIRY, ASTRONOMER ROYAL C.B. P.R.S.(2)

The investigation of the magnetic properties of iron and steel is exceedingly important, both for the advancement of science and for purposes of utility.(3) The phenomena presented by pieces of iron and steel when magnetized, demagnetized and remagnetized in various ways are comparable in point of intricacy to the phenomena presented by the same piece of iron when twisted, untwisted and retwisted, while at the same time the magnetic experiments may be repeated as often as we please without mechanical injury to the iron itself, such as takes place after many twistings and untwistings. Hence a method of determining the distribution of magnetization in a piece of iron or steel is of great scientific importance, since it is by such means that we may hope to obtain some knowledge of the molecular structure of these substances.

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(1) Royal Society, *Referees' Reports*, **7**: 152.

(2) George Biddell Airy, 'Experiments on the directive power of large steel magnets, of bars of magnetized soft iron, and of galvanic coils, in their action on external small magnets', *Phil. Trans.*, **162** (1872): 485–97. The paper was received by the Royal Society on 6 January 1872, and read on 8 February 1872; see the abstract in *Proc. Roy. Soc.*, **20** (1872): 158–9. The paper includes an 'Appendix containing an investigation of a galvanic coil on a small magnetic mass' by James Stuart (see Number 361). As a result of William Thomson's criticism in his referee report (see note (24)), Stuart revised and abbreviated his paper for publication: see the correspondence between Stokes, Airy and Stuart in June and July 1872 in Royal Greenwich Observatory Archive, ULC, Airy Papers 6/395, 145R–169R. Letters from Stokes to Airy of 10 and 13 December 1872 (Airy Papers 6/395, 180R–186R) explain how Stuart's revised appendix to Airy's paper had in error been read to the Royal Society as an independent paper on 5 December 1872 (and was to be published *in extenso* in *Proc. Roy. Soc.*, **21** (1872): 66–70), but that 'We have decided to reprint Mr Stuart's appendix that it may appear with your paper.'; see *Phil. Trans.*, **162** (1872): 493–6.

(3) See Numbers 466 and 467.

The method employed by our President appears to me to be remarkably well adapted to the particular case to which it was applied.(4) The direction and intensity of the magnetic force were ascertained at a number of points in two different surfaces completely surrounding the magnet. The graphical methods employed in dealing with the observations are also well worthy of study, as showing how results of amply sufficient accuracy may be obtained by means of easy processes with drawing instruments, and without the waste of calculating power, and risk of large errors, which often occurs in arithmetical processes.

I am therefore of the opinion that this paper should be printed in the Philosophical Transactions.

There are one or two points, however on which I should like to make a few remarks.

(1) The data given by the observations appear to me to furnish an excellent foundation for the application of what may be called the inverse method in the theory of attractions, that is the determination of the position and intensity of the attracting masses from the observation of the force at points in the surrounding space.

In the case before us two surfaces of revolution are described, surrounding the magnet at two different distances. In each of them, at intervals of $\frac{1}{10}$ of the length of the magnet, sections are taken, and the force determined at four points in each section. If we could devise or invent a mathematical function, the differential coefficients of which, with respect to the coordinates, would correspond to the forces at these points as found by experiment, then this would be the potential function of magnetic force for all space outside the magnet, and by *producing* the function inwards from the inner series of points towards the magnet, we might determine the force at the surface, and from this, since the distribution of imaginary magnetic matter is probably superficial, we could determine the density of this imaginary matter at every point of the surface.

But there is another method, which, like the method of this paper, is in the main graphical, and which would lead more directly to still more satisfactory results.

I shall first suppose that the magnetization is symmetrical about an axis as

(4) Compare William Thomson's judgment in his report on Airy's paper, dated 10 May 1872 (Royal Society, *Referees' Reports*, **7**: 151): 'The mode of measurement adopted, by a compass needle one inch long pivotted on a point, is not however capable of giving results of the accuracy desirable.... The results would, I believe, have been of much greater value if an exceedingly short needle hung by a silk fibre and carrying a light glass indicating arm, after the manner of Joule, had been employed.' On Joule's arrangement see Number 339 note (15).

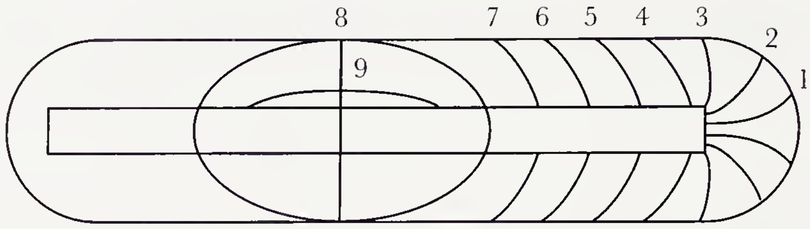


Figure 410,1

in a cylindrical magnet and then consider the modification, for a rectangular bar. The results given in the paper are sufficient to determine the resultant force at any point of the surfaces surrounding the magnet, and the resolved part of the force perpendicular to the surface.

By means of Simpsons Rule,[5] or otherwise, let the value of $\int N dS$ be calculated, where S is the area of the surface of revolution measured from one apex, and N is the resolved part of the magnetic force normal to the surface.

A table may thus be formed containing the values of $\int N dS$ for each position of the compass-centre. From this we may determine the positions of the compass-centre corresponding to any required number of equidistant values of $\int N dS$. Let 1 2 3 ... 8 be these points. Now take a *very short* magnetic needle, place its middle point at one of these points, and move it always in the direction of its own length, till it comes into contact with the magnet. It will thus trace out a *line of magnetic force*, the extremity of which should be marked on the magnet, or on the paper attached to it.

In this way a series of marks may be made on the magnet which will have the property, that the quantity of imaginary magnetic matter on a section of the magnet bounded by planes normal to its axis passing through two consecutive points of the series is the same for every such section.

Thus the distribution of magnetism may be determined for every part of the magnet except near its middle.

For this purpose it will be necessary to observe the longitudinal magnetic force at points on the line 89, perpendicular to the axis of the magnet, and to calculate $\int 2\pi r X dr$. Thus a series of points 9 &c may be found and from these lines of force may be drawn whose intersections with the surface of the magnet will determine the distribution of magnetism near its middle point. If the magnet is of rectangular section, the process must be applied separately to the longer and the shorter sides of the section.

(2) With respect to the history of the enquiry I think it may be noticed that Biot (Traité de Physique T.3 Ch 6) showed that the quantity of free magnetism in any section [of] a long thin magnet as determined by Coulombs experiments may be represented by the formula

$$A'(\mu'^{-x} - \mu'^{x})\, dx$$

(5) The method of determining the area under a curve by replacing the curve by a parabola, published by Thomas Simpson in his *Mathematical Dissertations* (London, 1743): 109–10.

where x is the distance of the section from the middle of the magnet and A' and μ' are constants.(6) ⌞See also D^r Kaspar Rothlauf 'Ueber Vertheilung des Magnetismus in Cylindrischen Stahlstaben' München 1861 also confirms this formula by induction experiments.⌟(7)

Green, in section 17 of his Essay on Electricity &c has given a very ingenious though faulty method of determining the distribution of the permanent magnetization of a cylinder originally magnetized to saturation, on the ordinary hypothesis about magnetic induction combined with that of a coercive force of constant value.(8)

Green finds the density (λ) of free magnetism per unit of length at a distance x from the middle point to be

$$\lambda = Apa\frac{e^{\frac{px}{a}} - e^{-\frac{px}{a}}}{e^{\frac{pl}{a}} + e^{-\frac{pl}{a}}}$$

where a is the radius of the cylinder, $2l$ its length, A a constant and p a quantity to be found from (Neumanns) coefficient of induction(9) from the equation

$$0.231863 - 2\log_e p + 2p = \frac{1}{\pi\kappa p^2}.^{(10)}$$

The whole quantity of free magnetism on the $+^{\text{ve}}$ side of the middle point is (when the cylinder is very long and thin) $\pi a A = M$.

Of this $\frac{1}{2}pM$ is on the flat end of the cylinder, and the distance of the centre of gravity of this free magnetism from the flat end is $\dfrac{a}{p}$.

When κ(11) is small (as in every substance except iron) p is large and the magnetism is almost all on the ends of the magnet. As κ increases, p diminishes and the magnetism extends further from the ends, as in iron. If κ is made infinite $p = 0$ and the magnetism at any section is simply proportional to the distance from the middle as in the hypothesis of p. 13* of the paper.(12)

(6) Jean Baptiste Biot, *Traité de Physique Expérimentale et Mathématique*, 4 vols. (Paris, 1816), **3**: 77. Maxwell states Biot's expression for free magnetism in the form as stated by George Green, *An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism* (Nottingham, 1828): 69, who gives the reference to Biot's *Traité* as stated by Maxwell.

(7) See also Kaspar Rothlauf, 'Bestimmung der magnetischen Vertheilung in cylindrischen Stahlstäben mittelst Magneto-Induction', *Ann. Phys.*, **116** (1862): 592–606.

(8) Green, *Essay on the Application of Mathematical Analysis*: 69; see Number 466. On coercive force see Number 442 esp note (5).

(9) See Number 327 note (4).

(10) Maxwell here, and in the next paragraphs, draws on his account in the *Treatise*, **2**: 68–9 (§439).

(11) Neumann's coefficient of magnetic induction: see Number 327 note (4).

(12) Airy, 'Experiments on the directive power of large steel magnets': 491–2; 'the supposition that the intensity of magnetism is proportional to the distance from the centre of the magnet'. P. 13* was a supplementary page inserted in the MS of Airy's paper.

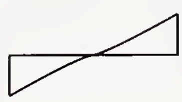
Greens method however is not perfect mathematically and indeed it leads to impossible results when κ is taken negative, as in diamagnetic substances. Thus[13]

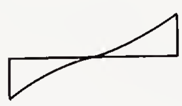
κ	p	κ	p
∞	0	11.8	0.07
62.02	0.02	9.1	0.08
48.42	0.03	7.5	0.09
29.48	0.04	6.3	0.10
20.18	0.05	0.1	1.00
14.79	0.06	0.0	∞
		negative	imaginary

Value of κ for soft iron from 30 to 33 by Thalèns experiments.[14]
Hence p is from $\frac{1}{25}$ to $\frac{1}{27}$.

Greens formula, however, represents very fairly the state of a long thin uniform cylinder magnetized to saturation by a powerful longitudinal force and then left to itself.

By comparing the observed values in the table at p. 13*[15] with those calculated on the supposition that the density of free magnetism is proportional to the distance from the middle point, and also with those for the galvanic coil it is easy to see that the magnetism of the real magnet is more confined to its extremities than that of the theoretical one but not so much as that of the galvanic coil.

1  Theoretical magnet of p13*
density of magnetism x

2  Green's formula
density of magnetism $e^{\frac{x}{a}} - e^{-\frac{x}{a}}$

3  Galvanic coil
magnetism at the ends

The practical value of investigations such as those of Green is greatly diminished by two results of experiment. In the first place the intensity of the

(13) See the table printed in the *Treatise*, **2**: 68 (§439).

(14) Tobias Robert Thalèn, 'Recherches sur les propriétés magnétique du fer', *Nova Acta Regiae Societatis Scientiarum Upsaliensis*, ser. 3, **4** (1863): esp. 36; see Number 327 note (4), and the *Treatise*, **2**: 54–5 (§430).

(15) Airy, 'Experiments on the directive power of large steel magnets': 492.

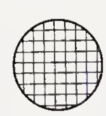
temporary magnetization produced in soft iron by a magnetic force, though nearly proportional to that force when it is feeble, ceases to be so when the force is considerable (such for instance as that employed to magnetize steel bars). Even in the simplest case the law assumes a complicated form, but if the iron has been previously subjected to magnetic action the effect of a new force depends not only on this force but on the whole previous history of the piece of iron.

In the second place the permanent magnetism of a steel bar is probably different, according as it is the result of the first magnetization of the bar, or the final result of several magnetizations and reversals of polarity. See the experiments of Wiedemann, Pogg. Ann C. p. 235 (1857).[16]

(3) It appears to me that the investigation by M^r^ Stuart of the magnetic action of a cylindrical coil[17] would be rendered more general as well as more easily intelligible by deferring the expansion in spherical harmonics to a later part of the investigation.

The magnetic potential of a circular current is expanded in a series of spherical harmonics, which agrees with that given in Thomson and Taits Natural Philosophy Art 546 (III).[18] To obtain the magnetic potential due to an assemblage of circular currents constituting a solenoid, the terms of the series have to be subjected to a process of double integration between limits, which involves a good deal of calculation, and in the case of most mathematicians, a considerable risk of error.

Now the case of a solenoid of any form of section may be treated as Ampère has done at p94 of his Théorie des Phenomèncs Electrodynamiques and at p188 of the same work.[19]

Break up each circuit into a great many elements, thus —  round each element let the current circulatc in the same direction. The circuit is thus transformed into a shell formed of elementary circuits. Now place such circuits one upon another so as to form a column. This column will be a column of solenoids of small section forming a solenoid of finite section, bounded by the two extreme sections and the coil of wire. The magnetic action at any point outside of the figure so bounded is, as Ampère has shown,

(16) Gustav Wiedemann, 'Ueber den Magnetismus der Stahlstäbe', *Ann. Phys.*, **100** (1857): 235–44.

(17) See note (2).

(18) Thomson and Tait, *Natural Philosophy*: 405–7, esp. 407; and see note (24).

(19) André Marie Ampère, *Théorie des Phénomènes Electro-dynamiques, uniquement déduite de l'expérience* (Paris, 1862): 94–108, 188–96 (esp. 95 for the term 'solénoide electro-dynamique'), and see Numbers 322 note (13) and 353 note (15).

identical with that of two uniform sheets of imaginary magnetic matter, forming the two extreme sections of the solenoid.(20) The numerical value of the surface-density of these sheets must be equal to the numerical value of the sum of the currents which flow across a line of unit length drawn on the surface of the coil parallel to its axis and if the column is vertical, and the current flows the way of the sun, the lower extremity must be coated with north-seeking magnetism.

The action of these two imaginary plane surfaces is identical with that of the coil for all points outside of the surface bounded by them and the coil.

For points within this closed surface, the resultant force due to the imaginary planes must be combined with a constant force parallel to the axis and equal to $4\pi\sigma$ where σ is the imaginary surface density on the sheet at the positive end of the axis.(21)

The magnetic force due to a solenoid of any form of section is thus identified with that due to two plane sheets.

If the solenoid consists of several layers of windings the corresponding plane sheets must be superposed, so as to make a sheet of variable density.

At this stage of the investigation we may introduce the expansion of the magnetic potential of the plane circular disk uniformly coated with imaginary magnetic matter. See Thomson & Tait §546.. II.(22)

$$V = \pi\sigma a\left\{\frac{a}{r} - \frac{1}{4}\frac{a^3}{r^3}Q_2 + \frac{1.3}{4.6}\frac{a^5}{r^5}Q_4 - \&c\right\}$$

where a is the radius and r the distance from the centre and Q_2, Q_4 &c are zonal harmonics, the argument of which is the angle between r & the axis.(23) If V' is the value of this expression when σ is made negative and when r is the distance of the given point from the centre of the negative end of the coil and Q the corresponding harmonic then the complete value of the magnetic potential outside the coil is

$$V+V'.$$

It is evident that in the case of a very long solenoid the magnetic force at any point in the plane of either end is directed towards the centre of that end, as if all the magnetism had been concentrated at the ends of the coil.

It is only with the hope that some of the work of calculation may be dispensed with that I have suggested any alteration in the order of M^r^ Stuarts

(20) See also the discussion by William Thomson, 'A mathematical theory of magnetism', *Phil. Trans.*, **141** (1851): 243–85 (= *Electrostatics and Magnetism*: 340–404); and Number 332 note (12).

(21) On the magnetic action of plane current sheets see the *Treatise*, **2**: 261–2 (§§652–3).

(22) Thomson and Tait, *Natural Philosophy*: 406.

(23) For Maxwell's similar discussion in the *Treatise* see *Treatise*, **2**: 280–1 (§676).

investigation. His result is of the first importance and it is expressed in a form which we may call simple in a case of such complexity.(24)

The statement at the end of p 11 does not seem quite correct as it stands. 'It is evident, from the remarks of Articles 6 & 7 that a magnet cannot in any wise be represented as a system of revolving galvanic currents'.(25) What is proved by experiment is that the magnetic force in the neighbourhood of a large steel magnet is distributed according to a different manner from that in the neighbourhood of a uniformly coiled solenoid. Ampère however has shown that for each particle of the magnet we may substitute a small circuit carrying a current which will produce on external points precisely the same effect as the magnetic particle.(26)

The combination of such particles into a mass will be a finite magnet and the result of combining the molecular currents will be a distribution of currents such that

Interior equations(27)	Surface equations
$4\pi u = \dfrac{dC}{dy} - \dfrac{dB}{dz}$	$4\pi U = mC - nB$
$4\pi v = \dfrac{dA}{dz} - \dfrac{dC}{dx}$	$4\pi V = nA - lC$
$4\pi w = \dfrac{dB}{dx} - \dfrac{dA}{dy}$	$4\pi W = lB - mA$

(24) Compare Thomson's judgment in his report (note (4)): 'The Appendix containing the investigation by Mr Stewart deals with a subject which has already been exhaustively worked out, and which belongs to the generally taught and known elements of electro-magnetism. See, for instance, Thomson & Tait's "Natural Philosophy" §546, in which a method closely resembling that of Mr Stewart, but more simple, is given.' On the circumstances of the publication of Stuart's 'Appendix' see note (2).

(25) Thomson commented: 'The remark...is not justified by the premises, inasmuch as the comparison is made by means of a single finite solenoid. In fact Ampère has proved in a complete manner that any distribution of magnetism whatever can be represented, so far as its external effects are concerned, by a distribution of galvanic currents.' In response to these comments Airy informed Stokes, in a letter of 3 July 1872 (Airy Papers 6/395, 162R–V) that 'My expression about the impossibility of representing a magnet by currents is, inadvertently too strong, and I am glad to have had attention called to it. I meant currents of equal strength and in uniform coils, such as those in the experimental coil. I shall look to this in Proof.' See Airy, 'Experiments on the directive power of large steel magnets': 490.

(26) Ampère, *Théorie des Phénomènes Electro-dynamiques*: 188–96; see also Maxwell's discussion in the *Treatise*, **2**: 419 (§833).

(27) See Maxwell's equations (E) for electric currents in the *Treatise*, **2**: 231 (§607).

where A, B, C are the rectangular components of the magnetization and $u\,v\,w$ those of the internal currents, & $U\,V\,W$ those of the surface currents $l\,m\,n$ being the direction cosines of the normal to the surface of the magnet.

In this way the action of any magnet whatever may be represented by that of a system of currents. I do not think that the statement in p. 11 is intended to deny this, though it seems at first sight to do so.

J. Clerk Maxwell
17 May 1872

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LETTER TO PETER GUTHRIE TAIT

24 MAY 1872

From the original in the University Library, Cambridge[1]

11 Scroope Terrace
24 May 1872

O. T′,

Yours received as per opposite page. With regard to my position in space we go to Glenlair (Dalbeattie) on Monday evening. Address so till further notice. With respect to the medal.[2] Colin Mackenzie W.S.[3] 28 Castle Street has charge of my worldly goods and will, I have no doubt, take the medal off your hands, and keep it till I see him.

I suppose the R S.E is now in a state of æstivation in its vacant intersessional cave or I should return it my thanks for its unlooked for bounty.

It is strange, not indeed that W. Weber could not correctly integrate

$$\int_0^\pi \cos\theta \sin\phi \, d\phi$$

where

$$\tan\theta = \frac{A\sin\phi}{B+A\cos\phi}$$

but that every one should have copied such a wild result as

$$\frac{B}{\sqrt{A^2+B^2}} \frac{B^4+\frac{7}{6}A^2B^2+\frac{2}{3}A^2}{B^4+A^2B^2+A^4}.^{(4)}$$

(1) ULC Add. MSS 7655, I, b/46.

(2) The Keith prize medal, awarded by the Royal Society of Edinburgh for Maxwell's paper 'On reciprocal figures, frames, and diagrams of forces', *Trans. Roy. Soc. Edinb.*, **26** (1870): 1–40 (= *Scientific Papers*, **2**: 161–207); see *Trans. Roy. Soc. Edinb.*, **26** (1870–72): viii.

(3) Maxwell's cousin and solicitor (see *Life of Maxwell*: x; and Volume I: xviii).

(4) Wilhelm Weber, 'Ueber den Zusammenhang der Lehre vom Diamagnetismus mit der Lehre von dem Magnetismus und der Elektricität', *Ann. Phys.*, **87** (1852): 145–89, on 167 (= *Wilhelm Weber's Werke*, 6 vols. (Berlin, 1892–4), **3**: 555–90). For Maxwell's discussion of the issue see the *Treatise*, **2**: 78n (§443), on Weber's expression for the relation between the intensity of magnetisation produced in a magnetisable substance and the magnetising force. See also Number 350.

Of course there are two forms of the result according as A or B is greater.(a)

11 Scroope Terrace
Cambridge
24 May 1872

Received from Professor Peter Guthrie Tait the sum of Forty seven Pounds nine shillings and one penny.

24 May 1872

£47..9′..1[d] J.C.M.

JAMES CLERK MAXWELL

(a) {Tait}

$$\int_0^\pi \frac{\sin\phi\,d\phi}{\sqrt{1+\left(\dfrac{A\sin\phi}{B+A\cos\phi}\right)^2}} = \int_0^\pi \frac{\sin\phi\,d\phi(B+A\cos\phi)}{\sqrt{B^2+A^2+2AB\cos\phi}}$$

$$\cos\phi = x, \text{ limits } -1, +1$$

$$\int_{+1}^{-1} \frac{-dx(B+Ax)}{\sqrt{B^2+A^2+2ABx}} = \int_{B+A}^{B-A} \frac{-\dfrac{1}{A}dy\,y}{\sqrt{(A^2-B^2)+2By}}$$

$$= -\frac{1}{2AB}\left(\frac{(2By+A^2-B^2-\overline{A^2-B^2})dy}{\sqrt{\quad}}\right.$$

$$= -\frac{1}{2AB}\left(\frac{1}{3B}(2By+A^2-B^2)^{\frac{3}{2}} - \frac{A^2-B^2}{2B}\sqrt{\quad}\right).$$

LETTER TO ROBERT DUNDAS CAY

27 MAY 1872

From the original in the Library of Peterhouse, Cambridge(1)

11 Scroope Terrace
Cambridge
27 May 1872

Dear Uncle Robert

I quite agree to your arrangement about the alterations and repairs to be paid by deducting £150 from the wayleave rents of this term and the same from the next, being £300 in all.

Your letter just arrived in time as we leave for Glenlair this evening. We shall be there, off and on, till we have to return to Cambridge in October.

I am to be 'Addle Examiner' on the new examination scheme next January(2) so I shall have plenty to do. This makes $3\frac{1}{2}$ Scotch examiners out of five, Ferrers being the $\frac{1}{2}$.(3)

The dissipations are now fiercely raging. Boat races being over balls begin, also flower shows processions, promenade concerts, peacocking on Kings Parade in splendour never seen in London out of doors, to say nothing of dinners and feasts ecclesiastical and secular, cleric and lay, wedded & celibate.

Katherine joins me in love to you all.

Your afft nephew
J. CLERK MAXWELL

---

(1) Peterhouse, Maxwell MSS (33).

(2) See *The Cambridge University Calendar for the Year 1873* (Cambridge, 1873): 26–8, for the regulations concerning the nomination by the Board of Mathematical Studies, and the subsequent appointment, of a third (Additional) examiner for the Mathematical Tripos.

(3) The other examiners were: William Davidson Niven (Trinity), George Pirie (Queens'), Norman Macleod Ferrers (Caius), and W. H. H. Hudson (St John's); see *Cambridge Calendar for 1873*: 521.

APPENDIX: THEOREM ON THE POTENTIAL FUNCTION FOR THE 1873 MATHEMATICAL TRIPOS[(4)]

circa SUMMER 1872

From the original in the Cavendish Laboratory, Cambridge[(5)]

[CONDITION OF MOTION IN THE INTERIOR OF A FLUID][(6)]

Shew that if within a certain region

$$\text{tr}\left\{\begin{array}{l}\dfrac{d^2V}{dx^2}+\dfrac{d^2V}{dy^2}+\dfrac{d^2V}{dz^2}=0\\[2ex] V \text{ is continuous and finite and satisfies the equation}\end{array}\right.$$

the quantity $R^2 = \left.\dfrac{dV}{dx}\right|^2 + \left.\dfrac{dV}{dy}\right|^2 + \left.\dfrac{dV}{dz}\right|^2$ cannot be a maximum at any point within the region.[(7)]

---

(4) See *Cambridge Calendar for 1873*: 548. 'Prove that / $\dfrac{d^2\psi}{dx^2}+\dfrac{d^2\psi}{dy^2}+\dfrac{d^2\psi}{dz^2}+4\pi\rho = 0$, / where ψ is the potential of a gravitating system and ρ is the mean density of a sphere whose centre is at the point (x, y, z) when its radius becomes infinitely small. / Prove that if ψ is continuous and finite, and satisfies the equation / $\dfrac{d^2\psi}{dx^2}+\dfrac{d^2\psi}{dy^2}+\dfrac{d^2\psi}{dz^2} = 0$, / throughout a given region, the quantity $\left.\dfrac{d\psi}{dx}\right|^2+\left.\dfrac{d\psi}{dy}\right|^2+\left.\dfrac{d\psi}{dz}\right|^2$ cannot have a maximum value at any point within the region.'

(5) Maxwell Notebook, Cavendish Laboratory, Cambridge; copy in ULC Add. MSS 7655, V, n/1.

(6) Compare Maxwell's discussion of conditions under which the potential function has a maximum and minimum value in fluid motion in his letter to William Thomson of 15 May 1855 (Volume I: 312–13).

(7) The potential function V cannot therefore be a maximum at a point in the interior of a fluid; the velocity of the fluid cannot be a maximum at any point in the fluid. See Horace Lamb's discussion of Maxwell's theorem (as set in the Mathematical Tripos, 1873) in his *A Treatise on the Mathematical Theory of the Motion of Fluids* (Cambridge, 1879): 39–40.

413

NOTE TO PETER GUTHRIE TAIT

circa LATE JUNE 1872[1]

From Knott, *Life of Tait*[2]

If your straight lines, parabolas[3] &c have no resemblance at all to those things which men call by those names, I would as soon be J. Stuart Mill as call them so. But if they differ very slightly, then T′ is enrolled among the Boyle and Charles of ΘH who remain unhurt by Regnault &c.[4] But in Physics we must equally avoid confounding the properties and dividing the substance. In the one case we fall into the sin of rectification (Eccl. i. 15)[5] and in the other we see in every zig zag a proof of transubstantiation.

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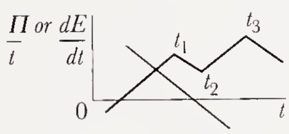
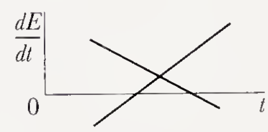
(1) Probably a reply to Tait's card of 11 June 1872 (see note (3)); and see his card of 28 June 1872 (see Number 414 note (2)).

(2) Knott, *Life of Tait*: 80.

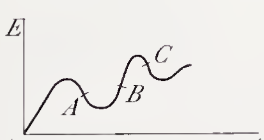
(3) In his card of 11 June 1872 (ULC Add. MSS 7655, I, a/27) Tait wrote: 'O dp/dt, Certain T.E. circuits give as below



instead of



the bits of the broken line being straight, and therefore



OA, AB, BC (in the expression for electromotive force) successive BITS of parabolas, all the axes being vertical. Does this not show that the wire giving the broken line is *one* substance from 0 to t_1, another from t_1 to t_2, yet another from t_2 to t_3 &^{c}? Answer this as soon as you have pondered it. Y^{rs} pv. / Have never yet got the length of C.M.'s office with the Medal. Am grinding away at these circuits all day in my Laboratory.' See P. G. Tait, 'On thermo-electricity: circuits with more than one neutral point', *Proc. Roy. Soc. Edinb.*, **7** (1872): 773–9, read 3 June 1872. E is the electromotive force, t the temperature, and Π the coefficient of the Peltier effect (see Number 322 esp. note (5)).

(4) For Tait's response see his card of 13 July 1872 (Number 417 note (2)). On Victor Regnault's work on the gas laws see Number 437 esp. note (8).

(5) Ecclesiastes chap. 1, verse 15; 'That which is crooked cannot be made straight: and that which is wanting cannot be numbered' (Authorised version).

414

POSTCARD TO PETER GUTHRIE TAIT

29 JUNE 1872

From the original in the University Library, Cambridge[1]

[Glenlair]

O T′.[2] That a metal should have any properties constant at various temperatures is an important discovery. That it should change these properties at certain temperatures is only to be expected. Exceptio probat regulam. But the investigation of these temperatures is important, especially if any other change of property takes place at them, e.g. resistance. I shall try and see Phil. Mag. for July.[3] Where are the Proofs! I shall send you shortly some remarks on T & T′ for next edition.[4] I have been overhauling the Equations of motion and have got a way of deducing them (in Hamiltons form) from the variables their velocities and the forces acting *on them* alone,[5]

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(1) ULC Add. MSS 7655, I, b/47.

(2) In reply to Tait's cards of 11 June 1872 (Number 413 note (3)), and of 28 June 1872 (ULC Add. MSS 7655, I, a/28): 'O dp/dt See next Phil. Mag., and say what I deserve for fighting *your* & T's battles. U don't answer about multiple neutral points. As you have reserved your oracular decision (only, I hope, till as now pressed to give it) add a few remarks on this theme. The curves give electromotive force in terms of diff^{ces} of temp.^{re} of junctions in circuits of iron with Platinum & diff^{t}. alloys of Plat. & Iridium. The latter can be plotted from the former by subtracting the ordinates of an oblique line drawn through the origin – i.e. the additions of Iridium seem to alter the neutral points without much altering the Kays (ks). .' The 'battles' Tait alludes to were with Clausius, specifically with Clausius' critique of Maxwell's *Theory of Heat*: see Numbers 402 and 403.

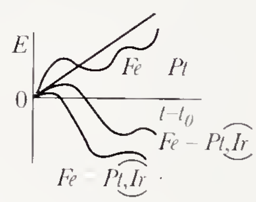

Figure 414,1

(3) In the Supplement to the *Philosophical Magazine* for July 1872 Tait published a paper 'On the history of the second law of thermodynamics, in reply to Professor Clausius', *Phil. Mag.*, ser. 4, **43** (1872): 516–18. Responding to Clausius' complaint about Maxwell's *Theory of Heat* (see Number 402, note (10)), Tait had published (in the May 1872 number of the *Phil. Mag.*) a 'Reply to Professor Clausius', *Phil. Mag.*, ser. 4, **43** (1872): 338. To contest Clausius' claims to priority over Thomson he introduced the problem of 'The behaviour of a thermoelectric circuit in which the hot junction is at a temperature higher than the neutral point, and where therefore heat *does of itself, pass from a colder to a hotter body*'. Clausius had discussed the problem of thermocouples in his paper 'Ueber die Anwendung der mechanischen Wärmetheorie auf die thermoelektrischen Erscheinungen', *Ann. Phys.*, **90** (1853): 513–44; and in his paper 'On the objection raised by Mr Tait against my treatment of the mechanical theory of heat', *Phil. Mag.*, ser. 4, **43** (1872): 443–6 (in the June 1872 number) he was able to meet Tait's argument. Tait's reply in the Supplement to the July number of the *Phil. Mag.* was to reiterate the claim that Thomson had first *correctly* formulated the second law of thermodynamics. See also Number 278 note (2).

(4) See Number 417 note (4).

(5) See the *Treatise*, **2**: 184–94 (§§553–67).

without considering the equations which give $\dot{x}$ in terms of $\dot{\phi}$, $\dot{\psi}$, or Φ in terms of X &c.[6] This is done by beginning with impulsive force.[7] It constitutes an improvement in my book and a preparation for Electrokinetics[8] and Magnetic action on Light.[9] Pray observe the dimensions of this card. Proove me my prooves and again I say reproove me my reprooves.

$$\frac{d}{dt}\frac{dT_p}{d\dot{q}}+\frac{dT_p}{dq}=\frac{dp}{dt}^{(10)}$$

(6) See Thomson and Tait, *Natural Philosophy*: 218–19; x, y, z are rectangular coordinates, $\dot{x}$, $\dot{y}$, $\dot{z}$ are component velocities; ψ, ϕ, θ are generalised coordinates of a material system, $\dot{\psi}$, $\dot{\phi}$, $\dot{\theta}$ are generalised velocity components; Ψ, Φ, Θ are generalised components of the force on the system, X, Y, Z are component forces of particles x, y, z.

(7) On the method of impulsive forces see Thomson and Tait, *Natural Philosophy*: 217–31.

(8) *Treatise*, **2**: 195–205 (§§568–77).

(9) *Treatise*, **2**: 399–417 (§§806–31).

(10) Compare Number 417.

415

REPORT ON A PAPER BY LATIMER CLARK[1] ON A STANDARD OF ELECTROMOTIVE FORCE

circa 2 JULY 1872[2]

From the original in the Library of the Royal Society, London[3]

REPORT ON Mr LATIMER CLARK'S PAPER ON A VOLTAIC STANDARD OF ELECTROMOTIVE FORCE[4]

The Author describes his search for a Voltaic Element whose Electromotive Force shall be, under circumstances easily obtainable, as constant as possible. He has obtained such an element by placing a paste composed of mercurous sulphate and zincic sulphate between pure mercury and pure zinc. He has compared together a number of such elements by means of an arrangement which is apparently the best hitherto published for comparing electromotive forces. The merit of this arrangement seems to me to be due to the Author. He calls it a Potentiometer.[5]

This comparison having established the constancy of his standard element, the author has gone on to determine the value of its electromotive force in the electromagnetic system of measurement, introduced by W. Weber[6] and adopted, with slight modifications, by the British Association.[7] He has made this determination by means of the electrodynamometer constructed for the

(1) Josiah Latimer Clark, electrical engineer (Boase).

(2) According to the Royal Society's *Register of Papers Received* Clark's paper was referred to Maxwell on 28 June 1872, and to Wheatstone on 5 July 1872.

(3) Royal Society, *Referees' Reports*, **7**: 162.

(4) Latimer Clark, 'On a voltaic standard of electromotive force' (Royal Society, AP. 54.4). The paper was received by the Royal Society on 30 May 1872, and read on 20 June 1872; see the abstract in *Proc. Roy. Soc.*, **20** (1872): 444–8. The paper was communicated to the Royal Society by Sir William Thomson: see his letter to Stokes of 25 May [1872] (ULC Add. MSS 7656, K 184A; printed in Wilson, *Stokes–Kelvin Correspondence*, **2**: 371–2). An abridged version of Clark's paper, under the title 'On a standard voltaic battery', was subsequently published in *Phil. Trans.*, **164** (1874): 1–14; see note (12) and Number 462.

(5) See also Latimer Clark, *An Elementary Treatise on Electrical Measurement* (London, 1868): 106–8.

(6) W. Weber, 'Messungen galvanischer Leitungswiderstände nach einem absoluten Maasse', *Ann. Phys.*, **82** (1851): 337–69; (trans.) 'On the measurement of electric resistance according to an absolute standard', *Phil. Mag.*, ser. 4, **22** (1861): 226–40, 261–9.

(7) 'Report of the Committee... on standards of electrical resistance', *Report of the Thirty-third Meeting of the British Association for the Advancement of Science* (London, 1864): 111–76.

Committee of the British Association[(8)] and also by means of a Sine Galvanometer of his own construction.[(9)] Both of these determinations seem to have been conducted with every precaution, and the results agree in a very satisfactory manner. The electromotive force of the standard cell was found to be 1.457 Volts[(10)] each Volt being 10^5 in the metric-gramme-second electromagnetic system.

It remains, of course, to be seen whether other persons, by following the authors directions, can obtain a standard element agreeing within one tenth per cent. with that of the author, but the experiments cited are sufficient to show that he has invented a most valuable material standard, and that he has submitted it to proper tests.[(11)]

I am therefore of opinion that this paper should be printed in the Philosophical Transactions.[(12)]

M[r] Clark was one of the earliest, and has remained one of the most persistent advocates of the adoption of electrical standards.[(13)] He has also been willing to adopt standards founded on considerations which, though

(8) See Number 416 note (3).

(9) See Latimer Clark, 'On a standard voltaic battery': 11–12. On sine galvanometers see Gustav Wiedemann, *Die Lehre vom Galvanismus und Elektromagnetismus*, 2 vols. (Braunschweig, 1861), **2**: 206–10.

(10) See also Clark, 'On a standard voltaic battery': 12.

(11) Clark acknowledged Maxwell's assistance in 'On a voltaic standard of electromotive force': ff. 30, 33, 40; and see Clark, 'On a standard voltaic battery': 9, 10, 12 for the same acknowledgments.

(12) In a report dated 3 September 1872 (Royal Society, *Referees' Reports*, **7**: 161) Sir Charles Wheatstone voiced a different opinion: 'I think it unnecessary to discuss the merits of M[r] Latimer Clark's paper as it, in my opinion, belongs to a class not suited for publication in the Philosophical Transactions. It contains little, or nothing, that has not already been known to the scientific public, and the additional details now brought forward do not appear to me to be of sufficient importance to give it that claim.... This paper might properly find a place in a special technical publication'. In a letter to Stokes of 1 January 1873 (ULC Add. MSS 7656, K 189; printed in Wilson, *Stokes–Kelvin Correspondence*, **2**: 380) Thomson asked if the Royal Society's consequent rejection of Clark's paper for publication in the *Phil. Trans.* was 'an irreversible decree'. In a subsequent letter of 21 January [1873] (ULC Add. MSS 7656, K 182; in Wilson, *Stokes–Kelvin Correspondence*, **2**: 396–7) Thomson states that he had forwarded one of Stokes' letters to Latimer Clark, 'as I believe you intended that I might do so', also noting that 'in one or two parts Clark['s] paper may admit of abbreviation with advantage'. In a memorandum for Stokes of 23 January 1873 (ULC Add. MSS 7656, RS 900) Clark responded to the objections raised to his paper; and subsequently submitted an abridged version to the Royal Society (see note (4)).

(13) Latimer Clark and Sir Charles Bright, 'On the formation of standards of electrical quantity and resistance', *Report of the Thirty-first Meeting of the British Association for the Advancement of Science* (London, 1862), part 2: 37–8.

truly scientific, are by no means obvious to the practical electrician. He has advocated the use of distinct short names for certain multiples or submultiples of these standards chosen so as to be of a convenient magnitude for practical work. Of these the Ohm, or practical standard of electric resistance, is represented by a wire having this resistance. The Farad, or standard of capacity, is represented by a condenser of this capacity.

The Volt, or standard of electromotive force is represented roughly by a Daniell cell,(14) or more accurately by $\frac{1}{1.457}$ of M^r^ Clarks new cell.

These three standards, therefore, are represented by material objects which the electrician may have always beside him, ready for use in electric measurements. There is, however, a fourth term introduced by M^r^ Clark to denote the quantity of electricity with which a Volt would charge a Farad, or which a Volt would send through an Ohm in one second. This quantity he calls a Veber.

I am by no means prepared to say that it would not be desirable to have a short name for a standard quantity of electricity, for the other three terms have been found more acceptable than I expected they would become. This last standard, however, is invisible, and cannot well be kept in a laboratory. We have also to consider whether we do honour to the name of the founder of electromagnetic measurement by cutting off one half of the initial letter of his name. It would be far better to pronounce the W in the English way than to transpose the word to a wrong division of the Dictionary.(15)

The system to which all these standards belong is the Electromagnetic system. There is another mode of obtaining a standard of electromotive force by electrostatic methods, and this has been developed to a great extent by Sir W. Thomson in his various Electrometers.(16) The construction, however, of a standard electrometer, and the reduction of its readings to absolute measure, is an operation of great labour and difficulty.

The discovery, therefore of a *small* standard of electromotive force is important to electrostatics, as enabling us to test the value of the scale-readings of any electrometer.

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(14) The electric battery which maintained a constant current invented by J. F. Daniell; see Number 235 note (11).

(15) In 'On a standard voltaic battery': 1 Clark omitted the 'Veber' from the list of units as given in 'On a voltaic standard of electromotive force': f. 2.

(16) William Thomson, 'Report on electrometers and electrostatic measurements', *Report of the Thirty-seventh Meeting of the British Association for the Advancement of Science* (London, 1868): 489–512 (= *Electrostatics and Magnetism*: 260–309).

Besides this, the determination of electromotive force of the new cell in electrostatic measure, compared with the value in electromagnetic measure, would lead to an independent value of the ratio of the electromagnetic to the electrostatic unit of electricity.

We must bear in mind, however, that the numerical value found by Mr Clark is affected by any error in the experiments by which the value of the Ohm was determined.(17)

J. Clerk Maxwell

(17) See the 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-fourth Meeting of the British Association for the Advancement of Science* (London, 1865): 345–67, esp. table facing 349.

416

LETTER TO GEORGE GABRIEL STOKES

8 JULY 1872

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
8 July 1872

My dear Stokes

The figures in Mr Latimer Clarks paper[2] are better than anything I could do. If therefore I can obtain the consent of the Society and of Mr Clark, I should like to obtain clichés of the engravings of the Electrodynamometer and its parts,[3] and in doing so I have supposed it best to begin with the Society for though I am acquainted with Mr Clark I have no direct relation with him in the capacity of referee of his paper.

I had already drawn figures, not so good as Mr Clarks, but they are not yet engraved, so that if there is any objection to my getting either the loan of the blocks or plates or impressions of them, my work will not be delayed.[4]

As some of the figures are already engraved I should think it likely that Mr Clark means to use them in some work of his own and that they have been engraved independently of the R.S. though they may first appear as illustrations in the Phil Trans.

With respect to the time at which I should have to send my last figures to the engraver I think the end of September is about the limit of safety.

Yours sincerely
J. CLERK MAXWELL

(1) ULC Add. MSS 7656, M 431. First published in Larmor, *Correspondence*, **2**: 33.

(2) 'On a voltaic standard of electromotive force'; see Number 415, esp. note (4).

(3) The engravings of the electrodynamometer and its torsion head, constructed for the British Association Committee on standards of electrical resistance, and referred to in the 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-seventh Meeting of the British Association for the Advancement of Science; held at Dundee in September 1867* (London 1868): 474–522, on 478, were printed in Latimer Clark's paper 'On a standard voltaic battery', *Phil. Trans.*, **164** (1874): 1–14, on 7–8. See Maxwell's report on this paper (Number 462).

(4) See the *Treatise*, **2**: 330–1 (§725), where Clark's drawings are reproduced.

417

POSTCARD TO PETER GUTHRIE TAIT

15 JULY 1872

From the original in the National Library of Scotland, Edinburgh(1)

[Glenlair]

(2)Let xyz be the currents from A to Z in 3 conductors
PQR the electromotive forces in these conductors
$a\,b\,c$ their resistances – then

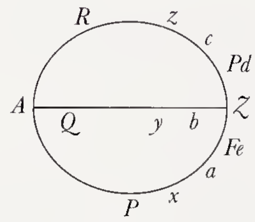

Figure 417,1

$$ax-P = by-Q = cz-R = E \text{ (say)}$$
$$x+y+z = 0$$
$$\therefore x = \frac{P-E}{a} \quad y = \frac{Q-E}{b} \quad z = \frac{R-E}{c}$$
$$e = \frac{\frac{P}{a}+\frac{Q}{b}+\frac{R}{c}}{\frac{1}{a}+\frac{1}{b}+\frac{1}{c}} \quad \text{and} \quad x = \frac{1}{a}\frac{P\left(\frac{1}{b}+\frac{1}{c}\right)-Q\frac{1}{b}-R\frac{1}{c}}{\frac{1}{a}+\frac{1}{b}+\frac{1}{c}}. \qquad (1)$$

Let x_c be the value of x when $b = \infty$ and x_b that when $c = \infty$ or when the second or third conductor is cut then by (1)

$$x_c = \frac{P-R}{a+c} \quad x_b = \frac{P-Q}{a+b} \text{ whence}$$
$$x = \frac{x_b\left(\frac{1}{a}+\frac{1}{b}\right)+x_c\left(\frac{1}{a}+\frac{1}{c}\right)}{\frac{1}{a}+\frac{1}{b}+\frac{1}{c}}$$

or
$$x = c\frac{x_b(a+b)+x_c(a+c)}{bc+ca+ab}.$$

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(1) National Library of Scotland, MS 1004 f. 41.

(2) For the context see Numbers 413 and 414. The card was written in reply to Tait's card of 13 July 1872 (ULC Add. MSS 7655, I, a/29): 'Pray dp/dt give me *again, at once*, and *in complete form*, your solution of this thermo-electric case. Either I have made an exceedingly stupid blunder, or I have made an exceedingly great discovery: & I wish to know *which*. This can only be done by my *not* telling you the result I have arrived at. The experiments are all made, & graphically delineated, so that there can be no mistake about *them*. Given, separately, the resistances of the Zn, the Cu, & the Fe (the latter including that of the galvanometer) calculate the current when the arrangement is complete from the observed currents when the zinc, & the copper, wires are (separately) cut. T′.'

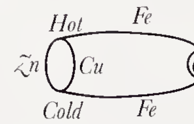


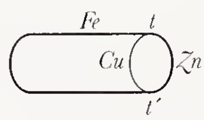
Figure 417,2

I can do no more to this.(3)

The binder of T & T′ has observed the maxim One stitch in nine saves time. See what a broad P.C. I have written to you with a new pen. I must go now to a swarm of bees in a yew tree. What of the $=^{\text{ns}}$ of motion?(4)

$$F+\frac{dT_{\dot{q}}}{dq}=\frac{dp}{dt}^{(5)}$$

(3) In his card of 16 July 1872 (ULC Add. MSS 7655, I, a/31) Tait replied: 'dp/dt thou that shirkest the question behold it *again*.



I don't require to be taught how to work resistances, and simple equations. My purpose was more awful. *What are* the electromotive forces in the three wires respectively, in terms of what they w.d be were each of the three wires separately cut? ꟼ.' In his card of 17 July 1872 (ULC Add. MSS 7655, I, a/32) Tait responded: 'O dp/dt I fear your Algebra, as well as your Reasoning, is in fault. You give me $x=c\dfrac{x_b(a+b)+x_c(a+c)}{bc+ca+ab}$. I had found (putting it into your notation) $x=\dfrac{x_b\,c(a+b)+x_c\,b(a+c)}{bc+ca+ab}$. But what I wanted to know from you was *why* there sh.d be any Fe–Zn electromotive force in the Cu branch of the triple arc. Goose that.— Y.rs ꟼ.' In a card of 19 July 1872 (ULC Add. MSS 7655, I, a/33) Tait added: 'O dp/dt I have proven today by experiment what I had lately been led to suspect was the true cause of double neutral points. It is the villain Fe, who is thermo-electrically $(-)$ as regards his σ up to a pretty high temperature & then suddenly gets his σ made $(+)$. I shot at him from the neutral point of Au–Pd, with double arc sometimes leaning to one side, then to the other: & here is the result.

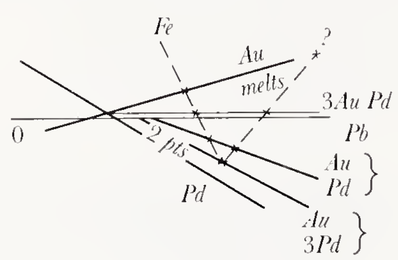


Figure 417,3

So you see my shots have taken effect. What say you to this on the firm ground of theory and of hypothesis? Also answer my other?. pv.'

(4) See Number 414. In a card of 15 July 1872 (ULC Add MSS 7655, I, a/30) Tait wrote: 'O dp/dt I return to you χηding ϑanξ for the νῶτς on T & T′. But don't claim originality till you can prove your case – vide *Phil. Mag.* for July (*Suppl.*) & see how Clausius fares after his many years boasting. Have you seen the German ed.n of T & T′ where Hamiltonianism & Lagrangism are remodelled on the advice of Strutt & Boltzmann – the former given almost immediately after the appearance of the Book, the latter two years ago? *I also* proposed to T. two years ago a method which is like as two peas to yours, but he would none of it – and sent his own to the Germans; or, rather, compelled me to do so – of course under violent protest, w.h may perhaps break out in the second (English) edition now preparing. To my χτρημ delight I caught

APPENDIX: POSTCARD TO PETER GUTHRIE TAIT

7 AUGUST 1872

From the original in the University Library, Cambridge(6)

[Glenlair]

O T′. Are you in the secular capital or in the metropolis? I address(7) to the latter.

Yours $\dfrac{dp}{dt}$

(by pure chance) on Saturday last a French nobleman who has shown me how to deposit (electrolytically) tenacious *feuilles* of Nickel, Cobalt, &c &c. I had made out for myself that morning the position of Nickel. It is as below. The question I asked you on Saturday really means 'Can we by 3 metals (in treble arc) sweep the field in abridged notation style? i.e. from $\alpha = 0$, $\beta = 0$, $\gamma = 0$, get by combination $A\alpha = B\beta = C\gamma = 0$?' If I am right I can take A, B, & C so as to *aim* at any point I like, and thus thoroughly investigate the mysterious course of the queer alloy. ϛ̂.' See Tait's paper 'On thermo-electricity: circuits with more than one neutral point', *Proc. Roy. Soc. Edinb.*, **7** (1872): 773–9.

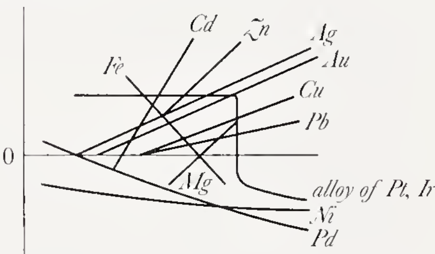


Figure 417,4

(5) The Lagrangian form of the equations of motion. See Number 419 and the *Treatise*, **2**: 192 (§564).

(6) ULC Add. MSS 7655, I, b/48.

(7) Proofs; to the 'Union Club, St Andrews'.

418

LETTER TO LATIMER CLARK

16 JULY 1872

From the original in the University Library, Cambridge[1]

Glenlair
Dalbeattie
16 July 1872

My dear Sir

I had lately the pleasure of reading your paper on a Standard Voltaic Element.[2] I shall certainly not be satisfied with merely reading about it, for I must make a nearer acquaintance with it.

I observed that you had engraved a figure of the Electrodynamometer[3] and my present object in writing to you is to ask if it would be convenient to you to allow me to have the block or a copy of it as an illustration to my book on Electricity.[4]

I had already made a diagram of it from memory, but a figure of this kind is unsatisfactory compared with a figure of an actual instrument.[5]

The experimental researches in your paper are of course known to me only by your report of them, but I think they are as good a piece of work as I have seen for some time.

The best test of the uniformity of your voltaic element would be to get it set up by people in different places, where they would get zinc from different sources and would differ slightly in their mode of preparing the mercurous sulphate. The use of mercury as the negative metal is a great merit, not only because the negative surface remains fluid and of uniform properties but because its effects when it gets in contact with the zinc are so different from what happens in the case of copper.

I have looked over all the formulae for the dynamometer and find them correct. I have not attacked the arithmetic.

I have taken the opportunity of this vacation to boil down a good deal of matter relating to electrical instruments which was in my book before, but it is now far more digestible.

I think I am right in saying that you use the word Farad to signify a certain capacity and that a Farad is a condenser having this capacity.[6]

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(1) ULC Add. MSS 7655, II/59.

(2) See Number 415.

(3) See Number 416 note (3).

(4) See the *Treatise*, **2**: 330–1 (§725).

(5) See Number 416.

(6) See Number 415.

Prof. Jenkin speaks of a Farad as a quantity of electricity.(7)

I think it is better to define it as a capacity as I think you do and then the Ohm is a piece of wire, the Volt a voltaic element (or a multiple thereof) and a Farad is a condenser.

These are all visible things.

Yours very truly
J. CLERK MAXWELL

(7) Fleeming Jenkin, *Electricity and Magnetism* (London, 1873): 160. On Maxwell's prior knowledge of Jenkin's book see his annotation to its proof (Number 385: Appendix).

DRAFT ON THE INTERPRETATION OF LAGRANGE'S AND HAMILTON'S EQUATIONS OF MOTION

circa JULY 1872[1]

From the original in the University Library, Cambridge[2]

ON THE INTERPRETATION OF LAGRANGE'S AND HAMILTON'S EQUATIONS OF MOTION

by J. Clerk Maxwell, Prof^r of Experimental Physics, Cambridge[3]

The following statement has nothing original in it,[4] but I think that our attention cannot be too often directed to the most important theorem in physical science – that which deduces, from the given motion of a connected system, the forces which act on it.

Our popular dynamical ideas are far too exclusively drawn from the dynamics of a particle. It is true that the most important ideas in dynamics may be illustrated by the motion of a single particle, but it is unfortunate that in expressing the relations between these ideas we have sometimes adopted a form of expression, which, though true for a particle, is not easily applicable to a connected system.

For instance, if, after defining velocity and momentum, we have to define the kinetic energy of a particle, we may do it in three different ways, thus,

$$\text{Twice the kinetic energy is} = \text{mass} \times \text{square of velocity}$$
$$= \frac{\text{square of momentum}}{\text{mass}}$$
$$= \text{velocity} \times \text{momentum.}$$

Of these three definitions, the first, which is the common form, involves the unimaginable concept of the square of a velocity, the second involves the equally unimaginable square of the momentum, whereas the factors in the third definition are both of them quantities of which we can form a distinct

(1) See Numbers 414 and 417; and see note (4).

(2) ULC Add. MSS 7655, V, e/9.

(3) Compare Maxwell's paper 'On the proof of the equations of motion of a connected system', *Proc. Camb. Phil. Soc.*, **2** (1873): 292–4, read 3 February 1873 (= *Scientific Papers*, **2**: 308–9). See also the *Treatise*, **2**: 184–94 (§§563–67).

(4) Compare Tait's comment in his card of 15 July 1872 (Number 417 note (4)).

idea. They are both vectors. In particle-dynamics they are coincident in direction. In the dynamics of a system their directions are, in general, different, and the multiplication must be performed on Hamiltonian principles, and the scalar part taken. In every case the result is a scalar quantity, the kinetic energy of the system.

In the case of a system having n degrees of freedom let

$q_1\ q_2 \ldots q_n$ be those variables on which the position of the system depends.
$\dot{q}_1\ \dot{q}_2 \ldots \dot{q}_n$ the velocities of those variables, that is to say the rate at which they increase
$p_1\ p_2 \ldots p_n$ the momenta of these variables, that is to say the impulses necessary to produce the actual motion.

Then T, the kinetic energy, may be *defined* as

$$T = \frac{1}{2}(p_1\dot{q}_1 + p_2\dot{q}_2 + \ldots + p_n\dot{q}_n). \tag{1}$$

But the quantities $p_1 \ldots p_n$ are homogeneous linear functions of $\dot{q}_1 \ldots \dot{q}_n$ so that T may be expressed in two other forms

$$T_{\dot{q}} = \frac{1}{2}P_{11}\dot{q}_1^2 + \frac{1}{2}P_{22}\dot{q}_2^2 + \&\text{c} + P_{12}\dot{q}_1\dot{q}_2 + \&\text{c} \tag{2}$$

$$T_p = \frac{1}{2}Q_{11}p_1^2 + \frac{1}{2}Q_{22}p_2^2 + \&\text{c} + Q_{12}p_1p_2 + \&\text{c}. \tag{3}$$

Of these two expressions for the kinetic energy, the first, $T_{\dot{q}}$ is that employed by Lagrange, the second, T_p was introduced by Hamilton.

The coefficients P_{11} &c in which the two suffixes are the same may be called moments of inertia and the coefficients P_{12} &c in which the suffixes are different may be called products of inertia.(5)

When the product of inertia corresponding to a given pair of variables is zero, these variables are said to be *conjugate* to each other.

In like manner we may call the coefficients Q_{11} &c the moments of mobility, and Q_{12} &c the products of mobility.

The system of coefficients P is inverse to the system Q.

We can also deduce p_r from $T_{\dot{q}}$ and $\dot{q}_r$ from T_p.(6)

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(5) See the *Treatise*, 2: 193 (§565).

(6) The two main sources for Maxwell's discussion are (see *Treatise*, 2: 184n (§553)): Arthur Cayley, 'Report on the recent progress of theoretical dynamics', in the *Report of the Twenty-seventh Meeting of the British Association for the Advancement of Science* (London, 1858): 1–42, esp. 2, 12, 14–15 (on the dynamical equations of Lagrange and Hamilton); and Thomson and Tait, *Natural Philosophy*: 217–31. See also Joseph Bertrand's note 'Sur les équations différentielles des problèmes de mécanique' in his edition of Lagrange's *Mécanique Analytique*, 2 vols. (Paris,

$$(4)\qquad p_r = \frac{dT_{\dot{q}}}{d\dot{q}_r} \qquad\qquad \dot{q}_r = \frac{dT_p}{dp_r} \qquad (5)$$

$$(6)\qquad P_{rs} = \frac{d^2T_{\dot{q}}}{d\dot{q}_r\,d\dot{q}_s} \qquad\qquad Q_{rs} = \frac{d^2T_p}{dp_r\,dp_s} \qquad (7)$$

$$\frac{dT_{\dot{q}}}{dq_r}+\frac{dT_p}{dq_r} = 0. \qquad (8)$$

The final equation as given by Lagrange, is

$$F_r = \frac{dp_r}{dt}-\frac{dT_{\dot{q}}}{dq_r}.^{(7)} \qquad (9)$$

As given by Hamilton it is

$$F_r = \frac{dp_r}{dt}+\frac{dT_p}{dq_r}.^{(8)} \qquad (10)$$

It is of great advantage to the student to be able to connect the equations as they thus stand with easily remembered dynamical ideas. The first term on the right hand of each equation expresses the fact that part of the force is expended in increasing the momentum p. The second term indicates that if the increase of the variable q has a direct effect in increasing the kinetic energy, a force will arise from this circumstance. According to Lagranges expression it would appear as if the kinetic energy had a tendency to increase, and to do work as it increases. This arises from the fact that the kinetic energy is expressed in terms of the velocities. Now it is not the velocities which obey Newton's law of persevering in their actual state, but the momenta or 'quantities of motion'. Hence if we wish to apply Newtons law we must express the kinetic energy in terms of the momenta and use Hamiltons form of the equations of motion.(9) We then see at once that the second term indicates that if a given displacement has a direct tendency to increase the kinetic energy, the momenta remaining the same, a quantity of work, equal to this increase of kinetic energy is performed by the external force F during the displacement.

We may give a still simpler form to Hamiltons equation by taking as the

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3 1853–5), **1**: 409–22 (see Number 408); Part II, Section IV of the *Mécanique Analytique*: 282–98; W. R. Hamilton, 'On a general method in dynamics', *Phil. Trans.*, **124** (1834): 247–308, esp. 260–2; and Hamilton, 'Second Essay on a general method in dynamics', *Phil. Trans.*, **125** (1835): 95–144, esp. 96–8.

(7) As in the *Treatise*, **2**: 192 (§564).

(8) As in the *Treatise*, **2**: 190 (§561).

(9) See P. M. Harman, 'Newton to Maxwell: the *Principia* and British physics', *Notes and Records of the Royal Society*, **42** (1988): 75–96, esp. 87–8.

displacement the actual displacement $\partial q = \dot{q}\partial t$ which takes place in the time ∂t. The equation then becomes an expression for the work done by the force F during this time.

$$\partial W_r = F_r \partial q = \dot{q}_r \partial t \frac{dp_r}{dt} + \frac{dT_p}{dq_r}\partial q_r \tag{11}$$

but since $\dot{q}_r = \dfrac{dT_p}{dp_r}$ we may write this

$$F_r \partial q_r = \frac{dT_p}{dp_r}\partial p_r + \frac{dT_p}{dq_r}\partial q_r. \tag{12}$$

Now the total work done by the external forces in the time ∂t is $F_1 \partial q_1 + F_2 \partial q_2 + \&c$ and the total increment of kinetic energy is

$$\left(\frac{dT_p}{dp_1}\partial p_1 + \frac{dT_p}{dq_1}\partial q_1\right) + \left(\frac{dT_p}{dp_2}\partial p_2 + \frac{dT_p}{dq_2}\partial q_2\right) + \&c.$$

The equality of these two quantities is expressed by the principle of the conservation of energy.

But the Hamiltonian equations enable us to assert that the terms composing the first quantity are equal to the corresponding terms of the second quantity, each to each, or

$$F_1 \partial q_1 = \frac{dT_p}{dp_1}\partial p_1 + \frac{dT_p}{dq_1}\partial q_1 \tag{13}$$

or in words

The work done by the force F_1 during the actual motion of the system in the time ∂t, is equal to the part of the actual increment of T_p which is due to the increment of the momentum p_1 and to the increment of the variable q_1.

Thus the increment of the kinetic energy is, by means of Hamilton's equations, divided into a number of parts, each of which is traced to the action of a particular force.

LETTER TO WILLIAM THOMSON

10 AUGUST 1872

From the original in the University Library, Cambridge(1)

Glenlair
Dalbeattie
10 Aug 1872

O T

I enclose an interesting account of M[r] & M[rs] Brown of Charles Street, Windsor particularly the latter, whose patient observations of the fire ball deserve all praise.(2)

Tell the committee of Electric Standards to abandon the attempt to explain the numerical value of the Ohm Volt & Farad in metres or centimetres grammes &c. The true interpretation of the Latimer Clark jargon*(3) is that electricians and telegraphists being accustomed to large distances and small weights, have adopted as unit of length a quadrant of the earths meridian or 10^7 metres and as unit of mass 10^{-11} grammes leaving the second of mean time in its accustomed place as unit of time.(4)

Latimer Clark tells me that you dissuaded him from defining a Farad as a dose of electricity and persuaded him to define it as the capacity of a (very large) condenser.(5)

Jenkin in his little book sticks to the dose.(6)

* Jargon – A rare and valuable stone from Ceylon probably useful to the electrician as well as to the spectroscopist.(3)

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(1) ULC Add. MSS 7655, II/61A.

(2) The enclosure is not extant. On 8 August 1872 *The Times* carried a report on 'The weather and the storm': 'A house occupied by Mr Brown, Charles Street Windsor, was struck by what its inmates described as a thunderbolt, and considerable damage done to the interior. Mr and Mrs Brown were at a window attending to some flowers when the latter saw a large ball of fire, about a foot in diameter, rushing through the air from the north-east; it was red and glowing, and revolved with great velocity as it approached. Mrs Brown drew back from the window as it neared the house. The ball of fire struck the chimney stack over the roof of the next room, shattered the chimney-pot and brickwork, and drove them into the room. Mrs Brown rushed into the apartment, but found it filled with blue flame and apparently on fire, while a strong sulphureous smell pervaded the place.'

(3) On the supposed discovery of an element 'jargonium' (by absorption spectroscopy) see H. C. Sorby, 'On jargonium, a new elementary substance associated with zirconium', *Proc. Roy. Soc.*, **17** (1869): 511–15. See also Number 468 notes (17) and (18).

(4) See the *Treatise*, **2**: 244–5 (§629) on electrical units.

(5) See Numbers 415 and 418; and see note (8).

(6) See Number 418 esp. note (7).

On the one hand the ohm and the Farad are both visible representations of secondary units, if the farad is a condenser and the ohm a wire while the volt is a Daniell cell of a certain degree of badness.(7)

On the other hand if the farad is a dose of electricity a farad condenser is one which holds a farad per volt and a farad current is one which transmits a farad per second, and in speaking of an electric dose we require a short word to give effect.

Thus – 'Mrs Brown, on a rough estimate, is of opinion, that the fire ball must have contained at least a farad of the electric fluid.

Its potential, at the instant of striking the chimney stack, must have been several megavolts. Mrs Brown, who, with more than the lightning's speed, pursued the fiery globe, had probably a resistance of not more than 1000 Ohms at her command. Her courage, therefore in making herself mistress of all the details of the phenomenon deserves a public recognition by the British Association'.(8)

I am now very near the point at which the definition of a Farad must be printed so I should like to hear from you about it.

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(7) See the *Treatise*, **2**: 244 (§629).

(8) Thomson's reply (ULC Add. MSS 7655, II/62) is dated 24 August 1872: 'Dear Maxwell / A pint is a pint whether there is liquor in it or not and it is *not* in the abstract a quantity of liquid. So of the microfarad. You may buy a microfarad (of tinfoil & paraffined paper, or of mica &c) and you may buy a pint or a quart measure, of pewter or silver. You might of course buy a pint of beer or of water; and when electrotyping, electric light, &c become commercial we may perhaps buy a microfarad or a megafarad of electricity (perhaps even of hody⟨*l*⟩* [* betraying the citizenship of the inventors of *V* dealers in such commodities] or meric, or omer† [† not to be confounded with the Home or Be.tray unit], as no doubt when we recognise the reality of these fluids we shall be able to measure them in microfarads). I am glad you agree with me that if there is to be a name given it had better be given to a real purchaseable tangible, object than to a quantity of electricity. I daresay Jenkin may be right enough however still in using the expression a microfarad of electricity, meaning as much electricity as a micro-farad holds when filled up till its gauge marks 1 volt. (A glass pint's measure would be filled not to over flowing, but up to a certain mark. Still the piece of glass so marked would be called a pint.) / Everett proposes to call forces metrims or centrims or decims or millims according to the fundamental mass! / I shall never have any thing to do with Veber, till Omer and Hody are generally accepted. / Ohms and Volts I suppose are *in* and they are undoubtedly convenient. So also is the microfarad, as the name of a condenser. But we don't want more names. Those are enough for all purposes, and it would be not any convenience, but rather the reverse, to bring in a separate name for the dose. On this account & from the analogy of a pint of beer we may (with F.J.) talk of a microfarad of elecy. Yours T. / I am taking two sisters in law &c for a cruise to West Highlands beginning Monday next & likely to last about a fortnight. Where are you to be after that? Address if posting on or before Wedy care of the Right Rev the Bishop of Argyll Bishopton Loch Gilphead. / I did not get your letter of the 10th till my arrival at the Be.tray last week & every day was prevented from answering it.' See Maxwell's discussion in the *Treatise*, **2**: 244–5 (§629); and on Jenkin see Number 450 esp. note (13).

As for poor Veber[9] it is too bad to cut off half his head during his life time. Ohm has been spared any such indignity and Volta and Faraday have only lost vowels and retain their places in the dictionary. I suppose the next thing out will be Walter for Volta to make all square.

I have received your proofs[10] in which we read Hydrok*e*netic passim. What new light is this?

Yours $\frac{dp}{dt}$

More corrections[11]

p517	5 from bottom	plane for place
523	twice	Plücker for Plücher
	'Ieames de la Pluche'	
534	last paragraph unsymbolic for non-analytic	

(9) See Number 415.

(10) Of Thomson's *Reprint of Papers on Electrostatics and Magnetism* (London, 1872).

(11) Only 'Plücker' (*Electrostatics and Magnetism*: 523) was corrected in the printed text.

LETTER TO JAMES THOMSON

2 SEPTEMBER 1872

From the original in the Library of The Queen's University, Belfast[1]

Glenlair
Dalbeattie
2 Sept 1872

My dear Thomson

With respect to the history of my knowledge of the path of rays in a medium of continuously variable index of refraction[2] I had it in my mind when I came to visit your brother Sir[a] William in December 1852 (I think).[3] I then thought it *easiest* to calculate the path of the ray by translating the problem into the emission theory and treating the ray as a moving body acted on by forces depending on the variation of the index of refraction.[4] This was done by making the potential $= 2\mu^2 + \text{constant}$. Of course I knew that this was only an artifice, justifiable only because the emission and undulation theories are mutually equivalent[b] when the proper alterations of the hypothesis are made.

I would therefore give Wollaston some credit for his paper[5] provided it is right on the emission theory. But your brother showed me how easy it is to begin with the right hypothesis, that is, by making the velocity *inversely* proportional to μ and calculating the change of wave-front.

In 1853 I sent the Cambridge & Dublin M.J. a problem[6] about the path

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(a) {William Thomson} ⟨Sir⟩

(b) {William Thomson} in respect to the path of rays

(1) James Thomson Papers, MS 13/22d, The Queen's University of Belfast Library.

(2) Prompted by a draft of James Thomson's paper, 'On atmospheric refraction of inclined rays, and on the path of a level ray', *Report of the Forty-second Meeting of the British Association for the Advancement of Science; held at Brighton in August 1872* (London, 1873), part 2: 41–5.

(3) In a letter to Maxwell of 11 January 1873 (copy in James Thomson Papers MS 13/22c), writing with reference to the draft 'postscript' to his paper 'On atmospheric refraction of inclined rays' (see note (12)), Thomson noted: 'You may observe that I refer to your visit to my brother as in Dec. 1851 or 1852 I was not sure which as in your letter to me you thought it was in 1852 & in your writing to D^r Everett lately I observe you mention it as having been about Xmas 1851 or so.' On Maxwell's proposed visit to William Thomson in December 1851, see a letter from J. P. Joule to W. Thomson, 3 December 1851 (ULC Add. MSS 7342, J 96).

(4) See P. S. Laplace, *Traité de Mécanique Céleste*, 5 vols. (Paris, An VII [1799]–1825), **4**: 231–76.

(5) W. H. Wollaston, 'Observations on the quantity of horizontal refraction; with a method of measuring the dip at sea', *Phil. Trans.*, **93** (1803): 1–11. See note (11).

(6) See the third of the 'Problems' in the *Camb. & Dubl. Math. J.*, **8** (1853): 188.

of a ray in a medium in which

$$\mu = \frac{\mu_0 a^2}{a^2 + r^2}$$

where μ_0 and a are constant and r is the distance from a fixed point.(7)

Such rays move in circles.

This problem was intended to illustrate the fact that the principal focal length of the crystalline lens is very much shorter than anatomists calculate it from the curvature of its surfaces and the index of refraction of its substance.(8)

If you measure the focal distance and curvatures of a sheep's lens the index of refraction as calculated comes out above 2 whereas no animal substance has an index above 1.45 or so.

The reason is, the increase of density towards the centre of the lens, so that the rays pass nearly tangentially through a place where the density is varying. I also set a question about the conditions of a horizontal ray of light having a greater curvature than that of the earth, in the Cambridge Examination for 1870.(9)

But I should prefer that you said nothing about me in your paper. I did not invent your very clear statement and proof about the radius of curvature, and your remarks about the advantage of using first principles instead of derived maxims is entirely original.

An immense heap of matter has been written about atmospheric refraction by Bessel Clairaut &c &c(10) and a question has been set on it on the first Tuesday afternoon after Jan 12th of each year at Cambridge for many years

(7) See [J. C. Maxwell,] 'Solutions to problems', *Camb. & Dubl. Math. J.*, **9** (1854): 7–11, esp. 9–11 (= *Scientific Papers*, **1**: 76–9); and for a draft see Volume I: 232–5. See Number 249.

(8) See Volume I: 235n.

(9) See *The Cambridge University Calendar for the Year 1870* (Cambridge, 1870): 502–3, question (3): 'A ray of light passes through a medium whose index of refraction varies continuously; prove that $\frac{d}{ds}\left(\mu\frac{dx}{ds}\right) = \frac{d\mu}{dx}$, s being the length of the path of the ray to a point whose coordinates are (x, y, z). / If in air $\mu - 1$ varies as the square of the density and if μ at a certain place is $\frac{3400}{3399}$ and if the height of the homogeneous atmosphere be five miles, prove that when the temperature is constant, the effect of refraction on distant horizontal objects is to increase the Earth's apparent radius as found from the dip from 4000 to 5230 miles: and that if the temperature over a frozen sea increase about 6 °F for every hundred feet of ascent, objects may be seen reflected in the sky.'

(10) For a comprehensive review (including an account of Bessel's work) see C. Bruhns, *Die Astronomische Strahlenbrechung in ihrer historischen Entwickelung* (Leipzig, 1861).

back, so the subject has been well twisted this way and that. I am surprised at Lloyd falling into the mistake you point out.(11)

What I have done in this matter belongs to the category of the confused calculations and Cambridge Questions not at all to that of your statement of first principles and your proof that the centre of curvature lies in that stratum which if μ increased uniformly would have the index 2μ.(c)

I do not see that I have any other connexion with your paper than as taking an interest in your enquiry when I heard you were thinking of horizontal rays in air, and much more now that I have read your very clear statement which puts not only atmospherical refraction but also the method of explaining physical phenomena by physical theories, quite in a new light, showing that it is better to go back to the very beginning than to rest in secondary principles.

I have marked for deletion what relates to me and I do not see that its omission will injure the paper in any way.(12)

Yours very truly
J. CLERK MAXWELL

(c) {William Thomson}?

(11) See Humphrey Lloyd, *Elements of Optics* (Dublin, 1849): 108–9, on atmospheric refraction. The point to which Maxwell is alluding is Thomson's discussion of the theory that a ray of light suffers refraction at each successive lamina of air: that 'its whole foundation, in oblique transition of the light across laminae with gradual change of density in those successively traversed, vanishes in the case of a horizontal ray' (Thomson, 'On atmospheric refraction of inclined rays': 41).

(12) With his letter of 11 January 1873 Thomson enclosed a 'postscript' (ULC Add. MSS 7655, II/224) which he proposed to append to the printed text of his paper 'On atmospheric refraction of inclined rays'. This 'postscript' repeats, in paraphrased and slightly re-ordered form, apposite passages from Maxwell's letter of 2 September 1872. In his letter of 11 January 1873 Thomson wrote in explanation of his decision to include reference to Maxwell in his paper: 'When I recv^d your letter of 2 September last from Glenlair relat[ing] to a reference I had made to your investigations or views as to the tending of rays of light in the atmos. or in other mediums of continuously varying index of refraction, in which you mentioned to me some particulars of what you had done & in which you said you would prefer I should say nothing about you in my paper, I altered the passage which had referred to you & quite omitted mention of you in it & then I sent the paper to M^r Griffiths to be printed. / However I afterwards showed a copy of the paper to my brother when I was over in Scotland with him & I showed him your letter & he said he would advise me notwithstanding what you said to annex to my paper an abstract of your letter and I have accordingly written a note proposed to be annexed at the end of my paper and this note I think is just such as he proposed that I should prepare. He thought the things you had done in the matter would be very desirable to be referred to; and I have myself always felt much interest in the conditions of the crystalline lens of the eye since you told me about it. So I hope I have done no harm and shall not have displeased you in making the mention of you and of your

communications with my brother on this subject in the note of which I send you a press copy on thin paper. /... I did not find time to prepare the proposed note till after the proof sheet of the paper came to me about a week ago and now it has been necessary for me to send to M^r G. (Ass^t Gen Sec^y Brit Assoc) the proof sheet corrected without waiting first to consult you about the foot note referring to you. / I send the copy however in the wish that if you do not like the foot note to be inserted you might favour me by writing *direct* to M^r Griffith... asking him to cancel that concluding note or asking him to make any alteration on it that you may think suitable. / ... You may understand that its omission would not in any way spoil or injure my paper.' In the event, Maxwell left Thomson's 'postscript' unaltered; see Thomson, 'On atmospheric refraction of inclined rays': 44–5.

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POSTCARD TO PETER GUTHRIE TAIT

4 OCTOBER 1872

From the original in the University Library, Cambridge[1]

O T′ How about electromagnetic 4^{nions} as in proof slip 106, 107? which please annotate and return. I suspect that I am not sufficiently free with the use of the Tensor symbol[2] in devectorizing such things as r (distance between two points). The great want of the day is a Grammar of 4^{nions} in the form of dry rules as to notation & interpretation not only of S, T, U, V but of . ( ) and the proper position of $d\sigma$ &c.[3] Contents, Notation, Syntax, Prosody, Nablody.

Yours $\frac{dp}{dt}$

Glenlair 4 Oct 1872

(1) ULC Add. MSS 7655, I, b/49. Previously published in Knott, *Life of Tait*: 151.
(2) See Number 353 note (9).
(3) See Number 353 note (9).

LETTER TO PETER GUTHRIE TAIT

9 OCTOBER 1872

From the original in the University Library, Cambridge[1]

Glenlair
9 October 1872

O T′

I am very sorry to hear the reason why I heard nothing of you for a while. I hope that home will be more conducive to health than even the metropolitan city.

I think I had better consecrate ρ to its prescriptive office of denoting indicating or reaching forth unto the point of attention $(x\,y\,z)$. I shall therefore say at 590[2]

If this vector be denoted by $\mathfrak{A}$[3] and if ρ denote the vector from the origin to a given point of the circuit, and $d\rho$ an element of the circuit

$$J = -S\mathfrak{A}\,d\rho^{(4)}$$

and we way write equation (2)[5]

$$p = \int\left(F\frac{dx}{ds}+G\frac{dy}{ds}+H\frac{dz}{ds}\right)ds$$

or

$$p = -\int S\mathfrak{A}\,d\rho.^{(6)}$$

Has ρ a *name*?

It is no ordinary vector carrying a point from one thing to another. It is rather the tentacle or feeler which reaches from the subject to the object.

Is he the Scrutator?[7]

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(1) ULC Add. MSS 7655, I, b/50. Previously published (in part) in Knott, *Life of Tait*: 151.

(2) See the *Treatise*, 2: 214 (§590), on electromagnetic action between two circuits.

(3) $\mathfrak{A}$ is the vector potential of a circuit at a point x, y, z, its components being F, G, H, and depends on the position of an element of the circuit ds in the electromagnetic field.

(4) Substituting for the element ds three components dx, dy, dz resolved in the directions of the axes x, y, z, Maxwell defines a quantity $J = F\frac{dx}{ds}+G\frac{dy}{ds}+H\frac{dz}{ds}$.

(5) *Treatise*, 2: 212 (§586). Considering the electromagnetic action between primary and secondary circuits Maxwell defines a quantity p which measures the part of the electrokinetic momentum of the secondary circuit depending on the primary current, and writes $p = \int J\,ds$.

(6) See note (4). He concludes: 'The vector $\mathfrak{A}$ represents in direction and magnitude the time-integral of the electromotive force which a particle placed at the point (x, y, z) would experience if the primary circuit were suddenly stopped. We shall therefore call it the Electrokinetic Momentum *at the point* (x, y, z).' (*Treatise* §590).

(7) Examiner of votes, at Cambridge (*OED*).

I am glad to hear of the 2nd edition of 4nion. I am going to try, as I have already tried, to sow 4nion seed at Cambridge. I hope and trust that nothing I have yet done may produce tares.(8)

But the interaction of many is necessary for the full development of a new notation for every new absurdity discovered by a beginner is a lesson. Algebra is very far from O.K. after now some centuries, and diff calc is in a mess and $\iiint$ is equivocal at Cambridge with respect to sign.

We put down everything, payments, debts, receipts, cash, credit, in a row or column and trust to good sense in totting up.

I send back slips 101–8 which please return soon as they are in the throes of revision and I have no more copies on hand.

Yours truly
J. Clerk Maxwell

Just received a Separat Abdruck from Klausius.(9)

(8) Injurious weed among corn (biblical and figurative usage, *OED*).

(9) Very likely Clausius' paper 'Ueber den Zusammenhang des zweiten Hauptsatzes der mechanischen Wärmetheorie mit dem Hamilton'schen Princip', *Ann. Phys.*, **146** (1872): 585–91, of which there is a reprint in Maxwell's library (Cavendish Laboratory, Cambridge). For Maxwell's comments on the controversy among 'learned Germans' about the reduction of the second law of thermodynamics to Hamilton's principle see Number 483, esp. note (28).

LETTER TO GEORGE BIDDELL AIRY

16 OCTOBER 1872

From the original in the Royal Greenwich Observatory Archive(1)

Glenlair
Dalbeattie
16 Oct 1872

Dear Sir(2)

I have instructed Messrs Macmillan(3) to forward a copy of my essay on Saturn's Rings to M. Faye.(4)

About the year 1864 I made an investigation (unpublished) of the condition of a ring consisting of imperfectly elastic bodies, in great numbers and colliding with one another.(5)

I found that such a ring, if composed of bodies having a coefficient of restitution above a certain value (which I forget) would be continually knocked about, so that the bodies would be describing paths of their own, and the ring would remain a ring of detached bodies, and therefore transparent.(6)

It would obtain the supply of energy required for the collisions by getting flatter and thinner.

But if the bodies were only as elastic as common stones, this great disturbance would not be kept up, and the stones would subside into contact with each other, forming a great flat cake of loose rubbish, kept together by

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(1) Royal Greenwich Observatory Archive, ULC, Airy Papers 6/259, 204R–V.

(2) In reply to a letter from Airy of 14 October 1872 (Airy Papers 6/259, 203R–V): 'Dear Sir / Having remarked in the French Comptes Rendus a notice of a paper by M. Hirn on the theory of Saturn's Rings considered as collections of discrete molecules, I called the attention of M. Faye to the circumstances that you had very completely investigated the state of the rings on that supposition:– but I could only refer him to an abstract of your paper which I had made in the Monthly Notices of the R. Astronomical Society. / M. Faye has alluded to my communication in an address to the Academy. / Could you send M. Faye a copy of your quarto paper? to the address (A Monsieur/M. Faye / President de l'Académie des Sciences / au Palais de l'Institut / à Paris. / Or, if you prefer it, send it to me that I may forward it to him. / The publication as a separate paper is very unfortunate:– such volatile tracts are very soon lost. / I am, dear sir, / Yours faithfully / G. B. Airy.'

On 16 September 1872 H. A. Faye had read to the Académie des Sciences a 'Note relative à un mémoire de M. Hirn sur les conditions d'équilibre et sur la nature probable des anneaux de Saturne', *Comptes Rendus*, **75** (1872): 645–6. Airy had written to him on 26 September 1872 (Airy Papers 6/259, 201R–V), commending Maxwell's 'very able paper' on Saturn's rings.

(3) On the circumstances of publication of Maxwell's *On the Stability of the Motion of Saturn's Rings* (Cambridge, 1859) (= *Scientific Papers*, **1**: 288–376) see Volume I: 599, 612.

(4) See note (2).

(5) Number 224.

(6) See Number 224 §3.

its mutual gravitation, but always grinding one part against another in a sluggish manner instead of working itself up, as a ring of glass balls would do, till they were all flying about like a swarm of bees.

Yours faithfully
J. Clerk Maxwell

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FROM A LETTER TO LEWIS CAMPBELL

19 OCTOBER 1872

From Campbell and Garnett, *Life of Maxwell*[1]

Glenlair
Dalbeattie
19 October 1872

... Lectures begin 24th. Laboratory rising, I hear, but I have no place to erect my chair, but move about like the cuckoo, depositing my notions in the chemical lecture-room 1st term; in the Botanical in Lent, and in Comparative Anatomy in Easter.

I am continually engaged in stirring up the Clarendon Press, but they have been tolerably regular for two months. I find nine sheets in thirteen weeks is their average. Tait gives me great help in detecting absurdities. I am getting converted to Quaternions, and have put some in my book, in a heretical form, however, for as the Greek alphabet was used up, I have used German capitals from $\mathfrak{A}$ to $\mathfrak{Z}$ to stand for Vectors, and, of course, ∇ occurs continually. This letter is called 'Nabla', and the investigation a Nablody.[2] You will be glad to hear that the theory of gases is being experimented on by Profs. Loschmidt[3] and Stefan[4] of Vienna, and that the conductivity of air and hydrogen are within 2 per cent of the value calculated from my experiments on friction of gases,[5] though the diffusion of one gas into another is '*in erglanzender ubereinstimmung mit* $\dfrac{dp}{ds}$ *schen Theorie.*'[6]

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(1) *Life of Maxwell*: 383–4.

(2) See Number 348 note (3).

(3) Joseph Loschmidt, 'Experimental-Untersuchungen über die Diffusion von Gasen ohne poröse Scheidewände', *Wiener Berichte*, **61**, Abtheilung II (1870): 367–80; *ibid.*, **62**, Abtheilung II (1870): 468–78. See Number 470.

(4) Josef Stefan, 'Über die Gleichgewicht und die Bewegung, insbesondere die Diffusion von Gasmengen', *Wiener Berichte*, **63**, Abtheilung II (1871): 63–124; Stefan, 'Untersuchungen über die Wärmeleitung in Gasen', *ibid.*, **65**, Abtheilung II (1872): 45–69; Stefan, 'Über die dynamische Theorie der Diffusion der Gase', *ibid.*, **65**, Abtheilung II (1872): 323–63.

(5) Stefan, 'Untersuchungen über die Wärmeleitung in Gasen': 47–8. Following Boltzmann's discovery of an arithmetical error in Maxwell's expression for the thermal conductivity of a gas (see Number 263 note (24)), Stefan discussed modifying Maxwell's theory: see his paper 'Untersuchungen über die Wärmeleitung in Gasen', *Wiener Berichte*, **72**, Abtheilung II (1876): 69–101.

(6) Garbled from Stefan, 'Über die dynamische Theorie der Diffusion der Gase': 324.

LETTER TO GEORGE BIDDELL AIRY

28 OCTOBER 1872

From the original in the Royal Greenwich Observatory Archive(1)

11 Scroope Terrace
Cambridge
28 Oct 1872

Dear Sir(2)

I should like very much to see the memoir of M. Hirn,(3) which I will return to you as soon as I can.

I see in Les Mondes of the 24th Oct that M. Hirn has become acquainted with your letter to M. Faye containing an account of my essay.(4)

Macmillan tells me the essay was sent at once to M. Faye(5) so M. Hirn may have seen it, though he might have acquired all that appears from his statement by reading your notice in the Astronomical Societies notices.(6)

The interest of the speculations on the constitution of Saturns rings is not likely to be soon exhausted for the solution of the various mathematical problems which it suggests takes a long time, and when these are solved we are liable to make mistakes in applying them to the physical problems and so we have to examine the whole speculation afresh, searching not for bad mathematics, but for bad reasoning.

Yours faithfully
J. Clerk Maxwell

The Astronomer Royal(7)

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(1) Royal Greenwich Observatory Archive, ULC, Airy Papers 6/259, 208R–V.

(2) In reply to a letter from Airy of 26 October 1872 (Airy Papers 6/259, 205R): 'Dear Sir / I have heard nothing from M. Faye about your paper concerning Saturn's rings. / M. Hirn has sent me a copy of his paper. Would you like to peruse it? I can place it in your hands only on loan, as I must retain it for the Observatory. / I am, dear Sir, / Faithfully yours / G. B. Airy.'

(3) See note (2); and G. A. Hirn, *Mémoire sur les Conditions d'Équilibre et sur la Nature Probable des Anneaux de Saturne* (Paris, 1872).

(4) See *Les Mondes*, **29** (24 October 1872): 288–9, a report of Faye's 'Note relative à un mémoire de M. Clerk-Maxwell, sur la stabilité des anneaux de Saturne', *Comptes Rendus*, **75** (1872): 793–4, giving an account of Airy's letter to him of 26 September 1872 (see Number 424 note (2)).

(5) See Number 424.

(6) See Airy's review 'On the stability of the motion of Saturn's rings', *Monthly Notices of the Royal Astronomical Society*, **19** (1859): 297–304.

(7) Airy's reply is dated 29 October 1872 (Airy Papers 6/259, 210R–V): 'Dear Sir / I send by Book Post my copy of M. Hirn's Essay on Saturn's Rings / I have just learned the address of

M. Hirn... Perhaps you could send *him* a copy of your Essay. / The mode of publication of the Adams Prize Essays was very unfortunate. Practically they are totally lost to the world. I suggested some years ago that arrangement should be made for their regular appearance in the Cambridge Phil. Soc. Transactions: but I know not whether any thing was done.' (See Volume I: 612n). Maxwell sent Hirn a copy of his Essay; acknowledged in a letter from Hirn to Maxwell of 25 November 1872 (ULC Add. MSS 7655, II/68).

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NOTE[1] TO PETER GUTHRIE TAIT

12 NOVEMBER 1872

From the original in the University Library, Cambridge[2]

[Cambridge]

Address 11 Scroope Terrace, Cambridge.

I hope the surgical operation is to be carried out on prooves only, not on mémoires. As for Trägheit,[3] none but himself can be his explanation. I defy you to explain him by central forces not involving velocities.

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(1) On proofs (of *Treatise* §§692–725), date as postmark.

(2) ULC Add. MSS 7655, IV/2.

(3) 'Laziness', Tait's nickname (see Knott, *Life of Tait*: 182–3) for the generalisation of the theorem in Thomson and Tait, *Natural Philosophy*: 217 (§312): 'The energy of the motion generated suddenly in a mass of incompressible liquid given at rest completely filling a vessel of any shape, when the vessel is suddenly set in motion, or when it is suddenly bent out of shape in any way whatever, subject to the condition of not changing its volume, *is less than the energy of any other motion it can have with the same motion of its bounding surface*.' Thomson's theorem on extremal conditions which determine the bounding surface of an incompressible fluid enclosed within a flexible and extensible envelope was first formulated in his 'Notes on hydrodynamics. V. On the vis-viva of a liquid in motion', *Camb. & Dubl. Math. J.*, **4** (1849): 90–4 (= *Math. & Phys. Papers*, **1**: 107–12). Thomson published a statement of the generalised minimum theorem, which Tait nicknamed 'Laziness', in his paper 'On some kinematical and dynamical theorems', *Proc. Roy. Soc. Edinb.*, **5** (1863): 113–15 (= *Math. & Phys. Papers*, **4**: 458–9): 'Given any material system at rest. Let any parts of it be set in motion suddenly with given velocities, the other parts being influenced only by their connections with those which are set in motion, the whole system will move so as to have less kinetic energy than belongs to any other motion fulfilling the given velocity conditions'. See *Natural Philosophy*: 217–25 for discussion and proof of the theorem.

LETTER TO PETER GUTHRIE TAIT

late 1872 – early 1873[(1)]

From the original in the University Library, Cambridge[(2)]

I hereby declare C. Neumann Not Guilty of *reading* T on the hydrokinetic illustration of Θ.H. currents.[(3)] He may have heard of it or *tried* to read it but if he had read it he would not have written such original bosch.[(4)]

Neumann's is not an illustration but a physical theory. He adopts the Unitarian view asserting, without a pang, that vitreous electricity is a fluidum, while resinous is inseparably joined with ponderable matter.[(5)]

This fluidum obeys two Princips the Potential ditto and that des isotropen Druckes[(6)] which last, be it observed, is nothing more than the Boyle & Charles gaseous laws, which being admitted introduce absolute charge to any extent and the squeezing of electricity under Druck.[(7)] In this abhandlung C. N. steers clear of this by considering only *steady* currents. Woe to him when he goes further. He will fare even worse!

The density of this fluidum varies with temperature, and namely in the same ratio as that of the ponderable body. But the absolute density of the fluidum at 0 °C is different for different metals.

Hence if we know the relative thermoelectric position of two metals at 0 °C, and also their dilatations per 1 °C their neutral point can be found. (Here the theory lies open to experiment.)

Be it carefully observed that this theory professes to account for nothing but

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(1) The letter relates to Tait's interest in thermo-electricity at this time, and to a recent paper by Carl Neumann (see notes (4) and (13)).

(2) ULC Add. MSS 7655, I, b/105.

(3) On Thomson's analogy see Number 393 note (7).

(4) Carl Neumann, 'Vorläufige Conjectur über die Ursachen der thermoelektrischen Ströme', *Berichte über die Verhandlungen der Königlich Sächsischen Gesellschaft der Wissenschaften zu Leipzig, Math.-Phys. Klasse*, **24** (1872): 49–64.

(5) That is: positive and negative electricity; see the *Treatise*, **1**: 31 (§27). On the concept of electricity as a fluid see note (10).

(6) Neumann bases his theory on 'das Princip der Potentiellen Kräfte und das Princip des isotropen Druckes'; see his 'Vorläufige Conjectur': 52. On his concept of pressure see note (7).

(7) Neumann states: 'dass das Princip des isotropen Druckes nicht nur in Hydrodynamik und Aërodynamik, sondern auch in andern Gebieten der theoretischen Physik eine Rolle zu spielen'; Neumann, 'Vorläufige Conjectur : 63.

Seebeck's effect,(8) Πeltiers(9) and Thomson's(10) are alike unreferred to. The word Thomson occurs in reference to his experimental researches(11) which are here said to confirm the doctrine of the slope of the line of a metal depending on its rate of dilatation.(12)

All things considered, this lucubration, if ever demonstrated to be true by the language of facts, will furnish me with arguments sufficient for the deglutition of my own hat, if not head, before I go on to swallow so crude a morsel.(13)

$$\frac{\partial p}{\partial t}$$

(8) The discovery 'of thermoelectric currents in circuits of different metals with their junctions at different temperature' (*Treatise*, **1**: 302 (§250)); see Thomas Seebeck, 'Magnetische Polarisation der Metalle und Erze durch Temperatur-Differenz', *Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin* (Aus dem Jahren 1822–23): 265–373.

(9) The discovery 'that, when a current of electricity crosses the junction of two metals, the junction is heated when the current is in one direction, and cooled when it is in the other direction' (*Treatise*, **1**: 300 (§249)); see J. A. C. Peltier, 'Nouvelles expériences sur la caloricité des courans électriques', *Ann. Chim. Phys.*, ser. 2, **56** (1834): 371–86. On Thomson's use of the symbol Π for the coefficient of the Peltier effect see Number 322 note (5).

(10) Thomson's discovery 'of the reversible effect of an electric current upon an unequally heated conductor of one metal' (*Treatise*, **1**: 305 (§253)); see William Thomson, 'On the electro-dynamic qualities of metals', *Phil. Trans.*, **146** (1856): 649–751, esp. 649–709 (= *Math. & Phys. Papers*, **2**: 189–327, esp. 189–266). Maxwell argues that Thomson's experiments, which established 'that the current produced opposite effects in copper and in iron', and 'shew that positive electricity in copper and negative electricity in iron carry heat with them from hot to cold', contradict the theory that either positive or negative electricity were a 'fluid, capable of being heated and cooled, and of communicating heat to other bodies, [for] we should find the supposition contradicted by iron for positive electricity, and by copper for negative electricity'; see the *Treatise*, **1**: 305 (§253).

(11) Thomson, 'On the electro-dynamic qualities of metals': 649–709.

(12) Neumann, 'Vorläufige Conjectur': 60.

(13) In his Rede Lecture on 'Thermo-electricity', *Nature*, **8** (1873): 86–8, 122–4, esp. 87–8, Tait echoes Maxwell's remarks on Neumann.

NOTES TO PETER GUTHRIE TAIT

circa DECEMBER 1872

From the microfilm of the originals in Edinburgh University Library[1]

[1] *For Science in 1872 in 'Belgravia' Dec 1872, p. 188.*

'Is Electricity Life?' by Henry Lake.[2] Here is a bit. The ocean, for instance, is compounded of water and salt; one is an electric, the other is not. The friction of these causes the phosphorescent appearances so often observed at sea. &c &c in same style.

Electricity of kissing – danger of mutual destruction obviated by the existence of electric atmospheres. A person who has the small pox cannot be electrified, while sparks of electricity may be drawn from a patient dying of cholera. The hand draws from the sensitive plant the electricity which it contains more than other plants; and its leaves at once fall flaccidly, until a new supply of electric force renders them once more turgid.

[2] *Astronomy at Cambridge in 1872*[3]

The Earth is now ascertained to have the form of an ellipse.

It revolves about its axes. In Winter it revolves about its longer axis AB, but in Summer about its shorter axis CD, thus producing the changing of the seasons.

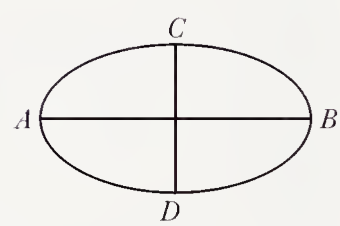

Figure 429,1

(1) Edinburgh University Library, Mic. M.134. Tait's Scrapbook (where these notes are preserved) is in private possession.

(2) Henry Lake, 'Is electricity life?', *Belgravia*, **9** (December 1872): 188–96.

(3) Possibly from a Natural Sciences Tripos examination paper. (Maxwell was not an examiner in 1872.)

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ARTICLE ON ELECTROMAGNETISM[1]

LATE 1872 – EARLY 1873

From the originals in the University Library, Cambridge[2]

ELECTROMAGNETISM[3]

The attraction of light particles by amber and that of iron by the loadstone were known to the ancients. Other phenomena were afterwards discovered to be related to those exhibited by amber and were classed together as Electric phenomena. A different set of phenomena were found to be related to those of the loadstone, and were called Magnetic phenomena. Many conjectures were formed as to the relation between these two sets of phenomena, but at last in 1822 Oersted discovered the mutual action between an electric current and a magnet,[4] and this discovery forms the starting point of the science of Electromagnetism which treats of electric and magnetic phenomena in their relation to each other. Ampère in 1823 discovered the mathematical form of the law of the mechanical force acting between two electric currents.[5] His investigation is in its way a model of scientific method,[6] and his formula is the foundation of that form of our science which admits that bodies may act on one another at a distance.

In the present article we shall follow the path pointed out by Faraday which leads to results mathematically identical with those of Ampère but never loses sight of the phenomena which take place in the ⟨media⟩ ⌊space⌋ between the bodies which are observed to act on each other.

Faraday had made considerable progress in illustrating the mutual action between electric currents and magnets, when in 1831 he made his great discovery of the fact that an electric current is produced in a closed circuit when a magnet, or a conductor carrying an electric current, is moved

(1) Published in the *English Cyclopaedia*, supplementary volume on 'Arts and Sciences' (London, 1873), columns 854–7.

(2) From the manuscript and corrected proofs in ULC Add. MSS 7655, V, c/18. The changes from the manuscript and Maxwell's corrections to the proofs are recorded.

(3) Compare 'Part IV. Electromagnetism' of the *Treatise*.

(4) See Number 238 note (17), and the *Treatise*, **2**: 128–9 (§§475–6).

(5) A. M. Ampère, 'Mémoire sur la théorie mathématique des phénomènes électrodynamiques uniquement déduite de l'expérience', *Mémoires de l'Académie Royale des Sciences*, **6** (1827): 175–388, esp. 252–3; and see the *Treatise*, **2**: 146–50 (§§502–8), and Volume I: 305–6n.

(6) 'The whole, theory and experiment, seems as if it had leaped, full grown and full armed, from the brain of the "Newton of electricity".' (*Treatise*, **2**: 162 (§§528)).

relatively to the closed circuit, or when the strength of the inducing current is varied. The first two series of Faradays *Experimental Researches* in which he describes his experiments, and deduces from them the laws of the phenomena, may be read as an example of a form of scientific method different from that of Ampère.[7]

Faraday soon afterwards found that an electric current acts on itself as well as on other currents. When it is increasing in strength this self inductive action tends to check the current. When it is diminishing it tends to maintain the current. Faraday was at once struck with the analogy between these phenomena of an electric current and those of a current of water in a pipe, which requires a force to start it and which continues in motion till it is stopped by some other force.[8]

In the case of the pipe these phenomena are attributed to the inertia of the water, which like all other material substances perseveres in its state of rest or motion, except in so far as that state is changed by the application of force.

In the case of the electric current in ⟨the state of⟩ which we find a persistence of exactly the same kind we naturally look for a similar explanation. But as Faraday shews, there are important differences between the two cases. The inertia of the water in the pipe depends only on the length and the section of the pipe and not on the form into which the pipe may be coiled or bent.

The apparent inertia of the electric current on the other hand depends on the shape of the wire which carries it. If the wire is made into a coil with its windings all in one direction the current has great persistence. If the wire is doubled on itself so that in adjacent portions the currents are opposite, the persistence is exceedingly small.

Besides this, we have the fact that a current in one circuit induces a current in a neighbouring circuit, a phenomenon which has no counterpart in the case of water in separate pipes.

Hence if Faradays phenomenon is due to the inertia of matter in motion this matter is not to be sought for in the wire itself but in the space surrounding the wire. It is to Sir William Thomson that the idea is due, that everywhere in the neighbourhood of an electric current, and wherever magnetic action can be traced there is matter of some kind the motion of which is determined by the currents and magnets, and that the phenomena

(7) Compare the *Treatise*, **2**: 164–8 (§§530–5).

(8) Compare Maxwell's discussion of the Ninth Series of Faraday's 'Experimental researches in electricity' (*Electricity*, **1**: 322–43; and see Number 238 note (15)) in the *Treatise*, **2**: 180–3 (§§546–52).

of induction are due to the persistence of this motion while those of attraction are due to its centrifugal force.[9] This idea has been developed by the writer of this article in a series of papers on the theory of Molecular Vortices in the Philosophical Magazine for 1861–2.[10]

But the connexion of all electromagnetic phenomena may be traced in a manner independent of this theory, by assuming only that a system of electric currents in given positions forms, with the surrounding medium, a connected system similar to those of which the equations of motion have been fully investigated by Lagrange.[11]

Lagrange supposes the position of every part of the system to be expressed in terms of a set of variables or coordinates x_1 x_2 &c, the number of which is equal to the number of degrees of freedom of the system. These coordinates vary with the time, and we shall for brevity write $\dot{x}_1$ for $\frac{dx_1}{dt}$, and call $\dot{x}_1$ the velocity of the coordinate x_1.

The whole dynamical theory of such a connected system depends on the value of the kinetic energy or vis viva of the system which is a homogeneous quadratic function of the velocities, the coefficients being in general functions of the coordinates. Hence

$$T = \tfrac{1}{2}L_1\dot{x}_1^2 + \tfrac{1}{2}L_2\dot{x}_2^2 + \&\text{c} + M_{12}x_1x_2 + \&\text{c} \tag{1}$$

where T is the kinetic energy of the system, L_1 L_2 &c functions of the coordinates which we may call moments of inertia and M_{12} &c other functions which we may call products of inertia.

If we differentiate the expression for T with respect to $\dot{x}_1$ we obtain

$$\xi_1 = \frac{dT}{d\dot{x}_1} = L_1\dot{x}_1 + M_{12}\dot{x}_2 + \&\text{c}. \tag{2}$$

This quantity ξ_1 is the momentum of the system with respect to the coordinate x_1. In the theory of the motion of unconnected particles the momentum of each particle depends only on its own motion, but when the motion is that of a connected system the momentum of each coordinate will depend on the velocity of other coordinates unless these coordinates are conjugate to each other.

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(9) In his paper 'Dynamical illustrations of the magnetic and the heliocoidal rotatory effects of transparent bodies on polarized light', *Proc. Roy. Soc.*, **8** (1856): 150–8.

(10) J. C. Maxwell, 'On physical lines of force', *Phil. Mag.*, ser. 4, **21** (1861): 161–75, 281–91, 338–48; *ibid.*, **23** (1862): 12–24, 85–95 (= *Scientific Papers*, **1**: 451–513).

(11) See the *Treatise*, **2**: 184–94 (§§553–67); and compare Number 419.

If X_1 denotes the external force applied to the system tending to increase x_1 then by Lagranges equation[(12)]

$$X_1 = \frac{d\xi_1}{dt} - \frac{dT}{dx_1}. \tag{3}$$

Hitherto we have introduced no distinction between different kinds of coordinates. We shall now suppose that the positions of the various circuits in the field are expressed by the coordinates x_1 &c their velocities by $\dot{x}_1$ &c, their momenta by ξ_1 &c the forces which are impressed on them by X_1 &c. We might also express by y_1 the position of a particle of electricity in one of these circuits, and by $\dot{y}_1$ its velocity but since one particle of electricity in the circuit is like another the value of y_1 can not enter into the dynamical equations and the state of things will be completely determined by $\dot{y}_1$ $\dot{y}_2$ &c the strengths of the electric currents in the circuits 1 2 &c.

Hence in the expression for T, the kinetic energy of the system the coefficients L, M &c will be functions of the mechanical coordinates x_1 x_2 &c and not of the electrical coordinates y_1 y_2 &c.

The expression will consist of three parts. The first part involves squares and products of the mechanical velocities and the second squares and products of the electric currents while the third involves products of mechanical velocities and electric currents.

The first part belongs to the ordinary dynamics of the system and it may be shown by experiment (Maxwell's Electricity Part IV Chap VI)[(13)] that the third part, if it exists, is of insensible value. Hence we have only to consider the second part of the kinetic energy, which involves squares and products of the electric currents.[(14)]

Let us take the case of two currents only, in which case

$$T = \tfrac{1}{2}L_1\dot{y}_1^2 + M\dot{y}_1\dot{y}_2 + \tfrac{1}{2}L_2\dot{y}_2^2. \tag{4}$$

Here T is the kinetic energy of the electric system, $\dot{y}_1$ and $\dot{y}_2$ are the currents, L_1 and L_2 are the moments of inertia (in an electrical sense) of the circuits and M is the product of inertia. The quantities L_1 M L_2 depend only on the form and ⌊relative⌋ position of the circuits, and may be calculated when these are given.

Writing η_1 and η_2 for the electromagnetic momenta of the circuits we find

$$\left.\begin{aligned} \eta_1 &= L_1\dot{y}_1 + M\dot{y}_2 \\ \eta_2 &= M\dot{y}_1 + L_2\dot{y}_2. \end{aligned}\right\} \tag{5}$$

(12) See the *Treatise*, **2**: 198–200 (§573).

(13) See the *Treatise*, **2**: 200–5 (§§574–77); and see Number 340 esp. note (9).

(14) See the *Treatise*, **2**: 206–10 (§§578–84); and Number 333.

If Y_1 Y_2 are the external electromotive forces in the two circuits

$$Y_1 = \frac{d\eta_1}{dt} \quad Y_2 = \frac{d\eta_2}{dt} \tag{6}$$

and if X_1 X_2 are the external mechanical forces on the conducting circuits required to overcome the forces arising from the action of the currents

$$X_1 = -\frac{dT}{dx_1} \quad X_2 = -\frac{dT}{dx_2}. \tag{7}$$

In the general expression for the force in equation (3) there are two terms. The second of these disappears in (6) because the expression for T does not involve y_1. The first disappears in (7) because the electric part of T does not involve $\dot{x}_1$ or ξ_1.

Equations (6) express the whole theory of the induction of electric currents, and equations (7) the whole theory of the mechanical action between circuits carrying electric currents.

For simplicity let us suppose the form of each circuit invariable so that L_1 and L_2 are constants but let their relative positions depend on the coordinate x, which we may call their distance. M will be a function of x which when the circuits are parallel and in the same direction will generally diminish as x increases.

Let us suppose that there is a current $\dot{y}_1$ in the first circuit and that there is no external electromotive force acting in the second except the resistance of the circuit to the current, which may be written $Y_2 = -R_2 \dot{y}_2$. Equation (6) becomes

$$-R_2 \dot{y}_2 = \frac{d\eta_2}{dt} \tag{8}$$

or integrating with respect to t

$$Ry_2 = (\eta_2) - [\eta_2] \tag{9}$$

where R denotes the resistance of the second circuit, y_2 the whole quantity of electricity which flows through it during a certain time, (η_2) the value of the momentum at the beginning and $[\eta_2]$ that at the end of the time.

If the second current $\dot{y}_2$ is zero both at the beginning and at the end of the time ⌊then by equation (5)⌋

$$Ry_2 = (M\dot{y}_1) - [M\dot{y}_1]. \tag{10}$$

Hence there is a positive induced current when the value of $M\dot{y}_1$ is diminished and a negative current when it is increased whether by the variation of the primary current $\dot{y}_1$ or by the variation of M due to relative motion of the circuits. All the phenomena of the induction of currents are included in this result.

Again X is the mechanical force which is required to balance the electric force acting between the circuits. Hence the force of the electric action is

$$-X = \frac{dT}{dx} = \frac{dM}{dx}\dot{y}_1\dot{y}_2. \tag{11}$$

Hence the force between the circuits is proportional to the product of the currents in them, and tends to increase M, that is, in the case of two parallel circuits in the same direction, to draw them together, or to attract them. All the phenomena of the mechanical action between currents are included in this result.

The problem of the mutual action of two circuits is thus reduced to the calculation of the coefficient M.(15) In the case of two linear closed curves of which the elements are ds_1 and ds_2

$$M = \iint \frac{ds_1\, ds_2 \cos\epsilon}{r} \tag{12}$$

where r is the distance between the elements ds_1 ds_2 and ϵ the angle between their directions and the integration is to be extended first round one circuit and then round the other.(16) The deduction of this formula from known facts and the calculation of the value of M in different cases will be found in the work on electricity already referred to.

According to the theory of this article, what we call an electric current is accompanied by a real motion of matter, which takes place not merely in the conducting wire but in the apparently empty space round it.

Admitting the existence of this invisible moving medium, we may find by mathematical methods the vis viva of any portion of it and the distribution of pressure in different directions. The action of these pressures is found to account for the observed attractions and repulsions of electric currents so that we have no need to assume that bodies can act on each other at a distance.

The velocity with which electric disturbances are propagated through this medium can be calculated from known experimental data, and it is found to agree very closely with the velocity of light. Besides this it can be shown that the only kind of electric disturbance which can be propagated through the medium is a transverse displacement and this is the kind of disturbance to which optical inquirers have traced the phenomena of light. Hence we have great reason to believe that light is an electromagnetic phenomenon, and that the radiation of light and heat as well as the forces of electricity and

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(15) The 'coefficient of mutual induction' between two circuits (*Treatise*, **2**: 210 (§584)); and see Number 350.

(16) See the *Treatise*, **2**: 159 (§524).

magnetism depend on one and the same medium, a medium truly material and capable of comparison with the kinds of matter with which we are more familiar though hitherto it has escaped the direct observation of our senses.

For the literature of this view of electromagnetism the reader is referred to Faradays Experimental Researches;(17) to Sir William Thomsons Electrical Papers(18) now being reprinted; to the Reports of the British Association 'On Electrical Standards' particularly those of 1863, 1867 and 1869,(19) and to Maxwell's paper on the Electromagnetic Field, Phil Trans. 1865.(20) For that of the ⟨opposite⟩ other way of treating the subject he is referred to Ampère, Mém de l'Institut 1823,(21) J. Neumann Berlin Trans 1845;(22) W. Weber Elektrodynamische Maasbestimmungen in the Leipzic Transactions from 1846 onwards,(23) a most important contribution to science. See also a masterly paper by Helmholtz on the equations of the motion of electricity in Crelle's Journal for 1870.(24) The whole subject is well treated in Wiedemann's *Galvanismus*.(25)

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(17) Faraday, *Electricity*.

(18) Thomson, *Electrostatics and Magnetism*.

(19) 'Report of the Committee on standards of electrical resistance', in the *Report of the Thirty-third Meeting of the British Association for the Advancement of Science* (London, 1864): 111–76; *Report of the Thirty-seventh Meeting*... (London, 1868): 474–522; and the *Report of the Thirty-ninth Meeting*... (London, 1870): 434–8.

(20) See Number 238 esp. note (1).

(21) See note (5).

(22) F. E. Neumann, 'Die mathematischen Gesetze der inducirten elektrischen Ströme', *Physikalische Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin*. Aus dem Jahre 1845 (Berlin, 1847): 1–87.

(23) Wilhelm Weber, 'Elektrodynamische Maassbestimmungen', in *Abhandlung bei Begründung der Königlichen Sächsischen Akademie der Wissenschaften* (1846): 211–378; in *Abhandlungen der Königlichen Sächsischen Gesellschaft der Wissenschaften*, **1** (1852): 199–381; and (with R. Kohlrausch) *ibid*., **3** (1857): 219–92 (= *Wilhelm Weber's Werke*, 6 vols. (Berlin, 1892–4), **3**: 25–214, 301–471, 609–76).

(24) Hermann Helmholtz, 'Ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper', *Journal für die reine und angewandte Mathematik*, **72** (1870): 57–128.

(25) Gustav Wiedemann, *Die Lehre vom Galvanismus und Elektromagnetismus*, 2 vols. (Braunschweig, $_2$1872–3).

NOTE ON FORBES' WORK ON COLOURS FOR THE *LIFE OF FORBES*[1]

circa 1872

From the original in the University Library, Cambridge[2]

[FORBES ON COLOURS]

In a paper entitled Hints towards a Classification of Colours read before the Royal Society of Edinburgh Dec 4, 1848 & Jan 15 1849[3] Forbes called attention to the importance of a method of defining colours with precision both for scientific and for artistic purposes. In this paper he adopted from Lambert and Mayer[4] not only their arrangement of colours in a pyramid or a triangle but their choice of the colours which are to be regarded as primary, namely red, yellow and blue. He afterwards attempted to form a permanent diagram of colours selected from the collection of artificial enamels employed in the Vatican fabric of mosaic pictures[5] by comparing these enamels with the tints formed by the mixture of the primaries on a rapidly revolving disk. He found, however on attempting to form a neutral gray by the combination of red blue and yellow, that the resulting tint could not be rendered neutral by any combination of these colours; and the reason was found to be that blue and yellow do not make green but a pinkish tint, when neither prevails in the combination. It was plain that no addition of red to this could produce a neutral tint.[6] The fact that green cannot be formed by a mixture of blue and

(1) J. C. Shairp, P. G. Tait and A. Adams-Reilly, *The Life and Letters of James David Forbes, F.R.S., D.C.L., LL.D., late Principal of the United College in the University of St Andrews, &c* (London, 1873): 464–5.

(2) ULC Add. MSS 7655, V, b/17.

(3) J. D. Forbes, 'Hints towards a classification of colours', *Phil. Mag.*, ser. 3, **34** (1849): 161–78; and see *Proc. Roy. Soc. Edinb.*, **2** (1848–9): 190, 214–16.

(4) Forbes, 'Hints towards a classification of colours': 161, 168–70; and see Tobias Mayer, 'De affinitate colorum commentatio', in *Opera Inedita*, ed. G. C. Lichtenberg (Göttingen, 1775): 33–42; and J. H. Lambert, *Beschreibung einer mit Calauischem Wachse ausgemalten Farbenpyramide* (Berlin, 1772).

(5) See Forbes, 'Hints towards a classification of colours': 177–8.

(6) This statement is drawn from a supplementary note on Forbes' experiments in Maxwell's paper 'Experiments on colour, as perceived by the eye, with remarks on colour-blindness', *Trans. Roy. Soc. Edinb.*, **21** (1855): 275–98, esp. 291–2 (= *Scientific Papers*, **1**: 145–6). On the circumstances under which this note was added to the proofs of Maxwell's paper, see Volume I: 302–3.

yellow was pointed out by E. C. Wünch* and by Young†[(7)] but the contrary was still believed by the highest optical authorities. The reason why mixtures of blue and yellow pigments are often green was soon after explained by Helmholtz;[(8)] and[(a)] one of Forbes pupils who[(b)] witnessed his experiments was led by them to make experiments on the mixtures of the colours of the solar spectrum which showed that a yellow equal to that of the spectrum can be produced by the mixture of green and red light.[(9)]

* Versuche und Beobachtungen über die Farben des Lichts, Leipzig 1792[(7)]

† Lecture XXXVII[(7)]

(a) {Tait} Clerk Maxwell who was

(b) {Tait} ⟨who⟩ and

(7) Young states: 'we may consider white light as composed of a mixture of red, green, and violet only'; see Thomas Young, *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, ed. P. Kelland, 2 vols. (London, 1845), **1**: 344. See also Chrétien-Ernst Wünsch, *Versuche und Beobachtungen über die Farben des Lichts* (Leipzig, 1792), where Young's selection of red, green and violet as the three primary colours is anticipated. (This work is cited in Young, *Course of Lectures*, **1**: 344n.)

(8) Hermann Helmholtz, 'Ueber die Theorie der zusammengesetzten Farben', *Ann. Phys.*, **87** (1852): 45–66. Helmholtz demonstrated that while the mixture of coloured lights is an additive process, pigment mixing is subtractive: see Volume **I**: 300n.

(9) J. Clerk Maxwell, 'On the theory of compound colours, and the relations of the colours of the spectrum', *Phil. Trans.*, **150** (1860): 57–84, esp. 77–8 (= *Scientific Papers*, **1**: 436).

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MANUSCRIPT ON THE CLASSIFICATION OF THE PHYSICAL SCIENCES[1]

LATE 1872 – EARLY 1873[2]

From the original in the University Library, Cambridge[3]

REMARKS ON THE CLASSIFICATION OF THE PHYSICAL SCIENCES

According to the original meaning of the word Physical Science would be that knowledge which is conversant with the order of nature, that is, with the regular succession of events whether mechanical or vital in so far as it has been reduced to a scientific form. The Greek word Physical would thus be the exact equivalent of the Latin word Natural.[4]

In the actual development, however, of modern science and its terminology, these two words have come to be restricted each to one of the two great branches into which the knowledge of nature is divided according to its subject-matter. Natural Science is now understood to refer to the study of organized bodies and their development while Physical Science investigates those phenomena primarily which are observed in things without life, though it does not give up its claim to pursue this investigation when the same phenomena take place in the body of a living being.

In forming a classification of sciences our aim must be to determine the best arrangement of these sciences in the state in which they now exist. We therefore make no attempt to map out a scheme for the science of future ages. We can no more lay down before hand the plan according to which science will be developed by our successors than we can anticipate the particular discoveries which they will make.

Still less would we found our classification on the order in time according to which different sciences have been developed. This would be no more scientific than the classification of the properties of matter according to the senses by which we have become acquainted with their existence.

It is manifest that there are some sciences, of which we may take arithmetic as the type, in which the subject matter is abstract, capable of exact

(1) Maxwell served as 'Physical Sciences' editor of the *Britannica*; this manuscript formed the substance of the article on 'Physical Sciences', in *Encyclopaedia Britannica* (9th edn) **19** (Edinburgh, 1885): 1–3. See note (14).

(2) See Maxwell's reference to the publication of the *Treatise*, *infra*; and note (13).

(3) ULC Add. MSS 7655, V, h/9.

(4) Compare Volume I: 419–31, 662–74.

definition, and incapable of any variation arising from causes unknown to us, which would in the slightest degree alter its properties.

Thus in arithmetic the properties of numbers depend entirely on the definitions of these numbers and these definitions may be perfectly understood by any person who will attend to them.

The same is true of theoretical geometry though, as this science is associated in our minds with practical geometry, it is difficult to avoid thinking of the probability of error arising from unknown causes affecting the actual measurement of the quantities.

There are other sciences again of which we may take biology as the type, in which the subject matter is concrete, not capable of exact definition and subject to the influence of many causes quite unknown to us.

Thus in biology many abstract words such as species, generation &c may be employed but the only thing which we can define is the concrete individual and the ideas which the most accomplished biologist attaches to such words as species or generation have a very different degree of exactness from those which mathematicians associate, say, with the class or order of a surface, or with the umbilical generation of conicoids.

Sciences of this kind are rich in facts, and will be well occupied for ages to come in the coordination of these facts, though their cultivators may be cheered in the mean time by the hope of the discovery of laws like those of the more abstract sciences, and may indulge their fancy in the contemplation of a state of scientific knowledge when maxims cast in the same mould as those which apply to our present ideas of dead matter will regulate all our thoughts about living things.

What is commonly called Physical Science occupies a position intermediate between the abstract sciences of arithmetic algebra and geometry and the morphological and biological sciences.

The principal Physical Sciences are as follows.

A The Fundamental Science of Dynamics or the doctrine of the Motion of Bodies as affected by Force.

The divisions of Dynamics are[5]

α Kinematics, or the investigation of the kinds of motion of which a body or system of bodies is capable without reference to the cause of these motions.[6] This science differs from ordinary geometry only in introducing the idea of motion, that is change of position going on continuously in space and time.

(5) See Number 266 and Volume I: 665–6.

(6) On the term 'kinematics' see Volume I: 665n.

Kinematics includes, of course, geometry, but in every existing system of geometry, the idea of motion is freely introduced, to explain the tracing of lines the sweeping out of surfaces and the generation of solids.

β Statics, or the investigation of the equilibrium of forces, that is to say, the conditions under which a system of forces may exist without producing motion of the body to which they are applied. Statics includes the discussion of systems of forces which are equivalent to each other.

γ Kinetics or the relations between the motions of material bodies and the forces which act on them.(7) Here the idea of matter as something capable of being set in motion by force, and requiring a certain force to generate a given motion is first introduced into physical science.

δ Energetics or the investigation of the force which acts between two bodies or parts of a body as dependant on the conditions under which action takes place between one body or part of a body and another so as to transfer energy from one to the other.(8)

The science of Dynamics may be divided in a different manner with respect to the nature of the body whose motion is studied. This forms a cross division.

1 Dynamics of a particle, including its kinematics or the theory of the tracing of curves, its statics, or the doctrine of forces acting at a point, its kinetics or the elementary equations of motion of a particle, and its energetics including, as examples, the theory of collision and that of central forces.

2 Dynamics of a connected system, including the same subdivisions. This is the most important section in the whole of Physical Science as every dynamical theory of natural phenomena must be founded on it. I shall be happy to make a short statement of this if desirable.

The subdivisions of this again are

2a Dynamics of a Rigid System, or a body of invariable form.

2b Dynamics of a Fluid, including the discussion

I of its possible motion

II of the conditions of its equilibrium (Hydrostatics)

III of the action of force in producing motion (Hydrodynamics) (Not so unsatisfactory since Helmholtz(9) Stokes & Thomsons(10) investigations.)

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(7) See Number 377 note (4).

(8) See W. J. M. Rankine, 'Outlines of the science of energetics', *Edinburgh New Philosophical Journal*, **2** (1855): 120–41.

(9) See especially: Hermann Helmholtz, 'Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen', *Journal für die reine und angewandte Mathematik*, **55** (1858): 25–55.

(10) See especially: G. G. Stokes, 'On the steady motion of incompressible fluids', *Trans.*

IV of the forces called into play by change of volume.
2c Dynamics of an Elastic Body.
2d Dynamics of a Viscous Body.

B The secondary Physical Sciences.

Each of these sciences consists of two divisions or stages. In the elementary stage it is occupied in deducing from the observed phenomena certain general laws, and then employing these laws in the calculation of all varieties of the phenomena.

In the dynamical stage the dynamical laws already discovered are analysed and shown to be equivalent to certain forms of the dynamical relations of a connected system (A.2) and the attempt is made to discover the nature of the dynamical system of which the observed phenomena are the motions.

This dynamical stage includes of course several other stages rising one above the other. For we may successfully account for a certain phenomenon, say the turning of a weather cock towards the direction of the wind, by assuming the existence of a force having a particular direction and tending to turn the tail of the cock in that direction. In this way we may account not only for the setting of the weathercock but for its oscillations about its final position. This therefore is entitled to rank as a dynamical theory.

But we may go on and discover a new fact that the air exerts a pressure and that there is a greater pressure on that side of the cock on which the wind blows. This is a further development of the theory as it tends to account for the force already discovered.

We may go on and explain the dynamical connexion between this inequality of pressure and the motion of the air regarded as a fluid.

Finally we may explain the pressure of air on the hypothesis that the air consists of molecules in motion, which strike against each other and against the surface of any body exposed to the air.

The dynamical theories of the different physical sciences are in very different stages of development, and in almost all of them a sound knowledge of the subject is best acquired by adopting, at least at first the method which we have called elementary, that is to say the study of the connexion of the phenomena peculiar to the science without reference to any dynamical explanations or hypotheses.

Thus we have

Camb. Phil. Soc., **7** (1842): 439–53; and their series of 'Notes on hydrodynamics', *Camb. & Dubl. Math. J.*, **2** (1847): 282–6; *ibid.*, **3** (1848): 89–93, 121–7, 209–19; *ibid.*, **4** (1849): 90–4.

I Theory of Gravitation

with discussion of the weight and motion of bodies near the earth the whole of physical astronomy and figure of the earth. There is a great deal of dynamics here but we can hardly say that there is even a beginning of a dynamical theory of the method by which bodies gravitate towards each other.

II Theory of the action of Pressure and Heat in changing the dimensions and state of Bodies.

This is a very large subject and might be divided into two parts one treating of the action of Pressure and the other of Heat. But it is much more instructive to study the action of both causes together, because they produce effects of the same kind, and therefore mutually influence each other. Hence the term Thermodynamics might be extended to the whole subject were it not that it is already restricted to a very important department relating to the transformation of energy from the thermal to the mechanical form and the reverse.

The divisions of the subject are

a Physical states of a substance, Gaseous, liquid, and solid.
Elasticity of volume in all three states.
Elasticity of figure in the solid state.
Viscosity in all three states Plasticity in the solid state.
Surface-tension or Capillarity.
Tenacity of solids, Cohesion of liquids, Adhesion of gases to liquids & solids.

b Effects of heat in
raising temperature
altering size and form
changing physical state

c Thermometry d Calorimetry

e Thermodynamics or the mutual convertibility of heat and work.

f Dissipation of energy by
Diffusion of matter by mixture
Diffusion of motion by internal friction of fluids
Diffusion of heat by conduction.

g Theory of propagation of Sound, vibrations of strings rods and other bodies.

See my book on Heat (Longmans)(11) for a sketch of this subject.

(11) J. Clerk Maxwell, *Theory of Heat* (London, 1871, $_2$1872).

III Theory of Radiance

a Geometrical Optics – Theory of conjugate Foci and of Instruments.
b Velocity of Light in different media.
c Prismatic analysis of Light – Spectroscopy.
Radiant Heat Visible Radiance Ultraviolet rays
Calorescence &c Fluorescence &c
d Colours of thin plates, diffraction &c.
d′ Proof of the existence of wave-lengths and wave periods (preparation for dynamical theory).
e Polarized Light radiant heat &c.
e′ The disturbance is transverse to the ray.
f Quantity of energy in the total radiation from a hot body. Prévosts Theory of Exchanges(12) &c.
g Theory of Three Primary Colours.

IV Electricity and Magnetism

a Electrostatics or distribution and effects of electricity in equilibrium.
b Electrokinematics or distribution of currents in conductors.
c Magnetism and Magnetic Induction (Diamagnetism &c).
d Electromagnetism or the effects of an electric current at a distance.

Under b we may discuss Electrochemistry or the theory of Electrolysis.

Under c Terrestrial Magnetism and Ships Magnetism and after d comes Electrokinetics or electromagnetic phenomena considered with reference to the fundamental science of dynamics.

There is also Faradays discovery of the effect of magnetism on light and the electromagnetic theory of light.

See my book on Electricity and Magnetism Clarendon press series Vol I is printed off & will be published when Vol II is printed.(13)

J. Clerk Maxwell

I quite approve of an article on Measurement,(14) including

a Systems of measures and National standards
b Methods of measurement.

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(12) Pierre Prevost, 'Mémoire sur l'équilibre du feu', *Journal de Physique*, **38** (1791): 314–22; Prevost, *Du Calorique Rayonnante* (Paris/Geneva, 1809).

(13) See Number 448, esp. note (5).

(14) An article on 'Measures and Measurement' is proposed in a draft 'Outline of Scheme of Physical Sciences' (ULC Add. MSS 7655, V, h/8) to which Maxwell's MS may be a response. See note (1).

There ought also to be some indication of the branches of mathematics which have hitherto been found most useful in Physical Science.

I have not included Chemistry in my list, because, though ⟨Physical⟩ Dynamical Science is continually reclaiming large tracts of good ground from the one side of Chemistry, Chemistry is extending with still greater rapidity on the other side, into regions where the dynamics of the present day must put her hand upon her mouth.

But Chemistry is a Physical Science, and that of very high rank. I do not, however, pretend to be able to go over its possessions and to show strangers the boundaries.

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LETTER OF REFERENCE FOR JAMES THOMSON[1]

7 JANUARY 1873

From a holograph copy in the University Library, Glasgow[2]

COPY OF CERTIFICATE BY PROFESSOR CLERK MAXWELL

Professor James Thomson of Belfast is a man of sound scientific attainments and is thoroughly acquainted with the principles of Engineering. He has proved this by the works he has executed, by the inventions he has patented and by the discoveries in science which he has published. Of all men I know I consider him the best qualified to succeed Professor Rankine in the University of Glasgow and to maintain the high character of the education of that University.

(signed) J. Clerk Maxwell
Professor of Experimental Physics
in the University of Cambridge

Cambridge, January 7th 1873

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(1) On the circumstances of James Thomson's candidature for the Professorship of Engineering in the University of Glasgow see S. P. Thompson, *The Life of William Thomson Baron Kelvin of Largs*, 2 vols. (London, 1910), **2**: 632–3.

(2) Glasgow University Library, Kelvin Papers, T 128. Previously published in A. T. Fuller, 'James Clerk Maxwell's Glasgow manuscripts: extracts relating to control and stability', *International Journal of Control*, **43** (1986): 1593–612, on 1611–12.

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POSTCARD TO WILLIAM THOMSON

22 JANUARY 1873[1]

From the original in the King's College London Archives[2]

[Glenlair]

The Tomlinson Correspondence is found.[3]

Our knowledge of magnetization is probably statistical. The doctrine that magnetic energy is potential ditto while electromagnetic energy is kinetic leads to hopeless confusion especially when currents act on magnets.[4] Now a current is certainly a kinetic thing Q.E.D. It is very remarkable that in spite of the *curl* in the electromagnetic equations of all kinds[5] Faradays twist of polarized light[6] will not come out without what the schoolmen call local motion.[7]

$$\frac{dp}{dt}$$

(a)

(a) {Thomson} Clerk Maxwell $\left(\frac{dp}{dt}\right)$ Keep K Seen Sep 22/99

(1) January 1873 seems a likely date for Maxwell's card to Thomson: see notes (3) and (7). No year is visible in the postmark. A note in the King's College archive records that the British Museum suggests 1872 as the likely date for its printing.

(2) Maxwell Papers, King's College London Archives. First published in Larmor, 'Origins': 748.

(3) Maxwell may well be alluding to his correspondence with Charles Tomlinson (in 1869) about the publication of the manuscripts of Henry Cavendish: see Numbers 435 and 459, and also Maxwell's letter to Thomson of 25 March 1873 (Number 448) on his intention to edit the Cavendish papers.

(4) See the *Treatise*, **2**: 246–51 (§§630–38).

(5) See the *Treatise*, **2**: 237–8 (§619) where Maxwell writes the general equations of the electromagnetic field in quaternion form, writing $V\nabla\sigma$ for the curl of the vector σ. On 'curl' see the *Treatise*, **1**: 28 (§25), and Number 347.

(6) On Faraday's discovery of the rotation of the plane of polarization of linearly polarised light in a magnetic field see the *Treatise*, **2**: 400 (§807); and Michael Faraday, 'On the magnetization of light and the illumination of magnetic lines of force', *Phil. Trans.*, **136** (1846): 1–20 (= *Electricity*, **3**: 1–26). In his paper 'Dynamical illustrations of the magnetic and the heliocoidal rotatory effects of transparent bodies on polarized light', *Proc. Roy. Soc.*, **8** (1856): 150–8, Thomson had argued that the phenomenon could be explained by a vortical theory of magnetism. See Maxwell's letter to Thomson of 10 December 1861 (Volume I: 692–8).

(7) Compare the *Treatise*, **2**: 416 (§831); 'I think we have good evidence for the opinion that some phenomenon of rotation is going on in the magnetic field, [and] that this rotation is performed by a great number of very small portions of matter, each rotating on its axis'. Proofs of *Treatise* §§801–30 (on the magnetic action on light and the theory of molecular vortices) are dated January 1873 (ULC Add. MSS 7655, IV/2).

DRAFT LETTER TO THE DUKE OF DEVONSHIRE[1]

LATE JANUARY – EARLY FEBRUARY 1873[2]

From the original in the University Library, Cambridge[3]

My Lord Duke

In the interest of science and at the suggestion of several scientific men I write to ask your help in securing the preservation of those manuscripts of Henry Cavendish which relate to electricity.

Mr Tomlinson informs me[4] that these papers are in the possession of Thomas Harris Esqre only son of the late Sir William Snow Harris.[5] They were put into the hands of Sir William by the Earl of Burlington.[6] Sir W. Snow Harris quoted them in his books on Electricity[7] and used to speak of their importance and of the use he intended to make of them.[8] At his death, however they passed into the hands of his son, || and Mr Tomlinson then made some efforts to get the papers deposited in the library of the Royal Society. There seems to have been some difficulty about giving up the papers but I should think this difficulty would be removed if the papers were asked for by your Grace who I understand to be the representative of the Earl of Burlington and of Henry Cavendish.

If the papers were in your own possession || or in that of a responsible public body men of science would be relieved from the anxiety which they must feel when such papers are liable to [....]

Many men of science are naturally anxious that the preservation of papers so important should not depend on the accidents attendant on the transmission of such manuscripts from hand to hand and all such anxiety would be removed if your Grace whom I understand to be the representative both of the Hon Henry Cavendish and of the Earl of Burlington were to take steps to obtain the papers from Mr Harris.

The permanent security of these papers depends at present on the mode in which they may be handed down from o[ne....]

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(1) William Cavendish, Seventh Duke of Devonshire, Chancellor of the University of Cambridge.

(2) See Numbers 434, 448 and 459.

(3) ULC Add. MSS 7655, II/90.

(4) In 1869; see Number 459.

(5) Sir William Snow Harris had died in 1867.

(6) The Duke himself, who had succeeded his grandfather as second Earl of Burlington in 1834, succeeding his cousin as Seventh Duke of Devonshire in 1858 (*DNB*); see Number 459.

(7) William Snow Harris, *A Treatise on Frictional Electricity*, ed. C. Tomlinson (London, 1867): 23, 45, 58, 121, 208, 223.

(8) See Maxwell's account in *The Electrical Researches of the Honourable Henry Cavendish, F.R.S.* (Cambridge, 1879): xl–xli.

Men of science are anxious that papers so important should be both safe and accessible. This would be the case if they were in the possession of your Grace whom I understand to be the representative of the author and of the Earl of Burlington who deposited them with Sir W. S. Harris.

M^r Harris is naturally unwilling to place them in the hands of persons who have no right to them, but if he were requested by your Grace to restore them to the family of the author I have no doubt he would do so.[(9)]

The following extract from a note in Sir W. Thomson's recent work on Electrostatics and Magnetism will indicate the importance of these papers not only to the ⟨history⟩ of science and to the biography of Cavendish but to the science of electricity in its present state.

Extract[(10)]

For the knowledge of the present ownership of I am entirely indebted to Charles Tomlinson Esq^re FRS 3 Ridgmount Terrace, Highgate, N, whose letter I enclose. He gives the address of Thomas Harris Esq^re Barrington House, Southsea.

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(9) But see Number 459.

(10) Compare *The Electrical Researches of... Cavendish*: xxxix, where Maxwell quotes Thomson's note of 2 July 1849, reproduced in his *Electrostatics and Magnetism*: 180n, on the accuracy of Cavendish's value for the ratio of the capacity of a disc to that of a sphere of the same radius; 'Cavendish's unpublished MSS... [are] a most valuable mine of results.... It is much to be desired that... [they] should be published complete'.

ON A PROBLEM IN THE CALCULUS OF VARIATIONS IN WHICH THE SOLUTION IS DISCONTINUOUS[1]

FEBRUARY 1873[2]

From the original in the University Library, Cambridge[3]

[ON THE RIDER ON THE THIRD QUESTION IN THE MATHEMATICAL TRIPOS PAPER OF WEDNESDAY AFTERNOON, 15 JANUARY 1873][4]

The question itself was suggested by some of the road making problems of Dupin[5] and was as follows.

If the velocity of a carriage along a road is proportional to the cube of the cosine of the inclination of the road to the horizon, determine the path of quickest ascent from the bottom to the top of a hemispherical hill, and shew that it consists of the spherical curve described by a point of a great circle which rolls on a small circle described about the pole with a radius $\frac{\pi}{6}$, together with an arc of a great circle. How is the discontinuity introduced into the problem?[6]

Taking a as the radius of the sphere and referring the curve to θ and ϕ the polar distance and azimuth and writing as usual p for $\frac{d\phi}{d\theta}$ we find for the element of length

$$ds = \sqrt{1+p^2\sin^2\theta}\,d\theta$$

and for the element of ascent

$$dz = -\sin\theta\,d\theta$$

(1) A problem mentioned by Maxwell in his letter to William Thomson of 21 March 1871 (Number 362; see esp. note (7) for a preliminary draft of the problem).

(2) This manuscript draft is a preliminary to Maxwell's paper 'On a problem in the calculus of variations in which the solution is discontinuous', *Proc. Camb. Phil. Soc.*, 2 (1873): 294–5 (= *Scientific Papers*, 2: 310); read 3 February 1873.

(3) ULC Add. MSS 7655, V, d/13.

(4) As stated in 'On a problem in the calculus of variations': 294.

(5) Charles Dupin, *Applications de Géométrie et de Mécanique* (Paris, 1822): 75–186.

(6) As stated in *The Cambridge University Calendar for the Year 1873* (Cambridge, 1873): 544.

whence we find putting α for the inclination of the curve to the horizon

$$\cos^2\alpha = \frac{\cos^2\theta+p^2\sin^2\theta}{1+p^2\sin^2\theta}$$

and the quantity to be made a minimum is $V = \int \frac{ds}{\cos^3\alpha}$ taken from the equator to the pole or in terms of θ only $V = \int_0^{\frac{\pi}{2}} \frac{(1+p^2\sin^2\theta)^2}{(\cos^2\theta+p^2\sin^2\theta)^{\frac{3}{2}}}\,d\theta.$

The solution by the calculus of variations is easy. The variable ϕ does not appear and therefore the sole condition is to be found by differentiating the expression under the integral sign with respect to p and equating the result to zero. This gives the equation

$$p\sin^2\theta(p^2\sin^2\theta+4\cos^2\theta-3) = 0$$

whence we find either

$$p = 0 \quad \text{or} \quad p = \pm\sqrt{3-\cot^2\theta}$$

whence it follows that

$$\cos\alpha = \cos\theta \quad \text{or} \quad \cos\alpha = \pm\tfrac{1}{2}\sqrt{3}.$$

When θ is less than $\frac{\pi}{6}$ the first solution corresponds to a minimum and the other two are impossible. When θ is greater than $\frac{\pi}{6}$ the first solution corresponds to a maximum and the other two to minima.

Hence the path must [be] a curve of constant inclination of 30° to the horizon from the bottom of the hill to that point at which the surface of the hill itself has this inclination. From this point to the top the path must be the great circle for which $p = 0$.

This solution would apply to the case of any solid of revolution with its axis vertical but in the case of a sphere the determination of the path of constant slope is easy. For the plane normal to the path passes through the centre of the sphere and is always inclined 30° to the vertical. It therefore always touches the small circle described about the pole with that radius and if a point fixed in the plane coincides with the centre of the sphere, while the sphere rolls on this circle, any point which coincides with the surface of the sphere will trace on that surface the path of constant slope required.

By suppressing [from] the rolling plane all but that circle of it which is always a great circle of the sphere the statement is reduced from solid to spherical geometry as in the statement of the question.

Note. It is manifest that if the inclination of the road is so small that it winds several times round the hill, the distance between consecutive windings is everywhere equal to the circumference of the small circle. In the case before us this circumference is equal to half a great circle.

If the time along a horizontal radius is taken for unity the time from the base of the hemisphere to the cusp is $\frac{8}{3}$ and the time up the arc of the great circle to the pole is

$$\frac{1}{3}+\frac{1}{4}\log_e 3$$

making the whole time of ascent

$$3+\frac{1}{4}\log_e 3.$$

The equation of the curve is

$$x = \cos\phi(1-4\sin^2\phi)$$

$$y = \sin\phi\left(\frac{5}{2}-3\sin^2\phi\right)$$

$$z = \frac{\sqrt{3}}{2}\sin\phi.$$

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LECTURE[1] ON FARADAY'S LINES OF FORCE[2]

EARLY 1873[3]

From the originals in the University Library, Cambridge[4]

[1][5] ON FARADAYS LINES OF FORCE

The statement has been made so often that I almost need your pardon for repeating it, that modern times have been distinguished from all that preceded them by the greater development of industry and by the greater progress of physical science. The men who, in recent times, have devoted themselves either to searching out the forces of nature, or to rendering these forces available for the supply of human wants, have made a deeper and more enduring mark on the face of the world than its rulers and conquerors, its philosophers and statesmen, its preachers and poets. Our meeting tonight is in commemoration of James Watt, the results of whose labours I have no intention to enumerate. It is true that tonight we have met in his native place, and that we have been reminded of his features by the statue at the door and of his name by the title of this building but in any other part of the habitable globe if we wish for something to remind us of James Watt we have only to look about us. Conquerors may have carried a people captive from their own land and planted them in another, but the steam engine has drawn away to its encampment on the coal measures all the craftsmen and smiths and every cunning workman, till the ancient rules and maxims of simple life, such as sufficed for the guidance of James Watt and his contemporaries, have broken down under the pressure of the multitude of the workers and the exigencies of their work. The legislator of the older type was counted famous when he had instituted laws and customs by which violence was checked, and every man was encouraged to abide in the condition and station wherein he was born. The development of industry has introduced new modes of gaining a livelihood and abolished others so that both employers and employed in their endeavour to walk in the paths of honour and virtue, have new problems set before them of a more complicated order than our fathers had to solve.

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(1) This draft apparently forms the text of a lecture to the Greenock Philosophical Society in honour of the anniversary of the birth of James Watt on 19 January 1736. See notes (9) and (10).

(2) The main text is a draft of Maxwell's lecture to the Royal Institution on Friday, 21 February 1873; see 'On action at a distance', *Proceedings of the Royal Institution of Great Britain*, **7** (1873–5): 44–54 (= *Scientific Papers*, **2**: 311–23); and see Number 438.

(3) See notes (1) and (2).

(4) ULC Add. MSS 7655, V, c/19, 20.

(5) ULC Add. MSS 7655, V, c/19.

There is no doubt that the invention of James Watt has like all other divine gifts greatly increased our responsibility. Every one of us requires more than ever, especially in those short intervals which labour allows us, to call to mind the nobler ends of human life. The beauty and dignity of a well ordered life are not the exclusive property of a primitive people but to preserve them amid the rattle of machinery and the press of business demands a more constant vigilance and involves a higher mental cultivation than was required by the contemporaries of James Watt.

James Watt is also connected both directly and indirectly with the other characteristic of our time, the growth of physical science. His own researches on Heat and on its action on water can hardly be separated from the use that he made of them – the economising of the forces at our disposal by turning them as much as possible into the channel of work useful to us.(6) The same principle, considered still with reference to the steam engine, is the foundation of the next great step in our scientific knowledge of heat – Carnots 'Reflexions on the motive power of fire'.(7) The most important contribution to our accurate knowledge of heat on various bodies is also by a Frenchman and is also connected with the steam engine, that of M. Regnault.(8)

But I understand that the progress of the science of heat since the time of Watt has been described to you already by D^r Joule,(9) who has himself made the most important measurements on which the theory of heat depends, and who has with a rare sagacity contributed to the future progress of almost every branch of physical science by his numerous measurements of energy in all its various forms.

Sir William Thomson also has laid before you some of the results of modern speculation as to the precise nature of the motion to which the phenomena of heat and electricity are due, and has shown that the explanation of these phenomena by the intestine motion of agitation of the particles of bodies has reached a stage in which it not only submits to the test of exact calculation,

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(6) J. P. Muirhead, *The Origin and Progress of the Mechanical Inventions of James Watt*, 3 vols. (London, 1854); Muirhead, *The Life of James Watt, with Selections from his Correspondence* (London, 1858).

(7) Sadi Carnot, *Réflexions sur la Puissance Motrice du Feu et sur les Machines propres à développer cette puissance* (Paris, 1824).

(8) H. V. Regnault, *Relation des Expériences entreprises ... pour déterminer les principales lois et données numériques qui entrent dans le calcul des machines à vapeur*, 3 vols. (Paris, 1847–70); reprinted from *Mémoires de l'Académie des Sciences de l'Institut de France*, **21** (1847): 1–767; *ibid.*, **26** (1862): iii–x, 3–928; *ibid.*, **37**, part 1 (1868): 3–575; and **37**, part 2 (1870): 599–968.

(9) Joule had delivered a lecture, 'On some facts in the science of heat developed since the time of Watt', to the Greenock Philosophical Society on 19 January 1865; see D. S. L. Cardwell, *James Joule, A Biography* (Manchester, 1989): 183, 313.

but leads of itself to the knowledge of new phenomena, the existence of which has been afterwards verified by experiment.(10)

I propose tonight to lead you into a different field of speculation, and to ask you to turn your minds to the familiar though mysterious phenomenon of the transmission of force. The mode in which Faraday was led by his peculiar genius and unremitting investigations to look on this phenomenon is different from that generally adopted by other modern inquirers, and my special aim will be to enable you to place yourselves at Faradays point of view, and to enable you to understand what he meant by the phrase 'Lines of Force' which we meet with so frequently in his writings.(11)

In order to do so I must have the indispensable help of your own best powers of thought. If I had merely to describe to you some new discovery in science, I should be able to avail myself of your previous knowledge as a foundation, and to erect thereon a representation of the new fact which you were to place beside those old ones which you knew before. The greater your previous knowledge, the easier would be my task. But what I have to do is something quite different. I have to shew you facts with which you are already acquainted in a light of a different character from that which the most illustrious philosophers have shed upon them – a light which the wisest among them would probably have avoided as deceptive and misleading had it been presented to him in his own time, because the slow yet steady progress of science has only in more recent times prepared us for its reception.

I have therefore to begin at the very beginning and any previous training you may have had in scientific ideas must be for the present as if it had never been.

We have to consider the mode in which one body acts on another, or rather the mode in which two bodies act on each other, for such action is always mutual. And here I would have you notice how very important right opinions on matters of this kind are. Wrong opinions in dynamics are the cause of errors which run through every department of life. The ordinary forms of speech which we cannot avoid using are the product of a time when men were satisfied with a few rough notions on matters of this kind, deduced from casual observation but sufficient for the purposes of ordinary life. I do not here allude to the phrases of the sun rising and such like, which are perfectly scientific methods of indicating an actual fact. I refer rather to the inveterate association between motion and effort which causes us when we see a body

(10) Possibly his lecture at Greenock in January 1869 on 'Elasticity viewed as a mode of motion'; see S. P. Thompson, *The Life of William Thomson Baron Kelvin of Largs*, 2 vols. (London, 1910), **2**: 1243.

(11) Compare Maxwell, 'On action at a distance': 44 (= *Scientific Papers*, **2**: 311).

moving in any direction to consider it as exerting a continued effort to travel in that direction, just as we ourselves would have to labour continuously if we had to make a long journey over a rough country.

Another association derived from the same source is that, because the traveller cannot help knowing in which direction he is toiling onwards, so we must always know the direction of our own motion, or at least whether we are in motion or at rest. It is impossible to overestimate the influence which the experience of smooth sailing has had on the minds of men in enabling them to get rid of these habits of thought. It is only by reading the works of the great founders of mechanical science and those of the philosophers of the old school that we can see how great was the labour of establishing right opinions on these points, and how tenaciously the wrong opinions were held by men, who, if they had lived now, would have certainly been eminent in science themselves. By these labours, however, the whole atmosphere of scientific thought appears to have been changed, and many statements, then thought paradoxical, are now accepted by the beginner without the slightest feeling of strangeness.

But the process is by no means complete, and a great deal has yet to be done before even the whole of what is called the scientific world is thoroughly free of dynamical errors.

And this will never be done merely by teaching everybody mathematics. What is necessary and sufficient is that the facts should be presented to the mind, and that the mind should be forced into contact with the facts.

There are certain facts then to which I wish you to apply your minds.

We see that bodies move, and that the direction and velocity of the motion of a particular body do not always remain the same. We find, however that when any change occurs in the motion of the body we can account for it by supposing that another body in the neighbourhood has in some way produced this change and we are confirmed in this supposition by observing that a change of motion of a kind exactly opposite has occurred in the other body.

The most familiar illustrations of these facts are those presented by the collisions of bodies projected through the air or of balls rolling on a smooth billiard table. In these cases both the bodies which act on each other are solid and visible bodies and their masses are comparable to each other. In those cases in which one of the bodies is the earth or a body connected with the earth the alteration of motion of the larger body is so exceedingly small that it is not perceived, and the reciprocal nature of the action is lost sight of. In other cases, as when a bullet flies through the air, the nature of the action is in some degree hidden, on account of the air, one of the bodies which act, being invisible.

Here again we may observe how important the observation of special phenomena has been in calling up in the minds of men those true ideas of things by which they may rightly understand not only these special phenomena but every ordinary occurrence.

The first true ideas about relative motion were derived from the experience of men who travelled by water, and the first true ideas about the reciprocity of force were probably derived from observations on games with balls. In ordinary circumstances, the motions of bodies are affected by so many different causes, that until some exceptionally simple phenomenon presents itself, we do not know what is the right thing to attend to first. As soon however as we become acquainted with this more simple phenomenon and we have been put in the right way of looking at it, either by our own efforts or by the teaching of others, then we know in ourselves that we have obtained a key to one department of the mystery of nature, and that however different any other phenomenon of motion may be from that which gave us the key, the key must nevertheless be equally appropriate to it.

Such exceptionally simple phenomena, when purposely produced, are called illustrative experiments. They are so called because they throw light on the subject, and enable the mind to see clearly those truths which it is unable to distinguish when they are exemplified in the more complicated form of ordinary occurrences.

This is the chief, and indeed the only value of an illustrative experiment. When once the mind has grasped the truth which it illustrates, the experiment has fulfilled its purpose. When we have seen the action and reaction of two balls striking each other we make no special measurements to prove that this action and reaction are equal and opposite. I am not aware that anyone ever even proposed to make such a measurement, or suggested a method of doing it. All that is necessary is to direct the mind to the experiment till it perceives that in this case the action between the two balls is reciprocal, and it immediately concludes that in all cases the action of bodies on each other may be reduced to reciprocal actions between pairs of the bodies.

These reciprocal actions are called forces, and the doctrine may be stated thus – that every force is *between* two bodies, that is to say, that whatever the force is which acts on one body, it is something which at the same time acts in the opposite direction on some other body.

We now come to the special subject of our consideration in this lecture. Admitting that the action between two bodies is always reciprocal, what is the condition that there shall be such action between two given bodies. Must the two bodies be in actual contact, or can they act on each other when at a distance? On the one hand it has been argued that matter has no power of acting where it is not, and it has been shown that in many cases in which force appears to be transmitted to a distance the action really takes place by means of a series of bodies, or parts of bodies, occupying the intervening space or moving across it.

Thus when we ring a bell by means of a bellrope and wire the successive parts of the rope and the wire are first tightened, and then moved, till at last the bell is rung at a distance by a process in which all the intermediate particles of the rope and the wire have taken part one after the other. We may also ring a bell by forcing a little air into a long tube at the other end of which is a piston which is made to fly out and strike the bell. Here the different portions of air in the tube are moved one after another and the action of the piston at the one end is communicated to the piston at the other end by a process of interaction between a series of portions of air every pair of which may be considered in actual contact.

It is clear therefore that, in certain cases, the action between bodies at a distance may be accounted for by a series of actions between a system of bodies which fill up the intervening space, and it is asked whether in cases in which we cannot perceive the intermediate agents it is not more philosophical to admit the existence of a medium which we cannot yet perceive than to admit that a body can act in a place where it is not.

Experiments
Tuning Fork
Corks in water

To a person ignorant of the existence and properties of air the transmission of force by means of that invisible medium must appear as unaccountable as any other example of action at a distance and yet we know that the action in this case is transmitted with a known velocity by the successive action of contiguous portions so that a sensible time elapses before the force is transmitted from the one end of the tube to the other. Why should we not conclude that the familiar mode of communication of motion by pushing and pulling with our hands is the type and exemplification of all action between bodies, even in cases in which we can observe nothing between the bodies which appears to take part in the action.

Experiments
on magnets

The advocates of the doctrine of action at a distance on the other hand have not been silenced by these arguments. What right say they have we to assert that a body cannot act where it is not? Do we not see instances of action at a distance in the case of a magnet, which acts on another magnet, not only at a distance, but with the utmost indifference to any object which may be placed in the intervening space. If this action depends on something between the two magnets it cannot surely be indifferent whether the space between them is filled with air or not or whether wood or glass be placed between the magnets. And do not the heavenly bodies act on one another across immense intervals which are certainly not filled with anything which can sensibly resist the motion of the filmiest tail of a comet.

Magnets

Besides this, the law of Newton, which every astronomical observation only tends to establish more firmly, asserts that when we know the masses of two bodies and the distance between them we can determine their mutual action from these facts alone, and that we require no additional information about the distribution of other bodies, around or between them. The one portion of matter may be a thousand miles deep in the interior of the earth and the other 100,000 miles deep in the interior of the sun and still the matter surrounding each portion will have no influence on their mutual action. Surely if a medium takes part in transmitting the action, it must make some difference whether the space between the bodies contains nothing but this medium, or whether it is full of dense matter?

But the advocates of action at a distance are not content with instances of this kind, in which the phenomenon even at first sight appears to favour their doctrine. They push their operations into the enemys camp, and assert that even in those cases in which the action is apparently that of contiguous portions of matter, the contiguity is only apparent – that a space always intervenes between the bodies which act on each other, and that so far from action at a distance being impossible it is the only kind of action which ever occurs. Even when one body supports another, they show that they are not in contact by actually measuring the distance between them. If I lay one lens of glass on another, I perceive a set of coloured rings near the place where the one rests on the other.[12] These rings were first explained by Newton who showed that the colour depends on the distance between the surfaces of the two glasses and that as this distance increases as we move further from the point where the one lens rests on the other the colours are arranged in rings about this point the colour of the ring thus forming a measure of the distance between the surfaces.[13] If the lenses are in actual contact, the central point of Newtons Rings is black. But when I simply lay the one lens on the other the central point is not black and it follows that the one lens supports the weight of the other although no part of the surfaces are in contact. To make sure of this I press the lenses together. The colour of the central spot changes, and as the pressure is increased a series of colours appears till at last the central spot becomes black and we then know that the surfaces are in optical contact. It follows that the surfaces were not in optical contact but at a measurable distance even when pressed together by a force considerably greater than the weight of the upper lens and that therefore one piece of glass can act on another piece of glass while a measurable distance intervenes between the nearest parts of their surfaces. This experiment succeeds equally well under the exhausted receiver of an air pump so that the air is certainly not the medium by means of which this action at a distance is effected.

Newton's Rings

(12) See Number 438.

(13) Isaac Newton, *Opticks* (London, $_3$1721): 168–98 (Book II, Part I).

Why then should we continue to maintain a doctrine founded only on the rough experience of a prescientific age that matter cannot act at a distance instead of admitting that all the facts from which our ancestors concluded that contact was essential to action were really cases of action at a distance in which the distance was too small to be discovered by their means of observation. If we are ever to discover the laws of nature we must deduce them by a careful reasoning from the facts of nature, according to our very best information, and not by dressing up in philosophical language the loose opinions of men who had no knowledge of the facts which throw most light on these laws.

And as for those who imagine etherial or other media to account for these actions, without any evidence of the existence of these media, and without first making sure that the media will do their work, and who fill all space three and four times over with ethers of different sorts, why, the less these men talk about their philosophical scruples in admitting action at a distance the better.

When the cumbrous celestial machinery of the Ptolemaic System was got rid of, when the comets had shattered to pieces those crystal spheres by which the planets were supposed to be carried round, and when the cycles and epicycles, orb in orb, by which astronomers had so long contrived to 'save appearances' were blown aside into the Paradise of Fools various attempts were made to supply their place in the heavens. Of these one of the most notable in its time was that of Descartes, who supposed the heavenly bodies to swim along in a medium which fills all space or rather, since he regarded space as essentially material, in that substance which we call space the fluidity of which, where it is fluid, arises from its moving in certain whirls or vortices, and the solidity of which, where it appears solid, arises simply from its quiescence, quiet being, as he remarks, infinitely superior to the best glue for holding things together.

The physical speculations of Descartes are as remarkable for their utter disregard of the first principles of dynamics as his mathematical writings are for the fertility of the genius they display.(14) He seems never to have understood what is meant by the mass of a body as distinguished from its size or its weight and in this ignorance he has been followed by Spinoza, and by most of the professed metaphysicians down to the present day. But the vigour of mind and the boldness of his methods have given a vitality to some of his erroneous opinions which is not even yet extinct, so that we find them springing up in the minds of men who certainly never came in contact with his writings.

(14) Compare Maxwell's comments on Descartes' *Principia Philosophiæ* in Number 350: Appendix, and Number 377.

When Newton demonstrated that the force which acts on each of the heavenly bodies depends on the distance and position of the other bodies, and is directed towards these bodies, and therefore may be described as an attraction, the new theory met with violent opposition from the advanced philosophers of the day. They described the doctrine of gravitation as a return to the exploded method of explaining everything by occult causes, attractive virtues, and the like.(15)

Newton himself, with that wise moderation which was characteristic of all his speculations, answered that he made no pretence of explaining the *mechanism* by which the heavenly bodies act on each other. What he had done was to determine the direction and magnitude of the force acting on each body, and to show how it may be deduced from a knowledge of their relative positions. This was the step in science which Newton asserted he had made. To explain the *cause* of this action was a quite distinct step, and this step Newton in his Principia does not attempt to make.

But so far was Newton from asserting that bodies really act on one another at a distance, independently of anything between, that in a letter to Bentley, which has often been quoted, he says

> It is inconceivable that inanimate brute matter should, without the mediation of something else, which is not material, operate upon and affect other matter without mutual contact; as it must do, if gravitation, in the sense of Epicurus be essential and Newton inherent in it.... That gravity should be innate, inherent, and essential to matter, so that one body can act upon another at distance though a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it.(16)

But the true history of Newtons speculations on this subject may be guessed at from one of his Optical queries relative to a medium or ether.

> Is not this medium much rarer within the dense bodies of the sun, stars and planets than in the empty celestial spaces between them. And in passing from them to great distances does it not grow denser and denser perpetually and thereby cause the gravity of those great bodies to one another, each body endeavouring to go from the denser parts of the medium to the rarer.(17)

And Colin Maclaurin one of his most illustrious disciples tells us

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(15) Leibniz's correspondence with Samuel Clarke is the classic source for the controversy.

(16) *Four Letters from Sir Isaac Newton to Doctor Bentley containing some Arguments in Proof of a Deity* (London, 1756): 25–6. In his lecture 'On action at a distance': 48 (= *Scientific Papers*, **2**: 316) Maxwell noted that Faraday had cited this passage, also in an attempt to provide a Newtonian pedigree for his denial of action at a distance. See Faraday, *Electricity*, **3**: 532n, 571.

(17) Newton, *Opticks*: 325 (Query 21).

It appears from his letters to M^r^ Boyle that this was his opinion early, and if he did not publish it sooner it proceeded from hence only, that he found he was not able, from experiment and observation to give a satisfactory account of its operation in producing the chief phenomena of nature. MacLaurin

Maclaurin goes on to say

Possibly some unskillful men may have fancied that bodies might attract each other by some charm or unknown virtue without being impelled or acted on by other bodies, or by any other powers of whatever kind and some may have imagined that a mutual tendency may be essential to matter, tho' this is directly contrary to the *inertia* of matter described above, but surely Sir Isaac Newton has given no grounds for charging him with either of these opinions.(18)

It appears therefore that both Newton and Maclaurin, so far from considering the law of gravitation a final explanation of the phenomena to which it refers, felt that if that law could be explained as the result of any action of something in the space intervening between the bodies this explanation would form a new and distinct step in science, and they would be ready to welcome it as filling up an acknowledged gap in our knowledge of things.

Newton, we have seen, attempted this step but found it was beyond his power with the means at his disposal, and I need scarcely tell you, that even with our vastly greater stock of scientific methods, the task has seldom been attempted, and never accomplished.

But another of Newtons most brilliant disciples Roger Cotes who edited the second edition of the Principia for Newton adopts quite a different tone of thought. He says that just as we infer, from experiments made in Europe, how bodies will behave in America, so when we find that all the bodies we know gravitate towards each other, we have as good a right to say that gravitation is an essential property of matter as to infer that any other property of matter such as extension mobility or impenetrability is essential to it in America because we find them in all bodies that we know in Europe.(19)

And when the Newtonian philosophy gained ground in Europe it was the opinion of Cotes, rather than that of Newton that became most prevalent, till at last Boscovich propounded a theory in which bodies were supposed to consist of a great number of mathematical points, each endowed with the power of attracting or repelling the other points according to fixed laws

(18) Colin MacLaurin, *An Account of Sir Isaac Newton's Philosophical Discoveries* (London, 1748): 110–11.

(19) See Cotes' 'Editoris praefatio in editionem secundam' to Issac Newton, *Philosophiæ Naturalis Principia Mathematica* (Cambridge, $_2$1713). On Cotes' preface to *Principia*, compare Maxwell's comment in Number 439.

depending on the distance.[20] How it happens that a congeries of mathematical points, when once set in motion, is able to persevere in its motion, and to do a fixed amount of work before it can be stopped, is not explained by the supporters of this theory, and I venture to say that the explanation will never be effected by endowing mere mathematical points with powers of attraction or repulsion however complicated.

But if we consider the history of science with respect to the extension of its boundaries, and leave out of account for the present the development of its ideas, it was most important that the great step made by Newton should be extended to every branch of science to which it is applicable, and this could only be done by studying the effects of forces between bodies at a distance, without attempting to explain how the force is transmitted. No men therefore were better fitted to apply themselves exclusively to the first part of the problem, than those who considered the second part quite unnecessary.

Hence those who during the last century and the early part of the present studied so successfully the laws of electricity and magnetism such as Cavendish, Coulomb, and Poisson,[21] paid no regard to those old notions of 'magnetic effluvia' and 'electrical atmospheres' which had been put forth in the previous century,[22] but turned their undivided attention to the determination of the law according to which the parts of electrified or magnetized bodies attract or repel each other. In this way the true laws of these actions were discovered, and this was done by men who never doubted that the action took place at a distance, without the intervention of any medium and who would have regarded the discovery of such a medium as complicating rather than as explaining the undoubted phenomena of attraction.

We have now arrived at the great discovery of the connexion between electricity and magnetism, by which the theory of action at a distance was to be far more severely tried. Professor Oersted of Copenhagen while lecturing to a private class on the electric current, made a wire red hot by transmitting the current through it, and brought it near a magnetic needle.[23] He had done so several times with the needle perpendicular to the current without noticing any effect, but this time he placed the current parallel to the needle, when, to his surprise, the needle moved so as to set itself perpendicular to the current. He found that the action of the current on the pole of a magnet was

(20) See the Appendix *infra* and note (47); and see also Number 294 note (32).

(21) See also Number 450 notes (6), (7) and (8).

(22) See J. L. Heilbron, *Electricity in the 17th and 18th Centuries* (Berkeley/Los Angeles/London, 1979); and R. W. Home's introduction to *Aepinus's Essay on the Theory of Electricity and Magnetism* (Princeton, 1979): 65–188.

(23) See Number 238 note (17).

neither an attraction nor a repulsion but a force tending to make a magnetic pole move at right angles to the plane passing through it and the wire. He expressed this by saying that 'the electric conflict acts in a revolving manner'.

The most obvious deduction from this new fact was that the action of the current on the magnet is not a push-and pull force, but a rotatory force, and the minds of many were set a speculating on vortices and streams of ether whirling round the current.

But Ampère, by a combination of mathematical skill with experimental ingenuity, first proved that two electric currents act on one another, and then analyzed this action into the resultant of a system of push and pull forces between the elementary parts of these currents.(24)

By a further hypothesis, that a reentering current exists in every molecule of a magnet,(25) he also fully explained the actions between currents and magnets and those of magnets on each other, illustrating every part of his theory by well devised experiments.

But the action at a distance, as defined by Ampère, though a push and pull force, depends for its magnitude, not only on the distance between the acting portions of the currents and on their strengths, but on their relative directions, that is, on the angles which they make with each other and with the line joining them, so that, considered as an ultimate fact, it is one of extreme complexity, as compared with the ordinary law of attraction; and it is not to be wondered at that many attempts have been made to resolve it into something of greater apparent simplicity. But before mentioning any of those attempts directly founded on the formula of Ampère, I shall direct your attention to the independent labours of Faraday to elucidate and illustrate the discovery of Oersted.

No man more conscienscously and systematically laboured to improve all his powers of mind than did Faraday from the very earliest period of his scientific career. But whereas the general course of development of science had begun with mathematics and astronomy, so that the ideas and methods of these sciences had been applied to every new investigation in turn, Faraday seems to have had no opportunity of prosecuting mathematics into its higher branches, and his knowledge of astronomy was derived merely from books. Hence though he knew enough of the history of astronomy to have a profound respect for the great discovery of Newton, he regarded the attraction of gravitation as a sort of sacred mystery, which, as he was not an astronomer, he had no right to gainsay or doubt, his duty being to believe it in the exact form in which it was delivered to him. In short though he believed in the

(24) See Number 430 note (5).

(25) See the *Treatise*, **2**: 419 (§833); and Number 410 note (19).

attraction of the heavenly bodies, his faith was dead, and he was not imbued with the doctrine of attraction, like the eighteenth century school and their followers.(26)

Besides this, the mathematical treatises of Poisson & Ampère in which the doctrine of attractions was applied with the greatest success to those electrical and magnetic phenomena into which Faraday threw the whole vigour of his powers, are of so technical a form, that, to derive any assistance from them, a man must have been thoroughly trained to mathematical study, and it is very doubtful if such a training can be begun with advantage in mature years.

Thus Faraday, with his original and penetrating intellect, his devotion to science, and his opportunities for experiment, was debarred from following the course of thought which had led to the great achievements of the French philosophers and was obliged to explain the phenomena to himself by a symbolism which he could understand, instead of adopting what had hitherto been the tongue of the learned.

The symbolism which, in the mind of Faraday, became so significant, and which, in his speech and writings, gave trouble to so many men of science, is that which I have spoken of as Faradays Lines of Force.

Filings

The idea of lines of force as shown for instance by iron filings is nothing new they had been observed repeatedly and investigated mathematically as an interesting curiosity of science. But let us hear Faraday himself as he introduces the method which became so powerful in his hands. Exp Res 3234.

It would be a voluntary and unnecessary abandonment of most valuable aid, if an experimentalist, who chooses to consider magnetic power as represented by lines of magnetic force, were to deny himself the use of iron filings. By their employment, he may make many conditions of the power, even in complicated cases, visible to the eye at once, may trace the varying direction of the lines of force and determine the relative polarity, may observe in which direction the power is increasing or diminishing; and in complex systems may determine the neutral point, or places where there is neither polarity nor power even when they occur in the midst of powerful magnets. By their use probable results may be seen at once, and many a valuable suggestion gained for future leading experiments.(27)

When a small piece of iron, such as a single filing, is placed in the magnetic field, that is to say in the space in the neighbourhood of magnets or currents, it becomes magnetized in a certain direction, depending on the distribution of magnetic force. Another filing placed in the magnetic field will also become a little magnet. Now if one of these filings is brought near the other, they will

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(26) See Maxwell's letter to Faraday of 9 November 1857 and Faraday's reply of 13 November; Volume I: 548–52.

(27) Faraday, *Electricity*, 3: 397.

act on each other the poles of opposite names which be nearest each other will approach and rush together and remain in contact and the other poles will turn themselves so as to be in the direction of the magnetic force. If any other filings are in the neighbourhood they will stick themselves on to the exposed poles so as to form a long fibre, the direction of which is that of the magnetic force. A new filing can only stick to the end of the fibre and not to any other part of its length for all the poles except the extreme ones are neutralised by opposite poles in contact with them.

If filings are scattered over a sheet of paper in the magnetic field then when the paper is gently tapped, the filings draw together, filing to filing, till the paper is covered not with mere dots of iron, but with little lines or fibres, and the direction of these fibres is plainly seen in every part of the field.

This is what we may call the physiological explanation of the lines of magnetic force, as delineated by iron filings.

To investigate them more closely, let us take a small magnet and carry it about the field. You observe that wherever the magnet is placed, its direction coincides with that of the fibres of iron filings. But the magnet tells us more than the filings did, for it has two ends, one marked N and the other marked S and by observing the direction of these ends we can distinguish the northern and the southern direction of the line of filings.

We can now define the lines of force somewhat more exactly. Take a very small compass needle and, starting from any point of the paper, go due north (as shown by the compass) and you will trace out part of a line of magnetic force ending usually in a north pole, a point where the north end of the compass will be drawn straight down and the compass will be indifferent in all horizontal directions.

If you steer due south you will trace out the other part of the line of force ending usually in a south pole.

The lines of force due to an electric current do not terminate in north and south poles but are closed or endless curves surrounding the current.

I have supposed the lines of force to be drawn on a flat piece of paper but it is evident that by using a needle free to turn in every direction vertically as well as horizontally we might draw lines of force through the air filling all the space round a magnet or current.

Let us now consider the ordinary scientific explanation of the lines of force. Let us take the case of a simple bar magnet having a north pole and a south pole. The north pole of the compass needle is repelled from the north pole of the bar magnet with a certain force depending on the distance and it is attracted towards the south pole of the bar with a force also depending on the distance. The actual force acting on the north pole of the compass needle is the mechanical resultant of these two forces. If the compass needle is very small compared with the distance of the bar, the forces acting on the south pole will be nearly equal and opposite to those acting on the north pole and their resultant also will be equal and opposite.

Hence the direction of a line of force at any point represents the direction of the resultant of two forces each directed towards or from a fixed point called a pole and each varying inversely as the square of the distance. It is an easy matter for a trained mathematician to calculate the forms of the lines of magnetic force from these conditions but to his mind the magnetic action is represented much more distinctly by the two forces directed to the two poles of the bar magnet than by the complicated system of curves, the investigation of which he regards only as a mathematical recreation.

But let us see what use Faraday made of these lines and to what results he was led and let us compare his method both as respects power accuracy and distinctness with that of the professed mathematicians.

The first thing to observe in Faradays use of the lines of force is that he uses them to represent not only the direction but the magnitude of the magnetic lines of force at every part of the field. It is evident even to the eye that when the lines converge the force is increasing in the direction of the convergence but the statements of Faraday relative to this subject show a constantly increasing clearness of definition which attains complete development in 3073 Series XXVIII.

A point equally important to the definition of these lines is, that they represent a determinate and unchanging amount of force. Though, therefore their forms, as they exist between two or more centres or sources of magnetic power may vary greatly, and also the space through which they may be traced, yet the sum of power contained in any one section of a given portion of the lines is exactly equal to the sum of power in any other section of the same lines however altered in form or however convergent or divergent they may be at the second place.[(28)]

That is to say that if we draw a number of these lines forming a little bundle[(29)] and if we measure the section of this bundle at different points of its length, the area of the section multiplied by the strength of the magnetic force will be the same at all these points.[(30)]

This conclusion which is mathematically true on the old theory of magnetism is one at which I am not aware that the professed mathematicians had arrived at the time when Faraday, without the help of technical mathematics, both saw its importance and proved its truth.

(28) Faraday, *Electricity*, **3**: 329.

(29) Compare Faraday, *Electricity*, **3**: 435; 'magnets may be looked upon as the habitations of bundles of lines of force'.

(30) On Maxwell's mathematical theory of lines of force see Volume I: 367–9, 371–5.

This remarkable property of the lines of force by which we may estimate the variation of the force as we pass along any bundle of lines by the degree of concentration of the lines leads to another conception of high mathematical significance and also due to Faraday.

By drawing the lines in a systematic manner it is evident that we might indicate the intensity of the magnetic force not only along a particular line but in every part of the field by the degree of concentration of the lines, that is by the *number* of the lines which pass through a square inch of a section taken perpendicular to their direction. The definite statement of this conception is given in one of his later memoirs Series XXVIII par 3122(31) but the frequent use of the expressions amount of the lines of force and number of lines of force in his earlier papers shows that he was guided by this conception from the first.(32)

Thus then we see that Faraday converted the lines of force from a mere scientific curiosity into a complete definition of the magnetic state of every part of the field.

The strength of the north pole of a magnet is measured at once by the number of lines which diverge from it. If the magnet is the only magnet in existence all these lines, whatever be their intermediate course converge again to the south pole of the magnet and return through the substance of the magnet to the north pole. If there are other magnets in the field some of the lines from the north pole of one magnet may go to the south pole of a different magnet but in all cases the number which meet at any pole measures the strength of that pole.

In the case of an electric current the lines of force form rings round the current.

If the strength of a magnet is increased Faraday conceives new lines of force as being developed within the magnet and causing the whole system to expand. If the strength of the magnet is diminished the lines of force contract towards the magnet and some of them disappear within it. In the same way when an electric current in a wire increases or diminishes, the system of lines of force round it expands or contracts.

By this remarkable mathematical conception, which is described in the

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(31) Faraday, *Electricity*, **3**: 349; 'the direction of the *lines of force*... and the relative amount of force, or of lines of force in a given space, [is] indicated by their concentration or separation, *i.e.* by their number in that space'.

(32) These expressions do not occur in Faraday's early papers, collected in his *Electricity*. Compare Maxwell's comments in the *Treatise*, **2**: 174–5 (§541) which qualify this assertion; and his claim that '[Faraday's] method of conceiving the phenomena was also a mathematical one', in the Preface to the *Treatise*, **1**: x, dated 1 February 1873.

second series of his researches (238, 239)[33] each line of force is regarded as having a continuous existence, so that it preserves its identity during all the changes of form which may arise from alterations in the position or strength of magnets or currents.

The diagrams which you see[34] are intended to illustrate the lines of force as definite in number as well as direction. The first represents a single magnetic pole whose strength is 36 placed among the lines of the earths magnetic force. The 36 lines of force belonging to the pole instead of proceeding from it in straight lines are all deflected towards the opposite pole of the earth and the lines of the earths force from the other pole are deflected from their originally straight directions.

Let us now briefly mention some of the uses which Faraday made of this conception of lines of force.

Let a conducting wire carrying a current be placed in the magnetic field, how will it move. The answer is simple. It will endeavour to embrace as many of the lines of force as possible so that they may pass through the electric circuit in the positive direction. To do this every portion of the wire will be urged across these lines of force with a mechanical force which is perpendicular both to the wire and to the lines of force.

Every question relating to the mechanical force acting on a current may be solved with mathematical accuracy by this simple rule that the work done during any motion of the current is equal to the number of new lines embraced by it multiplied by the strength of the current.

Again if a conducting circuit be placed in the magnetic field and any change occurs either in the position of this circuit or in the positions or the strengths of magnets or currents so that the number of lines of force embraced by the circuit is altered then an induced current will take place in the circuit, the total amount of which multiplied by the resistance of the circuit is equal to the diminution of the number of lines of force embraced by the circuit.

Another very remarkable proof of Faradays insight is given by his statement of the true form of the law of the force acting on diamagnetic bodies. In the second of his papers on diamagnetism he thus sums up the whole results of his experiments in language which in a mathematical point of view could hardly be improved.

All the phænomena resolve themselves into this, that a portion of such matter when under magnetic action, tends to move from stronger to weaker places or points of force.[35]

(33) Faraday, *Electricity*, **1**: 68–9.

(34) Not extant.

(35) Faraday, *Electricity*, **3**: 69 (par. 2418).

Here then we have the lines of magnetic force used as a powerful instrument by which Faraday was enabled to define with mathematical precision the whole laws of electromagnetism in language free from mathematical technicalities. His method then considered simply as a piece of mathematical apparatus for treating certain questions, has great merit. But he did not stop here. He went on from the conception of geometrical lines of force to that of physical lines of force and found wherever these lines of force existed that certain effects were there taking place intimately related to these lines of force. One of his most remarkable statements with respect to the physical character of the lines of magnetic force is that in every motion due to magnetic force the lines of force are shortened so that wherever there are lines of magnetic force there is a tendency to longitudinal contraction and lateral separation of the lines.(36)

Here then is a distinct physical state which Faraday thinks he perceives in the medium in which these lines exist. It consists of a tension like that of a rope in the direction of the lines of force combined with a pressure in all directions at right angles to these lines.

This is quite a new view of action at a distance reducing it to a phenomenon of the same kind as that action at a distance which we exert by means of our tools and ropes and connecting rods.

If we examine the lines of force in the diagrams or by means of iron filings we see that whenever the lines of force pass directly from one point to another, there is attraction between the poles and when the lines from the two poles avoid each other and are dispersed into space the poles repel each other, so that in both cases the poles are drawn in the direction of the resultant of the lines of force.

To test this theory of a physical tension along the lines of force we must examine whether such tension would produce the actually observed forces. To do this involves the study of the equilibrium of stresses in a body a subject which has been treated only within the present century and for the development of which we are indebted in the first place to engineers who studied it in order to know what forces are called into play in their machines and structures and in the second place to optical inquirers who have endeavoured to understand the forces involved in the propagation of light in an elastic medium.

Taking advantage therefore of the theory of stress as laid down for instance in Professor Rankines excellent work on Applied Mechanics(37) and supposing

(36) Faraday, *Electricity*, **3**: 437.

(37) W. J. M. Rankine, *A Manual of Applied Mechanics* (London/Glasgow, 1858): 92–3, 113–16.

that wherever there is magnetic force, there is a state of stress,(38) consisting of a tension in the direction of the lines of force combined with a pressure in all directions at right angles to these lines, the numerical value of the tension and the pressure being equal and both proportional to the *square* of the intensity of the magnetic force at the place then we find by a simple calculation
First that every part of the medium will be in equilibrium under the action of these stresses, provided it does not contain a magnetic pole or an electric current. Hence this hypothesis is quite consistent with the perfect mobility of the medium.
Second if a magnetic pole exists it will not be in equilibrium under the forces which act on it, but will be moved in the direction of the lines of force with a mechanical force the value of which is the product of the strength of the pole and the intensity of the magnetic force.
Third if an electric current crosses the lines of force, it will not be in equilibrium but will experience a mechanical force urging it across the lines of magnetic force and the value of this force agrees with what we know about it.

When the muscles of our bodies are excited by that stimulus which we are able in some unknown way to apply to them their fibres tend to shorten themselves and at the same time to expand laterally. If we knew the mechanical stress at every point of the muscle we could determine the force which it exerts in moving the limb though we might be quite unable to find out the cause of the tension of the muscle as produced at the will of its owner.

According to the theory of magnetic lines of force which I have been describing the magnetic field is in some way put into a state of stress like that of the muscle, the lines of force tending to shorten themselves and expand laterally and it appears that if this stress has a certain definite relation to the degree of concentration of the lines of force it will produce a mechanical effect exactly corresponding with what actually takes place.

Of course it does not follow that this is the actual mode in which magnetic attraction occurs. All we have done is to show that if a medium is in this state of stress it will produce the effects which are known to occur. If therefore we feel any desire to account for magnetic action at a distance by means of an action taking place in the intervening medium, this hypothesis deserves our consideration even though it furnishes no explanation of how the state of stress is produced or maintained.

In our bodily actions we are conscious of an act of will and we presently

(38) Maxwell had referred to Rankine's text in his paper 'On physical lines of force. Part I. The theory of molecular vortices applied to magnetic phenomena', *Phil. Mag.*, ser. 4, **21** (1861): 161–75, esp. 163 (= *Scientific Papers*, **1**: 453), in expounding a theory of magnetism as a result of '*stress* in the medium'.

become aware that a corresponding outward act has been done by us involving motions of our bodies and of other bodies extending to a considerable distance. We investigate the mechanism of our bodies and find that our muscles have been put into a certain state of stress and that motion has ensued the motion being communicated from one part to another till the required act was done.

This explanation is by no means complete for it furnishes no account of the method by which the will acts on the muscles nor does it even investigate the forces of cohesion which enable the muscles to support the state of tension. Nevertheless the simple fact that it substitutes an action which is propagated continuously along a material substance for one of which we know only a cause and an effect at a distance from each other induces us to accept it as a real addition to our knowledge.

For similar reasons we may be justified in bestowing much thought on this physical hypothesis about lines of force in the hope that it may substitute an action between consecutive parts of a medium for an action supposed to take place immediately at a distance.

[2](39) I wish in the last place to point out some results of the theory that electrical and magnetic forces act between bodies not directly but by the intervention of a medium. It follows from this that if by any means one of the bodies could be instantaneously annihilated or at least deprived of its electrical or magnetic properties, the ether would continue to be acted on by the medium for a certain time just as if the first body had still been there just as you continue to hear my voice for a certain time after I am here speaking, while the sound is still travelling through the air.

In fact it follows from our theory that electrical and magnetic forces require time for their transmission and that each body puts the medium round it into a certain state which is communicated from part to part of the medium at a certain rate.

The question therefore naturally arises – Have we any means of determining this rate? or velocity of transmission of magnetic force from one place to another? – The science of electrical measurements is indebted for its present degree of accuracy mainly to Prof W Weber, the fellow labourer of Gauss. From his results we can obtain data to calculate the speed of the transmission of magnetic force. The value calculated from his experiments is 314000000 metres or 19 miles in a second.(40) Now the velocity of light

(39) ULC Add. MSS 7655, V, c/20. Endorsed: 'Faraday's Lines of Force'.

(40) Weber's value for the ratio between the electrostatic and electromagnetic units is 310,740 mm/sec; see Number 238 note (22), and *Treatise*, **2**: 387 (§787). See also Volume I: 685.

according to Foucaults direct measurements is [298 millions of metres per second].(41) Both the measurement of the velocity of light and that of the quantities on which the rate of transmission of magnetic force depends are operations of great delicacy. The electromagnetic experiment has been repeated in two forms distinct from each other and from that of Weber by Sir W Thomson(42) and by myself.(43) We both obtained a value considerably less than that of Weber. The conclusion to which we are led is that whatever light is and whatever magnetism and electricity may be, they depend on the same thing – that light in fact is an electromagnetic phenomenon and that the waves of light consist of small alternating magnetic disturbances.

This theory of light leads us at once to the explanation of the polarization of light. It agrees in all points with the theory of undulations except that where the ordinary theory sees a displacement of ether our theory sees a displacement of electricity and where the ordinary theory sees a rotation of particles of ether our theory sees a magnetic force in the direction of the axis of rotation.

We are not therefore attempting to fill up all space three and four times over with new ethers to do this that and the other but are trying to understand how the one ether which the phenomena of light have compelled us to admit is capable also of other modes of action and that [light and electromagnetic] phenomena are manifestations of these.

And we must not consider this ether as something [like] vapour the wreaths of which have no power to bend. Even when it is employed only in transmitting the dazzling rays of the sun it is supporting very considerable forces. The ordinary magnetic force of the earth in this country is equivalent to a tension of about an eighth of a grain weight on a square foot and some of M^r^ Joules magnets can produce a magnetic force equivalent to a tension of about 200 lbs weight on the square inch.(44) These are very considerable forces to be exerted and supported by the ether.

I have already said that hardly any progress has been made in accounting for the attraction of gravitation. If however we propose to account for it in the

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(41) As stated in the published lecture 'On action at a distance': 53 (= *Scientific Papers*, **2**: 322); and see Number 238 note (25).

(42) As described in the 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-ninth Meeting of the British Association* (London, 1870): 434–8, on 434–6. See also *Treatise*, **2**: 372–3, 387 (§§772, 787).

(43) See Number 289.

(44) Compare *Treatise*, **2**: 258 (§646) where he gives a figure of 'about 140 pounds weight on the square inch'. Joule had described his magnets in his 'Description of a new electro-magnet', *Annals of Electricity*, **6** (1841): 431–3, and his 'Account of experiments with a powerful electro-magnet', *Phil. Mag.*, ser. 4, **3** (1852): 32–6.

same way as we have done for magnetism we must admit that there is pressure instead of tension along the lines of force and tension instead of pressure at right angles to them and that here where we sit the ether is supporting a vertical pressure of more than 37000 tons on the square inch. The strength of steel is nothing to this. I by no means assert this as a fact but as a specimen of the results to which we may be led by a theory which must be verified before we can believe it.

APPENDIX: MANUSCRIPT FRAGMENT ON DYNAMICAL PRINCIPLES

circa 1873[45]

From the original in the University Library, Cambridge[46]

[MATTER AND DYNAMICS]

In studying electrical and other phenomena for the purpose of explaining them on dynamical principles, there are certain axioms so well established in the better known parts of dynamics that we are entitled to expect them to prove universally true.

For instance, we obtain from ordinary dynamical considerations the idea of matter as a thing capable of motion but not changing its state of motion except in so far as it is acted on by external forces, that is, forces between the body in question and other bodies.

We must of course learn what is meant by a state of motion and how changes of that state are to be measured. We must also learn by experience that these changes, in the case of a material body depend on what we call the mass of the body itself as well as on those external relations to other bodies which we call forces. As soon as we clearly understand how the 'laws of motion' by which this dependence is expressed lead to results consistent with facts, we have formed an idea of mass as the quantitative aspect of matter which is as necessary a part of our thoughts as the triple extension of space or the continual flux of time.

I do not think it necessary to enquire whether this is the metaphysical idea of matter, or whether any metaphysician has come to the conclusion that the

(45) This MS fragment seems to have relation to the 1873 lecture 'On action at a distance': see note (47). The fragment is therefore reproduced here, though a date of early 1873 is conjectural.

(46) ULC Add. MSS 7655, V, c/10.

property commonly called inertia is the fundamental and inseparable part of matter. We may be satisfied with our dynamical reasons for asserting that the mass of a body is a measurable and constant quantity and that for all dynamical purposes a body must be measured by its mass and not by any other property such as its volume, its weight, or its chemical activity.

I have made these remarks because an opinion seems to be in circulation that all phenomena can be explained by the hypothesis that there are certain moveable points whose motions are determined by their relative positions and which are therefore called centres of force.(47) As long as it was supposed that the ultimate parts of bodies were themselves small bodies of finite size, these ultimate parts were called atoms, but as we have no evidence as to the size and shape of these atoms some have thought it more philosophical to speak of them as centres of force, without attributing to them any finite extension. This would be quite legitimate, provided each centre of force is admitted to have mass, so that the mass of all the centres taken together shall be equal to that of the body which they form. It seems, however, that the idea of mass is so bound up with that of extension in many minds that in giving up the size and shape of the atoms they have apparently lost sight of their mass.

Torricelli, in his fourth lecture to the Accademia della Crusca has expressed the relation between the idea of matter on the one hand and those of force and momentum on the other neither of which can exist without the other.(48)

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(47) In 'On action at a distance': 49 (= *Scientific Papers*, **2**: 317) Maxwell cites a review of Mary Somerville, *On Molecular and Microscopic Science*, 2 vols. (London, 1869), 'Mrs Somerville on molecular and microscopic science', *Saturday Review*, **27** (Feb. 13, 1869): 219–20, where the reviewer maintained that Boscovich 'had not quite got so far as the strict modern view of "matter" as being but an expression for modes or manifestations of "force"'. By contrast, Maxwell noted that Boscovich 'did not forget, however, to endow his mathematical points with inertia'. Compare Maxwell's discussion of Faraday's theory of 'all space as a field of force' in the *Treatise*, **2**: 164 (§529) where, alluding to Faraday's 'A speculation touching electric conduction and the nature of matter', *Phil. Mag.*, ser. 3, **24** (1844): 136–44, and 'Thoughts on ray-vibrations', *ibid.*, **28** (1846): 345–50 (= *Electricity*, **2**: 284–93; **3**: 447–52), he noted that '[Faraday] even speaks of the lines of force belonging to a body as in some sense part of itself', but maintained: 'This, however, is not a dominant idea with Faraday.'. See also Number 287.

(48) See Number 287 note (12); and Number 294: Appendix and note (29). Maxwell cites this passage in the concluding article of the *Treatise*, **2**: 438 (§866), in support of his claim that 'there must be a medium or substance in which the energy exists after it leaves one body and before it reaches the other'.

LETTER TO HENRY BENCE JONES[1]

4 FEBRUARY 1873

From the original in the Library of the Royal Institution[2]

11 Scroope Terrace
Cambridge
4 Feb 1873

Dear Sir

I send you by book post my lecture.[3] When you have done with it pray let me have it again. Some numbers &c must be inserted to agree with the experiments actually made at the lecture.

I enclose for M^r Wills[4] a list of the things which will be wanted. If any of them are not to hand I will devise something else if I know a day or two before.

I also enclose a diagram to explain Newtons Rings[5] which I think will make the subject plainer.

If, from your experience of lectures, you think mine will take more than the hour, pray let me know.

Yours faithfully
J. CLERK MAXWELL

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(1) Secretary of the Royal Institution.

(2) W. H. Bragg Box 28, Miscellaneous Papers, the Royal Institution, London.

(3) 'On action at a distance'; see *Proceedings of the Royal Institution of Great Britain*, **7** (1873–5): 44–54 (= *Scientific Papers*, **2**: 311–23). The lecture was read on 21 February 1873.

(4) Thomas Wills, assistant in the chemical laboratory; see *Proceedings of the Royal Institution*, **6** (1870–2): iii.

(5) See Maxwell, 'On action at a distance': 46–7 (= *Scientific Papers*, **2**: 313–14); and Number 437 note (13).

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ESSAY FOR THE ERANUS CLUB[(1)] ON SCIENCE AND FREE WILL

11 FEBRUARY 1873

From Campbell and Garnett, *Life of Maxwell*[(2)]

DOES THE PROGRESS OF PHYSICAL SCIENCE TEND TO GIVE ANY ADVANTAGE TO THE OPINION OF NECESSITY (OR DETERMINISM) OVER THAT OF THE CONTINGENCY OF EVENTS AND THE FREEDOM OF THE WILL?

11 FEBRUARY 1873

The general character and tendency of human thought is a topic the interest of which is not confined to professional philosophers. Though every one of us must, each for himself, accept some sort of a philosophy, good or bad, and though the whole virtue of this philosophy depends on it being our own, yet none of us thinks it out entirely for himself. It is essential to our comfort that we should know whether we are going with the general stream of human thought or against it, and if it should turn out that the general stream flows in a direction different from the current of our private thought, though we may endeavour to explain it as the result of a wide-spread aberration of intellect, we would be more satisfied if we could obtain some evidence that it is not ourselves who are going astray.

In such an enquiry we need some fiducial point or standard of reference, by which we may ascertain the direction in which we are drifting. The books written by men of former ages who thought about the same questions would be of great use, if it were not that we are apt to derive a wrong impression from them if we approach them by a course of reading unknown to those for whom they were written.

There are certain questions, however, which form the *pièces de résistance* of philosophy, on which men of all ages have exhausted their arguments, and

(1) 'Under the name of Erănus (or pic-nic) a club of older men ... who had been "Apostles" together in 1853–7, revived the habit of meeting together for the discussion of speculative questions' (*Life of Maxwell*: 366, 434). The club included Fenton John Anthony Hort and Joseph Barber Lightfoot (see Volume I: 314, 384, 506, 585; and *Life of Maxwell*: 434).

(2) *Life of Maxwell*: 434–44; reprinted and corrected (see note (17)) in *Life of Maxwell* (2nd edn): 357–66.

which are perfectly certain to furnish matter of debate to generations to come, and which may therefore serve to show how we are drifting. At a certain epoch of our adolescence those of us who are good for anything begin to get anxious about these questions, and unless the cares of this world utterly choke our metaphysical anxieties, we become developed into advocates of necessity or of free-will. What it is which determines for us which side we shall take must for the purpose of this essay be regarded as contingent. According to Mr F. Galton, it is derived from structureless elements in our parents, which were probably never developed in their earthly existence, and which may have been handed down to them, still in the latent state, through untold generations.(3) Much might be said in favour of such a congenital bias towards a particular scheme of philosophy; at the same time we must acknowledge that much of a man's mental history depends upon events occurring after his birth in time, and that he is on the whole more likely to espouse doctrines which harmonise with the particular set of ideas to which he is induced, by the process of education, to confine his attention. What will be the probable effect if these ideas happen mainly to be those of modern physical science?

The intimate connexion between physical and metaphysical science is indicated even by their names. What are the chief requisites of a physical laboratory? Facilities for measuring space, time, and mass. What is the occupation of a metaphysician? Speculating on the modes of difference of co-existent things, on invariable sequences, and on the existence of matter.

He is nothing but a physicist disarmed of all his weapons – a disembodied spirit trying to measure distances in terms of his own cubit, to form a chronology in which intervals of time are measured by the number of thoughts which they include, and to evolve a standard pound out of his own self-consciousness. Taking metaphysicians singly, we find again that as is their physics, so is their metaphysics. Descartes, with his perfect insight into geometrical truth, and his wonderful ingenuity in the imagination of

(3) Francis Galton, 'On blood-relationship', *Proc. Roy. Soc.*, **20** (1872): 394–402. For Maxwell's further discussion of Galton's concept of 'structureless elements', and his critique of Darwin's theory of 'pangenesis' (a theory of heredity in which cells of the body were supposed to give off 'gemmules') which Galton's paper investigates, see his article on 'Atom' in *Encyclopaedia Britannica* (9th edn), **3** (Edinburgh, 1875): 36–49, on 42 (= *Scientific Papers*, **2**: 461). At a meeting of the Cambridge Philosophical Society on 25 November 1872 Maxwell's comment on Darwin's theory of 'pangenesis' was recorded in the *Proceedings*: 'Professor Maxwell spoke of the difficulty of conceiving chemical molecules in sufficient quantity being packed in these small gemmules' (*Proc. Camb. Phil. Soc.*, **2** (1872): 289). Maxwell makes a similar point in his 'Atom' article.

mechanical contrivances, was far behind the other great men of his time with respect to the conception of matter as a receptacle of momentum and energy. His doctrine of the collision of bodies is ludicrously absurd. He admits, indeed, that the facts are against him, but explains them as the result either of the want of perfect hardness in the bodies, or of the action of the surrounding air. His inability to form that notion which we now call force is exemplified in his explanation of the hardness of bodies as the result of the quiescence of their parts.

Neque profecto ullum glutinum possumus excogitare, quod particulas durorum corporum firmius inter se conjungat, quàm ipsarum quies. *Princip.*, *Pars* II. LV.[4]

Descartes, in fact, was a firm believer that matter has but one essential property, namely extension, and his influence in preserving this pernicious heresy in existence extends even to very recent times. Spinoza's idea of matter, as he receives it from the authorities, is exactly that of Descartes; and if he has added to it another essential function, namely thought, the new ingredient does not interfere with the old, and certainly does not bring the matter of Descartes into closer resemblance with that of Newton.

The influence of the physical ideas of Newton on philosophical thought deserves a careful study. It may be traced in a very direct way through Maclaurin[5] and the Stewarts[6] to the Scotch School, the members of which had all listened to the popular expositions of the Newtonian Philosophy in their respective colleges. In England, Boyle and Locke reflect Newtonian ideas with tolerable distinctness, though both have ideas of their own.[7] Berkeley, on the other hand, though he is a master of the language of his time,

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(4) R. Descartes, *Principia Philosophiæ* (Amsterdam, 1644): 62 (Pars Secunda, Prop. LV) (nor assuredly are we able to devise any glue which could join together the particles of hard bodies more strongly than does their own rest).

(5) Colin MacLaurin, *An Account of Sir Isaac Newton's Philosophical Discoveries* (London, 1748), see also Number 437; *idem, A Treatise of Fluxions*, 2 vols. (Edinburgh, 1742).

(6) The Aberdeen (Marischal College) mathematics professor John Stewart published a translation and commentary on *Sir Isaac Newton's Two Treatises of the Quadratures of Curves and Analysis by Equations of an infinite Number of Terms, explained* (London, 1745); and 'Some remarks on the laws of motion, and the inertia of matter', in *Essays and Observations, Physical and Literary, read before a Society in Edinburgh and published by them*, 3 vols. (Edinburgh, 1754–71), **1**: 70–140. The Edinburgh mathematics professor Matthew Stewart was in the geometrical tradition (following Robert Simson), publishing 'A solution of Kepler's problem', in *Essays and Observations, Physical and Literary*, **2**: 105–44, and *The Distance of the Sun from the Earth determined by the Theory of Gravity* (Edinburgh, 1763). He was the father of Dugald Stewart, author of *Elements of the Philosophy of the Human Mind*, 3 vols. (Edinburgh, 1792–1827), which contains substantive discussion of mathematics, metaphysics and natural philosophy.

(7) On Locke see also Number 287.

is quite impervious to its ideas.[8] Samuel Clarke is perhaps one of the best examples of the influence of Newton;[9] while Roger Cotes, in spite of his clever exposition of Newton's doctrines, must be condemned as one of the earliest heretics bred in the bosom of Newtonianism.[10]

It is absolutely manifest from these and other instances that any development of physical science is likely to produce some modification of the methods and ideas of philosophers, provided that the physical ideas are expounded in such a way that the philosophers can understand them.

The principal developments of physical ideas in modern times have been –

1st. The idea of matter as the receptacle of momentum and energy. This we may attribute to Galileo and some of his contemporaries. This idea is fully expressed by Newton, under the form of Laws of Motion.

2nd. The discussion of the relation between the fact of gravitation and the maxim that matter cannot act where it is not.

3rd. The discoveries in Physical Optics, at the beginning of this century. These have produced much less effect outside the scientific world than might be expected. There are two reasons for this. In the first place it is difficult, especially in these days of the separation of technical from popular knowledge, to expound physical optics to persons not professedly mathematicians. The second reason is, that it is extremely easy to show such persons the phenomena, which are very beautiful in themselves, and this is often accepted as instruction in physical optics.

4th. The development of the doctrine of the Conservation of Energy. This has produced a far greater effect on the thinking world outside that of technical thermodynamics.

As the doctrine of the conservation of matter gave a definiteness to statements regarding the immateriality of the soul, so the doctrine of the conservation of energy, when applied to living beings, leads to the conclusion that the soul of an animal is not, like the mainspring of a watch, the motive power of the body, but that its function is rather that of a steersman of a vessel – not to produce, but to regulate and direct the animal powers.[11]

5th. The discoveries in Electricity and Magnetism labour under the same disadvantages as those in Light. It is difficult to present the ideas in an adequate manner to laymen, and it is easy to show them wonderful experiments.

6th. On the other hand, recent developments of Molecular Science seem

(8) Compare Volume I: 247.

(9) See Number 437 note (15).

(10) See Number 437 esp. note (19).

(11) Compare Maxwell's letter to Lewis Campbell of 21 April 1862 (Volume I: 712).

likely to have a powerful effect on the world of thought. The doctrine that visible bodies apparently at rest are made up of parts, each of which is moving with the velocity of a cannon ball, and yet never departing to a visible extent from its mean place, is sufficiently startling to attract the attention of an unprofessional man.

But I think the most important effect of molecular science on our way of thinking will be that it forces on our attention the distinction between two kinds of knowledge, which we may call for convenience the Dynamical and Statistical.

The statistical method of investigating social questions has Laplace for its most scientific(12) and Buckle for its most popular expounder.(13) Persons are grouped according to some characteristic, and the number of persons forming the group is set down under that characteristic. This is the raw material from which the statist endeavours to deduce general theorems in sociology. Other students of human nature proceed on a different plan. They observe individual men, ascertain their history, analyse their motives, and compare their expectation of what they will do with their actual conduct. This may be called the dynamical method of study as applied to man. However imperfect the dynamical study of man may be in practice, it evidently is the only perfect method in principle, and its shortcomings arise from the limitation of our powers rather than from a faulty method of procedure. If we betake ourselves to the statistical method, we do so confessing that we are unable to follow the details of each individual case, and expecting that the effects of widespread causes, though very different in each individual, will produce an average result on the whole nation, from a study of which we may estimate the character and propensities of an imaginary being called the Mean Man.

Now, if the molecular theory of the constitution of bodies is true, all our knowledge of matter is of the statistical kind. A constituent molecule of a body has properties very different from those of the body to which it belongs. Besides its immutability and other recondite properties, it has a velocity which is different from that which we attribute to the body as a whole.

The smallest portion of a body which we can discern consists of a vast number of such molecules, and all that we can learn about this group of molecules is statistical information. We can determine the motion of the centre of gravity of the group, but not that of any one of its members for the

(12) P. S. de Laplace, *Théorie Analytique des Probabilités* (Paris, 1812).

(13) Thomas Henry Buckle, *History of Civilization in England*, 2 vols. (London, 1857–61), esp. 1: 19–22. Maxwell had read the first volume of Buckle's *History* on its publication (see Volume I: 576).

time being, and these members themselves are continually passing from one group to another in a manner confessedly beyond our power of tracing them.

Hence those uniformities which we observe in our experiments with quantities of matter containing millions of millions of molecules are uniformities of the same kind as those explained by Laplace and wondered at by Buckle, arising from the slumping together of multitudes of cases, each of which is by no means uniform with the others.

The discussion of statistical matter is within the province of human reason, and valid consequences may be deduced from it by legitimate methods; but there are certain peculiarities in the very form of the results which indicate that they belong to a different department of knowledge from the domain of exact science. They are not symmetrical functions of the time. It makes all the difference in the world whether we suppose the inquiry to be historical or prophetical – whether our object is to deduce the past state or the future state of things from the known present state. In astronomy, the two problems differ only in the sign of t, the time; in the theory of the diffusion of matter, heat, or motion, the prophetical problem is always capable of solution; but the historical one, except in singular cases, is insoluble. There may be other cases in which the past, but not the future, may be deducible from the present. Perhaps the process by which we remember past events, by submitting our memory to analysis, may be a case of this kind.

Much light may be thrown on some of these questions by the consideration of stability and instability.(14) When the state of things is such that an infinitely small variation of the present state will alter only by an infinitely small quantity the state at some future time, the condition of the system, whether at rest or in motion, is said to be stable; but when an infinitely small variation in the present state may bring about a finite difference in the state of the system in a finite time, the condition of the system is said to be unstable.(15)

It is manifest that the existence of unstable conditions renders impossible the prediction of future events, if our knowledge of the present state is only approximate, and not accurate.

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(14) See Maxwell's letter to Francis Galton of 26 February 1879 (to be reproduced in Volume III).

(15) Compare Balfour Stewart, *The Conservation of Energy. Being an Elementary Treatise on Energy and its Laws* (London, 1874): 155–6; considering an egg balanced in unstable equilibrium he observed: 'Not... that its movements are without a cause, but... its movements are decided by some external impulse so exceedingly small as to be utterly beyond our powers of observation.' Maxwell referred to Stewart's text in his similar discussion of singularities in a dynamical system in his review in *Nature*, **19** (1879): 141–3, esp. 142 (= *Scientific Papers*, **2**: 760) of Balfour Stewart and P. G. Tait, *Paradoxical Philosophy. A sequel to the Unseen Universe* (London, 1878).

It has been well pointed out by Professor Balfour Stewart(16) that physical stability is the characteristic of those systems from the contemplation of which determinists draw their arguments, and physical instability(17) that of those living bodies,(18) and moral instability that of those developable souls, which furnish to consciousness the conviction of free will.

Having thus pointed out some of the relations of physical science to the question, we are the better prepared to inquire what is meant by determination and what by free will.

No one, I suppose, would assign to free will a more than infinitesimal range. No leopard can change his spots, nor can any one by merely wishing it, or, as some say, *willing* it, introduce discontinuity into his course of existence. Our free will at the best is like that of Lucretius's atoms – which at quite uncertain times and places deviate in an uncertain manner from their course.(19) In the course of this our mortal life we more or less frequently find ourselves on a physical or moral watershed, where an imperceptible deviation is sufficient to determine into which of two valleys we shall descend. The doctrine of free will asserts that in some such cases the Ego alone is the determining cause. The doctrine of Determinism asserts that in every case, without exception, the result is determined by the previous conditions of the subject, whether bodily or mental, and that Ego is mistaken in supposing himself in any way the cause of the actual result, as both what he is pleased to call decisions and the resultant action are corresponding events due to the same fixed laws. Now, when we speak of causes and effects, we always imply some person who knows the causes and deduces the effects. Who is this person? Is he a man, or is he the Deity?

If he is man – that is to say, a person who can make observations with a certain finite degree of accuracy – we have seen that it is only in certain cases that he can predict results with even approximate correctness.

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(16) B. Stewart and J. N. Lockyer, 'The sun as a type of the material universe. Part II: The place of life in a universe of energy', *Macmillan's Magazine*, **18** (1868): 319–27; Stewart, *Conservation of Energy*: 154–67.

(17) Reading: 'instability', following *Life of Maxwell* (2nd edn): 362–3.

(18) Stewart and Lockyer, 'The sun as a type of the material universe': 325–6; Stewart, *Conservation of Energy*: 161, noting that a human being or animal is 'a machine of a delicacy that is practically infinite, the conditions or motions of which we are utterly unable to predict'.

(19) Lucretius, *De Rerum Natura*, Book II, 289–93. Maxwell quotes the passage on the 'swerve' of Lucretian atoms in his MS on the 'Kinetic Theory of Gases' written for William Thomson in Summer 1871 (Number 377, see esp. note (5)); and see 'Molecules', *Nature*, **8** (25 September 1873): 437–41, on 440 (= *Scientific Papers*, **2**: 373).

If he is the Deity, I object to any argument founded on a supposed acquaintance with the conditions of Divine foreknowledge.

The subject of the essay is the relation to determinism, not of theology, metaphysics, or mathematics, but of physical science – the science which depends for its material on the observation and measurement of visible things, but which aims at the development of doctrines whose consistency with each other shall be apparent to our reason.

It is a metaphysical doctrine that from the same antecedents follow the same consequents. No one can gainsay this. But it is not of much use in a world like this, in which the same antecedents never again concur, and nothing ever happens twice. Indeed, for aught we know, one of the antecedents might be the precise date and place of the event, in which case experience would go for nothing. The metaphysical axiom would be of use only to a being possessed of the knowledge of contingent events, *scientia simplicis intelligentiæ* – a degree of knowledge to which mere omniscience of all facts, *scientia visionis*, is but ignorance.

The physical axiom which has a somewhat similar aspect is 'That from like antecedents follow like consequents.' But here we have passed from sameness to likeness, from absolute accuracy to a more or less rough approximation. There are certain classes of phenomena, as I have said, in which a small error in the data only introduces a small error in the result. Such are, among others, the larger phenomena of the Solar System, and those in which the more elementary laws in Dynamics contribute the greater part of the result. The course of events in these cases is stable.

There are other classes of phenomena which are more complicated, and in which cases of instability may occur, the number of such cases increasing, in an exceedingly rapid manner, as the number of variables increases. Thus, to take a case from a branch of science which comes next to astronomy itself as a manifestation of order: in the refraction of light, the direction of the refracted ray depends on that of the incident ray, so that in general, if the one direction be slightly altered, the other also will be slightly altered. In doubly refracting media there are two refracting rays, but it is true of each of them that like causes produce like effects. But if the direction of the ray within a biaxal crystal is nearly but not exactly coincident with that of the ray-axis of the crystal, a small change in direction will produce a great change in the direction of the emergent ray. Of course, this arises from a singularity in the properties of the ray-axis, and there are only two ray-axes among the infinite number of possible directions of lines in the crystal; but it is to be expected that in phenomena of higher complexity there will be a far greater number of singularities, near which the axiom about like causes producing like effects ceases to be true. Thus the conditions under which gun-cotton explodes are far from being well known; but the aim of chemists is not so much to predict the time at which gun-cotton will go off of itself, as to find a kind of gun-cotton which, when placed in certain circumstances, has never yet exploded, and this even when slight irregularities both in the manufacture and in the storage are taken account of by trying numerous and long continued experiments.

In all such cases there is one common circumstance – the system has a quantity of potential energy, which is capable of being transformed into motion, but which cannot begin to be so transformed till the system has reached a certain configuration, to attain which requires an expenditure of work, which in certain cases may be infinitesimally small, and in general bears no definite proportion to the energy developed in consequence thereof. For example, the rock loosed by frost and balanced on a singular point of the mountain-side, the little spark which kindles the great forest, the little word which sets the world a fighting, the little scruple which prevents a man from doing his will, the little spore which blights all the potatoes, the little gemmule which makes us philosophers or idiots. Every existence above a certain rank has its singular points: the higher the rank, the more of them. At these points, influences whose physical magnitude is too small to be taken account of by a finite being, may produce results of the greatest importance. All great results produced by human endeavour depend on taking advantage of these singular states when they occur.

> There is a tide in the affairs of men
> Which, taken at the flood, leads on to fortune.[20]

The man of tact says 'the right word at the right time', and, 'a word spoken in due season how good is it!' The man of no tact is like vinegar upon nitre when he sings his songs to a heavy heart. The ill-timed admonition hardens the heart, and the good resolution, taken when it is sure to be broken, becomes macadamised into pavement for the abyss.

It appears then that in our own nature there are more singular points – where prediction, except from absolutely perfect data, and guided by the omniscience of contingency, becomes impossible – than there are in any lower organisation. But singular points are by their very nature isolated, and form no appreciable fraction of the continuous course of our existence. Hence predictions of human conduct may be made in many cases. First, with respect to those who have no character at all, especially when considered in crowds, after the statistical method. Second, with respect to individuals of confirmed

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(20) Shakespeare, *Julius Caesar*, Act IV, Scene iii, 217–18.

character, with respect to actions of the kind for which their character is confirmed.

If, therefore, those cultivators of physical science from whom the intelligent public deduce their conception of the physicist, and whose style is recognised as marking with a scientific stamp the doctrines they promulgate, are led in pursuit of the arcana of science to the study of the singularities and instabilities, rather than the continuities and stabilities of things, the promotion of natural knowledge may tend to remove that prejudice in favour of determinism which seems to arise from assuming that the physical science of the future is a mere magnified image of that of the past.

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POSTCARD TO PETER GUTHRIE TAIT

12 FEBRUARY 1873

From the original in the University Library, Cambridge[1]

[Cambridge]

If there is any better way than by a long helix I should like to know it. It may be somewhat improved by connecting the ends by an iron bar, or by a tube as T does.

Begin with a thin tube (not of iron) put iron disks at both ends with holes in them. Wind the bobbin thus formed with wire, the diameter of the wire of different layers being proportional to the square root of the diameter of the layer and the resistances of the whole about equal to that of the battery & connexions. Then connect the iron disks by means of an iron bar outside the coil. This will make the external magnetic effect very small and throw the force on the space within the bobbin. Is it for rotation of plane of polarization?[2] I have not got to the Phil Mag[3] but will today. If you put 'immediate' or any other word beyond the direction on the obverse of a card you infringe H.M.'s gracious statutes.[4]

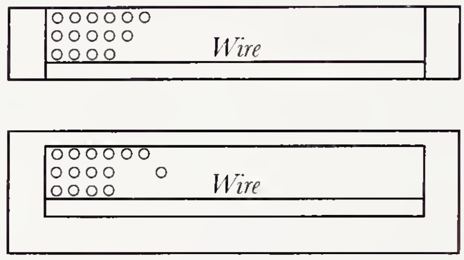

Figure 440,1

$\frac{\partial p}{\partial t}$

(1) ULC Add. MSS 7655, I, b/51.

(2) See Tait's experiments as reported in his paper 'On a possible influence of magnetism on the absorption of light, and some correlated subjects', *Proc. Roy. Soc. Edinb.*, **9** (1876): 118. On the Faraday magneto-optic effect see Number 434.

(3) In the February 1873 number of the *Philosophical Magazine* there was published a paper by Oliver Heaviside, 'On the best arrangement of Wheatstone's bridge for measuring a given resistance with a given galvanometer and battery', *Phil. Mag.*, ser. 4, **45** (1873): 114–20. There are notes by Maxwell on Heaviside's paper (ULC Add. MSS 7655, V, c/44) which outline his brief reference in the *Treatise* (2nd edn) §350. Of this paper Heaviside later recollected: 'Sent Maxwell a copy, and he noted it in his 2nd Edn'; from a Heaviside notebook cited by Bruce Hunt, *The Maxwellians* (Ithaca, NY/London, 1991): 58.

(4) See Number 356 esp. note (6).

LETTER TO PETER GUTHRIE TAIT

circa early 1873[1]

From the original in the University Library, Cambridge[2]

O T.! If a man will not read Lamé[3] how should he know whether a given thing is ν? Again, if a man throws in several triads of symbols & jumbles them up, pretending all the while that he has never heard of geometry, will not the broth be thick and slab?[4] If the problem is to be solved in this way by mere heckling of equations through ither[5] I doubt if you are the man for it as I observe that you always get on best when you let yourself and the public know what you are about.

I return your speculations on the $\phi(U\nu)\,ds$.[6] Observe, that in a magnet placed in a magnetic field the stress function is not in general self conjugate, for the elements are acted on by couples. But the $=^{n}$ of $=^{m}$ is very properly got as you get it.

Search for a physical basis for

$$S.\nabla^2\sigma\,\nabla\sigma^{(7)}$$

as a term of the energy developed in a medium by a variable displacement σ. When found make a note of, and apply to oil of turpentine, eau sucrée &c for it brings out the right sort of action on light of all colours.[8] But the mischief is $V\nabla\sigma$ which it is manifest can be produced without working any physical change inside a body. The very rotation of ⊕ produces it. Now $\nabla^2\sigma$ is a

(1) See notes (4) and (6).

(2) ULC Add. MSS 7655, I, b/103. Previously published in Knott, *Life of Tait*: 117, 147–8.

(3) Gabriel Lamé, *Leçons sur les Fonctions Inverses des Transcendantes et les Surfaces Isothermes* (Paris, 1857).

(4) Compare Maxwell's comments in his letter to Tait of 22 July 1873 (Number 468) on Tait's paper 'On orthogonal isothermal surfaces. Part I', *Trans. Roy. Soc. Edinb.*, **27** (1873): 105–23.

(5) ither: Scottish 'other' (*OED*). Knott (*Life of Tait*: 117n) comments: 'expressive Scottish phrase, meaning lack of method'.

(6) See P. G. Tait, 'Additional note on the strain function, &c', *Proc. Roy. Soc. Edinb.*, **8** (1873): 84–6, read 17 March 1873; ν is the normal vector of the surface element ds, U the versor (see Number 353 note (9)).

(7) See note (9).

(8) Maxwell is discussing the property of media 'to cause the plane of polarization to travel to the right or left, as the ray travels through the substance...[where] the property is independent of the direction of the ray within the medium, as in turpentine, solution of sugar, &c' (*Treatise*, **2**: 401 (§810)), in the context of his discussion of the Faraday magneto-optic effect.

vector.[9] Turn it alternately in the direction of $V\nabla\sigma$ & oppositely and you have increase & diminution of energy, & therefore a tendency to set like a magnet. The comfort is that $\nabla^2\sigma$ cannot subsist of itself.

Of course the resultant force on an element is of the form $V.\nabla^3\sigma$ and if σ is a function of z only, and $Sk\sigma = 0$

$$X = -\frac{d^3\eta}{dz^3}$$

$$Y = \frac{d^3\xi}{dz^3}.^{(10)}$$

This is the only explanation of terms of this form in an isotropic or fluid medium and since the rotation of plane of polarization is roughly proportional to the inverse square of the wave-length,[11] terms of this form must exist.

$$\frac{dp}{dt}$$

(9) See Number 468, where Maxwell writes $\nabla^2\sigma . V\nabla\sigma = S\nabla^2\sigma\,\nabla\sigma$, where $V\nabla\sigma$ is the 'curl' of the vector function σ (see Number 434 note (5)).

(10) See Number 468 for Maxwell's discussion of these relations; and the *Treatise*, **2**: 413–14 (§830), in the context of his treatment of the Faraday magneto-optic effect.

(11) Émile Verdet, 'Recherches sur les propriétés optiques développées dans les corps transparents par l'action du magnétisme', *Ann. Chim. Phys.*, ser. 3, **69** (1863): 415–91.

REPORT ON A PAPER BY FREDERICK GUTHRIE[1] ON THE ELECTRICAL PROPERTIES OF HOT BODIES

circa 25 FEBRUARY 1873[2]

From the original in the Library of the Royal Society, London[3]

REPORT ON PROF. GUTHRIE'S PAPER 'ON A NEW RELATION BETWEEN HEAT AND ELECTRICITY'[4]

In this paper are described a number of interesting experiments on the electrical properties of hot bodies as compared with those of cold ones, and on differences in these properties depending on the sign of the electrification of the hot solid body.

It is to be hoped that the author intends more fully to work out the subject, as he has now acquired sufficient data to direct him in choosing the proper points for investigation.

The theoretical position from which the author starts, and that to which he thinks his results lead him, are nowhere very clearly stated, and the reader is apt to attribute to the author opinions, which are, very likely, not held by him. I am by no means certain, therefore, whether all the following remarks really apply to the paper or not.

(1) In the experiments, a hot body, a ball or a wire, is electrified either by conduction or induction. When the body is white hot, this electrification is rapidly discharged, and the electrification of the inducing or 'inductric' body is also discharged.

When the body is only red hot, its electrification is discharged when positive but retained when negative, and at a dull red heat it discharges a negatively charged body but only when itself uninsulated.

In the account of these experiments the readers attention is directed to the

(1) Professor at the School of Science, South Kensington 1869, FRS 1871 (Boase).

(2) According to the Royal Society's *Register of Papers Received* Guthrie's paper was referred to Maxwell on 22 February 1873, and to Jenkin on 27 February 1873.

(3) Royal Society, *Referees' Reports*, **7**: 245.

(4) Frederick Guthrie, 'On a new relation between Heat and Electricity' (Royal Society, AP. 55.7); the paper is endorsed 'Archives June 19/73'. The paper was received by the Royal Society on 10 January 1873, and read on 13 February 1873; see the abstract in *Proc. Roy. Soc.*, **21** (1873): 168–9. Guthrie published a revise of his paper, 'On a relation between heat and static electricity', *Phil. Mag.*, ser. 4, **46** (1873): 257–66.

hot body and to the other electrified body, but the dielectric body, in this case the air, between the solid bodies is not explicitly referred to; and even in the concluding section (§63) where the conception of a 'coercitive' force (analogous to the so called 'coercitive' or 'coercive' force of hard steel for magnetism)[5] is introduced, it is not clearly stated in what body this force is to be looked for. The reader is led to look for it rather in the hot ball (which is a good conductor, and therefore destitute of such a power) than in the surrounding air.[6]

The authors own opinion is, I suppose, that this coercitive force is a power possessed by air and other dielectrics of resisting electromotive force up to a certain point, at which the power gives way, and discharge occurs; and that when the dielectric is heated the electromotive force required to produce discharge is much less than when the dielectric is cold.

In this opinion I, for one, agree, and I should think it would find general acceptance among all those who have studied the effect of heat on dielectrics. Thus the discharging power of the heated air in the voltaic arc, of flames, of hot glass (see Buff's experiments)[7] of warm gutta-percha &c are well known.[8] The discharging of power of a glowing slow-match is employed in Sir W. Thomson's portable electrometer[9] for ascertaining the potential of the air at the place of the burning match.

For the more complete investigation of the phenomena I would suggest that the attention should be confined to observation of what takes place in the air near a hot electrified surface, and in particular

1 by observations in the dark, to ascertain if there is any luminous discharge or glow
2 by observing what currents of air are formed and whether an artificial wind increases or diminishes the effect

(5) A force which prevents the loss of magnetism once induced; see the *Treatise*, **2**: 44–5 (§424).

(6) Guthrie, 'On a new relation between Heat and Electricity', f. 30; 'There is, with frictional electricity, a force comparable to what is called "coercitive" force with magnetism. This force is no more electric tension than magnetic coercitivism is magnetic strength. Like magnetic coercitive force it is overcome by heat.... The hot body furnishes a field of open gates into which, out of which and in which the electricities move untrammelled.'

(7) Heinrich Buff, 'Ueber die electrische Leitfähigkeit des erhitzten Glases', *Annalen der Chemie und Pharmacie*, **90** (1854): 257–83.

(8) See the *Treatise*, **1**: 423–4 (§§367–8).

(9) See Thomson's 'Report on electrometers and electrostatic measurements', *Report of the Thirty-seventh Meeting of the British Association for the Advancement of Science* (London, 1868): 489–512, esp. 501–7 (= *Electrostatics and Magnetism*: 292–302 and Plate II, Figs. 8, 9 and 10).

3 by observing the effect of the introduction of dust smoke and flame into the surrounding air.

As a useful guide in such researches the XII & XIII series of Faraday's researches[(10)] may be studied.

According to Faradays experiments which are in many points confirmed by those of Wiedemann & Rühlmann[(11)] the electric discharge in air and other gases begins where the electromotive force is most able to break down the resistance. In homogeneous gases this will be at the surface of the smaller electrode, and in unequally heated gases at the hot electrode.

If the electromotive force diminishes rapidly as we recede from the electrode, the discharge goes only a short distance, and charges the air. The charged air is carried off by currents, and new air arrives to be charged again.

To trace the course of the charged air, and to detect it clinging to oppositely electrified surfaces is an interesting research which has been only partially carried out by Thomson.[(12)]

The next point is the different results according to the positive or negative electrification of the hot electrode. Faraday found (1501)[(13)] that a negative surface can discharge air at a tension a *little* lower than a positive surface, but that when discharge takes place *much more* passes each time from the positive than from the negative surface.

This refers to air at the ordinary temperature, and was found by Wiedemann & Rühlmann to be the case also at low pressure, and with other gases.

Professor Guthries experiments agree together in showing that a hot surface has the greatest discharging power when positively electrified.

The interpretation of these results must, I think, be sought for in some difference in the nature of the dielectric at the opposite electrodes, produced by the action of the electromotive force. We know that in ordinary electrolytes the matter in contact with the + electrode differs from that in contact with

(10) Michael Faraday, 'Experimental researches in electricity. – Twelfth series. On induction', *Phil. Trans.*, **128** (1838): 83–123 (= *Electricity*, **1**: 417–72); Faraday, 'Experimental researches in electricity. – Thirteenth series. On induction', *Phil. Trans.*, **128** (1838): 125–68 (= *Electricity*, **1**: 473–532).

(11) G. Wiedemann and R. Rühlmann, 'Über den Durchgang der Electricität durch Gase', *Berichte über die Verhandlungen der Königlich Sächischen Gesellschaft der Wissenschaften zu Leipzig, math.-phys. Klasse*, **23** (1871): 333–85. See the *Treatise*, **1**: 424–5 (§370).

(12) William Thomson, 'Measurement of the electromotive force required to produce a spark in air between parallel metal plates at different distances', *Proc. Roy. Soc.*, **10** (1860): 326–38 (= *Electrostatics and Magnetism*: 247–59).

(13) Faraday, *Electricity*, **1**: 479; and see the *Treatise*, **1**: 425 (§370).

the — electrode, and in this way one electrode may be more favourable to discharge than the other. We also know from Sir B. C. Brodie's experiments[(14)] that an action, apparently electrolytic, occurs in several gases.

I make these remarks because in §22 the author uses language about different kinds of electricity which, at least at first sight, seems intelligible only on the hypothesis that the crudest form of the 'Two Fluid' theory is a physical fact; and that the operation of charging a body with a unit of + electricity can be physically distinguished from the act of removing a unit of — electricity from the same body.[(15)]

From what I have said, it is manifest that I do not agree with the title of the paper as a description of its contents. The experiments relate to the effects of heat on air as altering its electrical properties, particularly its insulating power. I cannot therefore recommend the present paper to be printed in the Transactions, though I think that the research, if successfully carried out, might furnish matter for a very valuable communication.[(16)]

J. Clerk Maxwell

(14) B. C. Brodie, 'An experimental inquiry on the action of electricity on gases. I. On the action of electricity on oxygen', *Phil. Trans.*, **162** (1872): 435–84.

(15) Guthrie, 'On a new relation between Heat and Electricity', f. 8; 'The whole aspect of the phenomena with hot iron balls which we have hitherto been considering, is of a nature to convince the actual experimenter that in all the above cases the collapse of the leaves [of an electroscope] is due to the release of their prevalent electricity rather than to the accession of electricity of the opposite kind.' Guthrie retained this paragraph (with only trivial verbal changes) in his 'On a relation between heat and static electricity': 260.

(16) In a letter to Stokes of 27 May 1873 (Royal Society, *Referees' Reports*, **7**: 244) Fleeming Jenkin was severely condemnatory of Guthrie's paper: 'I consider it unsuitable for publication. It contains numerous experiments all of which are clearly explicable on well established principles.... Prof[r] Guthrie's reasoning and explanations are quite without value and I think a judicious friend should give him a hint to withdraw the paper or only retain so much of it as is purely experimental – a short abstract of this is all that should ever be published.'

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POSTCARD TO PETER GUTHRIE TAIT

3 MARCH 1873

From the original in the National Library of Scotland, Edinburgh[1]

[Cambridge]

O T′. If, in your surface-integrals,[2] ds is an element of surface is not ds a vector? and does not multiplication by Uv[3] scalarize it? In your next edition tell us if you consider an element of surface otherwise than as $V\,d\alpha\,d\beta$ where α and β are vectors from the origin to a point in the surface defined by the parameters a, b.

Here the element of surface is a vector whose tensor is the area and whose versor is Uv.[4] These things I have written that our geometrical notions may in Quaternions run perpetual circle, multiform and mix and nourish all things. Such ideas are slowly percolating through the strata of Cartesianism trilinearity and determinism that overlie what we are pleased to call our minds. I hope you will read us a good rede in May.[5] Give it us hot and strong for our brains are soft and our hearts are hard and we need packing needles and saltpetre.

What day of May and what title?[6]

$$\frac{dp}{dt}$$

(1) National Library of Scotland, MS 1004 f. 42. Previously published (in part) in Knott, *Life of Tait*: 151.

(2) P. G. Tait, 'Additional note on the strain function, &c', *Proc. Roy. Soc. Edinb.*, **8** (1873): 84–6, read 17 March 1873.

(3) See Number 441 esp. note (6).

(4) See Number 353 note (9).

(5) Tait's Rede Lecture at Cambridge.

(6) Tait's Rede Lecture on 'Thermo-electricity' was read on 23 May 1873, and published in *Nature*, **8** (1873): 86–8, 122–4.

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POSTCARD TO PETER GUTHRIE TAIT

5 MARCH 1873

From the original in the University Library, Cambridge[1]

Why have *you* forgotten to send
Alice[2]. We remain in Wonderland
till she appears.
Till then no more from
yours truly
$\frac{dp}{dt}$

The text reads:
'Why have *you* forgotten to send Alice. We remain in Wonderland till she appears. Till then no more from

yours truly

$\frac{dp}{dt}$'.

(1) ULC Add. MSS 7655, I, b/52.

(2) Lewis Carroll, *Through the Looking-glass, and what Alice found there* (London, 1872).

LETTER TO PETER GUTHRIE TAIT

10 MARCH 1873

From the original in the University Library, Cambridge[1]

11 Scroope Terrace
10 March 1873

O T′[2]

Θαγξ φορ Αλλες.

(1) I have no Assistant. If I can do you any service well & good, if not, why not?

(2) Prof Liveing[3] will lend you his bags, give you his gases and furnish you with lime light. If you are particular about your lantern bring it yourself like Guy Fawkes or the man in the Moon.

The gases will go for half an hour if you want them for longer say so.

Bring your own galvanometer.

3 Thermopylæ exist but Peltier[4] only in the form of a repulsive electrometer and the effet Thomson[5] is an 'effect defective'.

The Senate House is a place to write in, to graduate in and to vote in. The Public Orator I believe can speak in it provided he employs the Latin Tongue.

What those venerable walls would say if the vernacular were sounded within them I dare not even think. If you have a good audience there will not be much echo from Geo II or Pitt[6] and if you erect a lofty platform the light spot on the screen, and the under side of your table may be seen by all.

5 If you do your Θ.H.[7] as you did your Quaternions to the British Asses[8] you will do very well always remembering that to speak familiarly of a 2nd Law as of a thing known for some years, to men of culture who have never even heard of a 1st Law, may arouse sentiments unfavourable to patient attention.

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(1) ULC Add. MSS 7655, I, b/52A. Previously published (in part) in Knott, *Life of Tait*: 251–2.

(2) On Tait's Rede Lecture on 'Thermo-electricity'; see Number 443 esp. note (6).

(3) G. D. Liveing, Professor of Chemistry at Cambridge (Venn).

(4) See Number 428 note (9).

(5) See Number 428 note (10).

(6) Cambridge University buildings near the Senate House.

(7) See note (2).

(8) Tait's 1871 address to Section A of the British Association; see the *Report of the Forty-first Meeting of the British Association for the Advancement of Science; held at Edinburgh in August 1871* (London, 1872), part 2: 1–8.

Your prop. about a distribution on a sphere is right. It is equivalent in the 1st instance to the distribution due a uniform shell of radius *a* which latter produces an attraction towards the centre of the shell or towards its image according as the point is outside or inside the sphere.

Both Moral & Intellectual Entropy are noble subjects though the dictum of Pecksniff concerning the idea of Todgers be unknown to me and not easily verified.(9)

I do not know much about reversible operations in morals. The science or practice depends chiefly on the existence of singular points in the curve of existence at which influences, physically insensible produce great results. The man of tact says the right word at the right time, and a word spoken in due season how good is it? The man of no tact is like vinegar upon natron when he sings his songs to a heavy heart. The ill timed admonition only hardens the conscience, and the good resolution, made just when it is sure to be broken, becomes macadamized into pavement for the abyss.*

'*καλὸν δὲ τὸ ζηλοῦσθαι εν καλῶ πάντοτε*'.(10)

Yrs $\frac{\partial p}{\partial t}$

* Sermons Vol III(11)

(9) Mr Pecksniff and Mrs Todgers are characters in Dickens' *Martin Chuzzlewit*. Tait may have been alluding to Pecksniff's exclamation (in Chap. 10): 'O my friend, Mrs Todgers! To barter away that precious jewel, self-esteem, and cringe to any mortal creature – for eighteen shillings a week!'

(10) The sentence ('The emulation of a good man is always a good thing') appears twice in the writings of John Chrysostom, in his commentaries on 'Galatians' and 'Hebrews'. (Source: 'Ibycus' and with the assistance of the Department of Classics, Harvard University.)

(11) Possibly: Joseph Butler's sermon 'Upon human nature'; see *The Works of... Joseph Butler*, 2 vols. (Oxford, 1849–50), **2**: 29–37. See also Number 439.

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POSTCARD TO PETER GUTHRIE TAIT

12 MARCH 1873

From the original in the University Library, Cambridge[1]

[Cambridge]

I hope you have my letter on ways and means.[2] I shall do the best I can to make it work. Only tell me the time of your appearing that we may prepare our arches and send round to them that are bidden.

$\frac{\partial p}{\partial t}$

(1) ULC Add. MSS 7655, I, b/53.
(2) Number 445.

REPORT ON A PAPER BY JAMES JAGO[1] ON EXPERIMENTS ON VISION

circa 24 MARCH 1873[2]

From the original in the Library of the Royal Society, London[3]

REPORT ON Dr JAGO'S PAPER ON 'VISIBLE DIRECTION'[4]

This paper contains a description of a number of interesting experiments on vision when one or both eyes are subjected to pressure. I have found that my own sensations agree with the description given in the paper in those cases which I have tried. I have not, however, in any case, obtained a displacement of 30° by means of pressure, as I was unwilling to employ the requisite degree of force. Nevertheless I have no doubt of the correctness of the description of most of the phenomena.[5]

It is when the author draws his conclusions that I fail to follow him. I have been unable to find the slightest connexion between the experiments and the doctrine finally laid down – that visible direction is determined by the direction of the optic nerve where it enters the eye.[6] On the contrary, all the

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(1) St John's 1835, MD Oxford 1859, FRS 1870 (Venn).

(2) According to the Royal Society's *Register of Papers Received* Jago's paper was referred to Maxwell on 21 March 1873, and to Burdon-Sanderson on 26 March 1873.

(3) Royal Society, *Referees' Reports*, **7**: 247.

(4) James Jago, 'Visible direction. Being an elementary contribution to the study of monocular and binocular vision' (Royal Society, AP. 55.8). The paper was received by the Royal Society on 12 February 1873, and read on 13 March 1873; see the abstract in *Proc. Roy. Soc.*, **21** (1873): 213–17. In the archives of the Royal Society there is a letter from Jago to T. H. Huxley (Secretary of the Royal Society) of 11 March 1873 (Royal Society, AP. 55.9) excusing his absence when the paper was read, and enclosing some supplementary material (AP. 55.10). The paper is endorsed 'Archives May 15/73'.

(5) In a report dated 12 May 1873 (Royal Society, *Referees' Reports*, **7**: 246) John Scott Burdon-Sanderson (Professor of Practical Physiology and Histology at University College, London 1870, FRS 1867 (*DNB*)) disagreed. He reported that: '[Jago] considers it possible by means of the "wedge" to press on the posterior half of the eyeball so as to cause it to move forwards. I have endeavoured to perform the manipulation as directed by the Author, and have thus satisfied myself (1) that it is not possible by these means to press upon the posterior half of the eye unless an amount of violence is used which I cannot suppose that the Author contemplated; and (2) that the principal and only important effect of the manipulation is to *deform* the eyeball, the amount of *displacement* produced being very inconsiderable.'

(6) Jago, 'Visible direction', f. 15; 'Visible direction is a function of the terminal direction of the Optic nerve.'

phenomena described seem to agree with the doctrine that in forming our opinion of the position of bodies in space we are guided chiefly by the muscular sensations arising from our efforts to look directly at these bodies with one or with both eyes.(7)

Dr Jago, indeed, contributes additional evidence of the fact that the object we look at has its image on a definite point of the retina so that we 'bisect' the object looked at in the same sense that the astronomer 'bisects' a star. He has also shown that the power of keeping the axis of collimation (that is the line joining the optic centre of the eye with the centre of the 'foramen centrale')(8) fixed on an object is not much interfered with by lateral pressure, and that in fact we may continue to look steadily at the same object while the pressure is being applied.(9) But he does not seem to have noticed what happens to the free eye during the effort of the pressed eye to maintain its parallelism. I find that the free eye moves in the same direction in which the muscles of the pressed eye must act to maintain the pressed eye in its position, or in other words in the opposite direction to that in which the applied pressure tends to turn the pressed eye.

This arises, I suppose, from the habit we have of working the muscles of both eyes at the same time, so as to keep the eyes parallel.

The subject of visible direction, or the conclusions we draw as to the position of objects in space, is rather psychological than physiological and does not appear to be a strong point with our author.(10) He is accurate with respect to the theory of corresponding points of the two retinæ, which is a purely optical question, but seems to have a half-expressed opinion concerning the relation of the position of certain fibres of the optic nerve to the position we attribute to objects seen by means of these fibres.

With respect to the final doctrine – of visible direction as determined by the direction of the extremity of the optic nerve, Dr Jago tells us that the optic nerve itself cannot be excited by acting on points in its course and it has yet

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(7) Hermann Helmholtz, 'On the normal motions of the human eye in relation to binocular vision', *Proc. Roy. Soc.*, **13** (1864): 186–99; Helmholtz, *Handbuch der physiologischen Optik* (Leipzig, 1867): 457–86. See Number 279.

(8) A depression of the retina of the eye. For Maxwell's discussion (with reference to colour vision and Haidinger's brushes) see Volume I: 201, 318, 636, 652.

(9) Jago, 'Visible direction', ff. 4–5, where he declares this 'fact' to be 'fundamental to the inquiry'.

(10) Compare Burdon-Sanderson's comment (see note (5)): 'the Author regards the apparent direction of an object as dependent on the "flexure" of the terminal portion of the optic nerve. I object to this hypothesis... if there were any such flexure as the Author supposes it would be entirely contrary to physiological experience to suppose that it would have any effect on the character of the impressions conveyed by the nerve to the centre'.

to be shown that it has any other function than that of transmitting impressions received on the retina, and in particular that it shares the power which the muscles undoubtedly possess, of acquainting us with their state of contraction or relaxation. The muscles of the eyes, being used for no other purpose than to regulate the position of the eyes, have all their associations connected with visual impressions; and the mode, (well described by the author of the paper) in which we *feel* our way to the stereoscopic union of two pictures, shows the importance of the muscular sense in the act of vision.

I think, therefore, that before transferring to the optic nerve a function hitherto supposed to be confined to the nerves of muscles, we require more evidence than is here presented; and I do not think the paper as it stands should be printed in the Philosophical Transactions.

James Clerk Maxwell

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LETTERS TO WILLIAM THOMSON

25 MARCH 1873[(1)]

From the originals in the University Library, Glasgow[(2)]

[1] Observe the address { Glenlair
Dalbeattie
25 March 1873

Dear Sir William

I am requested by the Board of Mathematical Studies to ascertain from you, whether, in the event of your being nominated by the Board as Additional Examiner for Mathematical Honours in 1874, and the nomination confirmed by the Senate, you would undertake the duties of the office. (See Cambridge Calendar p26).[(3)]

Yours truly
J. Clerk Maxwell
President

[2] The Chancellor is now fairly engaged to collect the Cavendish papers.[(4)] I think he will give them to the University Library. I am just going to walk the plank with them in the interest of physical science.

I am going to Glenlair tomorrow which accounts for the false date of my formal epistle.

I expect my book will reach you this week.[(5)] There has been some costiveness about the binding, as it was bound to be out some time ago.

$\frac{dp}{dt}$

(1) See Maxwell's appended note *infra*.

(2) Glasgow University Library, Kelvin Papers, M 33, 34.

(3) Thomson accepted the appointment; see *The Cambridge University Calendar for the Year 1874* (Cambridge, 1874): 26, 163.

(4) See Number 435.

(5) According to the Clarendon Press advertisement in *Nature*, **7** (27 March 1873): cv, the *Treatise* was published that week.

FROM A LETTER TO LEWIS CAMPBELL

3 APRIL 1873

From Campbell and Garnett, *Life of Maxwell*(1)

Glenlair
Dalbeattie
3 April 1873

The roof of the Devonshire Laboratory is being put on, and we hope to have some floors in by May, and the contractors cleared out by October.(2) We are busy electing School Boards here.(3) The religious difficulty is unknown here. The chief party is that which insists on keeping down the rates; no other platform will do. All candidates must show the retrenchment ticket.

The Cambridge Philosophical Society have been entertained by Mr Paley on Solar Myths, Odusseus as the Setting Sun, etc.(4) Your Trachiniæ(5) is rather in that style, but I think Middlemarch(6) is not a mere unconscious myth, as the Odyssey was to its author, but an elaborately conscious one, in which all the characters are intended to be astronomical or meteorological.

Rosamond is evidently the Dawn. By her fascinations she draws up into her embrace the rising sun, represented as the Healer from one point of view, and the Opener of Mysteries from another; his name, Lyd Gate, being compounded of two nouns, both of which signify something which opens, as the eye-lids of the morn, and the gates of day. But as the sun-god ascends, the

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(1) *Life of Maxwell*: 385–7.

(2) See Numbers 463 and 464.

(3) Following the Elementary Education Act of 1870.

(4) F. A. Paley, 'On the name "Odusseus" signifying "setting sun" and the Odyssey as a solar myth', *Proc. Camb. Phil. Soc.*, **2** (1873): 295–7; read 17 February 1873. Paley deployed F. Max Müller's theory of myths as narratives deriving from words originally descriptive of the sun; see note (8) and F. M. Turner, *The Greek Heritage in Victorian Britain* (New Haven/London, 1981): 104–12. See also Number 359 note (4).

(5) See *Sophocles. The Text of the Seven Plays*, ed. Lewis Campbell (Oxford, 1873). In his subsequent edition of Sophocles's 'Trachiniae' in his *Sophocles. The Plays and Fragments*, 2 vols. (Oxford, 1871–81), **2**: 240, Campbell remarked: 'Whatever truth may underlie this theory [that the tale of Hercules is a solar myth], it can have no bearing, as Mr Paley would be the first to admit, on the interpretation of the Trachiniae.'

(6) George Eliot's novel *Middlemarch, A Study of Provincial Life*, 4 vols. (London/Edinburgh, 1871–2). For comment on Maxwell's 'usual brilliant immediacy' in his analysis of *Middlemarch* in relation to solar mythology, see Gillian Beer, '"The death of the sun": Victorian solar physics and solar myth', in J. B. Bullen, ed., *The Sun is God: Painting, Literature and Mythology in the Nineteenth Century* (Oxford, 1989): 159–80, on 174.

same clouds which emblazoned his rising, absorb all his beams, and put a stop to the early promise of enlightenment, so that he, the ascending sun, disappears from the heavens. But the Rosa Munda of the dawn (see Vision of Sin) reappears as the Rosa Mundi in the evening, along with her daughters ♀ and ☿,(7) in the chariot of the setting sun, who is also a healer, but not an enlightener.(8)

Dorothea, on the other hand, the goddess of gifts, represents the other half of the revolution. She is at first attracted by and united to the fading glories of the days that are no more, but after passing, as the title of the last book expressly tells us, 'from sunset to sunrise',(9) we find her in union with the pioneer of the coming age, the editor.

Her sister Celia, the Hollow One, represents the vault of the midnight sky, and the nothingness of things.

There is no need to refer to Nicolas Bulstrode, who evidently represents the Mithraic mystery, or to the kindly family of Garth, representing the work of nature under the rays of the sun, or to the various clergymen and doctors, who are all planets. The whole thing is, and is intended to be, a solar myth from beginning to end.

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(7) Venus and Mercury.

(8) In his essay on 'Comparative mythology' Friedrich Max Müller argued that 'Mythology is only a dialect, an ancient form of language'; see his *Chips from a German Workshop*, 2 (London, 1867): 143. The theory of mythology as deriving from words descriptive of the sun is central to his argument: 'The simple story of nature which inspired the early poet.... When he was the Sun kissing the Dawn, he dreamt of days and joys gone for ever. And when the Dawn trembled, and grew pale, and departed, and when the Sun seemed to look for her... the Sun seemed to die away in the far West... the tragedy of nature... is the lifespring of all the tragedies of the ancient world.... The Sun freshens the Dawn, and dies at the end of the day, according to an inexorable fate, and bewailed by the whole of nature.... There was but one name by which they could express love... it was the blush of the day, the rising of the sun.... And this... is fully confirmed by an analysis of ancient speech.'; see *Chips from a German Workshop*, 2: 106–8, 129–30.

(9) *Middlemarch*, Book Eight: 'Sunset and Sunrise'.

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REVIEW OF FLEEMING JENKIN, *ELECTRICITY AND MAGNETISM*[1]

circa APRIL 1873

From *Nature* (15 May 1873)[2]

ELECTRICITY AND MAGNETISM. BY FLEEMING JENKIN, F.R.S.S.L. AND E., M.I.C.E., PROFESSOR OF ENGINEERING IN THE UNIVERSITY OF EDINBURGH. (LONDON: LONGMANS AND CO., 1873)[3]

The author of this text-book tells us with great truth that at the present time there are two sciences of electricity – one that of the lecture-room and the popular treatise; the other that of the testing-office and the engineer's specification. The first deals with sparks and shocks which are seen and felt, the other with currents and resistances to be measured and calculated. The popularity of the one science depends on human curiosity; the diffusion of the other is a result of the demand for electricians as telegraph engineers.

The text-book before us, which is the work of an engineer eminent in telegraphy, is designed to teach the practical science of electricity and magnetism, by setting before the student as early as possible the measurable quantities of the science, and giving him complete instructions for actually measuring them.

> The difference between the electricity of the schools and of the testing office has been mainly brought about by the absolute necessity in practice for definite measurement. The lecturer is content to say, under such and such circumstances, a current flows or a resistance is increased. The practical electrician must know how much resistance, or he knows nothing; the difference is analogous to that between quantitative and qualitative analysis.[4]

It is not without great effort that a science can pass out of one stage of its existence into another. To abandon one hypothesis in order to embrace another is comparatively easy, but to surrender our belief in a mysterious agent, making itself visible in brilliant experiments, and probably capable of accounting for whatever cannot be otherwise explained; and to accept the notion of electricity as a measurable commodity, which may be supplied at a

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(1) The review is anonymous, but the style and content point strongly to Maxwell as the reviewer: but the *Nature* archives cannot confirm this attribution.

(2) *Nature*, **8** (1873): 42–3.

(3) See Number 385.

(4) Fleeming Jenkin, *Electricity and Magnetism* (London, 1873): vii.

potential of so many Volts at so much a Farad,[5] is a transformation not to be effected without a pang.

It is true that in the last century Henry Cavendish led the way in the science of electrical measurement,[6] and Coulomb invented experimental methods of great precision.[7] But these were men whose scientific ardour far surpassed that of ordinary mortals, and for a long time their results remained dormant on the shelves of libraries. Then came Poisson[8] and the mathematicians,[9] who raised the science of electricity to a height of analytical splendour, where it was even more inaccessible than before to the uninitiated.

And now that electrical knowledge has acquired a commercial value, and must be supplied to the telegraphic world in whatever form it can be obtained, we are perhaps in some danger of forgetting the debt we owe to those mathematicians who, from the mass of their uninterpretable symbolical expressions, picked out such terms as 'potential',[10] 'electromotive force'[11] and 'capacity',[12] representing qualities which we now know to be capable of direct measurement, and which we are beginning to be able to explain to persons not trained in high mathematics.

Prof. Jenkin has, we think, made great progress in the important work of reducing the cardinal conceptions of electromagnetism to their most intelligible form, and presenting them to the student in their true connection.

The distinction between free electricity and latent, bound, combined, or dissimulated electricity, which occurs so frequently, especially in continental works on electricity, is not, so far as we can see, even alluded to in these pages; so that the student who takes Prof. Jenkin as his sole guide will not have his mind infected with a set of notions which did much harm in their day. On the other hand, terms which are really scientific – the use of which has led to a clearer understanding of the subject – are carefully defined and rendered familiar by well-chosen illustrations.

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(5) On these terms see Numbers 415, 418, 420.

(6) Compare the reference to Cavendish in Maxwell's lecture 'On Faraday's Lines of Force' (Number 437); and his draft letter to the Duke of Devonshire on Cavendish's electrical manuscripts (Number 435).

(7) See Volume I: 354.

(8) See Volume I: 354.

(9) A. M. Ampère, F. E. Neumann and W. E. Weber: see Volume I: 255, 262, 305.

(10) George Green, *An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism* (Nottingham, 1828); and C. F. Gauss, 'Allgemeine Lehrsätze...', in *Resultate aus den Beobachtungen des magnetischen Vereins in Jahre 1839* (Leipzig, 1840): 1–51. See Volume I: 258n, 261–2n.

(11) Franz Neumann, 'Die mathematischen Gesetze der inducirten elektrischen Ströme', *Physikalische Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin*, Aus dem Jahre 1845 (Berlin, 1847): 1–87.

(12) On Cavendish see Number 435 note (10).

Thus we find that men of the most profound scientific acquirements were labouring forty years ago to discover the relation between the nature of a wire and the strength of the current induced in it. By the introduction of the term 'electromotive force' to denote that which produces or tends to produce a current, the phenomena can now be explained to the mere beginner by saying that the electromotive force is determined by the alterations of the state of the circuit in the field, and is independent of the nature of the wire, while the current produced is measured by the electromotive force divided by the resistance of the circuit. To impress on the mind of the student terms which lead him in the right track, and to keep out of his sight those which have only led our predecessors, if not ourselves, astray, is an aim which Prof. Jenkin seems to have kept always in view.

To the critical student of text-books in general, there may appear to be a certain want of order and method in the first part of this treatise, the different facts being all thrown into the student's mind at once, to be defined and arranged in the chapters which follow. But when we consider the multiplicity of the connexions among the parts of electrical science, and the supreme importance of never losing sight of electrical science as a whole, while engaged in the study of each of its branches, we shall see that this little book, though it may appear at first a mighty maze, is not without a plan, and though it may be difficult to determine in which chapter we are to look for any particular statement, we have an excellent index at the end to which we may refer.

The descriptions of scientific and telegraphic instruments have all the completeness and more than the conciseness which we should look for from a practical engineer, and in a small compass contain a great deal not to be found in other books. The preface contains an outline of the whole subject, traced in a style so vigorous, that we feel convinced that the author could, with a little pains bestowed here and there, increase the force of his reasoning by several 'Volts', and at the same time diminish by an 'Ohm' or two the apparent stiffness of some of the paragraphs, so as to render the book more suitable to the capacities of the 'Microfarads'(13) of the present day.

(13) See Number 420 esp. note (8); on a 'microfarad' see Jenkin, *Electricity and Magnetism*: 159.

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LETTER TO PETER GUTHRIE TAIT

2 MAY 1873

From the original in the University Library, Cambridge(1)

11 Scroope Terrace
Cambridge
2 May 1873

Dear Tait

Will you dine with us on Friday 23rd May at 7 oClock?(2) or on Saturday 24th (Victoria Reg. et Vid).(3)

What stay do you make in Cambridge? Can I do anything for you in the way of preparation percunctation(4) or operation?

Yours truly
J. CLERK MAXWELL

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(1) ULC Add. MSS 7655, I, b/54.
(2) The day of Tait's Rede Lecture: see Number 443 note (6).
(3) Queen Victoria's birthday.
(4) *viz*, delay; see Number 465 note (8) and *OED* 'percunctorily'.

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LETTER TO PETER GUTHRIE TAIT

7 MAY 1873

From the original in the University Library, Cambridge[1]

11 Scroope Terrace
Cambridge
May 7 1873

Dear Tait

It will give M^rs^ Maxwell and myself much pleasure if you and M^rs^ Tait will favour us with your company at dinner on Friday 23^rd^ May at 7 o'Clock p.m.[2]

You can have bags of O and H (separate) about 2 cubic feet or more of each. You would have to get a Grace of the Senate if you wished Knallgas[3] and the Coroner would be in attendance outside with the Curator of the Anatomical Museum.

I am going to ascertain the facilities for the introduction of gas for your Bunsen. You can always get up a good heat with H & O.

There are a couple of concave mirrors for radiant heat which will be scoured up.

The size of diagrams is determined by that of their minimum visible which should not be less than 1.5 inch for a symbol 0.5 for the breadth of a line.

A thermoelectric diagram of $4^{ft} \times 6^{ft}$ would I think be visible.

The V.C. is no judge of such matters as the relative value of written forms committal and extempore. You should rather ask a Presbytery or the congregation of Kilmalcolm. You may, if you please, print it off first and then give us something quite different vivâ voce.

Do you bring a divided scale for the light spot to wag on?

What do you mean by referring to D^r^ Redtail[4] as an 'effusion'? I am indeed but slightly acquainted with him nevertheless I never doubted that he came into existence by ordinary generation. How then was he 'effunded'? And how have I, still in life I hope, passed into his Form?

M^rs^ Maxwell desires me to say that if M^rs^ Tait comes with you she would be happy if you would stay with us and so escape the rigour of those statutes which forbid ladies staying in College.

Yours truly
J. CLERK MAXWELL

(1) ULC Add. MSS 7655, I, b/55. (2) See Number 451 note (2).

(3) A detonating mixture of oxygen and hydrogen.

(4) Sir William Thomson's parrot; see S. P. Thompson, *The Life of William Thomson Baron Kelvin of Largs*, 2 vols. (London, 1910), **2**: 630–1, 634.

LETTER TO GEORGE GABRIEL STOKES

13 MAY 1873

From the original in the Library of the Royal Society, London(1)

11 Scroope Terrace
Cambridge
13 May 1873

My dear Stokes

I have read Sir George Airy's 'Magnetical Observations on the Britannia and Conway Tubular Iron Bridges'.(2)

I consider that these observations supply valuable data with respect to the distribution of magnetism in any iron structure of great size and tolerably regular form and that besides their present value these observations may be found valuable hereafter in determining the question of the permanence or the gradual alteration of the properties of iron structures exposed to great strain.(3)

I therefore recommend this paper to be printed in the Philosophical Transactions of the Royal Society.

Yours very truly
J. Clerk Maxwell

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(1) Royal Society, *Referees' Reports*, **7**: 221.

(2) George Biddell Airy, 'Magnetical observations in the Britannia and Conway tubular iron bridges', *Phil. Trans.*, **163** (1873): 331–9. The paper was received by the Royal Society on 12 October 1872 (with a 'Postscript' received on 22 October), and read on 19 December 1872; see the abstract in *Proc. Roy. Soc.*, **21** (1872): 85–6.

(3) Airy recorded an anomalous reading for the tube of the Britannia Bridge on the Anglesey side of the Menai Strait. As he recorded in the 'Postscript' to his paper, subsequent inquiry led him to attribute the anomaly to an accident which had occurred during the construction of the bridge; see Airy, 'Magnetical observations': 337–9. In a letter to Stokes of 21 April 1873 (Royal Society, *Referees' Reports*, **7**: 220) William Thomson commented: 'The point referred to in the Postscript seems to me particularly interesting and valuable. I think there can be little if any doubt but that the cause of the peculiarity of magnetic action found in the Anglesea Water tube is there truly explained. This very remarkable discovery is a lesson of faith and practice to all investigators:– never carelessly or unconscientiously to slur over any "anomaly".'

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LETTER TO PETER GUTHRIE TAIT

15 MAY 1873

From the original in the University Library, Cambridge[1]

11 Scroope Terrace
Cambridge
15 May 1873

My dear Tait

If you wish a lime light and a thermoelectric pile on which you can rely you had better fetch them with you. You can get gas bags here.[2]

You can have a tripod erected steady enough for your galvanometer but you have given no specification as to height.

Also if you wish the upper windows of the Senate House pasted up with brown paper it shall be done, on demand.

Yours truly
J. CLERK MAXWELL

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(1) ULC Add. MSS 7655, I, b/56.

(2) See Numbers 445 and 452.

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REPORT ON A PAPER BY DUGALD M'KICHAN[1] ON THE DETERMINATION OF THE NUMBER OF ELECTROSTATIC UNITS IN THE ELECTROMAGNETIC UNIT OF ELECTRICITY

circa 20 MAY 1873[2]

From the original in the Library of the Royal Society, London[3]

REPORT ON Mr M'KICHAN'S PAPER DETERMINATION OF THE NUMBER OF ELECTROSTATIC UNITS IN THE ELECTROMAGNETIC UNIT[4]

The aim of the experiments described in this paper is the determination of a physical constant of the nature of a velocity, the absolute magnitude of which is independent of the particular system of units of time and space used in the experiments. The scientific importance of the determination is of the highest order. Not only does it enable us to pass from any measurement made on the electrostatic system to the corresponding one on the electromagnetic system but it enables us to compare our standard measures with what we have great reason to believe is one of the most permanent magnitudes in nature.

The difficulty of the investigation arises from the largeness of the ratio to be measured. To do this by few steps requires batteries of great electromotive force, condensers of great capacity, wires sometimes of great resistance, and sometimes of great conductivity, and in all cases electrometers and galvanometers of great delicacy. To divide the measurements into convenient stages requires the construction of a number of new instruments, none of which can be perfected without much time and trouble.

The determination made by Mr M'Kichan depends on three kinds of data,

(1) A Glasgow student of William Thomson's (who communicated the paper to the Royal Society); see S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 525, **2**: 1026–7.

(2) According to the Royal Society's *Register of Papers Received* M'Kichan's paper was referred to Maxwell on 16 May 1873, and to Jenkin on 22 May 1873.

(3) Royal Society, *Referees' Reports*, **7**: 249.

(4) Dugald M'Kichan, 'Determination of the number of electrostatic units in the electromagnetic unit made in the Physical Laboratory of Glasgow University', *Phil. Trans.*, **163** (1873): 409–27. The paper was received by the Royal Society on 15 April 1873, and read on 15 May 1873; see the abstract in *Proc. Roy. Soc.*, **21** (1873): 290–2.

derived from the Electrometer, the Electrodynamometer, and the British Association's Unit of Resistance respectively.

It differs from the method described by myself in 1868[(5)] in using separate methods to determine the electrostatic and the electromagnetic effects of the same current, instead of balancing the one force against the other, as in my experiment.

This involves the use of Sir W. Thomsons Absolute Electrometer[(6)] and of his admirable heterostatic method, in which the electromotive force to be measured is deduced from the difference of two distances between the attracted surfaces and does not require an exact measurement of either.[(7)] This gives a very great advantage over the method used by me, which required the determination, always very precarious, of the absolute distance between two nearly parallel surfaces, one of which was freely suspended.

The electromagnetic measurement was also performed by means of an instrument capable of far more precise measurement than that which I employed.[(8)]

The other datum used in the calculation was the B.A. Unit as determined in 1864.[(9)] Any correction hereafter discovered to be necessary in the received value of this unit must be also applied to the number arrived at in this paper.

All things considered I regard this investigation as probably far more accurate than any yet made, and that both the matter and the method render the paper worthy of a place in the Philosophical Transactions.[(10)]

J. Clerk Maxwell

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(5) Number 289.

(6) See Number 289 note (11). M'Kichan employed Thomson's 'new absolute electrometer'; see Thomson's supplementary note ('added May 1870') to his 'Report on electrometers and electrostatic measurements' (1867) in his *Reprint of Papers on Electrostatics and Magnetism* (London, 1872): 287–92, and Plate III facing 287.

(7) Thomson classified electrometers as either 'idiostatic', where 'the whole electric force depends on the electrification which is itself the subject of the test', or 'heterostatic', where 'besides the electrification to be tested, another electrification maintained independently of it is taken advantage of' (*Electrostatics and Magnetism*: 308). The 'new absolute electrometer' was described as being 'heterostatic', for 'the potential of the auxiliary charge is tested and maintained... by an idiostatic arrangement forming part of the instrument itself' (*ibid.*: 287).

(8) M'Kichan acknowledged Maxwell's suggestion of a method of comparison of the two large coils and the third suspended coil of the electrodynamometer, so as to determine the magnetic moment of the suspended coil. See M'Kichan, 'Determination of the number of electrostatic units in the electromagnetic unit': 425–6.

(9) 'Report of the Committee on standards of electrical resistance', *Report of the Thirty-fourth Meeting of the British Association for the Advancement of Science* (London, 1865): 345–67, esp. table facing 349.

(10) In a brief letter to Stokes of 30 May 1873 (Royal Society, *Referees' Reports*, **7**: 248) Fleeming Jenkin also recommended publication.

Electromagnetic constant v centimetres per second		Velocity of Light centimetres per second	
Weber & Kohlrausch 1856[11]	310.74×10^8	Fizeau[12]	314×10^8
Maxwell 1868[13]	288×10^8	Foucault[14]	298.36×10^8
M'Kichan 1872	293×10^8	Cornu (by Fizeaus method) 1872[15]	298.5×10^8

Note

The only part of the paper which I think might be made more clear with a little pains is that which describes the method used to eliminate the effects of terrestrial magnetism from the observations of the Electrodynamometer.[16] The axis of the suspended coil, when in equilibrium, appears to have been nearly in the magnetic meridian, for the + and − readings are not very different. But in the one case the constant multiplier requires to be increased, and in the other diminished, on account of the terrestrial magnetism either conspiring with, or acting against, the elastic moment of torsion of the wire of suspension.

The heating of the wire by the current does not seem to have sensibly affected its elasticity.

(11) See Number 238 note (22).

(12) See Number 238 note (24).

(13) See Number 289 note (14).

(14) See Number 238 note (25).

(15) Alfred Cornu, 'Détermination nouvelle de la vitesse de la lumière', *Comptes Rendus*, **76** (1873): 338–42; (trans.) 'A new determination of the velocity of light', *Phil. Mag.*, ser. 4, **45** (1873): 394–7.

(16) By observing the deflections of the suspended coil when a given current passed first in one direction and then in the other; magnets were fixed near the coils to neutralise the action of terrestrial magnetism. See M'Kichan, 'Determination of the number of electrostatic units in the electromagnetic unit': 412–13.

LETTER TO ROBERT DUNDAS CAY

22 MAY 1873

From the original in the Library of Peterhouse, Cambridge[1]

11 Scroope Terrace
Cambridge
May 22 1873

Dear Uncle

I enclose receipt.[2] We are very glad to hear that Aunt Jane is so much better. This is the first mild day we have had. I hope it will do Aunt Jane good, though 'Assembly weather'[3] has a very bad name.

This seat of learning is at present in a wild turmoil of boat races, concerts, flower shows, Divine Services pic-nics processions feasts, organ recitals, Rede Lectures, balls, syndicates Cam pollution boards Swedenborgian lectures to young men, young mens lectures to young women, military bands in the College gardens, London organists in the College chapels, pianos and red cloth in the College halls, nothing at all in the College lecture-rooms. Senate house full of youths whom we know, as in manners so in doctrine, to be fit to be admitted to the title of Inceptor in Arts and so on, with love to Aunt Jane and all the family from Katherine & myself.

Your affectionate $\boxed{\text{nephew}}$
J. CLERK MAXWELL

(1) Peterhouse, Maxwell MSS (36).
(2) See Volume I: 682.
(3) The annual meeting of the General Assembly of the Church of Scotland.

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ON THE EFFECT OF GRAVITY ON THE TEMPERATURE OF A COLUMN OF GAS: REPLY TO FRANCIS GUTHRIE(1)

circa 25 MAY 1873(2)

From *Nature* (29 May 1873)(3)

CLERK-MAXWELL'S KINETIC THEORY OF GASES(4)

Your correspondent, Mr. Guthrie, has pointed out an, at first sight, very obvious and very serious objection to my kinetic theory of a vertical column

(1) A letter from Francis Guthrie (Boase), addressed from Graaf Reinet College, Cape Colony (South Africa), was headed 'Kinetic theory of gases' in *Nature*, **8** (22 May 1873): 67. 'On page 300 of the second edition of Maxwell's excellent little text-book on the "Theory of Heat", it is stated, as a result of the kinetic theory of gases therein set forth, that "gravity produces no effect in making the bottom of the column" (of gas) "hotter or colder than the top." / I cannot see how this result follows from the kinetic theory of gases. On the contrary, it seems obvious that thermal equilibrium can only subsist according to the kinetic theory, where the molecules encounter each other with equal average amounts of *work* or *vis viva*, and in order that this may be the case, the velocity of the molecules (and consequent temperature) of any upper layer must be less than that of the molecules in the layer next below; since, in order to encounter each other, the former must descend, and acquire velocity, while the latter must ascend and lose it. This would establish a diminution of temperature from the bottom to the top of a column of air at the rate (in the absence of any counteracting cause) of 1 °F. for 113 ft. of height, as can easily be verified from the fact that on account of the specific heat of air 1 lb. requires 183 foot-pounds to raise its temperature 1 °F. Radiation may diminish this and tend to produce equilibrium, but nevertheless it seems obvious from these two opposing tendencies a residual inequality of thermal condition would result, and that the top of a column would be cooler than the bottom. That this would be the case if the air were in general motion in the form of upward and downward currents, will not, I presume, be disputed; and surely molecular [?] is on the same footing. If the particles of air are moving in every direction with great absolute velocity, in what respect does this differ from air currents? In fact, all the particles which at any epoch of time are moving in any given direction constitute an air-current in that direction, mingled, it is true, with currents in other directions, but moving with accelerated velocity if descending, and with retarded velocity if ascending, and thus always tending to produce a diminution of temperature with height as a condition of gaseous thermal equilibrium.' In his *Theory of Heat* (London, $_2$1872): 300 Maxwell had concluded that: 'We find that if a vertical column of gas were left to itself, till by the conduction of heat it had attained a condition of thermal equilibrium, the temperature would be the same throughout, or, in other words, gravity produces no effect in making the bottom of the column hotter or colder than the top.... This result... proves that gravity has no influence in altering the conditions of thermal equilibrium in any substance, whether gaseous or not.'

(2) See notes (1) and (3).

(3) *Nature*, **8** (29 May 1873): 85.

(4) The title under which Maxwell's letter to *Nature* was published.

of gas. According to that theory, a vertical column of gas acted on by gravity would be in thermal equilibrium if it were at a uniform temperature throughout, that is to say, if the mean energy of the molecules were the same at all heights. But if this were the case the molecules in their free paths would be gaining energy if descending, and losing energy if ascending. Hence, Mr. Guthrie argues, at any horizontal section of the column a descending molecule would carry more energy down with it than an ascending molecule would bring up, and since as many molecules descend as ascend through the section, there would on the whole be a transfer of energy, that is, of heat, downwards; and this would be the case unless the energy were so distributed that a molecule in any part of its course finds itself, on an average, among molecules of the same energy as its own. An argument of the same kind, which occurred to me in 1866,(5) nearly upset my belief in calculation, and it was some time before I discovered the weak point in it.(6)

The argument assumes that, of the molecules which have encounters in a given stratum, those projected upwards have the same mean energy as those projected downwards. This, however, is not the case, for since the density is greater below than above, a greater *number* of molecules come from below than from above to strike those in the stratum, and therefore a greater number are projected from the stratum downwards than upwards. Hence since the total momentum of the molecules temporarily occupying the stratum remains zero (because, as a whole, it is at rest), the smaller number of molecules projected upwards must have a greater initial velocity than the larger number projected downwards. This much we may gather from general reasoning. It is not quite so easy, without calculation, to show that this difference between the molecules projected upwards and downwards from the same stratum exactly counteracts the tendency to a downward transmission of energy pointed out by Mr. Guthrie. The difficulty lies chiefly in forming exact expressions for the state of the molecules which instantaneously occupy a given stratum in terms of their state when projected from the various strata in which they had their last encounters. In my paper in the *Philosophical Transactions*, for 1867, on the 'Dynamical Theory of Gases',(7) I have entirely avoided these difficulties by expressing everything in terms of what passes through the boundary of an element, and what exists or takes place inside it.(8) By this method, which I have lately carefully verified

(5) See Numbers 259 §9 and 260.

(6) See Numbers 259 note (33), 263 and 266 note (8).

(7) J. Clerk Maxwell, 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88 (= *Scientific Papers*, **2**: 26–78).

(8) Maxwell, 'On the dynamical theory of gases': 86–7 (= *Scientific Papers*, **2**: 75–6). See note (6).

and considerably simplified, Mr. Guthrie's argument is passed by without ever becoming visible. It is well, however, that he has directed attention to it, and challenged the defenders of the kinetic theory to clear up their ideas of the result of those encounters which take place in a given stratum.[9]

J. Clerk Maxwell

(9) See Numbers 472, 473 and 481.

LETTER TO JOHN WILLIAM STRUTT

26 MAY 1873

From the original in private possession(1)

11 Scroope Terrace
Cambridge
26 May 1873

My dear Strutt

I am glad to hear you are writing a book on Acoustics. (Why not call it Theory of Sound?)(2) The Clarendon Press published Donkins book.(3) They appear to act very nearly in the same way as other printers. They print, I think, very well but very slow. I had great difficulty at first in getting them to use tall capitals instead of those small capitals which are no bigger than small letters. In such cases I did not make much impression unless I wrote both to the Secretary of the Delegates(4) and to the printer. When these things were settled I had no more difficulties with the printers, and they were very intelligent & careful. Thomson & Tait seem to think the Oxford press no better than other presses.(5)

You speak modestly of a want of Sound books in English. In what language are there such, except Helmholtz,(6) who is sound, not because he is German but because he is Helmholtz. ⌊The next best book is Herschel(7) whom you may regard as of German & organic descent.⌋ Observe Deschanels theory that the sound of an organ pipe is excited by the vibrations of the thick wooden lip of the mouthpiece. 'This lip is itself capable of vibrating in unison with any note lying within a wide range, and the note which is actually

(1) Rayleigh Papers, Terling Place, Terling, Essex. Published in part in R. J. Strutt, *John William Strutt, Third Baron Rayleigh* (London, 1924): 80–1.

(2) J. W. Strutt, Baron Rayleigh, *The Theory of Sound*, 2 vols. (London, 1877–8).

(3) W. F. Donkin, *Acoustics. Theoretical. Part I* (Oxford, 1870).

(4) Bartholomew Price: see Number 367 note (3).

(5) Thomson and Tait were in serious dispute with the Clarendon Press arising from the publication of their *Treatise on Natural Philosophy* in 1867; the second edition was published by Cambridge University Press in 1879. In a letter to Thomson of 6 January 1870 Tait declared – with reference to the demands from the Press for payment for corrections in proof – that 'as we cannot spare time for constant disputes about money matters the sooner our connection is terminated the better'; and on 25 April 1875 he exulted: 'Hurrah! We are at last our own masters!' (Edinburgh University Library, Gen. 2169: 199, 208).

(6) Hermann Helmholtz, *Die Lehre von den Tonempfindungen, als physiologische Grundlage für die Theorie der Musik* (Braunschweig, 1863).

(7) J. F. W. Herschel, 'Sound', in *Encyclopædia Metropolitana; or Universal Dictionary of Knowledge... Second Division. Mixed Sciences*, **2** (London, 1830): 747–824.

emitted is determined by the resonance of the column of air in the pipe.'[(8)]
This is what organists desirous of scientific knowledge have to receive and believe.

Another doctrine delivered to such persons is that any one with a mere smattering of science ought to understand completely the motion of the air at the mouth hole, say, of a flute.

Many thanks for your paper on Bessels functions.[(9)]

I do not expect to be in London on 12th June as I shall be in Scotland then. We go about the 3rd or 4th.

Yours very truly
J. Clerk Maxwell

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(8) A. Privat Deschanel, *Elementary Treatise on Natural Philosophy*, translated and edited, with extensive additions, by J. D. Everett, 4 parts (London, 1870–2), Part IV: 838. Everett subsequently rewrote and corrected the passage; see Deschanel, *Natural Philosophy* (London, $_4$1877): 838.

(9) J. W. Strutt, 'Notes on Bessel functions', *Phil. Mag.*, ser. 4, **44** (1872): 328–44.

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LETTER TO CHARLES TOMLINSON[1]

29 MAY 1873

From the original in the Library of the Institution of Electrical Engineers[2]

11 Scroope Terrace
Cambridge
29 May 1873

My dear Sir

I think we shall need your kind assistance again in the matter of the Cavendish Papers. The Duke of Devonshire to whom I communicated the information I received from you in 1869 wrote to M^r^ Harris, explaining the circumstances under which the papers had been placed in his father's hands and requesting him to return them to him.[3] This was six weeks ago and the Duke has received no answer and is afraid that it looks as if M^r^ Harris does not mean to part with the papers if he can help it. The Duke has therefore asked me to consult you on the subject as the person most likely to be able to render assistance.

In the first place would the address 'Thomas Harris Esq^re^ Barrington House Southsea', be safe to carry the letter to him. If not, we must improve the direction of the next letter.

I have no clear idea of the objections which M^r^ Harris may have to return the M.SS to the person who lent them to his father, and who is also the lawful owner of them.

With regard to the question how far the experiments of Cavendish may be supposed to have anticipated those of Sir W. S. Harris, I should myself expect, judging from the published writings of the two persons that the unpublished writings of Cavendish would be found to have nothing in common with the publications of Harris.

Harris' experimental methods have all the aspect of being his own. He gradually improved them till he could get consistent results, just as Sir W. Thomson has improved methods somewhat similar[4] by the help of his

(1) Scientific author and lecturer, FRS 1867 (Boase).

(2) Institution of Electrical Engineers, London, Special Collection MSS 3.

(3) See Number 435.

(4) The reference is to Thomson's addition of a guard-ring (see his *Electrostatics and Magnetism*: 281–6, and Number 289 note (11)) to the attracted disc electrometer first constructed by Snow Harris; see W. Snow Harris, 'On some elementary laws of electricity', *Phil. Trans.*, **124** (1834): 213–45. Compare Maxwell's discussion in the *Treatise*, **1**: 266–9 (§§216–17).

mathematical powers till they have attained still greater accuracy, and can estimate the quantities in absolute measure.[5]

I hope you will be able to suggest some method of making his Grace's application effectual.

Yours very truly
J. Clerk Maxwell

(5) In his 'Report on electrometers and electrostatic measurements' (1867) Thomson commented: 'it occurred to me to take advantage of the fact noticed by Harris, but easily seen as a consequence of Green's mathematical theory, that the mutual attraction between two conductors used as in his experiments is but little influenced by the form of the unopposed parts' (*Electrostatics and Magnetism*: 282). In his paper 'On the mathematical theory of electricity in equilibrium. I. On the elementary laws of statical electricity', *Camb. & Dubl. Math. J.*, **1** (1846): 75–95 (= *Electrostatics and Magnetism*: 15–37), Thomson commented on Harris' style of experimentation. Referring to Harris' paper 'Inquiries concerning the elementary laws of electricity', *Phil. Trans.*, **126** (1836): 417–52, Thomson noted the absence of 'precautions... in the experiments described in Mr Harris's memoir... [thus] the results are accordingly unavailable for the accurate *quantitative* verification of any law, on account of the numerous unknown disturbing circumstances by which they are affected' (*Electrostatics and Magnetism*: 24–5).

REPORT ON A PAPER BY JOHN WILLIAM STRUTT ON THEOREMS RELATING TO VIBRATIONS

26 JUNE 1873

From the original in private possession[1]

REPORT ON 'SOME GENERAL THEOREMS RELATING TO VIBRATIONS' – BY THE HON J. W. STRUTT[2]

I have long thought that the deduction of general theorems on vibrations from the general equations of motion of a connected system and the enunciation of these theorems in intelligible language would be one of the greatest services that a mathematician could do to Natural Philosophy. I am not well read in the literature of the subject but I have never met with dynamical discussions either of vibrations or undulations which take the full advantage of Lagranges method of dynamical reasonings by keeping clear of all hypotheses as to the mechanism by which the connexions of the system are maintained.

The theorems before us are just of the kind required to guide the speculative enquirer in his meditations on the vibrations of compound molecules, the radiation & absorption of light &c. A comparison with Sir J. F. W. Herschels justly celebrated theorem on forced vibrations[3] will show that since that time a real improvement in method has taken place.

If the mathematician can put the natural philosopher in possession of a method by which he may reason directly from the phenomena to the forces which produce them (as is done in the *differential* equations of Dynamics) this will answer his ends much better than if by integrating the equations the result of any particular hypothesis could be calculated.

I hope that the theorems of this paper when they appear in the work on

(1) Rayleigh Papers, Terling Place, Terling, Essex.

(2) J. W. Strutt, 'Some general theorems relating to vibrations', *Proceedings of the London Mathematical Society*, **4** (1873): 357–68; read 12 June 1873.

(3) J. F. W. Herschel, 'Sound', in *Encyclopædia Metropolitana... Second Division. Mixed Sciences*, **2** (London, 1830): 747–824, esp. 811–13 (§§323–31); 'provided the time elapsed since the commencement of the vibrations be long enough to allow of our regarding the number of previous vibrations as infinite, or which comes to the same, long enough to have allowed all traces of the initial vibrations to have been destroyed by resistance, friction, &c, these last will either exactly destroy each other, or, if they leave a residue, that residue will consist in a vibratory motion, having the same period with the primary impulse.' See J. W. Strutt, Baron Rayleigh, *The Theory of Sound*, 2 vols. (London, 1877–8), **1**: 108–9.

Acoustics[4] may be stated a little more explicitly for the general public than they are here for the Mathematical Society (to suppose whom ignorant of any 'known' theorem would be an impertinence).

For instance, there would be no harm in showing that the general equation of vibration of a conservative system is linear with respect to $\overline{\frac{d}{dt}}\Big|^2$ so that when expressed as a product of factors, each factor is of the form $\left(\overline{\frac{d}{dt}}\Big|^2+p^2\right)$.[5]

Thus the determination of the normal types is equivalent to the solution of this equation.

When dissipation exists the equation contains odd powers of $\frac{d}{dt}$ but the roots are of course still in pairs each of which pairs corresponds to a normal type of decaying vibration.

At p. 5 'the corresponding theorem relating to alteration of the potential energy'[6] should be actually enunciated for the benefit of natural philosophers not members of L.M.S. It is proved that the addition of a mass to any part of the system either increases or leaves unaffected every moment of inertia belonging to the system considered as having its type determined by a set of fixed multipliers of the variables. If we suppose this mass added continuously the types of the normal vibrations will change continuously, but since the moment of inertia is stationary at these normal vibrations as regards the variation of the multipliers the moment of inertia of the normal vibrations must increase as the mass increases.

Constraint of any kind is shown at p [7] to shorten the period of the fundamental vibration.[8] If it be objected that the fixation of the Sun would lengthen the year, it may be replied that if this fixation is effected by the application of a force binding the Sun to a fixed point and if this force rises from 0 to ∞ then there are at first two periods in the system, of which one is at first ∞.

The subject of harmonic types has great light thrown on it in this paper. The solution of physical problems on vibration conduction of heat, distribution of electricity &c depends on the discovery of the series of harmonic

(4) Strutt stated that his paper was preliminary to the 'preparation of a work on Acoustics'; see 'Some general theorems relating to vibrations': 357; and also Number 458, esp. note (2).

(5) p is the period of vibration.

(6) See Strutt, 'Some general theorems relating to vibrations': 359, where the text was apparently unchanged for publication.

(7) Space in MS.

(8) Strutt, 'Some general theorems relating to vibrations': 359.

types proper to the problem. We know the form of these types for a uniform closed curve, a sphere, certain sectors of a sphere, a cylinder and a few other cases, but the student can find but little information as to the general theory of harmonic types and their use when found. The fact that their use depends upon each harmonic type being *conjugate* to every other is given in this paper(9) and should be stated as clearly as it deserves.

The existence and properties of the Dissipation Function are clearly made out.(10) This is a matter of great importance. Many excellent investigations are rendered worthless from a neglect of dissipation,(11) amplitudes of forced vibrations come out zero or infinite in an absurd way. For equations containing dissipation terms see my paper on Governors Proc R.S. 1868.(12)

In Acoustics and Radiation there is a peculiar kind of dissipation which does not involve viscosity namely dissipation of undulations into an infinite medium, as when sound from an organ pipe escapes into the Atmosphere, or along an infinite tube as in Question 7, Jan 18, 1867, $1\frac{1}{2}$ to 4 Cambridge Math Tripos paper.(13) See also Qu ix Jan 21 1869 $1\frac{1}{2}$ to 4 for undulations in a dissipative medium,(14) showing that the refrangibility is irregular near an absorption band(15) ⟨where the dispersion is shown to be above the average so that the light is weakened not only by absorption but by dispersion at the band⟩. The ⟨refrangibility⟩ dispersion may become negative near an absorption

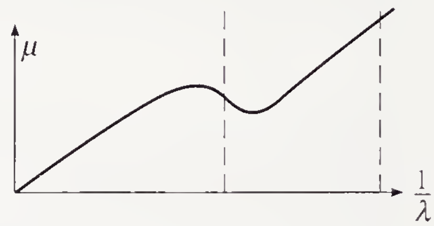

Figure 460,1

(9) Strutt, 'Some general theorems relating to vibrations': 361–3.

(10) Strutt, 'Some general theorems relating to vibrations': 363–6.

(11) See Strutt, 'Some general theorems relating to vibrations': 364, where he states that the 'existence of the [dissipation] function F does not seem to have been recognised hitherto, and indeed is expressly denied in the excellent "Acoustics" of the late Prof. Donkin (p. 101)'; compare W. F. Donkin, *Acoustics* (Oxford, 1870): 101.

(12) J. Clerk Maxwell, 'On governors'; *Proc. Roy. Soc.*, **16** (1868): 270–83, esp. 275, equation (6) and 278, equations (13) (= *Scientific Papers*, **2**: 111, 115).

(13) *The Cambridge University Calendar for the Year 1867* (Cambridge, 1867): 492; see Number 299 note (14).

(14) *The Cambridge University Calendar for the Year 1869* (Cambridge, 1869): 502. See Number 300: Appendix.

(15) Maxwell's examination question was later reprinted by Strutt; see Lord Rayleigh, 'The theory of anomalous dispersion', *Phil. Mag.*, ser. 6, **48** (1899): 151–2, who stated that: 'I have lately discovered that Maxwell, earlier than Sellmeier or any other writer, had considered this question. His results are given in the Mathematical Tripos Examination for 1869 (see 'Cambridge Calendar' for that year).' On the discovery of anomalous dispersion, see C. Christiansen, 'Ueber die Brechungsverhältnisse einer weingeistigen Lösung des Fuchsins; brieflicher Mittheilung', *Ann. Phys.*, **141** (1870): 479–80; and on its interpretation in terms of the

band so that a spot of light if refracted in a horizontal plane by the absorptive medium, and in a vertical plane by a glass prism might have the form shown in the margin.(16)

A collection of reciprocal properties such as that of section 3 and other physical cases of reciprocity would be useful if the mathematical foundation of the reciprocity were expressed in its simplest terms.(17)

E.g. If a series of imperfect elastic balls be placed not in contact with their centres in a straight line and if the first ball be projected against the second with velocity V and if after any number of impacts among the balls the last goes off with momentum M then if this last ball were projected in the reverse direction against the original system with velocity V the first will go off with momentum M.

Note 1. Sir W. Thomson is gone cable-laying for 3 months(18) and has not had time to peruse the paper, which he regrets as he is busy with vibrations.
Note 2. I regard this paper as proper to be printed by the Society.
Note 3. Address till October Glenlair, Dalbeattie.

J. Clerk Maxwell
26 June 1873

interaction of ether vibrations with the oscillatory frequencies of molecules, see W. Sellmeier, 'Ueber die durch Aetherschwingungen erregten Mitschwingungen Körpertheilchen und deren Rückwirkung auf die ersteren, besonders zur Erklärung der Dispersion und ihrer Anomalien', *Ann. Phys.*, **145** (1872): 399–421, 520–49; *ibid.*, **147** (1872): 386–403, 525–54. See Number 461. Strutt had himself referred to the phenomenon of anomalous dispersion in a paper 'On the reflection and refraction of light by intensely opaque matter', *Phil. Mag.*, ser. 4, **43** (1872): 321–38, on 322; 'Below the absorption-band the material vibration is naturally the higher, and hence the effect of the associated matter is to increase (abnormally) the virtual inertia of the aether, and therefore the refrangibility. On the other side the effect is the reverse.', citing a paper by Sellmeier, 'Zur Erklärung der abnormen Farbenfolge im Spectrum einiger Substanzen', *Ann. Phys.*, **143** (1871): 272–82. For discussion see Lord Kelvin, *Baltimore Lectures on Molecular Dynamics and the Wave Theory of Light* (Cambridge, 1904): 76–9.

(16) μ is the refractive index, λ the wavelength.

(17) Strutt, 'Some general theorems relating to vibrations': 366–8. See Numbers 480 and 482.

(18) See S. P. Thompson, *The Life of William Thomson Baron Kelvin of Largs*, 2 vols. (London, 1910), **2**: 636–9.

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MANUSCRIPT ON THE THEORY OF ANOMALOUS DISPERSION

circa 1873[(1)]

From the original in the University Library, Cambridge[(2)]

[CONNEXION OF ABSORPTION AND DISPERSION][(3)]

[1] Let ξ denote the displacement of the æther, x_1 x_2 &c the displacements of different constituents of a molecule.

If we suppose the action between the æther and a body in it to be like that of a liquid on an immersed body, the force acting between them will depend not on their relative displacement $\xi - x$ but on their relative acceleration $\ddot{\xi} - \ddot{x}$.

The vibrations of molecules which have definite periods, and which produce emission and absorption of particular kinds of light are due to forces between the parts of the molecule and not to forces between the molecule and the æther. For they are in great measure independent of the physical state of the molecule.

We may therefore assume as a probable form of the equations

$$0 = \ddot{\xi} + \alpha \frac{d^2\xi}{dy^2} + \beta_1(\ddot{\xi} - \ddot{x}_1) + \beta_2(\ddot{\xi} - \ddot{x}_2) + \&\text{c}$$

$$0 = m_1 \ddot{x}_1 + \gamma_{12}(\dot{x}_1 - \dot{x}_2) + \delta_{12}(x_1 - x_2)$$

$$0 = m_2 \ddot{x}_2 + \gamma_{12}(\dot{x}_2 - \dot{x}_1) + \delta_{12}(x_2 - x_1).$$

We may consider only x_1 and x_2 at present as two variables are enough to give a special time of vibration. But we may deduce the equations for molecules of any degree of complexity by considering their kinetic energy per unit of volume T their potential energy V[(4)] and their function of dissipation F[(5)] each of which is a homogeneous quadratic function, the first of the $\dot{x}$s the second and third of the xs.

Assuming that the solution is of the form

$$\xi = Ae^{ly} \cos n(t + \mu y)$$

(1) The document may well have been written consequent on Number 460: see notes (4) and (5).

(2) ULC Add. MSS 7655, V, f/9.

(3) Endorsed thus.

(4) Symbols employed by J. W. Strutt, 'Some general theorems relating to vibrations', *Proceedings of the London Mathematical Society*, **4** (1873): 357–68, esp. 357; see Number 460.

(5) See Number 460, esp. note (11).

where n is the frequency of vibration and μ is the index of refraction we find

$$\begin{vmatrix} -n^2(1+\beta_1+\beta_2)+(n^2\mu^2-l^2+2iln\mu), & n^2\beta_1, & n\beta_2 \\ n\beta_1^2, & -(m_1+\beta_1)\,n^2+\delta+i\gamma n, & -(i\gamma n+\delta) \\ n\beta_2^2, & -(i\gamma n+\delta), & -(m_2+\beta_2)\,n^2+\delta+i\gamma n \end{vmatrix} = 0$$

to determine μ the index of refraction and l the coefficient of absorption in terms of n the frequency $=\dfrac{2\pi}{\lambda}$.

Putting $m = 1+\beta_1+\beta_2$ $\quad m_1' = m_1+\beta_1$ $\quad m_2' = m_2+\beta_2$ and remembering that if the ray is not altogether absorbed in the first millimetre of the medium l must be very small and also γ we find

$$\mu^2 = m+\frac{\delta(\beta_1+\beta_2)^2-(m_1'\beta_2^2+m_2'\beta_1^2)\,n^2}{m_1'm_2'n^2-(m_1'+m_2')\,\delta}.$$

[2] In the Senate house examination Thursday afternoon Jan 21, 1869 I set a question of this kind modified to make the result as simple as possible.(6)

One kind of atoms was introduced each connected to a particle of the æther by a force varying as the distance. No force between the atoms. The æther elastic.

If ρ = density and E elasticity of æther displacement ξ

σ = density of atoms

force between atoms of æther in unit of volume $\sigma p^2(x-\xi)+\sigma R\dot{x}-\dot{\xi}$.

$$\rho\ddot{\xi}-E\frac{d^2\xi}{dy^2}+\sigma R(\dot{\xi}-\dot{x})+\sigma p^2(\xi-x) = 0 \quad \text{for æther}$$

$$\sigma\ddot{x}+\sigma R(\dot{x}-\dot{\xi})+\sigma p^2(x-\xi) = 0 \quad \text{for atoms.}$$

Assuming that the solution is of the form $Ce^{ly}\cos n(t-\mu y)$

$$\frac{d}{dt} = -in \qquad \frac{d^2}{dt^2} = -n^2$$

$$\frac{d}{dy} = l-in\mu \qquad \frac{d^2}{dy^2} = l^2-n^2\mu^2-2il\mu n$$

$$+\rho n^2+E(l^2-n^2\mu^2-2il\mu n) \qquad n^2\sigma$$

(6) See Number 460; and Number 300: Appendix.

REPORT ON A PAPER BY LATIMER CLARK ON A STANDARD VOLTAIC BATTERY[1]

26 JUNE 1873

From the original in the University Library, Cambridge[2]

REPORT ON M^r LATIMER CLARKS PAPER 'ON A STANDARD VOLTAIC BATTERY'[3]

I consider that the invention of a voltaic cell whose electromotive force is so constant that it may be employed as a standard for the comparison of other electromotive forces is one of the greatest services which can be done to electrical science.

In the absence of such a standard, the value of each electromotive force must be deduced from the results of a set of experiments which require delicate and costly instruments, and a laboratory free from vibration and from magnetic disturbances, not to speak of an amount of skill and of leisure of which few are possessed.

By the use of M^r Clark's cell, any practical electrician, with his ordinary instruments and methods, may determine the value of an electromotive force at once.

For the verification of the constancy of this cell M^r Clark has employed a method founded on that of Poggendorff[4] but capable I think of greater accuracy.

The chief merit, however, of the paper, is the determination, in electromagnetic measure, of the value of the electromotive force of the standard cell. I am not acquainted with any instance in which such great care

(1) See Number 415, Maxwell's report on the first version of Clark's paper.

(2) ULC Add. MSS 7655, V, c/54. The manuscript is endorsed: '1873. Latimer Clark by Clerk Maxwell' (and is a stray from the Royal Society's *Referees' Reports*).

(3) Latimer Clark, 'On a standard voltaic battery', *Phil. Trans.*, **164** (1874): 1–14. The paper was received by the Royal Society and read on 19 June 1873; see the abstract in *Proc. Roy. Soc.*, **21** (1873): 422. For the circumstances of the publication of the paper see Number 415 notes (4) and (12).

(4) J. C. Poggendorff, 'Methode zur quantitativen Bestimmung der elektromotorischen Kraft inconstanter galvanischer Ketten', *Ann. Phys.*, **54** (1841): 161–91. Poggendorff's method is described by Gustav Wiedemann, *Die Lehre vom Galvanismus und Elektromagnetismus*, 2 vols. (Braunschweig, ₂1872–3), **1**: 351–60.

has been bestowed upon the management of the Electrodynamometer[5] and the Sine Galvanometer,[6] or in which such consistent results have been obtained.

I think therefore that this paper deserves a place in the Philosophical Transactions, as one which shews a marked advance in the science of electrical measurement.[7]

J. Clerk Maxwell
Glenlair 26 June 1873

(5) See Number 416 note (3).

(6) See Number 415 note (9).

(7) In a report dated 1 July 1873 (Royal Society, *Referees' Reports*, **7**: 232) Sir Charles Wheatstone voiced a contrary opinion: 'I have already reported on M^r Latimer Clark's paper which is now presented with some abridgements.... I can only repeat the objections I formerly made to it'. For Wheatstone's comments on the first version of Clark's paper see Number 415 note (12). For confirmation of the accuracy of Clark's standard cell see a letter from W. Bottomley to Stokes of 26 March 1873 (Royal Society, *Referees' Reports*, **7**: 231).

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FIXTURES AND INSTRUMENTS IN THE CAVENDISH LABORATORY

JUNE 1873(1)

From the original in the University Library, Cambridge(2)

CAVENDISH LABORATORY(3)

FIXTURES & PRINCIPAL INSTRUMENTS IN THE DIFFERENT ROOMS(4)

Ground floor. (1) Under Lecture Room
Furnace, oven & boiler
iron retort for oxygen, &c

2 large gas holders
copper cylinders 2 feet diameter 6 feet deep in wooden vats. Pullies & weights for d°

Blowpipe & table for glass blowing

Battery of 80 trays Thomson's arrangement zincs 20 inches square: connexions for same

Working Table 6′ × 3′, 3 feet high.
Large Drawers for Copper-sulphate, &c with place above for carboys & acids

2 Small room next the last
Turning lathe
Bench & tools
Screw tap &c

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(1) See note (2) and Number 464.

(2) ULC, CUR, 55.2 (187). A holograph copy (ULC Add. MSS 7655, V, j/2) is endorsed: 'List of Instruments for Cavendish Laboratory by Prof[r] Maxwell June 1873'.

(3) The laboratory had been previously referred to as the 'Devonshire Laboratory': see Number 449.

(4) This list constitutes a statement of Maxwell's aims (see Number 464), rather than an account of the instruments actually acquired at the commencement of the Laboratory. Compare Maxwell's list of 'Instruments in the Cavendish Laboratory' (ULC Add. MSS 7655, V, j/4; to be published in Volume III) dated 29 April 1874. See Number 365 for a preliminary outline of the design of the Laboratory; and the account of 'The new physical laboratory of the University of Cambridge', *Nature*, **10** (1874): 139–42.

3 Heat Room

Stone table

Barometer by Casella (I have this)

Aneroid Barometer

Thermometers (All Centrigrade) Standard to 100 °C.

— delicate for atmospheric temperatures

— for boiling points

— Wet & dry bulb

Dew point instrument by Casella

Column of mercury (in the tower) for great pressures, connected to Heat room.

Bunsen's air pump(5) by falling water (in the tower, connected to Heat room and to lecture room).

Sprengel's air pump(6)

Joule's air pump by mercury(7)

Deloeils air pump(8) for exhaustion or compression (glass cylinders and long pistons working loosely in them)

Bramah's press(9) (in gun metal)

Water engine, Schmid's Patent N°. III (Makers in Gloucester, I cannot yet ascertain the name of the firm.)

Water tank, 4 feet diameter, 3 feet deep

Calorimeter (after Regnault)(10)

Bunsens Ice Calorimeter(11)

Apparatus for viscosity of gases (belongs to me at present).(12)

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(5) Robert Bunsen, *Gasometry: Comprising the leading Physical and Chemical Properties of Gases*, (trans.) H. E. Roscoe (London, 1857): 14.

(6) Hermann Sprengel, 'Researches on the vacuum', *Journal of the Chemical Society*, **3** (1865): 9–21; Sprengel, 'The invention of the water air-pump', *Phil. Mag.*, ser. 4, **45** (1873): 153–4. See S. P. Thompson, *The Development of the Mercurial Air-Pump* (London, 1888): 14–15.

(7) J. P. Joule, ['On a mercurial air-pump',] *Proceedings of the Literary and Philosophical Society of Manchester*, **12** (1873): 43, 55–6, 57–8. See Thompson, *Mercurial Air-Pump*: 9.

(8) J. A. Deleuil, 'Machine pneumatique construite sur un nouveau principe', *Comptes Rendus*, **60** (1865): 571–2; (trans.) 'Air pump constructed on a new principle', *Phil. Mag.*, ser. 4, **29** (1865): 487.

(9) A hydraulic press invented in 1795 by Joseph Bramah; see *Encyclopaedia Britannica* (11th edn), **4** (Cambridge, 1910): 417–18.

(10) H. V. Regnault, 'Sur les chaleurs latentes de la vapeur aqueuse à saturation sous diverses pressions', *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, **21** (1847): 635–728, esp. 665–7 and Plate VII Fig. 10.

(11) Robert Bunsen, 'Calorimetrische Untersuchungen', *Ann. Phys.*, **141** (1870): 1–31 and Plate I; (trans.) 'Calorimetric researches', *Phil. Mag.*, ser. 4, **41** (1871): 161–82 and Plate V. See J. Clerk Maxwell, *Theory of Heat* (London, 1871): 61–2.

(12) See Number 252 and Plate IV.

4 Balance Room

Balance to 5 Kilo. Secretan 1372
— 4 Kilo — 1378
Set of British weights
Set of French weights
Several rough balances (Mordan's patent)
Standard yard and pound
Standard mètre and kilogramme
Ten foot rod
4 mètre sliding 'wire parlant'
Gauge up to 1 foot
Micrometer screw gauge (Becker)
Rolling disk (Becker)
Dividing engine
Spherometer
Kathetometer (Stäudinger, Giessen)

5 Pendulum Room

Standard clock with electric connexion to all the clocks in the Museum
Stone pillar for the same
8 common clocks in connexion
2 or more journeyman clocks in connexion for experiments
Vacuum case for pendulum experiments
Stone foundation & pillar
Steinheil's 'Passage Prisma'(13) in window
Jenkins governor & driving gear & chronograph barrel(14)
Suspension wire, mirror & scale for determining moments of inertia

6 Magnetic Room

Kew magnetometer,(15) by Becker
Dip needle, Joule's suspension, with circle(16)
Joules pair of magnets for horizontal intensity(17)
Theodolite by Eichens of Paris called 'Aba' 360 fr.

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(13) C. A. von Steinheil, 'Ueber das Passage prisma', *Astronomische Nachrichten*, **24** (1846): cols. 269–74.

(14) See Numbers 210 notes (2) and (3) and 219 note (8).

(15) Maxwell's letters (dated 20 March and 4 May 1874) concerning the magnetometer, written to G. M. Whipple of the Kew Observatory, will be published in Volume III. See also E. Sabine, 'Results of the magnetic observations at the Kew Observatory from 1857–8 to 1862 inclusive', *Phil. Trans.*, **153** (1863): 273–84.

(16) See Number 339 notes (15) and (16).

(17) See Number 327 note (7).

Stone foundation for theodolite
4 mirrors 2 inches square fixed in walls
Revolving Coil, driving gear
Governor, telescope, scale } From Brit. Ass.
Wheatstones bridge and galvanometer
the property of British Association.[18]
Base for the coil of brick with stone top, 2 feet high 3 feet square
Electrodynamometer (Belongs to Brit. Ass.)[19]
Large standard coil 50 cm diameter
Helmholtz tangent galvanometer[20]
Thomson's suspended coil for magnetic intensity[21]

7 Lecture Room
3 Black boards hung like sash windows
2 scales 8′ × 1 with hinged screens to fold over them, painted white without but black within.

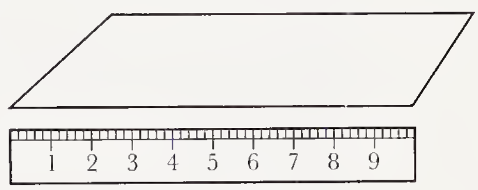

Figure 463,1

Thomson's Quadrant Electrometer[22]
— Reflecting Galvanometer[23]
2 lime lights
In the table are 5 taps for gas 3 for water 2 for oxygen 1 for steam and 1 connected with Bunsens air pump in the Tower.
3 sinks for water 2 flues for noxious flumes
2 pairs of electrodes of great battery below.
Above on the wall are a pair of electrodes connected with electric machine in the dry electric room, and one connected with the atmospheric collector on the Tower.
Fly wheel driven by a winch with pulley and band.
In the loft above lecture room
Suspension of Foucaults Pendulum[24]

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(18) See Numbers 210 note (2) and 219 note (9).
(19) See Number 416 note (3).
(20) Described by Gustav Wiedemann, *Die Lehre vom Galvanismus und Elektromagnetismus*, 2 vols. (Braunschweig, 1861), **2**: 197; and see the *Treatise*, **2**: 318–19, 330 (§§713, 725).
(21) See the *Treatise*, **2**: 328 (§724).
(22) Described by William Thomson in his 'Report on electrometers and electrostatic measurements', *Report of the Thirty-seventh Meeting of the British Association* (London, 1868): 489–512, esp. 490–7 and Plate 5 (= *Electrostatics and Magnetism*: 262–81). See the *Treatise*, **1**: 272–4 (§219); and G. Green and J. T. Lloyd, *Kelvin's Instruments and the Kelvin Museum* (Glasgow, 1970): 22–4 and Plate 5.
(23) See Numbers 379 note (4) and 399 note (7).
(24) See Number 364 note (7).

— Blackburns Pendulum[25]
Pulley & shaft on friction wheels for Thomson's endless chain[26]
Clock on East Side of Lecture room
White screen for projection
Electric Lamp
Large 'aquarium' tank of glass 4′ × 1′..6″ × 1′..6″

8 Professor's Laboratory
Drawers & cases for glass apparatus
Tap & sink for washing the same
Set of chemicals for ordinary testing

9 Apparatus Room
Cabinets with drawers below to the height of 3 feet and glass doors above to the height of 8 feet.
Large electromagnet with glass case and suspensions for diamagnetic experiments.
Large induction coil
Magneto electric machine (Gramme?[27] Wild?)[28]

10 Private Room
Book case for special library of the Laboratory
Desk Working-table

11 Large Laboratory
10 Working tables
1 Frame for measurements electric & other
Reflecting galvanometer & curtains
Cases for instruments in constant use
Tap & sink

12 Electric Room
Endless web of flannel to dry the air worked by clockwork, weights or water.[29]

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(25) The pendulum arrangement due to Hugh Blackburn (see Volume I: 238n), consisting of a spherical pendulum suspended by strings; see P. G. Tait and W. J. Steele, *A Treatise on the Dynamics of a Particle* (London, $_3$1871): 224–5.

(26) See Number 328.

(27) Z. T. Gramme, 'Sur une machine magnéto-électrique produisant des courants continus', *Comptes Rendus*, **73** (1871): 175–8.

(28) H. Wilde, 'On some improvements in electromagnetic induction machines', *Phil. Mag.*, ser. 4, **45** (1873): 439–50 and Plate VIII.

(29) See 'The new physical laboratory of the University of Cambridge': 141; 'Mr. Latimer Clark's contrivance...[to absorb] moisture from the air...so that the electrical instruments in the room are preserved in a highly insulating condition'.

Winters electrical machine[30]
Holtz[31] — connexion to Lecture Room
Varleys[32] —
Thomson's Reciprocal Electrophorus by water[33]
— Mouse mill[34]
— Portable Electrometer[35]
— Absolute Electrometer[36]
— Electroplatymeter[37]
Connexion to Collector of Atmospheric Electricity on the Tower
Large Tin Condenser
Microfarad (Worden & Co.)
Ohm
Latimer Clarks Standard Cell[38]
Spherical condenser
Electrophorus[39]
Maxwell's electric balance (made)[40]

13 Photographic Room
Window of yellow glass
Shelves for chemicals
Tap & sink

14 (15) 2 Optical Rooms
Foucaults Heliostat[41]

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(30) Karl Winter, ['Ein neuer Electrophor-Apparat',] *Berichte über die Mittheilungen von Fremde der Naturwissenschaften in Wien*, **2** (1847): 449–50.

(31) See Number 260 note (16).

(32) See Number 260 note (17).

(33) See Number 302 note (2).

(34) See S. P. Thompson, *The Life of William Thomson Baron Kelvin of Largs*, 2 vols. (London, 1910), **1**: 573; and Green and Lloyd, *Kelvin's Instruments*: 24 and Plate 7.

(35) See William Thomson, 'Report on electrometers and electrostatic measurements': 501–7 and Plate 6 Figs 8–10; and Green and Lloyd, *Kelvin's Instruments*: 22 and Plate 4.

(36) See William Thomson, 'Report on electrometers and electrostatic measurements': 497–501; and Number 289 note (11).

(37) See William Thomson, 'On new instruments for measuring electrical potentials and capacities', *Report of the Twenty-fifth Meeting of the British Association for the Advancement of Science; held at Glasgow in September 1855* (London, 1856), part 2: 22; and Thomson, 'On the measurement of electrostatic capacity', *Journal of the Society of Telegraph Engineers*, **1** (1873): 394–9, esp. 396–7.

(38) See Number 415.

(39) See the *Treatise*, **1**: 255–6 (§208).

(40) See Number 243 and Plates III and XI.

(41) Léon Foucault, 'Sur un moyen d'affaiblir les rayons du soleil au foyer des lunettes', *Comptes Rendus*, **63** (1866): 413–15; (trans.) 'On a means of weakening the solar rays in the focus of telescopes', *Phil. Mag.*, ser. 4, **32** (1866): 396–7.

Spectroscope (made)
Compound colour apparatus (made)
Scale for measuring focal lengths
Steinheils fluid prism
— reading telescope
Fizeau's 'conjugate telescopes'(42)
Microscope with graduated vertical movement
Noberts grating(43)
Prisms for bisulphide of carbon (made)(44)

16 Radiant Heat Room
Thermopile and galvanometer of small resistance
Metallic mirrors &c.

17 Calculating Room
Drawing board parallel ruler & scales
Beam compass Protractor
Amsler's Planimeter(45)

18 Acoustics
Organ bellows & wind chest
Organ pipe one side glass
Monometric gas jets (Koenig)
Tuning forks and resonators
Monochord
Helmholtz' Siren(46)

19 Tower
Mast at top with water dropping } connected to Electricity &
Collector for atmospheric electricity } Lecture room
Lightning conductor
In the stair
Bunsen's airpump by falling water connected to Heat and Lecture Room
Column of mercury for high pressures connected to Heat room

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(42) See Number 227 note (4).
(43) F. A. Nobert, 'Die Interferenz-Spectrumplatte', *Ann. Phys.*, **85** (1852): 80–2.
(44) See Number 355 esp. note (10).
(45) Jakob Amsler, 'Ueber das Polar-Planimeter', *Polytechnisches Journal*, **140** (1856): 321–7.
(46) To demonstrate the interference of sound and beats; see Hermann Helmholtz, *Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik* (Braunschweig, 1863): 241–3.

20 Store Room

Apparatus for strength of materials effect of strains on iron & magnets
Shaft for winding coils
Wire drawing and straightening
Glass tubes
Copper wire
Gutta percha covered wire
Steel piano wire
Caouchouc tubes

464

FROM A DRAFT LETTER TO HENRY WILKINSON COOKSON[(1)]

5 JULY 1873

From Campbell and Garnett, *Life of Maxwell*[(2)]

Glenlair
5 July 1873

I enclose a provisional list of fixtures and apparatus required for the Laboratory.[(3)]

At present I am not able to estimate the prices of many of the articles.

Some of them are in the market, and have simply to be ordered; others require to be constructed specially for the Laboratory.

I have begun with a list arranged according to the places and rooms in the Laboratory, but, of course, all small things must be kept in cases, either in the apparatus room, or in the special rooms.

The special duty of the professor of experimental physics is to teach the sciences of heat and electricity, and also to encourage physical research. The Laboratory must therefore contain apparatus for the illustration of heat and electricity, and also for whatever physical research seems most important or most promising.

The special researches connected with heat which I think most deserving of our efforts at the present time are those relating to the elasticity of bodies, and in general those which throw light on their molecular constitution; and the most important electrical research is the determination of the magnitude of certain electric quantities, and their relations to each other.

These are the principles on which I have been planning the arrangement of the Laboratory. But if in the course of years the course of scientific research should be deflected, the plans of work must vary too, and the rooms must be allotted differently.

I agree with you that the income of the Museums must be largely increased in order to meet the demands of this and other new buildings, and I am glad that the University is able to increase it.

It is impossible to procure many of the instruments, as they are not kept in stock, and have to be made to order. Some of the most important will require a considerable amount of supervision during their construction, for their whole value depends on their fulfilling conditions which can as yet be determined only by trial, so that it may be some time before everything is in working order.

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(1) Peterhouse 1827, Fellow 1836, Master 1847–76, Vice-Chancellor 1848, 1863–4, 1872–3 (Venn).
(2) *Life of Maxwell*: 352–3.
(3) Number 463.

465

POSTCARD TO PETER GUTHRIE TAIT

circa 8 JULY 1873

From the original in the University Library, Cambridge[1]

[Glenlair]

O T′.[2] T is selfconsistent.[3] For take XVI and find α by XIV.[4]

$$\alpha = \frac{dH}{dy} - \frac{dG}{dz} = \frac{1}{3}\alpha - \frac{1}{3}\int\frac{d\beta}{dy}dx - \frac{1}{3}\int\frac{d\gamma}{dz}dx + \frac{1}{3}\alpha\left(+\frac{1}{3}\alpha - \frac{1}{3}\int\frac{d\alpha}{dx}dx\right)$$

$$= \alpha + 0 \quad \text{(by (a) p. 401).}^{(5)}$$

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(1) ULC Add. MSS 7655, I, b/57.

(2) Maxwell was replying to Tait's card of 7 July 1873 (ULC Add. MSS 7655, I, a/35): 'O *dp*/*dt* See T, *Reprint of Electrostatics*, p. 402. The integrals in the values of F, G, H have a factor ($\frac{1}{3}$) w^h was not in the original paper. 4^ions say the factor should be $\frac{1}{2}$. What do you say? It is curious that T sh^d be *twice* wrong. Thus $V.\nabla\sigma = \tau$ gives $\sigma = \frac{1}{2}\int V\tau d\rho + \nabla u$; for $\nabla\int V\tau d\rho = \Sigma(\iota\nabla\tau\iota) = 3\tau - \tau = 2\tau$. You have said nothing of Ångström. Shall I send you my proofs on the subject? ☛ / Thermal Cond^y of Argentan varies by temp^re about $\frac{1}{5}$^th as much as that of Fe.' For Maxwell's comments on Ångström's work on thermal conductivity, and on Tait's paper, see his card of 24 July 1873 (Number 469, esp. notes (7), (8) and (9)). Tait had first requested Maxwell's comments in a card of 30 June 1873 (ULC Add. MSS 7655, I, a/34): '38 George Sq. Edin^h 30/6/73. O *dp*/*dt*, See'st thou what a splendid lark you have missed at Cambridge? On pondering farther on the question I should prefer to write – $(S.\nabla\nabla_1)\,\phi\rho$ in place of $\nabla S.\nabla_1\phi\rho$. But neither of them is good, as the ρ & ∇ *together* amount to mere artificial introduction of i, j, k: and therefore really do not introduce variables at all. It raises, however, a novelty as regards $\iiint d_\varsigma(\ \ )$, & $\iint ds(\ \ )$. / Did I ever ask you about the extraordinary error in Ångström's Conductivity (Thermal) paper? He takes an integral for the diff. =^n w^h is true only if rad^n & convection are 0. ☛. / If χ be the strain-function the work done is expressible as $\Sigma[S\chi\alpha\chi\beta - S\alpha\beta]\,[S\chi\gamma\chi\delta - S\gamma\delta]$. This has 21 terms.' Tait is alluding to his paper 'Additional note on the strain function, &c', *Proc. Roy. Soc. Edinb.*, **8** (1873): 84–6.

(3) Tait's reference (see note (2)) is to Thomson's 'A mathematical theory of magnetism', *Phil. Trans.*, **141** (1851): 243–85, esp. 283–4 (= *Electrostatics and Magnetism*: 341–404, esp. 402, where Thomson introduced the correction to his equations (XV) and (XVI) as mentioned by Tait in his card of 7 July 1873).

(4) For a solenoidal distribution of magnetism $\frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = 0$ (Thomson's equation (a)), where α, β, γ are the components of the intensity of magnetisation at any internal point in a magnet, Thomson states equations (XIV), $\alpha = \frac{dH}{dy} - \frac{dG}{dz}, \beta = \frac{dF}{dz} - \frac{dH}{dx}, \gamma = \frac{dG}{dx} - \frac{dF}{dy}$, 'where F, G, H are three functions to a certain extent arbitrary', and he obtains (XV), $F = \frac{1}{3}\iint dy\,dz\left(\frac{d\beta}{dy} - \frac{d\gamma}{dz}\right) + \frac{d\psi}{dx}$, and similar equations for G and H, where ψ denotes an arbitrary function, and obtains (XVI), $F = \frac{1}{3}\int(\beta dz - \gamma dy) + \frac{d\psi}{dx}$, and similar equations for G and H (the factor $\frac{1}{3}$ in (XV) and (XVI) being appended to the 1872 reprint of the paper).

(5) See note (4).

But how un4$^{\text{ionic}}$ is all this and indeed it is only expressible in base Cartesians.

$$F = \frac{1}{3}\int_0^z \beta dz - \frac{1}{3}\int_0^y \gamma dy + \frac{d\psi}{dx}.^{(6)}$$

I cannot interpret $\frac{1}{2}\int V\tau d\rho$ as having a definite value at a point in space independent of how you get to it. T's integrations are all conducted in rectangular trammels. Express, (if you can,) (XV)(7) in 4$^{\text{ions}}$!

I admit that $\Sigma(\iota V\tau\iota) = 2\tau$ but deny that $\int V\tau d\rho$ is a quantity capable of ∇-tion, if ρ (the cunctator)(8) is free to poke about as him pleases. But here is wisdom. Let α β γ be components of τ and let $S\nabla\tau = 0$(9) not only within the magnet but everywhere. Then $V.\nabla\sigma = \tau$(10) gives $\sigma = -\dfrac{1}{4\pi}\displaystyle\iiint \frac{V\nabla\tau}{\tau(\rho-\rho')}$.

What are the coordinates of T?

$\dfrac{dp}{dt}$

(6) Thomson's equation (XVI); see note (4).

(7) See note (4).

(8) One who hesitates (cognomen for Quintus Fabius Maximus).

(9) The solenoidal condition; see Number 396 note (8).

(10) The curl of σ; see Number 396 note (8).

LETTER TO HENRY AUGUSTUS ROWLAND[(1)]

9 JULY 1873

From the original in the Library of The Johns Hopkins University[(2)]

Glenlair
Dalbeattie
9 July 1873

Dear Sir

Your letter and M.S. have been forwarded to me this morning. I have read your paper with great interest and I think the whole of it is of great value and should be published as soon as possible because the subject is one of great importance, and the value of results such as yours is just beginning to be appreciated.[(3)]

I am sorry that your paper has arrived too late to be communicated to the Royal Society of London, and there will be no more meetings till November otherwise it would have given me great pleasure if you would allow me to communicate your paper to the Society. I hope however that if you have any other papers of the same sort you will give us an opportunity of making them known in this country.

I gather however from your letter that you would consider the Philosophical Magazine a suitable place for your paper to appear in.[(4)] It is certainly the best medium of publication for any researches in exact science. The are several other scientific periodicals but most of them circulate among a class of readers such that their Editors are apt to be suspicious of any article involving exact methods.

Your M.S. is so clear both in style and penmanship that I think the correction of the proofs will be a very easy matter. If I send it to the Phil. Mag., and if it is likely to appear at once, would you rather have the proofs sent to you or will it be sufficient if I look them over?

I may be mistaken in glancing over the paper, but I have not seen clearly

(1) Of the Rensselaer Polytechnic Institute in Troy, New York.

(2) Henry Augustus Rowland Papers MS. 6, Milton S. Eisenhower Library, The Johns Hopkins University, Baltimore.

(3) Rowland's study of the properties of magnetic metals, and his postulation of a magnetic analogue to Ohm's law for electric circuits, were not understood by the editors of the *American Journal of Science*; see J. D. Miller, 'Rowland's magnetic analogue to Ohm's law', *Isis*, **66** (1975): 230–41. See also D. W. Jordan, 'The magnetic circuit model, 1850–1890: the resisted flow image in magnetostatistics', *British Journal for the History of Science*, **23** (1990): 131–73, esp. 150–2.

(4) Henry A. Rowland, 'On magnetic permeability, and the maximum of magnetism of iron, steel, and nickel', *Phil. Mag.*, ser. 4, **46** (1873): 140–59.

whether your magnetization formula rests on a purely empirical foundation, or whether you can give any physical reason for adopting it.(5) You have also an investigation about a cylindric magnet.(6) If as I suppose the work is similar to Green's(7) I should like to see it in full, for if you have satisfactorily got over a piece of Greens work which is very precarious mathematics(8) you deserve great credit.(9)

Yours very truly
J. Clerk Maxwell

Address till October as above afterwards 11 Scroope Terrace, Cambridge

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(5) Rowland, 'On magnetic permeability': 144. Rowland explained the reasoning behind his derivation of his expression for magnetisation in his paper 'Studies on magnetic distribution', *Phil. Mag.*, ser. 4, **50** (1875): 257–77, 451–9, esp. 258–63.

(6) Rowland, 'On magnetic permeability': 144–6.

(7) George Green, *An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism* (Nottingham, 1828): 69 (= *Mathematical Papers of the Late George Green*, ed. N. M. Ferrers (Cambridge, 1871): 111).

(8) See Number 410; and the *Treatise*, **2**: 68 (§439).

(9) In his 'Studies on magnetic distribution': 258, Rowland noted that 'Green, in his "Essay", has obtained a formula which gives the same distribution; but he obtains it by a series of mathematical approximations which it is almost impossible to interpret physically'.

LETTER TO HENRY AUGUSTUS ROWLAND

12 JULY 1873

From the original in the Library of The Johns Hopkins University(1)

Glenlair
Dalbeattie
12 July 1873

Dear Sir

Mr Francis, the Editor of the Phil. Mag, will be very glad to have your paper, for the Magazine.(2)

From the largeness of some of your values of λ & μ(3) I suspected some difference between your units and the received ones, but on more careful examination I find that these quantities are really much larger for certain values of M(4) than has been supposed. Thalèn(5) used the magnetic force of the Earth, which although it is most important for ships and is most easily obtained is too small to bring out the largest value of μ.

If $\kappa = 32$(6) $\mu = 403$ nearly,(7) which lies between your values for different pieces of iron for small values of M.(8)

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(1) Henry Augustus Rowland Papers MS. 6, Milton S. Eisenhower Library, The Johns Hopkins University, Baltimore.

(2) Maxwell enclosed a letter of 11 July 1873 which he had received from William Francis (Rowland Papers): 'I shall be much pleased to receive Mr Rowland's paper for insertion in the Phil. Mag. The subject is undoubtedly one of the highest importance and from your description of it must be an exceedingly valuable one. Directly I receive the MS I will have it put into type, and shall feel much indebted to you for the perusal of the proofs.'

(3) Henry A. Rowland, 'On magnetic permeability, and the maximum of magnetism of iron, steel, and nickel', *Phil. Mag.*, ser. 4, **46** (1873): 140–59, esp. 141, 152–3. Rowland adopts Thomson's term 'magnetic permeability' (*Electrostatics and Magnetism*: 482–6). He follows Maxwell in the *Treatise*, **2**: 54 (§430), where μ is termed 'Coefficient of Magnetic Induction'; and from Maxwell's relation $\mu = 4\pi\kappa + 1$ (where κ is 'Neumann's Coefficient of Magnetization by Induction'), Rowland writes $\mu = \lambda/4\pi$. See Number 327 esp. note (3).

(4) Rowland, 'On magnetic permeability': 144, 152–3. Rowland defines M as the 'magnetizing-force of helix'. In his subsequent paper 'On the magnetic permeability and maximum of magnetism of nickel and cobalt', *Phil. Mag.*, ser. 4, **48** (1874): 321–40, esp. 322n, Rowland defined his notation in relation to that employed by Maxwell in the *Treatise*, stating that Maxwell's quantity $\mathfrak{H}$ (the 'magnetic force') corresponded to $4\pi M$ in his former paper.

(5) T. R. Thalén, 'Recherches sur les propriétés magnétiques du fer', *Nova Acta Regiae Societatis Scientiarum Upsaliensis*, ser. 3, **4** (1863). See Number 327 note (4).

(6) See note (3) and Number 327.

(7) From Maxwell's relation $\mu = 4\pi\kappa + 1$ (see note (3)).

(8) Rowland, 'On magnetic permeability': 152 (Table II).

There seems to be a mistake or rather perhaps miscopying at p 13 in the calculation of Q'[9] for a ring.

It should be

$$Q' = 2n'i\mu \int_{-R}^{+R} \frac{\sqrt{R^2-x^2}}{a-x}\,dx$$
$$= 4\pi n'i\mu\,(a-\sqrt{a^2-R^2}).^{(10)}$$

The expression developed in a series is correct as you have given it.

At p. 21 the reason why some take the direct action of the helix into account is that they are seeking for κ, not for μ.[11]

Now μ represents the permeability of what is inside the helix, that of air being unity.

κ represents the *increase* of permeability due to the substitution of iron for air, this increase being divided by 4π.[12]

The only disadvantage of using a ring of metal is the difficulty of determining its actual magnetic state. You can only determine *differences*.[13] In the case of a bar you can determine the number of lines of force at the middle of the bar by placing your coil there, and sliding it off the bar quickly. I should think this might be done with a bar long enough to get rid of the direct action of the ends at the middle point and not too long to perform the sliding off of the coil in a short enough time.

There are two other advantages of straight bars.

(1) The magnetizing helix may be made once for all and with great care.
(2) The magnetism of the bar may be disturbed by passing longitudinal electric currents through it, by tension pressure or torsion &c.

I understand from your paper the peculiarity of *magnetic* and *burnt* iron.[14]

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(9) In 'On magnetic permeability': 144, Rowland defines Q' as the 'lines of force *in* bar at any point'; and in his subsequent paper 'On the magnetic permeability and maximum of magnetism of nickel and cobalt': 322n, he explained that Maxwell's 'magnetic induction' $\mathfrak{B}$ corresponded to Q in his former paper.

(10) Compare Rowland, 'On magnetic permeability': 145, where the first expression has the coefficient 4. i is the strength of current, n' and R the number of coils and resistance per metre, a the area of the ring.

(11) Compare Rowland, 'On magnetic permeability': 148–9.

(12) See the *Treatise*, 2: 54 (§430), where Maxwell states the relation $\kappa = \dfrac{\mu-1}{4\pi}$.

(13) See Rowland, 'On magnetic permeability': 147. In his paper 'On the magnetic permeability and maximum of magnetism of nickel and cobalt': 336 Rowland acknowledged Maxwell's point.

(14) Iron 'burnt' by welding; see Rowland, 'On magnetic permeability': 149.

But I think that if a characteristic curve (say Table II & III)[(15)] of each were plotted it would be more easily understood.[(16)]

The behaviour of hard steel should also be compared with soft iron both when normal and when longitudinally magnetized.

There are important differences between the effects of a new magnetizing force according as it acts in the same or in the opposite direction to the residual magnetism.

It would be interesting also to trace the effects of previously magnetizing the iron transversely as D^r Joule did with a gun barrel by passing a current down the tube and up outside so making it a ring-magnet.[(17)]

The molecules are then placed with their axes mostly transverse to the length and should therefore be easily acted on by a longitudinal magnetizing force. Hence I suppose by magnetizing first transversely and then measuring λ for longitudinal magnetization you will obtain a very large value of λ.

You have seen, no doubt, in the journals that M. Jamin has succeeded in making a very powerful magnet.[(18)] A knowledge of the best kind of steel for magnets and how to temper and magnetize them is of great importance to science and very few scientific men or instrument makers know anything about it.

Would you like to have separate copies of your paper? & how many?

Yours truly

J. Clerk Maxwell

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(15) Rowland, 'On magnetic permeability': 152. Table II gives values for 'iron, magnetic', Table III for 'iron, burnt'.

(16) Compare Rowland's comments in 'On magnetic permeability': 149, 153–4.

(17) Joule had described various experiments on magnetisation (though not the arrangement mentioned by Maxwell), in his 'Account of experiments demonstrating a limit to the magnetizability of iron', *Phil. Mag.*, ser. 4, **2** (1851): 306–15, 447–56.

(18) Jules Jamin, 'Sur la théorie de l'aimant normal et sur le moyen d'augmenter indéfiniment la force des aimants', *Comptes Rendus*, **76** (1873): 789–94; (trans.) 'On the theory of the normal magnet, and the means of augmenting indefinitely the power of magnets', *Phil. Mag.*, ser. 4, **45** (1873): 432–7.

LETTER TO PETER GUTHRIE TAIT

22 JULY 1873

From the original in the University Library, Cambridge[1]

Glenlair 22 July 1873

O. T.

I beg leave to report that I consider the first two pages of Prof. Taits Paper on Orthogonal Isothermal Surfaces[2] as deserving and requiring to be printed in the Transactions of the R.S.E. as a rare and valuable example of the manner of that Master in his Middle or Transition Period, previous to that remarkable condensation, not so say coagulation, of his style which has rendered it impenetrable to all but the piercing intellect of the author in his best moments.

Nemo repente fuit Turpissimus.[3]

Airys Equations[4] are of the form for Turps[5]

$$\rho\frac{d^2\xi}{dt^2} = X = A\frac{d^2\xi}{dz^2} - B\frac{d^3\eta}{dz^3}$$

$$\rho\frac{d^2\eta}{dt^2} = Y = A\frac{d^2\eta}{dz^2} + B\frac{d^3\xi}{dz^3}.^{(6)}$$

These under the hypothesis $\zeta = 0$;[7] ξ & η functions of z and t only.

Hence in the potential energy of the system there must be terms of the form $-\xi\frac{d^3\eta}{dz^3}$ & $\eta\frac{d^3\xi}{dz^3}$[8] but in as much as we suppose pot energy to depend on relative, not on absolute displacement, these must be integrated by parts leaving internal parts $\frac{d\xi}{dz}\frac{d^2\eta}{dz^2}$ & $-\frac{d\eta}{dz}\frac{d^2\xi}{dz^2}$.

(1) ULC Add. MSS 7655, I, b/58.

(2) P. G. Tait, 'On orthogonal isothermal surfaces. Part I', *Trans. Roy. Soc. Edinb.*, **27** (1873): 105–23.

(3) 'No one ever suddenly became depraved'; Juvenal, *Satires*, ii, 83.

(4) Airy's mathematical treatment of the Faraday magneto-optic effect (on which see Number 434); see G. B. Airy, 'On the equations applying to light under the action of magnetism', *Phil. Mag.*, ser. 3, **28** (1846): 469–77, esp. 472. Airy cites James MacCullagh, who had previously obtained equations containing terms of the form d^3/dz^3 in his paper 'On the laws of the double refraction of quartz', *Transactions of the Royal Irish Academy*, **17** (1837): 461–9. Compare Maxwell's discussion of Airy's and MacCullagh's equations of motion in his treatment of the Faraday magneto-optic effect in the *Treatise*, **2**: 413–14 (§830).

(5) Turpentine: see Number 441 esp. note (8).

(6) ξ and η are displacements at time t parallel to the x and y axes.

(7) ζ is the displacement parallel to the z axis.

(8) Compare Number 441.

The question then arises. Since in Turps there are no fixed axes, these terms must form part of an invariant which is reduced to this form when $\zeta = 0$ & ξ & η independent of x & y. Now $S\nabla\sigma$ is an invariant but will not do but σ, $V\nabla\sigma$ $\nabla^2\sigma$ $V\nabla^3\sigma$ &c are vectors(9) the scalar part of the product of any two of which is independent of the directions of the axes of $x\ y\ z$.

Now $\nabla^2\sigma \,.\, V\nabla\sigma$ $(= S\nabla^2\sigma\nabla\sigma)$(10) is of the right dimensions in ∇ (or d) and is when expanded of the form (never mind signs)

$$\left(\frac{d^2\xi}{dx^2}+\frac{d^2\xi}{dy^2}+\frac{d^2\xi}{dz^2}\right)\left(\frac{d\zeta}{dy}-\frac{d\eta}{\delta z}\right)$$
$$+\left(\frac{d^2\eta}{dx^2}+\frac{d^2\eta}{dy^2}+\frac{d^2\eta}{dz^2}\right)\left(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\right)$$
$$+\left(\frac{d^2\zeta}{dx^2}+\frac{d^2\zeta}{dy^2}+\frac{d^2\zeta}{dz^2}\right)\left(\frac{d\eta}{dx}-\frac{d\xi}{dy}\right)^{(11)}$$

which is an invariant and when $\zeta = 0$ & ξ & η independent of x & y is reduced to

$$\frac{d\xi}{dz}\frac{d^2\eta}{dz^2}-\frac{d\eta}{dz}\frac{d^2\xi}{dz^2}$$

the form required.

The fact that the rotation is *roughly* as inverse square of wave length(12) with corrections for dispersion(13) shows that this is the form of the principal part of the term in **B**.(14)

But what if the body is turned about axis of x through a few degrees. Then values of $\frac{d\eta}{dz}$ arise without any strain of the body.

Moral: – These things are molecular & not to be explained by the diff: co: and ∇ of a continuous medium.

For your diamagnetic experiment(15) the best thing you can do is to throw your polarized light into a mass of Jargon.

The absorption lines are much more distinct in scientific Jargon(16) than in

(9) See Numbers 347 and 396 note (8).
(10) Compare Number 441.
(11) ξ, η, ζ are the components of the vector σ; and see Number 441 note (9).
(12) Émile Verdet, 'Recherches sur les propriétés optiques développées dans les corps transparents par l'action du magnétisme. De la dispersion des plans de polarisations des rayons de diverses couleurs', *Ann. Chim. Phys.*, ser. 3, **69** (1863): 415–91, esp. 438. See the *Treatise*, **2**: 413 (§830).
(13) Verdet, 'Recherches sur les propriétés optiques…': 439–41.
(14) Magnetic induction.
(15) See Number 440 esp. note (2).
(16) See notes (17) and (18).

ordinary materials. One very sharp one appears in light polarized one way and not in the other kind of polarized light.

See Sorby on Absorption.[17]

I believe that what is called scientifically Jargon and vulgarly Zircon[18] is derived from some of the settlements containing Christians of S Thomas and indeed contains Didymium.[19]

I think all recriminations between men of science on opposite sides of the Atlantic should be refused admittance into *Nature* or any other paper till they have been passed through the cable.[20] If one cabling is not sufficient to defæcate them, give them two more, and then if necessary send them round by Peru & Pernambuco till they are either cleansed themselves or have corroded the cables, in wh: case let the authors spend their time in discovering and mending the faults. You will see presently in Phil: Mag: a meritorious research of a modern Trojan[21] into the magnetizability of Iron Steel & Nickel.[22] He has hit on a good way of shooting at the maximum of magnetization by means of a curve, which as the result of plotting is good, but as the result of an equation is ludicrous.

L'Equation du Beau
exprimée par la formule Arithmétique

$$B = 2^{\pm m}\,(1\times 3^{\pm n}\times 5^{\pm p}).$$

(17) H. C. Sorby claimed to discover an element 'jargonium' from investigation of the absorption spectra of zirconium ore; see his paper 'On jargonium, a new elementary substance associated with zirconium', *Proc. Roy. Soc.*, **17** (1869): 511–15.

(18) Sorby admitted that the 'jargon' spectral lines were due to uranium; see his paper 'On some remarkable spectra of compounds of zirconium and the oxides of uranium', *Proc. Roy. Soc.*, **18** (1870): 197–207.

(19) A rare metal found in association with lanthanum and cerium, difficult to separate from lanthanum, and hence named 'didymium' (*διδυμος*, twin); see Henry Watts, *A Dictionary of Chemistry*, 5 vols. (London, 1863–9), **2**: 321.

(20) Maxwell is alluding to the virulent exchanges, currently filling the letters column of *Nature*, between Alexander Agassiz (of Harvard) and George Forbes (of St Andrews) over the relations – both scientific and personal – between their fathers Louis Agassiz and James David Forbes in the formulation of theories of glacial motion in the early 1840s. See Alexander Agassiz, 'Originators of glacial theories', *Nature*, **8** (1873): 24–5; George Forbes, 'Agassiz and Forbes', *ibid.*: 44; Forbes, 'Forbes and Agassiz', *ibid.*: 64–5; Agassiz, 'Agassiz and Forbes', *ibid.*: 222–3. This correspondence had been sparked off by the publication of *The Life and Letters of James David Forbes* (London, 1873), written jointly by J. C. Shairp, P. G. Tait and A. Adams-Reilly. See also Number 477.

(21) 'Rowland of Troy, that doughty knight' (*Life of Maxwell*: 645).

(22) See Numbers 466 and 467.

'Le bon goût semble être l'apanage de la France dans les expositions universelles. Edouard Lagout.'
Voyez Les Mondes 17 July[(23)]

I return the epistle of the Knight.

$$\frac{\partial p}{\partial t}$$

(23) See a report in *Les Mondes*, **31** (17 July 1873): 480–96, esp. 486–7, on a theory of rules of beauty.

POSTCARD TO PETER GUTHRIE TAIT

24 JULY 1873

From the original in the University Library, Cambridge[1]

[Glenlair]

O T   Can you supply me with the no. of grammes of Hydrogen in a litre at a named temperature and pressure.[2] I require the value of $\frac{p}{\rho}$ in absolute measure and I have not the data here.

Also do you know if L. Boltzmann has done any thing new in electricity?[3] Wiedemann[4] in a letter seems to imply it. I only know Boltzmann as a student of the ultimate distribution of vis viva in a swarm of molecules.[5]

Viscosity in centimetre-gramme-second measure, deduced from Loschmidt on Diffusion of gases by the elastic-sphere theory compared with $\frac{dp}{dt}$ direct.[6]

	Loschmidt	$\frac{dp}{dt}$ direct
Hyd	0.0001112	0.0000967
Ox	0.0002581	
CO	0.0002076	
CO_2	0.0002049	0.0001612

Note on Ångström[7] received today. No explanation of α α' α_2 &c.[8]

(1) ULC Add. MSS 7655, I, b/59. Previously published in *Molecules and Gases*: 493–4.

(2) See Number 470, esp. notes (1) and (2).

(3) See especially Ludwig Boltzmann, 'Experimentaluntersuchung über die elektrostatische Fernwirkung dielektrischer Körper', *Wiener Berichte*, **63**, Abtheilung II (1873): 81–155.

(4) Gustav Wiedemann; the letter is not extant.

(5) Ludwig Boltzmann, 'Studien über das Gleichgewicht der lebendigen Kraft zwischen bewegeten materiellen Punkten', *Wiener Berichte*, **58**, Abtheilung II (1868): 517–60.

(6) Compare the values cited in Number 470.

(7) P. G. Tait, 'Note on Ångström's method for the conductivity of bars', *Proc. Roy. Soc. Edinb.*, **8** (1873): 55–61, read 17 February 1873.

(8) See A. J. Ångström, 'Neue Methode das Wärmeleitungsvermögen der Körper zu bestimmen', *Ann. Phys.*, **114** (1861): 513–30; (trans.) 'New method of determining the thermal conductibility of bodies', *Phil. Mag.*, ser. 4, **25** (1863): 130–42, esp. 132–3. α, α' are terms in Ångström's equations for the conducting power of bodies.

$$\text{Write } q_n = \sqrt{\frac{\pi n}{kT}}(1-e).^{(9)}$$

How long were the bars?(10)

If you go 17 miles per minute and take a totally new course 1700,000,000 times in a second where will you be in an hour?

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(9) See Tait, 'Note on Ångström's method': 56. T is the period of heating and cooling of a metal bar, k the conducting power, and q_n a term in the equation for the periodic heating of a bar.

(10) On Tait's continuing interest see his paper 'Thermal and electrical conductivity', *Trans. Roy. Soc. Edinb.*, **28** (1878): 717–40.

DRAFT OF 'ON LOSCHMIDTS EXPERIMENTS ON DIFFUSION'(1)

LATE JULY 1873(2)

From the original in the University Library, Cambridge(3)

ON THE APPLICATION OF LOSCHMIDTS EXPERIMENTS ON THE DIFFUSION OF GASES(4) TO THE DETERMINATION OF MOLECULAR QUANTITIES

There are three phenomena from which data may be obtained for the investigation of the nature of gases on the kinetic theory of their molecular constitution.

According to this theory the molecules of a gas are in a state of very rapid motion. Each individual molecule would in consequence of this motion pass from one side to another of the containing vessel with a speed of several hundreds of metres per second if it were not interrupted in its course by collisions with other molecules.

These interruptions however occur so often that the molecule in spite of its rapid motion travels very slowly through the crowd of other molecules against which it is always jostling. But though the molecules are thus made to spend a long time on their journey they still travel about, and a process of diffusion is always going on so that any particular set of molecules which were in a certain part of the vessel at first will become gradually scattered throughout the whole vessel.

If these molecules are of a different chemical nature from the rest the fact of their diffusion may be verified by chemical analysis of the contents of different parts of the vessel.

This has been done by Graham who obtained the first rough estimate of the

(1) Published in *Nature*, **8** (1873): 298–300 (= *Scientific Papers*, **2**: 343–50).

(2) The paper was published in the issue of *Nature* dated 14 August 1873; and see Maxwell's postcards to Tait of 24 and 30 July 1873 (Numbers 469 and 471). The available documentary evidence – see notes (19) and (25) — suggests that this draft was written during the last week of July 1873.

(3) ULC Add. MSS 7655, IV, 2 *verso*. Published in part in *Molecules and Gases*: 477–82.

(4) Joseph Loschmidt, 'Experimental-Untersuchungen über die Diffusion von Gasen ohne poröse Scheidewände', *Wiener Berichte*, **61**, Abtheilung II (1870): 367–80; *ibid.*, **62**, Abtheilung II (1870): 468–78.

rate of diffusion.[5] The coefficients of diffusion of several pairs of gases have since been determined with great accuracy by Prof. Loschmidt of Vienna, and in making a revision of the kinetic theory of gases and comparing these results of Prof. Loschmidt with that theory I obtained evidence of the consistency of these results with each other and with the other experimental data of the theory which I consider encouraging both to experimenters and speculators.

Besides the diffusion of matter with which we are now dealing there are two other kinds of diffusion on which experiments have been made – that of momentum or the lateral communication of sensible motion from one portion of a gas to another, and that of kinetic energy.

The diffusion of momentum gives rise to internal friction or viscosity, that of energy to the conduction of heat. Investigations on viscosity have been made by Graham[6] O. E. Meyer[7] and myself.[8] They involve the consideration of the mutual action between the moving gas and the surfaces of the solids over which it moves. Hence in all these investigations it is only by carefully arranged methods of comparison that trustworthy results can be obtained. The conduction of heat in air has recently been investigated experimentally by Prof. Stephan of Vienna who finds his results in striking agreement with the kinetic theory[9] but the practical difficulties of the investigation are even greater than in the case of viscosity.

In Prof Loschmidts experiments on diffusion on the other hand everything appears favourable to the accuracy of the results. He appears to have got rid of all disturbing currents, and his methods of measurement, founded on those of Bunsen[10] are most precise. The interdiffusing gases are left to themselves

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(5) Thomas Graham, 'A short account of experimental researches on the diffusion of gases through each other, and their separation by mechanical means', *Quarterly Journal of Science*, **20** (1829): 74–83; and Graham, 'On the molecular mobility of gases', *Phil. Trans.*, **153** (1863): 385–405.

(6) Thomas Graham, 'On the motion of gases', *Phil. Trans.*, **136** (1846): 573–632; Graham, 'On the motion of gases. Part II', *Phil. Trans.*, **139** (1849): 349–401.

(7) O. E. Meyer, 'Ueber die Reibung der Flüssigkeiten', *Ann. Phys.*, **113** (1861): 55–86, 193–228, 383–425; Meyer, 'Ueber die innere Reibung der Gase', *Ann. Phys.*, **125** (1865): 177–209, 401–20, 564–99; Meyer, 'Ueber die Reibung der Gase', *Ann. Phys.*, **127** (1866): 253–81, 353–82.

(8) J. Clerk Maxwell, 'On the viscosity or internal friction of air and other gases', *Phil. Trans.*, **156** (1866): 249–88 (= *Scientific Papers*, **2**: 1–25). See Number 252.

(9) Read: Josef Stefan, 'Untersuchungen über die Wärmeleitung in Gasen', *Wiener Berichte*, **65**, Abtheilung II (1872): 45–69. See Number 425, esp. note (5).

(10) Robert Bunsen, *Gasometrische Methoden* (Braunschweig, 1857); (trans. H. E. Roscoe), *Gasometry. Comprising the Leading Physical and Chemical Properties of Gases* (London, 1857).

and are not disturbed by the presence of any solid body. The results of different experiments with the same pair of gases are very consistent with each other. They prove conclusively that the coefficient of diffusion varies inversely as the pressure a result in accordance with the kinetic theory whatever hypothesis we assume for the mode of action between the molecules.

They also show that the coefficient of diffusion increases as the temperature rises, but the range of temperature in the experiments was too small to enable us to decide whether it varies as T^2 which it does according to the theory of a force varying inversely as the fifth power of the distance(11) or as $T^{\frac{3}{2}}$ as it does according to the theory of elastic spherical molecules.(12) In comparing the coefficients of diffusion of different pairs of gases Prof. Loschmidt has adopted a formula which is simple enough but which does not appear to me to agree with the kinetic theory and from which he deduces values of a quantity k' which according to him should be constant for all gases(13) but these values do not agree together in the manner which we should expect from the accuracy of the experiments.

According to the kinetic theory as deduced from the collisions of elastic spheres the coefficient of diffusion between two gases at standard pressure and temperature

$$D_{12} = \frac{p_1 p_2}{\rho_1 \rho_2} \frac{M_1 + M_2}{\pi s^2 V}$$

$$D_{12} = \sqrt{\frac{1}{W_1} + \frac{1}{W_2}} \frac{\mathfrak{M} V}{N} \frac{1}{2\sqrt{6\pi}} \frac{1}{s_{12}^2}$$

where W_1 & W_2 are the molecular weights of the gases that of hydrogen being unity $\mathfrak{M}$ is the mass of a molecule of hydrogen V the velocity of mean square for hydrogen $= \sqrt{\frac{3p}{\rho}}$ at standard pressure and temperature N the number of molecules in unit of volume (the same for all gases),(14) and s_{12} the distance between the centres at collision.

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(11) J. Clerk Maxwell, 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88, esp. 75 (= *Scientific Papers*, **2**: 60). See Number 263.

(12) The theory proposed by Maxwell in his 'Illustrations of the dynamical theory of gases', *Phil. Mag.*, ser. 4, **19** (1860): 19–32; *ibid.*, **20** (1860): 21–37 (= *Scientific Papers*, **1**: 377–409).

(13) Defining $k' = k_0/\sqrt{m_1 m_2}$, where k_0 is the diffusion coefficient at standard temperature and pressure, and m_1 and m_2 are the masses of the molecules of two gases; see Loschmidt, 'Experimental-Untersuchungen über die Diffusion von Gasen', *Wiener Berichte*, **62**, Abtheilung II (1870): 476.

(14) 'Loschmidt's number'; see Number 259 note (13).

Hence if we write $A^2 = \dfrac{\mathfrak{M}V}{N 2\sqrt{6\pi}}$

and $$\Sigma_{12}^2 = \frac{1}{D_{12}}\sqrt{\frac{1}{W_1}+\frac{1}{W_2}}$$

We find $s_{12} = A\Sigma_{12}$.

The quantity A which is constant for all gases contains two hitherto unknown quantities, the mass of a molecule of hydrogen and the number of molecules in unit of volume. The product of these quantities is the density of hydrogen, a known quantity but their ratio which appears in A is not fully ascertained though Prof Loschmidt himself was the first to estimate it roughly(15) which has been since repeated independently by Mr Stoney(16) and Sir W Thomson.(17)

The quantity Σ_{12} is proportional to the distance between the centres of the molecules at the instant of collision or to $r_1 + r_2$ if r_1 and r_2 are the radii of the molecules. Now from Prof Loschmidts data we can deduce the values of Σ for the six pairs of gases which may be made up from the four gases H, O, CO and CO_2. These according to our theory are not independent, being the six sums of pairs of the four independent quantities r_1 r_2 r_3 r_4.

Accordingly assuming

$$2r\,(\mathrm{H}) = 1.739 \qquad 2r\,(\mathrm{O}) = 2.283$$
$$2r\,(\mathrm{CO}) = 2.461 \qquad 2r\,(\mathrm{CO_2}) = 2.775$$

we find

	By calculation $\Sigma_{12} = r_1 + r_2$	By Loschmidt $\Sigma_{12} = \sqrt{\sqrt{\frac{1}{W_1}+\frac{1}{W_2}}\frac{1}{D_{12}}}$
Σ (H, O)	2011(18)	1992
Σ (H, CO)	2100	2116
Σ (H, CO_2)	2257	2260
Σ (O, CO)	2372	2375
Σ (O, CO_2)	2529	2545
Σ (CO, CO_2)	2618	2599

(15) Joseph Loschmidt, 'Zur Grösse der Luftmolecüle', *Wiener Berichte*, **52**, Abtheilung II (1865): 395–413.

(16) G. J. Stoney, 'The internal motions of gases compared with the motions of waves of light', *Phil. Mag.*, ser. 4, **36** (1868): 132–41.

(17) William Thomson, 'The size of atoms', *Nature*, **1** (1870): 551–3 (= *Math. & Phys. Papers*, **5**: 289–96).

(18) Read: 2.011 in centimetre-second measure.

The agreement of these numbers furnishes I think strong evidence in favour of the kinetic theory of gases. But we may derive evidence of a higher order by a comparison between experiments of two entirely different kinds, those on diffusion, already spoken of and those on viscosity.

If μ is the viscosity of a gas and ρ its density at standard temperature and pressure the theory gives

$$\frac{\mu}{\rho} = A\sqrt{\frac{2}{W}}\frac{1}{(2r)^2}$$

so that we have the following relation between the viscosities of two gases and their coefficient of diffusion

$$2D_{12} = \frac{\mu_1}{\rho_1} + \frac{\mu_2}{\rho_2}.$$

Calculating on this system the viscosities of the gases experimented on by Loschmidt in centimetre gramme second measure and comparing them with those deduced by O E Meyer from his own experiments and those of Graham and with my own we find[19]

	μ	Meyer[20]	Maxwell
H	0.000116	0.000134	0.0000971
O	270	306	
CO	217	266	
CO_2	214	231	0.000161

The numbers do not appear to agree very well with each other. The numbers given by Meyer are greater than those deduced from diffusion. Mine, on the other hand, are smaller. I have no doubt however that the best method of determining all these quantities is by the comparison of the diffusion coefficients of a great many pairs of gases.

I have shown that by this method we may obtain a set of numbers which are proportional to the diameters of the molecules of different gases. From these we can determine their relative volumes and since we already know their relative masses we can determine the relative densities of the molecules.

(19) The values here cited correspond with those in the published paper 'On Loschmidt's experiments on diffusion': 300 (= *Scientific Papers*, 2: 347), but differ from those in the postcard to Tait of 24 July 1873 (Number 469), suggesting late July 1873 for the date of composition of this draft.

(20) O. E. Meyer, 'Ueber die Reibung der Gase', *Ann. Phys.*, **127** (1866): 253–81, 353–82, on 378–9.

These densities have been compared by Loschmidt and Lorenz Meyer[21] with the densities of the same substances in the liquid condition and with the 'molecular volumes' of the substance in its compounds as estimated by Kopp.[22] It appears from these comparisons that the relative molecular volumes of the gases whose viscosity has been determined are on the whole roughly proportional to those of the same substances in the liquid state or in their liquid compounds.

It is manifest, however that the density of the molecules must be greater than that of the liquified substance, for even if we could determine the density of the substance at $-273\,°C$ and at an infinite pressure there would still be interstices between the spherical molecules. Indeed the method of estimating molecular volume by observations on liquids at the boiling point under a pressure of 76 cm of mercury seems a very arbitrary one, for there is no reason why the average pressure of our atmosphere at the level of the sea should be placed on any very high rank as a physical constant.

It is probable that at all observed temperatures the molecules of bodies are kept further apart by their motion of agitation than they would be if in actual contact. It is therefore more likely that we should obtain consistent results if we measured the molecular volume of substances when that volume is the smallest attainable.

The volume relations of potassium its oxide and its hydrated oxide as described by Faraday[23] seem to indicate that the whole theory of molecular volume is not quite understood.

If, however we assume as the molecular volume of oxygen that deduced by Kopp from that of oxide of tin namely 2.7 when $O = 16$ which is the smallest of those quoted by L. Meyer[24] we find for the number of molecules of any gas in a cubic centimetre at 760 mm and 0 °C

$$\mathcal{N} = 19 \times 10^{18}$$

Hence the side of a cube which would on an average contain one molecule is

$$\mathcal{N}^{-\frac{1}{3}} = 37.47 \text{ tenth metres.}^{(25)}$$

(21) Read: Lothar Meyer, 'Ueber die Molecularvolumina chemische Verbindungen', *Annalen der Chemie und Pharmacie*, **5** Supplementband (1867): 129–47.

(22) Hermann Kopp, 'Ueber Atomvolum, Isomorphismus und specifisches Gewicht', *Annalen der Chemie und Pharmacie*, **36** (1840): 1–32.

(23) Michael Faraday, 'A speculation touching electric conduction and the nature of matter', *Phil. Mag.*, ser. 3, **24** (1844): 136–44, esp. 139–40 (= *Electricity*, **2**: 288–9).

(24) L. Meyer, 'Ueber die Molecularvolumina chemischer Verbindungen': 145.

(25) Tenth metre is 10^{-10} metre, as noted by Maxwell on the *verso* of a letter from George Griffith of 27 July 1873 (ULC Add. MSS 7655, II/72). This measure had been introduced by Stoney, 'The internal motions of gases compared with the motions of waves of light':

The mass of molecule of hydrogen is

$$= 4.607 \times 10^{-24} \text{ grammes}$$

and its diameter is 5.8 tenth metres.

O	7.6
CO	8.3
CO_2	9.3

These estimates are much smaller than those of Prof Loschmidt Mr Stoney and Sir W Thomson. This arises from the molecular volume of oxygen being assumed much smaller than is usually done.

There is another quantity, however which plays a considerable part in the kinetic theory as developed by Clausius namely the mean length of the uninterrupted path of a molecule.(26) The determination of this quantity does not involve any estimate of such doubtful matters as the density of a molecule. We find the length of the mean path

for	Hydrogen	$l =$ 965 tenth metres
	Oxygen	560
	Carbonic Oxide	482
	Carbonic acid	430.

The length of a wave of green light is about ten times the mean length of path of a molecule of oxygen at 760 mm & 0 °C.(27)

138–9n. Maxwell calculated this value on the *verso* of Griffith's letter; again suggesting late July 1873 for the composition of the draft.

(26) Rudolf Clausius, 'Ueber die mittlere Länge der Wege...', *Ann. Phys.*, **105** (1858): 239–58.

(27) Compare Stoney, 'The internal motions of gases compared with the motions of waves of light'.

POSTCARD TO PETER GUTHRIE TAIT

30 JULY 1873

From the original in the University Library, Cambridge[1]

[Glenlair]

O T. $\Theta\alpha\gamma\xi$ for the density of H.[2] What do you expect me to do with Ewing & M^c^Gregor on Salts?[3] Figures & curves not sent to me but I see that the ordinates are densities. Now the best ordinates are Volumes of so much of the stuff as contains 1 of the original water. As for Å, if he neglects H, he does so at his peril. How can I save him? Let him sink![4] Have you seen Clausius ueber einen neuen mechanischen Satz in Bezug auf stationäre Bewegungen.[5] For the absolute values of molecular constants to be used for Viscosity diffusion & conduction I think diffusion exp^ts^ as done by Loschmidt for the best and least interfered with by sides of vessel &c. Thus for diameters of molecules of H, O, CO, CO_2.[6]

		Calculated	By diffusion	diff[7]
H	1739	$\frac{1}{2}(H+O) = 2011$	1992	-19
O	2283	$\frac{1}{2}(H+CO) = 2100$	2116	$+16$
CO	2461	$\frac{1}{2}(H+CO_2) = 2257$	2260	$+3$
CO_2	2775	$\frac{1}{2}(O+CO) = 2372$	2375	$+3$
CH	2605	$\frac{1}{2}(O+CO_2) = 2529$	2545	$+16$
NO	3063	$\frac{1}{2}(CO+CO_2) = 2618$	2599	-19
SO_2	3109			

Mass of Hydrogen molecule not less than 10^{-27} gramme.

(1) ULC Add. MSS 7655, I, b/60. Previously published in *Molecules and Gases*: 495–6.

(2) See Number 469.

(3) Maxwell had been asked to referee a paper by J. A. Ewing and J. G. MacGregor, 'On the electrical conductivity of certain saline solutions, with a note on the density', *Trans. Roy. Soc. Edinb.*, **27** (1873): 51–70. See Number 474.

(4) See Number 469.

(5) Rudolf Clausius, 'Ueber einen neuen mechanischen Satz in Bezug auf stationäre Bewegungen', *Sitzungsberichte der Niederrheinischen Gesellschaft für Natur- und Heilkunde zu Bonn*, **30** (1873): 136–54 (of which there is a reprint in Maxwell's library (Cavendish Laboratory)).

(6) See Number 470 note (18).

(7) See Tait's comment in his reply of 31 July 1873 (Number 474 note (3)).

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DRAFTS OF 'ON THE FINAL STATE OF A SYSTEM OF MOLECULES IN MOTION SUBJECT TO FORCES OF ANY KIND'[1]

circa AUGUST 1873[2]

From the originals in the University Library, Cambridge[3][4]

[1] [5][a]ON THE MOTIONS AND ENCOUNTERS OF MOLECULES. APPLICATION OF HAMILTONS METHOD OF THE HODOGRAPH TO REPRESENT VELOCITIES OF MOLECULES.

1 Method of representing velocities of molecules

The velocity of a body is a Vector that is a quantity which is determinate in direction and magnitude. It may therefore be conveniently represented by the finite straight line which would be traced by the body in unit of time if the velocity of the body remained the same in magnitude and direction during that time.

The position of the first point of this straight line is a matter of indifference so long as the line is considered only with respect to its magnitude and direction; so that although it might appear most natural to draw the line from the actual position of the body at the given time it is more convenient to draw all lines representing velocities from one point, called the Origin.

(a) {Maxwell} Rough notes which you may make any use of, but return before May.[6]

(1) Published in *Nature*, **8** (23 October 1873): 537–8 and in the *Report of the Forty-third Meeting of the British Association for the Advancement of Science; held at Bradford in September 1873* (London, 1874), part 2: 30–2 (= *Scientific Papers*, **2**: 351–4).

(2) For the circumstances of composition see Numbers 457 and 481; and see note (4).

(3) ULC Add. MSS 7655, V, f/7, 11.

(4) In notes on the 'Dynamical Theory of Gases', which form part of his notes on 'Maxwell's lectures/Oct term 1873' (ULC DAR. 210. 22), George Howard Darwin recorded Maxwell's presentation of the kinetic theory of gases. Maxwell began with an account of the application of the hodograph to represent the velocities of gas molecules. Darwin's notes on Maxwell's lectures on gas theory – part of a course on 'Heat and elasticity' (see *Cambridge University Reporter* (14 October, 1873): 23) – record Maxwell's presentation of the theory in terms similar to the argument of the manuscript drafts printed here. At the conclusion of these notes Darwin appended: 'See Nature for some week in Nov or Oct 1873 for this subject – *Maxwell*'. See also Number 482 note (5).

(5) ULC Add. MSS 7655, V, f/11.

This method has long been known in the construction of what is called the 'parallelogram of velocities' but its value was first clearly shown by Sir W. R. Hamilton who by drawing from one point a series of lines representing the successive velocities of a body, determined a series of points in a curve which he called the Hodograph.(7)

The Hodograph is a curve each point of which corresponds to a point in the path of the body, and by studying the correspondence of these curves the force acting on the body and the whole circumstances of the motion may be ascertained.

In our present investigation we use the same method to compare the simultaneous velocities of different bodies as well as the successive velocities of each.

We may regard this method as an example of one of the most powerful instruments of mathematical research – the simultaneous contemplation of two systems so related to each other that every element in the one has its corresponding element in the other. In pure geometry this study of corresponding elements in two figures has led to the establishment of a Geometry of Position by which results are obtained by pure reasoning without calculation the verification of which by the Cartesian analysis would fill many pages with symbols.

In Statics the same method has enabled us to construct diagrams of stress, by which without calculation the stresses of the pieces of a frame are all represented and in Geometrical Optics the study of the correspondence between the object and the image has been of almost equal service to the theory of optical instruments and to pure geometry.(8)

In all these instances we have to construct a figure the relative position of whose elements indicates not the relative *position* of the corresponding elements of the original system but their relative velocity, the force acting between them or some other physical quantity not apparent to the eye in the original system.

In molecular science this method is especially valuable when we want to form a mental representation of the motion of an immense number of molecules at a given instant. Instead of confusing the image we have already formed of the positions ⌊configuration⌋ of the system [of] molecules by trying to attach to each molecule an arrow or some other symbol to indicate its velocity, we form our image of the velocities on an entirely new field in which

(6) Maxwell's pencilled note was probably addressed to H. W. Watson: see note (12).

(7) W. R. Hamilton, 'A new mode of geometrically conceiving, and of expressing in symbolical language, the Newtonian law of attraction', *Proceedings of the Royal Irish Academy*, 3 (1847): 344–53. See Number 276 note (2).

(8) See Numbers 273 note (2) and 480.

their positions are not represented at all. In this figure, to every molecule corresponds a point, and the velocity of the molecule is represented in magnitude and direction by a line drawn from the origin to this point.

Since in all the motions of real bodies the phenomena depend not on their absolute but on their relative velocities, and as these are indicated in the diagram of velocities by the distances between the points of the figure, the position of the origin itself may be transferred from one part of the figure to another without altering the relative position of the points just as the ⌊motion [of a]⌋ system of molecules as a whole may be varied while the relative motion of the molecules may be unaltered.

If there are a great number of molecules having velocities different from each other, the diagram of velocities will contain as many points distributed over the diagram. If we take a small element of volume of the diagram and consider those points which lie within it, these points correspond to molecules whose velocities differ little from each other, either in magnitude or direction.

In studying the motion of the system it is found convenient to divide the molecules into groups according to their velocities, those molecules whose velocities lie within certain limits with respect to magnitude and direction being placed in the same group.

In the diagram of velocities these magnitudes are indicated at once by the points which correspond to them being included within a certain small region of the diagram, the boundaries of this region corresponding to the given limit of velocity.

We shall also find it convenient to use the term velocity-density to indicate the result of dividing the number of molecules whose velocities lie between the given limits by the volume of the corresponding region in the diagram of velocities.

[2](9) ENCOUNTER OF TWO MOLECULES

If two molecules act on each other only when at a very small distance apart, and if the kinetic energy of the system is not altered by the encounter then if OA represents the velocity of the first and OB that of the second, BA will represent the velocity of A with respect to B. If G is the centre of gravity of the two molecules when placed at A and at B respectively OG will represent the velocity of the centre of ⟨gravity⟩

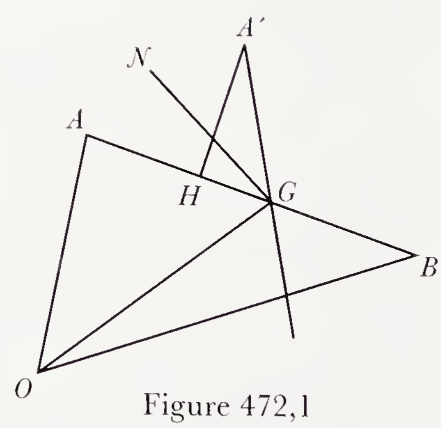

Figure 472,1

(9) ULC Add. MSS 7655, V, f/7. Previously published in *Molecules and Gases*: 398–9 (but there printed as a draft fragment preliminary to 'On the dynamical theory of gases' (1867)).

⟨inertia⟩ mass of the two molecules which is not altered by their mutual action.

During the whole motion, therefore, in whatever manner the velocities of the two molecules represented by the lines OA and OB may alter the centre of ⟨inertia⟩ mass, G, will remain fixed.

We shall also assume that the action between the molecules is such that after the encounter the kinetic energy of the system is the same as before. The kinetic energy of the system of two molecules may be divided into two parts the first being the kinetic energy of a mass equal to that of the system and having the velocity of its centre of ⟨gravity⟩ ⟨inertia⟩ mass, and the second being the kinetic energy arising from the motion of the parts of the system relatively to the centre of ⟨inertia⟩ mass.

The first part is necessarily unaffected by any mutual action of the parts of the system. Hence if the whole kinetic energy is the same before and after the encounter the second part must be so.

Now the kinetic energy of the motion relative to the centre of inertia is $\frac{1}{2}(A.\overline{GA}^2+B.\overline{GB}^2)$ before the encounter or since $A.GA = B.\overline{BG}$ the energy is $\frac{1}{2}\frac{A.B}{A+B}\overline{AB}^2$ and if OA', OB' represent the velocities after the encounter the kinetic energy is $\frac{1}{2}(A.\overline{GA'}^2+B.\overline{GB'}^2) = \frac{1}{2}\frac{A.B}{A+B}\overline{A'B'}^2$.

Now the ratio of GA to GB is the same before and after the encounter so that if this part of the kinetic energy remains the same after the encounter we must have

$$AB = A'B', \quad GA = GA' \;\&\; GB = GB'.$$

The velocity of the molecules relative to the centre of inertia is therefore unaltered and the result of the encounter is therefore completely defined if the *direction* of this relative velocity be given.

This direction is determined by the angle $AGA' = 2\theta$, and the angle, ϕ, which the plane of AGA' makes with a plane through AG parallel to the axis of x. This plane which may be called the plane of the encounter may be determined by drawing through the line AB a line parallel to the line joining the molecules at any instant. The plane through these two lines is the plane of the encounter. It is manifest that all values of the angle ϕ which determines the angular position of this plane round the line AB are equally probable.

The angle AGA' or 2θ, between the directions of the line GA before and after the encounter depends on the mode in which the force is exerted between the bodies and on their angular momentum about G.

[3][10] [THE MOTIONS AND ENCOUNTERS OF MOLECULES]

The elements therefore by which the circumstances of an encounter are completely defined are as follows.

The motion of the centre of mass of the two molecules. This motion is not affected by the encounter and the other elements of the encounter are not affected by it. We need not therefore take it into consideration till we have to do with the relation of the encountering molecules to other bodies.

In what follows we shall consider the centre of mass of the two molecules as the origin of the diagram of configurations and of that of velocities. The lines of motion of the two molecules will then be parallel and in opposite directions.

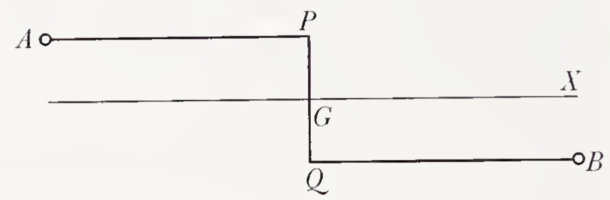

Figure 472,2

The velocity of the molecule A with respect to B before the encounter is called the velocity of approach and the direction of this velocity is called the direction of approach. The velocity of A with respect to B after the encounter is called the velocity of separation and its direction the direction of separation.

The distance, PQ between the lines of motion of the molecules before the encounter is called the arm of approach. The corresponding distance after the encounter is called the arm of separation.

The plane containing the direction of approach and the arm of approach is called the plane of the encounter. During the whole encounter the line joining the molecules remains parallel to this plane.

The angle between the direction of approach and the direction of separation is called the deviation.

The direction and velocity of approach is determined when we know the velocities of the two molecules.

To determine the arm of approach we require also to know the relative position of the two molecules at any instant before the encounter.

The plane of the encounter is thus determined. It is manifest that of all the planes passing through the direction of approach any one is equally likely to

(10) ULC Add. MSS 7655, V, f/11. The introductory portion of the manuscript has been published in *Molecules and Gases*: 402–4 (but there printed as a draft fragment preliminary to 'On the dynamical theory of gases' (1867)).

be the plane of the encounter. The angle of deviation depends on the velocity of approach and on the arm of approach. If the force with which the molecules act on each other does not pass through their centres of mass the angular positions of the molecules at the instant of encounter may affect the deviation, but we do not attempt to take account of this kind of irregularity otherwise than by classing those encounters as of the same kinds in which this irregularity as well as the other elements has the same value.

Neglecting this irregularity the velocity of separation is equal to that of approach but its direction is turned through the angle of deviation in the plane of the encounter.

Whatever be the circumstances of an encounter it is always possible to arrange the positions of the molecules for a second encounter such that the velocities of approach and separation in the second encounter shall be those of separation and approach in the first. For if we suppose the whole figure of the encounter turned round through two right angles in the plane of the encounter, and the directions of motion reversed, the velocity of approach in the original figure will be equal and parallel to the velocity of separation in the inverted figure and *vice versâ*.

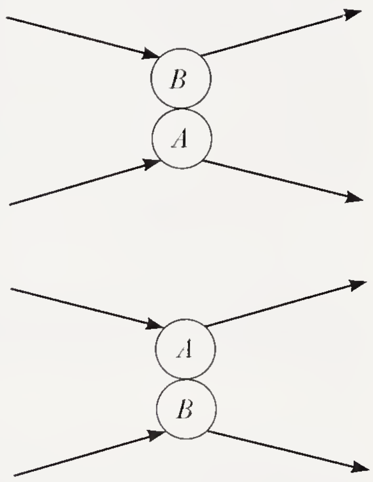

Figure 472,3

The arm of approach in the one figure is equal and parallel to that in the other figure, but it is drawn in the opposite direction.

We are now prepared to investigate the fundamental problem in the theory of molecules – the determination of the ultimate state of a multitude of moving molecules confined in a vessel in such a way that neither the molecules themselves, nor their energy, can escape.

For greater generality we shall suppose the molecules to be of several different kinds and we shall distinguish the quantities belonging to the different sets of molecules by different suffixes. We shall also suppose that the molecules are acted on by attractions or repulsions towards fixed points. The effect of gravity on the motion of a set of molecules will thus be included in our solution but for the sake of generality we shall only assume that the force acting on any set of the molecules is such that it may be derived from a potential and we may even suppose the form of this potential to be different for different sets of molecules.

Let x, y, z be the coordinates of a molecule of mass M and let ξ, η, ζ be the components of its velocity and let N be the number of such molecules in unit of volume and let ψ be the potential of the force which acts on this set of molecules.

The number of molecules of this kind which on an average have their coordinates between the limits x and $x+dx$, y and $y+dy$, z and $z+dz$ and also the components of their velocity between ξ and $\xi+d\xi$, η and $\eta+d\eta$, ζ and $\zeta+d\zeta$ must be a function of x, y, z; ξ, η, ζ; dx, dy, dz and $d\xi, d\eta, d\zeta$ of the form

$$\mathfrak{N} = f(x\ y\ z\ \xi\ \eta\ \zeta)\,dx\,dy\,dz\,d\xi\,d\eta\,d\zeta. \tag{1}$$

We have to investigate the form of this function.

We shall begin by determining the manner in which this function depends on the components of velocity (ξ, η, ζ) before we proceed to investigate in what manner it depends on the coordinates (x, y, z).

For this purpose we shall consider the effect of an encounter between two molecules which we shall assume to be of the first and the second kind respectively, distinguished by the suffixes 1 and 2. The whole number of molecules of the first kind in unit of volume of the given place which have their component velocities within the given limits may be written

$$f_1(\xi_1\ \eta_1\ \zeta_1)\,d\xi_1\,d\eta_1\,d\zeta_1 = n_1. \tag{2}$$

The number of molecules of the second kind selected in a similar manner according to the velocities may be written

$$f_2(\xi_2\ \eta_2\ \zeta_2)\,d\xi_2\,d\eta_2\,d\zeta_2 = n_2. \tag{3}$$

The number of pairs which can be formed by taking one molecule out of each of these groups is $n_1 n_2$.

The result of an encounter between the two molecules forming one of these pairs will depend on the elements of position of the molecules, that is to say on the arm of approach and on the plane of the encounter if the action between the molecules passes through their centres of mass. If the mode of action depends on the angular position of each molecule about its centre of mass then this also must be taken into account.

Let us class all encounters as of the same kind when these elements of position differ from certain specified values by less than certain given small quantities.

Now it is shewn in treatises on dynamics*(11) that if in any motion a certain

* Thomson & Taits Natural Philosophy Vol 1 p 250(11)

(11) Thomson and Tait, *Natural Philosophy*: 250. Thomson and Tait there (§328) discuss Hamilton's 'characteristic function', and establish that for a system of rigid bodies or connected particles, if ξ, η are momenta corresponding to coordinates ψ, χ, then '$\frac{d\xi}{d\chi} = \frac{d\eta}{d\psi}$ if both coordinates belong to one configuration, or $\frac{d\xi}{d\chi} = -\frac{d\eta}{d\psi}$, if one belongs to the initial configuration, and the other to the final'. Compare Maxwell's similar argument in his paper 'On Boltzmann's

variation δq_a in one of the elements of position of the initial motion causes a variation $\delta p'_b$ in one of the elements of momentum in the final motion then if the direction of motion be reversed a variation δq_b equal in magnitude to δq_a in the initial element of position will cause a variation $-\delta p'_a$ equal in magnitude to δp_b in the final element of momentum

$$\frac{dp'_a}{dq_b} = -\frac{dp_b}{dq'_a}.$$

We have already shown that if the system be turned round 180° in the plane of the encounter and the direction of motion reversed we have an encounter in which the initial velocities agree in direction and magnitude with the final velocities in the original encounter and in which the final velocities agree with the original velocities in the first encounter.

We now see that equal variations in the initial elements of position lead to equal variations in the final elements of velocity in the two encounters.

Let us now consider a set of encounters all the elements of which except the velocity of the centre of mass agree within very close limits but let the velocity of the centre of mass of the two molecules be anywhere between the limits $\bar{\xi}$ and $\bar{\xi}+d\xi$, $\bar{\eta}$ and $\bar{\eta}+d\eta$ and $\bar{\zeta}$ and $\bar{\zeta}+d\zeta$.

Since the velocities relative to the centre of mass are the same for all the encounters the limits of the velocity of the molecule A before the encounter will be ξ_1 & $\xi_1+d\xi$, η_1 and $\eta_1+d\eta$, ζ_1 and $\zeta_1+d\zeta$ and those of B will be ξ_2 and $\xi_2+d\xi$, η_2 and $\eta_2+d\eta$, ζ_2 and $\zeta_2+d\zeta$. We shall call these the original limits.

If the symbols of the velocities after the encounter are distinguished by accents their limits will be

ξ'_1 and $\xi'_1+d\xi$ $\quad$ η'_1 and $\eta'_1+d\eta$ $\quad$ ζ'_1 and $\zeta'_1+d\zeta$ for A

and $\quad$ ξ'_2 and $\xi'_2+d\xi$ $\quad$ η'_2 and $\eta'_2+d\eta$ $\quad$ ζ'_2 and $\zeta'_2+d\zeta$ for B.

We shall call these the final limits.

The number of pairs of molecules which have their original velocities lying within the given limits is

$$f_1(\xi_1\ \eta_1\ \zeta_1)f_2(\xi_2\ \eta_2\ \zeta_2)\ (d\xi\, d\eta\, d\zeta)^2. \tag{4}$$

The proportion of these whose elements of position lie within the assigned limits may be represented by the factor ρ.

Hence the number of pairs which change their velocities from the given original to the given final velocities is

$$\rho f_1(\xi_1\ \eta_1\ \zeta_1)f_2\,(\xi_2\ \eta_2\ \zeta_2)\ (d\xi\, d\eta\, d\zeta)^2. \tag{5}$$

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theorem on the average distribution of energy in a system of material points', *Trans. Camb. Phil. Soc.*, **12** (1879): 547–70, esp. 551 (= *Scientific Papers*, **2**: 718).

But we have seen that it is possible for two molecules to have an encounter in which the original and final velocities are the final and original velocities of the first encounter and that the factor which expresses the condition that the elements of the encounter shall be within given limits is the same in both cases. Hence the number of encounters of the second kind is

$$\rho f_1(\xi_1' \; \eta_1' \; \zeta_1') f_2(\xi_2' \; \eta_2' \; \zeta_2') \, (d\xi \, d\eta \, d\zeta)^2. \tag{6}$$

In these encounters the velocities change from the final to the original limits. Thus there is an exchange of molecules between the two groups whose velocities lie within the original and the final limits respectively. In the ultimate state of the system as many pairs of molecules must pass from the final to the original as from the original to the final velocities. Hence in the ultimate state we may equate (5) and (6) or omitting the common factors

$$f_1(\xi_1 \; \eta_1 \; \zeta_1) f_2(\xi_2 \; \eta_2 \; \zeta_2) = f_1(\xi_1' \; \eta_1' \; \zeta_1') f_2(\xi_2' \; \eta_2' \; \zeta_2') \tag{7}$$

One and only one necessary relation exists between the variables before and after the encounter namely that which expresses the conservation of the kinetic energy of the system

$$M_1 V_1^2 + M_2 V_2^2 = M_1 V_1'^2 + M_2 V_2'^2 \tag{8}$$

where the symbol V denotes the velocity and

$$V^2 = \xi^2 + \eta^2 + \zeta^2. \tag{9}$$

Since $f(\xi, \eta, \zeta)$ cannot depend on the directions assumed for the axes of $\xi \; \eta \; \zeta$ we may write it

$$f(\xi, \eta, \zeta) = e^{F(V)} \tag{10}$$

Hence taking the logarithm of equation (7) it becomes

$$F_1(V_1) + F_2(V_2) = F_1(V_1') + F_2(V_2') \tag{11}$$

whatever be the values of $V_1 \;\; V_2 \;\; V_1' \;\; V_2'$

provided $$M_1 V_1^2 + M_2 V_2^2 = M_1 V_1'^2 + M_2 V_2'^2. \tag{12}$$

From this we obtain

$$F_1(V_1) = B_1 - AM_1 V_1^2, \quad F_2(V_2) = B_2 - AM_2 V_2^2 \tag{13}$$

where A is a constant common to both functions and B_1, B_2 are independent.

We may now write the expression (1) in the more definite form

$$\mathfrak{N} = e^{B - AMV^2} d\xi \, d\eta \, d\zeta \, dx \, dy \, dz \tag{14}$$

where B is a function of x, y, z which may be different for different kinds of molecules while A, though it may, for ought we know be a function of $x \; y \; z$ is the same for the different kinds of molecules.

The relation between the velocities immediately before and immediately after a sudden encounter is not affected by the continuous action of finite forces on the molecules. Hence the above expression for the distribution of velocities at any given point is true whether external forces act or not. The effect of external forces is to alter the velocity of the molecules while they are describing their free paths.

Let us now confine our attention to molecules of the first kind and let us suppose that they are acted on by a force whose potential is ψ_1.(12)

Consider the $\mathfrak{N}$ molecules whose coordinates of position are $x\,y\,z$ and whose components of velocity are $\xi\;\eta\;\zeta$.

Let us follow any one of these molecules in its motion. The rates of variation of the coordinates are

$$\frac{dx}{dt}=\xi \quad \frac{dy}{dt}=\eta \quad \frac{dz}{dt}=\zeta. \tag{15}$$

The rates of variation of the component velocities are

$$\frac{d\xi}{dt}=-\frac{d\psi}{dx} \quad \frac{d\eta}{dt}=-\frac{d\psi}{dy} \quad \frac{d\zeta}{dt}=-\frac{d\psi}{dz}. \tag{16}$$

Hence if we consider $\mathfrak{N}$ as a function of $x\,y\,z\,\xi\,\eta\,\zeta$

$$\begin{aligned}\frac{d.\log\mathfrak{N}}{dt} &= \frac{dB}{dx}\xi+\frac{dB}{dy}\eta+\frac{dB}{dz}\zeta \\ &\quad +2AM\left(\xi\frac{d\psi}{dx}+\eta\frac{d\psi}{dy}+\zeta\frac{d\psi}{dz}\right) \\ &\quad -MV^2\left(\frac{dA}{dx}\xi+\frac{dA}{dy}\eta+\frac{dA}{dz}\zeta\right).\end{aligned} \tag{17}$$

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(12) On Maxwell's acknowledgment to Boltzmann for this method see Number 481. See Ludwig Boltzmann, 'Studien über das Geichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten', *Wiener Berichte*, **58**, Abtheilung II (1868): 517–60, esp. 555–60. In his *A Treatise on the Kinetic Theory of Gases* (Oxford, 1876): 12–13, H. W. Watson discussed the problem of deriving Maxwell's distribution law for a system of molecules in the presence of an external field of force, noting that his theorem was 'due originally to Dr. Ludwig Boltzmann and subsequently modified by Professor Maxwell.' He was here alluding to Maxwell's published paper 'On the final state of a system of molecules in motion subject to forces of any kind' (see note (1)). Watson considers any number of molecules, divided into two sets in a unit of volume, the molecules being acted on by impressed forces tending to fixed centres, and expressed as space variations of a potential function. His method of presentation, making reference to Thomson and Tait's dynamical argument (see note (11)), is however closer to Maxwell's argument in the present draft than to the procedure in the published paper (compare the later draft, Number 473, which is closer in style to the published paper). Watson acknowledged his 'access to some of his [Maxwell's] manuscript notes on this subject, from which I have taken many valuable suggestions' (*Kinetic Theory of Gases*: iv). It seems likely that Maxwell had made available MS notes now collected in ULC Add. MSS 7655, V, f/11; see also Number 478 §§5 and 6 and esp. note (31).

Now though the molecules have by their motion passed into this new condition their number must remain the same as before, so that the right hand member of the above equation (17) must be identically zero, whatever be the values of ξ, η, ζ.

Hence $$\frac{dA}{dx} = 0 \quad \frac{dA}{dy} = 0 \quad \frac{dA}{dz} = 0 \tag{18}$$

or A is independent of x, y, z and is constant throughout the system.

Also $$\frac{dB}{dx} = -2AM\frac{d\psi}{dx} \quad \frac{dB}{dy} = -2AM\frac{d\psi}{dy} \quad \frac{dB}{dz} = -2AM\frac{d\psi}{dz} \tag{19}$$

whence $$B = C - 2AM\psi \tag{20}$$

where C is independent of x, y, z.

Hence we obtain

$$\mathfrak{N}_1 = e^{C_1 - AM_1(2\psi_1 + \xi^2 + \eta^2 + \zeta^2)} dx\, dy\, dz\, d\xi\, d\eta\, d\zeta^{(13)} \tag{21}$$

as the number of molecules of the first kind which at a given instant lie within the element of volume $dx\,dy\,dz$ and have their velocities represented by points within the element $d\xi\,d\eta\,d\zeta$ of the diagram of velocities.

In this expression C_1 is a constant for each kind of molecule but may be different for different kinds of molecules, A is a constant which is the same for all kinds of molecules in the alternate state of the system, ψ_1 is the potential of the force which acts on molecules of the first kind. The other kinds of molecules may be acted on by forces having potentials different from ψ_1.

To find the whole number of molecules within the element $dx\,dy\,dz$ we must integrate (21) between the limits $\pm\infty$ with respect to ξ η and ζ so as to include all values of the velocity.

Dividing the result by $dx\,dy\,dz$ we obtain N_1 the number of molecules of the first kind in unit of volume

$$N_1 = \left(\frac{\pi}{AM_1}\right)^{\frac{3}{2}} e^{C_1 - AM_1 2\psi_1}. \tag{22}$$

From this it appears that the density of the first medium which is the product of the number of molecules in unit of volume into the mass of a molecule, varies from one part of the vessel to another according to a law which is independent of the existence of molecules of other kinds in the vessel.

For instance if a certain quantity of oxygen is placed in a closed vessel it will be distributed according to the well known law of a gas under the action of gravity, being denser below and rarer above. If now a quantity of nitrogen be

(13) Compare the result stated in Number 474, which corresponds more closely to that obtained in the published paper (*Scientific Papers*, **2**: 354).

added to the oxygen in the vessel and time be allowed for a thorough diffusion to take place the distribution of the oxygen will be exactly as before. That of the nitrogen will follow its own law diminishing in density in the upper part of the vessel but at a slower rate than in the case of oxygen because the mass of its molecule is less.

This is Dalton's law of mixed gases according to which if several portions of gas whether of the same kind or of different kinds occupy the same region each portion behaves as a vacuum to the rest.(14)

The kinetic energy of the medium in unit of volume is found by multiplying $\mathfrak{N}$ by $\frac{1}{2}M_1 V_1^2$ and integrating as before. By dividing this by N_1 we get the mean kinetic energy of a molecule

$$\frac{1}{2}M_1\overline{V_1^2} = \frac{3}{2}\frac{1}{A}. \tag{23}$$

As the quantity A is in the ultimate state of the system the same for every kind of molecule and for every part of the vessel, it follows that the mean kinetic energy is the same for all molecules whatever be their masses.

Now when two bodies are placed in communication their temperatures tend to become equal and the temperature of each depends in some manner on the agitation of its parts. The relation between the kinetic energy of agitation and temperature is not known for all bodies but in the case of two gases mixed together we now see that what tends to become equal is the mean kinetic energy of a single molecule in each gas. Hence we may assert that in gases the temperature is a function of the kinetic energy of agitation of a single molecule.

It follows from this that in the ultimate state of the system of molecules acted on by gravity the temperature is the same in all parts of the vessel.(15) The effect of gravity is to increase the density in the lower parts of the vessel but not to make any difference in the mean velocity of the molecules. This may appear in contradiction to the fact that in the free motion of any one molecule its velocity increases as it descends and diminishes as it ascends. To show that the contradiction is only apparent requires a little consideration which will form a good exercise for the student.

Another and still more important result is that equal volumes of different gases at equal temperatures and pressures contain equal numbers of molecules. This is Gay-Lussacs law of the molecular volumes of gases.(16)

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(14) John Dalton, 'Experimental essays on the constitution of mixed gases, ...', *Memoirs of the Manchester Literary and Philosophical Society*, 5 (1802): 535–602.

(15) See note (2).

(16) On Gay-Lussac and 'Avogadro's hypothesis' see Number 259 §4 and notes (13) and (14) and Number 263.

For the pressure on unit of area is

$$p = \frac{1}{3}MN\overline{V^2} = \frac{1}{2}\frac{N}{A}$$

where A is a function of the temperature. Hence when the temperature and the pressure are given, $N = 2Ap$ is given or the number of molecules in unit of volume is independent of the nature of the gas.

Again when heat is communicated to a gas that part of it which is spent in increasing the motion of agitation of the centres of the molecules is for each molecule the increment of $\frac{1}{2}M_1\overline{V^2}$ or of $\frac{3}{2}\frac{1}{A}$ which is the same for all gases for the same increment of temperature. Hence the specific heat per molecule is the same for all gases except in so far as their thermal energy may depend on the internal motions of each molecule. This is the law of molecular specific heats discovered by Dulong & Petit.(17)

(17) A. T. Petit and P. L. Dulong, 'Sur quelques points importants de la théorie de la chaleur', *Ann. Chim. Phys.*, ser. 2, **10** (1819): 395–413, esp. 405. See Number 377 note (18).

473

DRAFT OF 'ON THE FINAL STATE OF A SYSTEM OF MOLECULES IN MOTION SUBJECT TO FORCES OF ANY KIND'[1]

AUGUST 1873[2]

From the original in the University Library, Cambridge[3]

[ON THE FINAL STATE OF A SYSTEM OF MOLECULES IN MOTION][4]

Let molecules of several kinds be in motion within a vessel with elastic sides and let each kind of molecules be acted on by forces which have a potential the form of the potential being in general different for different kinds of molecules. Let the coordinates of a molecule be $x\,y\,z$ and the components of its velocity ξ, η, ζ and let it be required to determine the number of molecules of the first kind which, on average, have their coordinates between x & $x+dx$ y and $y+dy$ and z and $z+dz$ and also their component velocities between ξ and $\xi+d\xi$ η and $\eta+d\eta$ and ζ and $\zeta+d\zeta$. This number must depend on the coordinates $x\,y\,z$ on the component velocities $\xi\,\eta\,\zeta$ and on the limits and we may therefore write it

$$f_1(x_1\,y_1\,z_1\,\xi_1\,\eta_1\,\zeta_1)\,dx\,dy\,dz\,d\xi\,d\eta\,d\zeta.$$

We shall begin by investigating the law according to which this number depends on the velocity (ξ, η, ζ) before we proceed to determine in what manner it depends on the position (x, y, z).

For this purpose we consider the mode in which the velocity of a molecule of the first kind will be changed in consequence of a collision with a molecule, say, of the second kind. The whole number of molecules of the first kind in unit of volume at the given place which have velocities within given limits may be written

$$f_1(\xi_1\,\eta_1\,\zeta_1)\,d\xi_1\,d\eta_1\,d\zeta_1 = n_1.$$

The number of those of the second kind within their limits of velocity are

$$f_2(\xi_2\,\eta_2\,\zeta_2)\,d\xi_2\,d\eta_2\,d\zeta_2 = n_2.$$

The number of pairs which can be formed by taking one molecule of each kind is $n_1 n_2$.

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(1) For publication details see Number 472 note (1). The present draft is closer to the published paper than the drafts reproduced as Number 472.

(2) See Number 472 note (13).

(3) ULC Add. MSS 7655, V, f/12.

(4) For the circumstances of composition see Number 472 note (2).

We have already shown(5) that when two molecules encounter each other the velocity of their centre of gravity remains unchanged in magnitude and direction but that the line AB representing on the diagram of velocities the relative velocity of the molecule B with respect to A is turned about G as a fixed point without change of magnitude into a new direction which we may suppose to be defined by the angular coordinates θ & ϕ.

The values of θ and ϕ depend upon the direction and magnitude of the angular momentum of B with respect of A and on the direction of the axis about which this angular momentum exists, and the number of pairs [of] molecules for which θ lies between θ and $\theta+d\theta$ and ϕ between ϕ and $\phi+d\phi$ will be

$$n_1 n_2 F(\theta,\phi)\, d\theta\, d\phi.$$

Now let us suppose the components of relative velocity $(\xi_2-\xi_1)$, $(\eta_2-\eta_1)$, $(\zeta_2-\zeta_1)$ to remain constant while the components of absolute velocity of both molecules are made to vary then $d\xi_2 = d\xi_1 \quad d\eta_2 = d\eta_1 \quad d\zeta_2 = d\zeta_1$.

Let us also suppose that θ and ϕ remain constant during the variation then if the values of the component velocities after collision be distinguished by accents

$$d\xi_1' = d\xi_2' = d\xi_2 = d\xi_1 = d\xi$$

and the same for the other differentials.

Hence the number of molecules of the first kind whose velocities as represented in the diagram lie within the element A which encounter molecules of the second kind whose velocities lie within the element B equal to A and which have encounters such that θ and ϕ lie between given limits

$$f_1(\xi_1\ \eta_1\ \zeta_1) f_2(\xi_2\ \eta_2\ \zeta_2)\, F(\theta,\phi)\, (d\xi\, d\eta\, d\zeta)^2 d\theta\, d\phi$$

and after collision the velocities are represented by points lying within the elements A' and B', each equal to A.

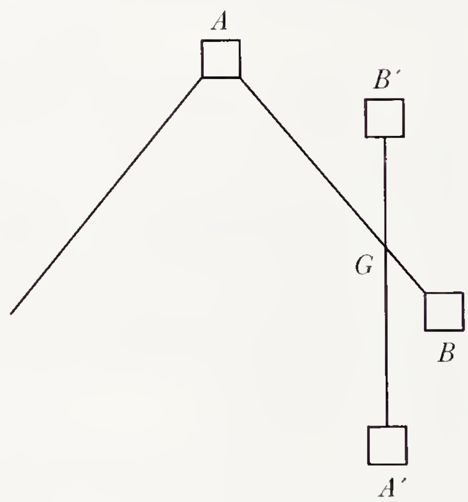

Figure 473,1

Let us now consider the number of encounters between pairs of molecules the original velocities of which lie within $d\xi\ d\eta\ d\zeta$ of $-\xi_1'\ -\eta_1'\ -\zeta_1'$ and $+\xi_2'\ +\eta_2'\ +\zeta_2'$ respectively and for which θ' and ϕ' lie between the same limits as before except that $\phi' = \phi+\pi \quad \theta' = \pi-\theta$. Their number is evidently

$$f_1'(-\xi_1'\ -\eta_1'\ -\zeta_1')\, f_2(+\xi_2'\ +\eta_2'\ +\zeta_2')\, F(\theta,\phi)\, (d\xi\, d\eta\, d\zeta)^2 d\theta\, d\phi.$$

(5) See Number 472 §2.

These pairs of molecules will change their velocities from OA' OB' to OA OB.

If the system is in a permanent state as many pairs will change from AO OB to OA' OB' as change from OA' OB' to AO OB or dividing out the common factors

$$f_1(\xi_1\ \eta_1\ \zeta_1)f_2(\xi_2\ \eta_2\ \zeta_2) = f_1(\xi_1'\ \eta_1'\ \zeta_1')\ f_2(\xi_2'\ \eta_2'\ \zeta_2').$$

One and only one necessary relation exists between the variables before and after collision namely

$$\begin{aligned} &M_1(\xi_1^2+\eta_1^2+\zeta_1^2)+M_2(\xi_2^2+\eta_2^2+\zeta_2^2) \\ &= M_1(\xi_1'^2+\eta_1'^2+\zeta_1'^2)+M_2(\xi_2'^2+\eta_2'^2+\zeta_2'^2). \end{aligned} \qquad [(1)]$$

Writing $\xi = Vl \quad \eta = Vm \quad \zeta = Vn$

this becomes $M_1\,V_1^2+M_2\,V_2^2 = \text{const.}$

We may also write $f(\xi, \eta, \zeta) = F(V^2, l, m, n)$

whence taking the logarithm of ([(1)])

$$F_1(M_1\,V_1^2\ l_1\ m_1\ n_1)+F_2(M_2\,V_2^2\ l_2\ m_2\ n_2)$$

is constant provided $M_1\,V_1^2+M_2\,V_2^2$ is constant.

Since l_1 m_1 n_1 are independent of l_2 m_2 n_2 we must have

$$F_1\,(M_1\,V_1^2) = AM_1\,V_1^2\ \&\ F_2\,(M_2\,V_2^2) = AM_2\,V_2^2$$

or

$$f_1(\xi_1\ \eta_1\ \zeta_1) = C_1\,e^{AM_1V_1^2}$$
$$f_2(\xi_2\ \eta_2\ \zeta_2) = C\,e^{AM_2V_2^2}.$$

Since the relation between the velocities immediately before and immediately after collision is not affected by the continuous action of finite forces on the molecules the distribution of velocities at any given point of the vessel must be such that the number of molecules in the element $dx\,dy\,dz$ having velocities between ξ & $\xi+d\xi$ &c is

$$dN = C_1\,e^{AM(\xi_1^2+\eta_1^2+\zeta_1^2)}\,d\xi\,d\eta\,d\zeta\,dx\,dy\,dz^{(6)}$$

where C is a function of $x\,y\,z$ which may be different for different kinds of molecules while A though it may be a function of the position of the element is the same for the different kinds of molecules.

Let us now suppose that a force whose potential is ψ_1 acts on the molecules of the first kind which we are now alone considering and let us consider the variation of dN during the time δt the quantities $x\,y\,z\,\xi\,\eta\,\zeta$ varying in the

(6) See the similar result in the published paper in *Nature*, **8** (1873): 537 (= *Scientific Papers*, **2**: 353).

same way as the coordinates and the component velocities of a molecule do, that is, let

$$\delta x = \xi \delta t \qquad \delta y = \eta \delta t \qquad \delta z = \zeta \delta t$$

$$\delta \xi = -\frac{d\psi}{dx}\delta t \quad \delta \eta = -\frac{d\psi}{dy}\delta t \quad \delta \zeta = -\frac{d\psi}{dz}\delta t.$$

We thus find

$$\begin{aligned}\frac{\delta \log \delta N}{\delta t} = \frac{dC_1}{dx}\xi + \frac{dC_1}{dy}\eta + \frac{dC_1}{dz}\zeta \\ -2AM\left(\xi\frac{d\psi}{dx} + \eta\frac{d\psi}{dy} + \zeta\frac{d\psi}{dz}\right) \\ +M(\xi^2+\eta^2+\zeta^2)\left(\frac{dA}{dx}\xi + \frac{dA}{dy}\eta + \frac{dA}{dz}\zeta\right).\end{aligned}$$

Now since the same set of molecules have by their motion passed into this new condition the variation of N must be zero and this must be true whatever the particular values of $\xi\ \eta\ \zeta$. Hence

$$\frac{dA}{dx} = 0 \quad \frac{dA}{dy} = 0 \quad \frac{dA}{dz} = 0$$

or A is a constant throughout the vessel.

Also $$\frac{dC}{dx} = 2AM\frac{d\psi}{dx} \qquad \frac{dC}{dy} = 2AM\frac{d\psi}{dy} \qquad \frac{dC}{dz} = 2AM\frac{d\psi}{dz}$$

or $$C = 2AM(\psi + B).$$

474

LETTER TO PETER GUTHRIE TAIT

circa AUGUST 1873[1]

From the original in the University Library, Cambridge[2]

Glenlair
Dalbeattie

Dear Tait[3]

(1) I have sent a report on Ewing & MacGregor[4] to Prof Balfour.[5]

(3) The person most injured in the Contemporary Review,[6] (next to JT)[7] is Principal Shairp,[8] who has been subjected to textual criticism and also to comparison with the author of the article.

(7) Does T drag at each remove a lengthened chain, and leave a continuity of copper between himself and the old world, or is his conversation confined to the new?[9]

(5) By the study of Boltzmann[10] I have become unable to understand him. He could not understand me on account of my shortness and his length was and is an equal stumbling block to me. Hence I am very much inclined to join the glorious company of supplanters and to put the whole business in about 6 lines.

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(1) See note (3) and Number 471; and the date given by Knott, *Life of Tait*: 114.

(2) ULC Add. MSS 7655, I, b/60A.

(3) The following numbered paragraphs indicate that this letter was written in reply to Tait's postcard post marked 31 July 1873 (and to another missing letter or card): 'O dp/dt; This is what is required of thee – / 1. Report to Balfour whether Ewing and MacGregor's paper is worthy to go into Trans. R.S.E. / 2. Report to ϛ. (at St Andrews) whether his formulas for periodic temperature in bars are *correct*, and whether he has employed proper courtesy to Å. / 3. Look at the *Contemp-Review* and advise whether it is worth while to answer, or whether it would be better to hand the author over at once to the son of the injured man for what he calls chastisement. / 4. Communicate any doubts or difficulties or discoveries of your own about ∇ for the benefit of my final chapter, now about to pass through the Clarendon Press. ϛ. / The diffusion numbers certainly show a splendid agreement. Why are the differences symmetrical? Is it a new Semal-bis-ter law?????'. (ULC Add. MSS 7655, I, a/36). On (1) see note (4). On (2) see Number 469 esp. note (7). On (3) see note (6). On (4) see P. G. Tait, *An Elementary Treatise on Quaternions* (Cambridge, ${}_2$1873): 260–88, on the quaternion operator ∇; the first edition of 1867 was published by the Clarendon Press, Oxford, the second edition by Cambridge University Press. On Tait's final comment see Maxwell's table of molecular diameters in his post card of 30 July 1873 (Number 471).

(4) See Number 471 esp. note (3).

(5) See Number 349 note (1).

(6) See Number 477 esp. note (1).

(7) John Tyndall: see Number 477.

(8) See Number 468 note (20).

(9) See Number 460 esp. note (18).

(10) See Numbers 472 note (12) and 481.

Boltzmanns aim is to settle the equilibrium of kinetic energy among a finite number of bodies.

But if the question is restricted to the average number of bodies M, within the element $dx\ dy\ dz$ which have velocity components $\xi\ \eta\ \zeta$ between limits $d\xi$, $d\eta$, $d\zeta$, there being N_1 bodies of mass M_1 acted on by a force whose potential is ψ_1, N_2 bodies of mass M_2 acted on by a force whose potential is ψ_2 &c the answer is

$$\epsilon^{-AM_1(\xi_1^2+\eta_1^2+\zeta_1^2+2\psi_1+B_1)}\, dx\, dy\, dz\, d\xi\, d\eta\, d\zeta.$$

The values of the constants A_1 B_1 must be found after the integration over the vessel containing the bodies. B is different for each set of bodies A is the same for all.[11]

Hence (1) the mean kinetic energy of the body is the same for all sizes of bodies and for all points in the vessel.

(2) The distribution of each kind of body in the vessel is the same as if all the other kinds had been removed, each taking its share of kinetic energy out of the common stock.

In thermal language.

Temperature uniform, in spite of crowding to one side by forces.

Molecular volume of all gases equal.

Equilibrium of mixed gases follows Daltons law of each gas acting as vacuum to the rest. (In fact it acts as vacuum to itself also.)

In my former treatise[12] I got these results only by way of conclusions. Now they come out before any assumption is made as to the law of action between molecules.

$$\frac{dp}{dt}$$

Is Joule very ill? He must count on not being able to be President of the Asses in Sept. which is not just yet.[13] It is a great pity for the Asses who have had and lost the opportunity of being led by a Lion.

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(11) See J. Clerk Maxwell, 'On the final state of a system of molecules subject to forces of any kind', *Nature*, **8** (1873): 537–8 (= *Scientific Papers*, **2**: 351–4, esp. 354); and Numbers 472 and 473.

(12) J. Clerk Maxwell, 'On the dynamical theory of gases', *Phil. Trans.*, **157** (1867): 49–88 (= *Scientific Papers*, **2**: 26–78).

(13) In a letter to Maxwell of 27 July 1873 George Griffith, Assistant General Secretary of the British Association for the Advancement of Science, informed Maxwell that: 'We have only lately heard from Joule that owing to his health he will be unable to act. Our Council have proposed Prof. Williamson of Univ. Coll. London in his place.' (ULC Add. MSS 7655, II/72). See also Number 478 note (2).

475

ON ATOMS AND ETHER: REPLY TO ALBERT JULIUS MOTT[1]

13 AUGUST 1873

From *Nature* (4 September 1873)[2]

ATOMS AND ETHER[3]

I am not enough of a metaphysician to say whether a substance which can be compressed and expanded *necessarily* contains void spaces.

If so, the idea of air, furnished to a beginner by instruction in 'Boyle's Law', is self-contradictory; and any molecular theory afterwards developed in order to account for 'Boyle's Law', may claim not only ingenuity but necessity in order to abate a crying grievance to all right-minded persons.

I do not myself believe in Prof. Challis's æther,[4] but at the same time I do not believe in the power of the human mind to pronounce that a continuous medium capable of being compressed is an impossibility.

But, on the other hand, I am sure that a medium consisting of molecules is essentially viscous; that is, any motions on a large scale which exist in it are

(1) A. J. Mott, 'Atoms and ether', *Nature*, **8** (1873): 322, written in response to Maxwell's anonymous review of 'Challis's "Mathematical Principles of Physics"', *Nature*, **8** (1873): 279–80 (= *Scientific Papers*, **2**: 338–42): 'Attempts to dispense, in physics, with the ideas of direct attraction and repulsion, however interesting, lead generally to a *petitio principii*, and I fear Prof. Challis's view, to which attention is called in *Nature*, of August 7, cannot be received as an exception. / For an ether of which the density can be varied is a substance that can be compressed and expanded, and what idea is in our minds when we speak of compression and expansion in a really continuous substance? Continuity implies space, and space that is full. Can space be more than full? When we say that a fluid is compressible and elastic, do we mean anything else than it is made of parts which can be pushed closer together, and which, being so pushed, will push each other back? But this is repulsion and action at a distance. We do not alter the fact by calling the substance ether, and relieving it from the influence of gravitation. / Is a continuous substance, which is capable of compression, conceivable? I think not; or if it is, the conception is at once more difficult and more opposed to sensible experience than that of attraction and repulsion. / The substance of a bar of iron is not continuous. If I draw one end of it towards me, why does the other end follow? What can be the relation between the movement of my end of the bar and the ethereal vibrations which must propel the other end and all intermediate parts in the same direction?'

(2) *Nature*, **8** (1873): 361.

(3) As published in *Nature* under this title: see note (1).

(4) See Maxwell's review (see note (1)) of James Challis, *An Essay on the Mathematical Principles of Physics* (Cambridge, 1873).

always being converted into molecular agitation, otherwise called heat, so that every molecular medium is the seat of the dissipation of energy, and is getting hotter at the expense of the motions which it transmits. Hence no perfect fluid can be molecular. So far as I can see, Prof. Challis intends his æther to be a perfect fluid, and therefore continuous (see p. 16 of his Essay),(5) though he does not himself pronounce upon its intimate constitution.

Hansemann*(6) makes his æther molecular, and in fact a gas with the molecules immensely diminished in size.

With regard to Mr. Mott's iron bar, when he pulls one end he diminishes, in some unknown way, the pressure between the particles of the iron, and allows the pressure of the æther on the other end to produce its effect.

N.B. This is only the language of a theory, and that theory not mine; nevertheless, I think it is consistent with itself.

J.C.M.

Glenlair, Aug. 13

* Die Atome und ihre Bewegungen, von Gustav Hansemann. E. H. Mayer: Coln, 1871.(6)

(5) See note (4).

(6) See Number 377 note (36).

LETTER TO JOHN WILLIAM STRUTT, LORD RAYLEIGH(1)

28 AUGUST 1873

From the original in private possession(2)

Glenlair
Dalbeattie
28 August 1873

Dear Lord Rayleigh

I have left your papers on the light of the sky &c(3) at Cambridge and it would take me even if I had them some time to get them assimilated sufficiently to answer the following question which I think will involve less expence to the energy of the race if you stick the data into your formula and send me the result (not worked out arithmetically I can do that).

Suppose that there are N spheres of density ρ and diameter s in unit of volume of the medium find the index of refraction of the compound medium and the coefficient of extinction of light passing through it.(4)

The object of the enquiry is of course to obtain data about the size of the molecules of air.(5) Perhaps it may lead also to data involving the density of the ether.

The following quantities are known being combinations of the unknowns.

$$\left.\begin{array}{rl} M = & \text{mass of molecule of hydrogen} \\ N = & \text{number of molecules of any gas in a cubic} \\ & \text{centimetre at } 0\,^{\circ}\text{C \& 760 B}^{(6)} \\ s = & \text{diameter of molecule of any gas} \end{array}\right\} \quad \text{Three unknowns}$$

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(1) John William Strutt succeeded his father as the third Baron Rayleigh in June 1873 (*DNB*).

(2) Rayleigh Papers, Terling Place, Terling, Essex. Published in part in Lord Rayleigh, 'On the transmission of light through an atmosphere containing small particles in suspension, and on the origin of the blue of the sky', *Phil. Mag.*, ser. 5, **47** (1899): 375–84, esp. 376n.

(3) J. W. Strutt, 'On the light from the sky, its polarization and colour', *Phil. Mag.*, ser. 4, **41** (1871): 107–20, 274–9.

(4) Strutt, 'On the light from the sky': 113; 'If the primary light be unpolarized, the intensity in a direction making an angle β with its course becomes $A^2\dfrac{(D'-D)^2}{D^2}(1+\cos^2\beta)\dfrac{mT^2}{\lambda^4 r^2}$', where D and D' are the original and altered densities of the medium, m the number of particles, T the volume of the disturbing particle, λ the wave-length, and r the distance from the disturbing particle.

(5) See Numbers 470 and 471.

(6) See Number 470 note (14). Barometric pressure in mm. of mercury.

Known combinations

$MN =$ density

Ns^2 from diffusion or viscosity

Conjectural combination

$$\frac{6M}{\pi s^3} = \text{density of molecule} \propto \begin{matrix}\text{density of liquid?}\\ \text{density in compounds?}\end{matrix}$$

If you can give us 1° the quantity of light scattered in a given direction by a stratum of a certain density & thickness and 2° the quantity cut out of the direct ray and 3° the effect of the molecules on the index of refraction which I think ought to come out easily we might get a little more information about these little bodies.(7)

You will see by Nature Aug 14 1873 that I make the diameter of molecules about $\frac{1}{1000}$ of a wavelength.(8)

The inquiry into scattering must begin by accounting for the great observed transparency of air. I suppose we have no numerical data about its absorption.

But the index of refraction can be numerically determined, though the observation is of a delicate kind, and a comparison of the result with the dynamical theory may lead to some new information.

Yours very truly
J. Clerk Maxwell

P.S. What vowel did the echo at Bedgebury Park return an octave higher?(9) There are some echoes here from trees which act best when the leaves are on. They are very different and more ringing than from walls but I must try them again by your new light.

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(7) Rayleigh subsequently published his own solution in his 'On the transmission of light through an atmosphere containing small particles in suspension': 377–84. See also Maxwell's letter to Rayleigh of 22 November 1873 (Number 482).

(8) See Maxwell, 'On Loschmidt's experiments on diffusion in relation to the kinetic theory of gases', *Nature*, **8** (1873): 298–300 (= *Scientific Papers*, **2**: 343–50); and Number 470.

(9) [Lord] Rayleigh, 'Harmonic echoes', *Nature*, **8** (21 August 1873): 319–20. Rayleigh reported 'an echo at Bedgebury Park.... The sound of a woman's voice was returned from a plantation of firs, situated across a valley, with the pitch *raised an octave*... it soon occurred to me that the explanation was similar to that which I had given of the blue of the sky'.

NOTE TO PETER GUTHRIE TAIT(1)

LATE AUGUST – EARLY SEPTEMBER 1873(2)

From the original in the National Library of Scotland, Edinburgh(3)

Can a man do *good* service in popularising certain parts of science and *thereby* lose his claim to scientific authority?(4) If a man has a claim to scientific authority the only way he can lose it is by writing bosch. If he writes it in a dry manner it is bad enough, but the harm is confined to students. But if he seasons it for the public, and the public swallow it (like the Saturday Reviewer)(5) then it is a sad misuse of words to say that this is a useful work.

Unless indeed it was a good work when the D–l invented popular tunes, because the pious were thereby enabled to set hymns thereto. If so, are you prepared to write an orthodox Libretto of the Tyndallic lectures(6) containing the spirit which gives liveliness, and avoiding the letter which would pluck any man.

(1) An annotation to the proofs of Tait's 'Tyndall and Forbes' dated 20 August 1873 and published in *Nature*, **8** (11 September 1873): 381–2. Tait was responding to John Tyndall's 'Principal Forbes and his biographers', *Contemporary Review*, **22** (August 1873): 484–508, where Tyndall defended his relations with J. D. Forbes on the theory of glaciers (see Number 319 notes (11) and (12)). Tyndall was responding to Tait's account of Forbes' glacier theory in J. C. Shairp, P. G. Tait and A. Adams-Reilly, *The Life and Letters of James David Forbes* (London, 1873): 492–520; and see also Number 468 note (20).

(2) See note (1).

(3) National Library of Scotland, MS.1709 f. 78.

(4) In his 'Tyndall and Forbes': 382 Tait had resorted to personal attack on Tyndall, holding up to ridicule Tyndall's discussion in his *Six Lectures on Light. Delivered in America in 1872–1873* (London, 1873): 25, of whether a 'rainbow which spans a tranquil sheet of water is ever seen reflected in it'. Tait commented: 'Dr. Tyndall has, in fact, martyred his scientific authority by deservedly winning distinction in the popular field'. In his bitter response (*Nature*, **8** (1873): 399) Tyndall replied that 'Mr Tait's criticism of my "popular" writings... is the product of mere ignoble spite'.

(5) The reviewer of 'Tyndall on Light' in *The Saturday Review*, **36** (26 July 1873): 115–16, had singled out Tyndall's discussion of the 'reason why a rainbow is never seen reflected in the sheet of water, however tranquil, which it spans', in praising the *Lectures on Light* as being marked with Tyndall's 'freshness of thought, clearness of exposition, and firm grasp of physical truth'.

(6) See note (4).

DRAFTS OF LECTURE ON 'MOLECULES'(1)

circa AUGUST – SEPTEMBER 1873(2)

From the originals in the University Library, Cambridge(3)

[1](4) [MOLECULAR PHENOMENA]

[In a liquid the diffusion of the molecules takes place with](5) extreme slowness, whereas internal friction, or the lateral diffusion of momentum from one stratum to another, is rather favoured by the molecules being close together and the conduction of heat which takes place in liquids not so much by the transference of molecules as by the communication of energy from one molecule to another with which it is as it were in gear takes place still more freely.

Another kind of diffusion which can be studied best in liquids is that which takes place under electric action. Here is a solution of iodide of potassium through which an electric current passes. Iodine appears at one electrode and potassium at the other but as potassium cannot exist in contact with water we have instead potass and hydrogen.

Clausius has thrown great light on this phenomenon which is called electrolysis(6) by his theory that the molecules of iodide of potassium are always dancing about in the solution and that with such vigour that every

(1) A lecture delivered to the meeting of the British Association for the Advancement of Science in Bradford on 22 September 1873; see J. Clerk Maxwell, 'A discourse on molecules', *Phil. Mag.*, ser. 4, **46** (1873): 453–69, esp. 453n (= *Scientific Papers*, **2**: 361–78). The MS of the text of the published lecture (first published as 'Molecules', *Nature*, **8** (25 September 1873): 437–41) is in ULC Add. MSS 7655, V, f/14.

(2) It is likely that Maxwell prepared the lecture in late August or early September 1873. According to a letter to Maxwell from George Griffith (Assistant General Secretary of the British Association) of 27 July 1873 Maxwell had tried to escape from his commitment to present the lecture on 'Molecules': 'I am very sorry to hear that it will be extremely inconvenient for you to deliver a Discourse at the Bradford meeting. I will have a conference with my colleagues on Tuesday & we will endeavour to find a substitute but I fear that it will be very difficult to do so.' (ULC Add. MSS 7655, II/72). In *Nature*, **8** (7 August 1873): 292 it was stated that 'It is also hoped that Prof. Clerk-Maxwell will deliver a discourse on "Molecules"'. See also note (12).

(3) ULC Add. MSS 7655, V, f/11, 13. Published in part in *Molecules and Gases*: 133–6, 257–9, 260–1 and 400–1.

(4) ULC Add. MSS 7655, V, f/13.

(5) Maxwell, 'Molecules': 439 (= *Scientific Papers*, **2**: 370).

(6) Rudolf Clausius, 'Ueber die Elektricitätsleitung in Elektrolyten', *Ann. Phys.*, **101** (1857): 338–60; (trans.) 'On the conduction of electricity in electrolytes', *Phil. Mag.*, ser. 4, **15** (1858): 94–109.

now and then they bounce up against some other molecule with such force that the iodine and potassium part company and dance about through the crowd seeking partners among the other dissociated molecules who have suffered like things. It is under these circumstances according to Clausius that the electromotive force produces the effects you have seen. As long as the molecules are in pairs the electromotive force which pulls the two molecules in opposite directions can do nothing, and by itself it is not sufficient to tear them asunder; but when by some violent shock the molecules are once parted the electromotive force exerts its guiding influence and bends the course of each of the unattached molecules each towards its proper electrode till the moment when meeting an unappropriated molecule of the opposite kind it enters into a new and closer alliance in which it is indifferent to mere electric suasion.

Another class of molecular phenomena from the study of which we may derive much light is the evaporation of liquids and the condensation of vapours. According to the theory of Clausius both these phenomena are always going on at the surface of every liquid.(7) Molecules are continually darting out of the liquid and other molecules from the gaseous mass above are darting into the liquid. When more molecules leave the liquid than enter it we call the process evaporation, when more enter it than leave it we call it condensation.

The conditions under which evaporation and condensation take place have been long studied. Certain correct statements about them are to be found in every elementary book, so that we are in danger of thinking we know all that is to be known but we need only to consider the vast accession to our knowledge of the relations between gases and liquids which we owe to Dr Andrews(8) to see that however well rounded our scientific doctrines may appear their true interpretation may involve some principle so profound that we are not even conscious that it yet remains to be discovered.

[2] [THE DATA OF MOLECULAR SCIENCE]

[The average distance travelled by a molecule between one collision and another](9) is very small. Roughly speaking it is about one tenth of a wave-length of light.(10)

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(7) R. Clausius, 'Ueber die Art der Bewegung, welche wir Wärme nennen', *Ann. Phys.*, **100** (1857): 353–80, esp. 361–3; (trans.) 'On the kind of motion which we call heat', *Phil. Mag.*, ser. 4, **14** (1857): 108–27, esp. 113–16.

(8) Thomas Andrews, 'On the continuity of the gaseous and liquid states of matter', *Phil. Trans.*, **159** (1869): 575–90.

(9) Maxwell, 'Molecules': 439 (= *Scientific Papers*, **2**: 369).

(10) See Number 470.

From the mean path and the mean velocity it is easy to calculate the number of collisions which each molecule must undergo in a second in a gas of standard temperature and pressure. These numbers are also given in the table.(11) They are reckoned by thousands of millions in a second. The numbers of vibrations of light is only about a hundred thousand times greater.

Having advanced thus far in the study of molecules let us spend a moment in taking account of the knowledge we have obtained. We have determined the relative masses of the molecules of different gases, their absolute velocities in metres per second and the amount of energy which takes the form of ⟨internal⟩ or rotatory or vibratory motion of the parts of the molecule about its centre of gravity.

These data of molecular science are obtained from those experiments on the pressure density and specific heat of gases which have long been recognised as the regular business of a laboratory.

In the second rank we must place the determination of the relative size of the molecules of different gases, and the absolute values of their mean paths and of the number of collisions in a second. For these quantities we must have recourse to experiments of a more difficult order, on the diffusion of gases, their viscosity and their conductivity for heat. Some progress has already been made in obtaining accuracy in researches of this kind, but we must remember that the numerical results of such experiments cannot as yet be regarded as so precise as those of the first rank.

But the results already obtained agree with each other sufficiently to show that the molecular theory of gases is worth the attention of scientific men.

There is another set of quantities which we must place in the third rank, as our knowledge of their numerical magnitude is neither precise, as in the first rank nor approximate as in the second, but is only as yet of the nature of a probable conjecture. These are the absolute mass and dimension of the molecules and the number of them in a cubic centimetre.

The most direct way of calculating these quantities is by a comparison of the volume of the substance as a gas at the standard temperature and pressure with its volume when the molecules are packed as closely together as possible.(12) We cannot be certain that in any case the molecules are in actual

(11) See Maxwell, 'Molecules': 441 (= *Scientific Papers*, **2**: 378).

(12) In a letter to Maxwell of 4 September 1873 (addressed from Lindau, Bavaria), Alexander Crum Brown (Professor of Chemistry at Glasgow University) gave Maxwell some information on molecular volume, responding to a letter of Maxwell's of 18 August 1873: '1st as to molecular (and atomic) volume of *solids*. The *atomic* volume of similar *elements* is often nearly the same, i.e. the sp. gr. varies as the *atomic* weight.... By adding 5.2 for each atom of oxygen, to the atomic volume of a metal, the molecular vol. of the oxide is obtained in a *good many*

contact. Indeed we are certain that in liquids they are not in contact, for the phenomena of diffusion show that they not only have room to move about among each other, but that they are always moving about even when the liquid is apparently at rest. But we have already shown that we can ascertain the relative dimensions of the molecules from experiments on diffusion viscosity and conduction. We can also compare the volumes to which a cubic centimetre of different gases is reduced when in the liquid form. In the case of those gases which have not been observed ⟨directly⟩ in the liquid form Kopp has calculated their molecular volume from the volumes of their liquid compounds.(13)

Now Loschmidt(14) and Lorenz Meyer(15) have shown that there is a remarkable though by no means perfect correspondence between the ratios of the volumes of the molecules deduced from experiments on viscosity and the molecular volumes of the substances in the liquid form. It would appear therefore that in most liquids the molecules are nearly in the same state of relative condensation so that the same proportional condensation would reduce them to the ideal state of maximum density in which no room at all is left between the molecules.

What the amount of this further condensation may be it is hard to say but we cannot suppose that any liquid is capable of any very great degree of condensation.

Now Loschmidt has shown in a very simple way that if the mean path of a molecule were shortened in the same proportion as the volume of the gas

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cases.... But this breaks down in many cases besides that which you mention of the alkaline metals. In these as you mention, the mol. vol. is diminished by the addition of oxygen. 2nd. mol. vol. of liquids. Owing to the large coeff. of expansion of liquids, no comparison can be made that is of any use unless at corresponding temperatures. Kopp compares them at temperatures at which they have the same vapour tension that is at the boiling points under the same pressure, and has made out a number of regularities or indications of regularities, but nothing like a law. You ask if the mol. volumists have a dodge to explain the anomalies. They have, and I have indicated it above. It is that we do not know the molecular weight of solids or liquids, but only of gases.' (ULC Add. MSS 7655, II/73). On work on molecular volumes by Kopp and Lothar Meyer see notes (13) and (15). The letter is published in *Molecules and Gases*: 506–8.

(13) Hermann Kopp, 'Beiträge zur Stoichiometrie der physikalischen Eigenschaften chemischer Verbindungen', *Annalen der Chemie und Pharmacie*, **96** (1855): 1–36, 153–85, 303–35; *ibid.*, **100** (1856): 19–38.

(14) Joseph Loschmidt, 'Experimental-Untersuchungen über die Diffusion von Gasen ohne poröse Scheidewände', *Wiener Berichte*, **61**, Abtheilung II (1870): 367–80; *ibid.*, **62**, Abtheilung II (1870): 468–78.

(15) Read: Lothar Meyer, 'Ueber die Molecularvolumina chemische Verbindungen', *Annalen der Chemie und Pharmacie*, **5** Supplementband (1867): 129–47.

would be diminished if it were reduced to its ideal condensation this shortened mean path would be about one eighth of the diameter of a molecule.[16]

It was in this way that Loschmidt in 1865[17] first announced that the diameter of a molecule of either of the gases contained in air is about a millionth of a millimetre or ten tenth-metres.[18] Independently of him and of each other Mr Stoney in 1868[19] and Sir W. Thomson in 1870[20] published results of a similar kind, those of Sir W. Thomson being deduced not only by the consideration of the volume of a liquefied gas but from the phenomena of soap bubbles and the electric properties of metals.

According to an estimate which I have made on Loschmidts plan[21] the size of the molecules of hydrogen is such that about two million of them placed in a row would be about a millimetre long and a million million million million of them would weigh about four grammes. In a cubic centimetre of gas at standard pressure and temperature there are nineteen million million million molecules and if they were placed in regular cubical order the distance between consecutive molecules would be 37 tenth-metres. We must remember that all these numbers which I have placed in the third rank are conjectural. In order to warrant us in putting any confidence in numbers of this kind we should have to show that independent calculations founded on data obtained from experiments on many different gases, lead to consistent results.

But what we have already obtained is enough to show that we have some foundation for our conjectures about the weight and dimensions of molecules, and that our knowledge of the subject has made much progress since the time when Graham began to experiment on Diffusion[22] and Transpiration.[23] The adventurers who now undertake the quest of the ultimate atom are at

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(16) Joseph Loschmidt, 'Zur Grösse der Luftmolecüle', *Wiener Berichte*, **52**, Abheilung II (1865): 395–413, esp. 398.

(17) Loschmidt, 'Zur Grösse der Luftmolecüle'.

(18) Tenth-metre is 10^{-10} metre; see Number 470 note (25).

(19) G. J. Stoney, 'The internal motions of gases compared with the motions of waves of light', *Phil. Mag.*, ser. 4, **36** (1868): 132–41.

(20) William Thomson, 'The size of atoms', *Nature*, **1** (1870): 551–3 (= *Math. & Phys. Papers*, **5**: 289–96).

(21) J. Clerk Maxwell, 'On Loschmidt's experiments on diffusion in relation to the kinetic theory of gases', *Nature*, **8** (1873): 298–300 (= *Scientific Papers*, **2**: 343–50). See Number 470.

(22) Thomas Graham, 'On the law of the diffusion of gases', *Trans. Roy. Soc. Edinb.*, **12** (1832): 222–58.

(23) Thomas Graham, 'On the motions of gases', *Phil. Trans.*, **136** (1846): 573–632; Graham, 'On the motions of gases. Part II', *ibid.*, **139** (1849): 349–401.

least aware of some of the phenomena which they have to study and they may be sure that in following up the paths already known they will light on others hitherto unthought of.

Some years ago the homoeopathists in their endeavour to administer small enough doses of medicine prescribed them in the dilution. (Here describe the process.)[24]

If the estimate we have formed of the size and weight of the molecules of simple substances be a just one the chance of the patient receiving even one molecule of the drug would be but small.

There are other men who have made a study of the transmission of the characteristic features and peculiarities of individuals from one generation to another and of the remarkable manner in which these peculiarities sometimes reappear after having been apparently lost for one or more generations.[25] Some of the results of these enquiries have been expressed in terms of the hypothesis that a large number of [....]

[3] [THE METHOD OF MOLECULAR SCIENCE]

Thus far we have been describing scientific work from the point of view of the worker himself. The aim we have kept in view is the study of the constitution of bodies and we have estimated the value of experiments methods and theories according to the help they have afforded us in this study.

But though the professed aim of scientific work is to unravel the secrets of nature it has another result hardly less important in its reaction on the mind of the worker. It leaves him in possession of methods which he could never have developed without the incitement of the pursuit of natural knowledge and it places him in a position from which many regions of nature besides that which he has been studying appear under a different aspect. The study of molecules has developed a method of its own and it has also opened up new views of nature.

To describe mathematical methods is at all times tedious and I have no intention of making use of a black board. But I think that the method which we are forced to adopt in the study of molecular phenomena affords one of the

(24) In his letter of 4 September 1873 (see note (12)) Crum Brown commented: 'I understand the homœopathists to mean by a drug in the n^{th} dilution, a substance containing $1/(100)^n$ of the active stuff. You take 1 part of your drug and 99 of sugar of milk if the drug be solid, 99 of water or spirit if it be liquid, mix *well* the result is the first delusion or dilution take 1 part of that & do it again &c &c. I think they sometimes go to the 10^{th} dilution!'

(25) Francis Galton, *Hereditary Genius: an Inquiry into its Laws and Consequences* (London, 1869). See Maxwell's reference to Galton in Number 439.

best illustrations of the nature and limitations of a large part of human knowledge.

The molecular theory of gases resolves itself into that of the motions and encounters of the molecules. As long as we have to deal only with one pair of molecules and to determine from their known motion before the encounter what will be their motion after the encounter we require no methods but those of elementary dynamics.

When air is compressed the sides of the vessel are moving to meet the molecules like a cricket bat swung forward to meet the ball, and the molecules like the cricket ball rebound with a greater velocity than before. When air is allowed to expand the sides of the vessel are retreating from the molecules like the cricketers hands when he is stopping the ball and the molecules rebound with diminished velocity. Hence air becomes warmer when compressed and cooler when allowed to expand. The observed amount of this heating and cooling however is less than it would be if the only motion of the molecules were that of their centres of inertia. Hence as Clausius has shown(26) the molecules must have other motions such as rotation and vibration as well as motion of simple translation and a definite portion of the energy of the gas arises from these rotatory or vibratory motions of the molecules which have no effect on the pressure of the gas.

Thus far any person acquainted with the most elementary dynamics may pursue the kinetic theory of gases. It is easy to understand the effects of the collisions of the molecules against the sides of the vessel. But when we attempt to trace the motion of each molecule among innumerable others and to determine the effects of all the collisions which must occur in the flying crowd we find that special methods are requisite for the treatment of so intricate a problem.

Lucretius has given us a hint how to form a mental representation of the dance of atoms by looking at the motes which we see chasing each other through a sunbeam in a darkened room.(27) Their number their minuteness

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(26) Clausius, 'Ueber die Art der Bewegung, welche wir Wärme nennen'.

(27) *Titi Lucreti Cari De Rerum Natura Libri Sex*, ed. and trans. H. A. J. Munro, 2 vols. (Cambridge, ₂1866), **1**: 87–8 (Book II, 114–22): 'contemplator enim, cum solis lumina cumque / inserti fundunt radii per opaca domorum: / multa minuta modis multis per inane videbis / corpora misceri radiorum lumine in ipso / et velut aeterno certamine proelia pugnas / edere turmatim certantia nec dare pausam, / conciliis et discidis exercita crebris; / concicere ut possis ex hoc, primordia rerum / quale sit in magno iactari semper inani'; ('observe whenever the rays are let in and pour the sunlight through the dark chambers of houses: you will see many minute bodies in many ways through the apparent void mingle in the midst of the light of the rays, and as in never-ending conflict skirmish and give battle combating in troops and never halting, driven about in frequent meetings and partings; so that you may guess from this what it is for the first-beginnings of things to be ever tossing about in the great void'; *ibid.*, **2**: 31).

and the variety and perpetual alteration of the motion of each atom might well confuse the [....]

[4] [THE STATISTICAL METHOD]

[When the working members of Section F[28] get hold of a report of the Census, or any other document containing the numerical][29] data of Economic or Social Science they obtain their results by distributing the whole population of a country into groups according to age, income tax, reading and writing, number of convictions and so forth. They do so because the number of individuals is far too great to allow of their tracing the history of each separately, so that in order to reduce their labour within human compass they concentrate their attention on artificial groups so that the varying numbers in each group and not the varying state of each individual is the primary datum from which they work. ⟨This may not be the way in which Shakespeare obtained his knowledge of human nature.⟩ This is of course not the only method of studying human nature.

We may observe the conduct of individual men and compare it with that conduct which their previous character, as judged according to the best attainable rules and maxims would lead us to expect. In this way we may correct and improve these rules and maxims just as an astronomer corrects the elements of a planet by comparing its actual position with that deduced from the received elements.

This method which we may call the astronomical or dynamical method is evidently of a higher order than the statistical method, but we can employ it only in the case of the limited number of individuals of whose history we have sufficient knowledge.

Now in our physical researches every portion of matter which we can subject to experiment consists of millions of millions of molecules not one of which ever becomes individually sensible to us. Hence the statistical method is the only available one.

What is the nature of our data. We can take a drop of water and weigh it. We thus obtain a measure of the sum of the masses of all the molecules it contains, though we may be ignorant of the mass of any one of them. Again by multiplying this mass by the apparent velocity of the drop we obtain its momentum and this is the resultant of the momenta of all the individual

(28) The section concerned with 'Economic Science and Statistics'; see the *Report of the Forty-third Meeting of the British Association for the Advancement of Science; held at Bradford in September 1873* (London, 1874), part 2: 174.

(29) Following the published text of 'Molecules': 440 (= *Scientific Papers*, 2: 373).

molecules though in point of fact these molecules may be moving in every direction within the drop.

[5][30] [THE STATISTICAL METHOD]

In passing from the consideration of the motions of individual molecules to that of the medium which consists of multitudes of moving molecules we are forced, on account of our limited powers of observation and even of imagination, to abandon the strict dynamical method of tracing the course of every molecule and to adopt the statistical method of dividing the molecules into groups according to some system, and then confining our attention to the number of molecules in each group. This is a step the philosophical importance of which cannot be overestimated. It is equivalent to the change from absolute certainty to high probability. The kinetic theorems by which the motion of a single molecule is expressed are, no doubt, founded on axioms absolutely certain but as soon as we loose sight of the individual molecule and assert anything of groups of molecules which are continually exchanging molecules one with another our assertions can lay claim to nothing more than a high probability.

In the first place let us form a group of molecules by confining our attention to those which at a given instant are within a given region bounded by a closed surface of any form but large enough to contain a very great number of molecules.

The mass of this group of molecules is the sum of the masses of the individual molecules which at the given instant are within the given region. The only way in which the mass of the group can change is by molecules entering or leaving the given region.

The numerical value of the density of the medium within this region is obtained by dividing the number representing the mass by the amount representing the volume of the region. The density of the matter at any mathematical point within the region is [...]. There is no point within the region at which the actual density of matter has this value for if the point is within a molecule the density is much greater and if it is not within a molecule the density is zero. The density we have obtained is therefore an average density and is our first example of a statistical quantity. If there are several kinds of molecules within the region forming a mixture of several media the mass and the density of each medium may be estimated separately.

(30) ULC Add. MSS 7655, V, f/11.

[6][31] [THE STATISTICAL METHOD]

It is easy to see that if encounters take place among a great number of molecules their velocities even if originally equal will become unequal for except under conditions which can be only rarely satisfied two molecules having equal velocities before their encounters will have unequal velocities after the encounter.

Every molecule changes both its velocity and its direction of motion at every encounter so that unless we are supposed to be able to calculate the elements of the motion of every other molecule which it encounters these changes of motion must appear to us very irregular if we follow the course of a single molecule.

As long as we have to deal with only two molecules and have all the data of the encounter given us, we can calculate the result of their mutual action, but when we have to deal with millions of molecules each of which has millions of encounters in a second, the complexity of the problem seems to shut out all hope of a legitimate solution.

We are therefore obliged to abandon the strictly kinetic method and to adopt the statistical method.

According to the strict kinetic or historical method as applied to the case before us we follow the whole course of every individual molecule. We arrange our symbols so as to be able to identify every molecule throughout its whole motion and the complete solution of the problem would enable us to determine at any given instant the position and motion of every given molecule from a knowledge of the positions and motions of all the molecules in their initial state.

According to the statistical method the state of the system at any instant is ascertained by distributing the molecules into groups, the definition of each group being founded on some variable property of the molecules. Each individual molecule is sometimes in one of these groups and sometimes in another but we make no attempt to follow it. We simply take account of the number of molecules which at a given instant belong to each group.

Thus we may consider as a group those molecules which at a given instant lie within a given region in space. Molecules may pass into or out of this

(31) ULC Add. MSS 7655, V, f/11. The first part of the MS draft here printed as §6 – up to and including the sentence on Laplace, and with the second paragraph omitted – was published in H. W. Watson's 'Introduction' to his *A Treatise on the Kinetic Theory of Gases* (Oxford, 1876): v–vii. Making only trivial modifications to Maxwell's text, Watson acknowledged the source as 'MS. notes by Professor Clerk Maxwell' (*Kinetic Theory of Gases*: vii footnote). See also Number 472 note (12).

region but we confine our attention to the increase or diminution of the number of molecules within it. In the same way the population of a watering-place considered as a mere number varies in the same way whether its visitors return to it season after season or whether the annual flock consists each year of fresh individuals.

We might also form our groups out of those molecules which at a given instant have velocities lying within given limits. When a molecule has an encounter and changes its velocity it passes out of one of these groups and enters another, but as other molecules are also changing their velocities the number of molecules in each group varies very little from a certain average value.

We thus meet with a new kind of regularity – the regularity of averages – a regularity which when we are dealing with millions of millions of individuals is so unvarying that we are almost in danger of confounding it with absolute uniformity.

Laplace in his theory of Probability[(32)] has given many examples of this kind of statistical regularity and has shown how this regularity is consistent with the utmost irregularity among the individual instances which are enumerated in making up the results. In the hands of Mr Buckle facts of the same kind were brought forward as instances of the unalterable character of natural laws.[(33)] But the stability of the averages of large numbers of variable events must be carefully distinguished from that absolute uniformity of sequence according to which we suppose that every individual event is determined by its antecedents.

For instance if a quantity of air is enclosed in a vessel and left to itself we may be ⟨perfectly⟩ ⌞morally⌟ certain that whenever we choose to examine it we shall find the pressure uniform in horizontal strata and greater below than above, that the temperature will be uniform throughout, and that there will be no sensible currents of air in the vessel.

But there is nothing inconsistent with the laws of motion in supposing that in a particular case a very different event might occur. For instance if at a given instant a certain number of the molecules should each of them encounter one of the remaining molecules and if in each case one of the molecules after the encounter should be moving vertically upwards and if in addition the molecules above then happened not to get into the way of these upward moving molecules, – the result would be a sort of explosion by which a mass of air would be projected upwards with the velocity of a cannon ball

(32) P. S. de Laplace, *Théorie Analytique des Probabilités* (Paris, 1812).

(33) Thomas Henry Buckle, *History of Civilization in England*, 2 vols. (London, 1857–61), **1**: 19–22.

while a larger mass would be blown downwards with an equivalent momentum. We are morally certain that such an event will not take place within the air of the vessel however long we leave it. What are the grounds of this certainty.

The explosion will certainly happen if certain conditions are satisfied. Each of these conditions by itself is not only possible but is in the common course of events as often satisfied as not. But as the number of conditions which must be satisfied at once is to be counted by millions of millions the improbability of the occurrence of all these conditions amounts to what we are unable to distinguish from an impossibility.

Nevertheless it is no more improbable that at a given instant the molecules should be arranged in one definite manner than in any other definite manner. We are as certain that the exact arrangement which the molecules have at the present instant will never again be repeated as that the arrangement which would bring about the explosion will never occur.

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POSTCARD TO PETER GUTHRIE TAIT

2 SEPTEMBER 1873

From the original in the University Library, Cambridge[1]

[Glenlair]

O T! 0 can be finer than T′′s bow. It is decidedly a long one. But no man in his senses would undertake to sanction a *whole affair* between 2 other men, in any other respect than where grammar &c occur.[2]

Beware of the Redding Strake.[3]

Observe in Les Mondes Aug 28 M. Belpaire on $\Theta.\Delta$ as reported on by that eminent savant M. Folie.[4] Also the hardness of solids calculated to 6 places of decimals and the effects of a moral discourse on a dogue.[5] If the observed values of $a+b$, $a+c$, $a+d$, $b+c$, $b+d$, $c+d$ are p, q, r, s, t, u, and if $f = p+u$ $g = q+t$ $h = r+s$ then the most probable value of $6a$ is

$2p+2q+2r-s-t-u$ and the (observed–probable) value

of p is $\frac{1}{6}(2f-g-h)$ = that of u

of q $\frac{1}{6}(2g-h-f)$ = that of t

r $\frac{1}{6}(2h-f-g)$ = that of s.

Whence follows the curious arrangement of errors in my diffusion calculations.[6]

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(1) ULC Add. MSS 7655, I, b/61.

(2) See Number 477 note (1).

(3) See Sir Walter Scott, *Guy Mannering*, chap. 27 (note): 'a blow received by a peacemaker who interferes betwixt two combatants, to red or separate them, is proverbially said to be the most dangerous blow a man can receive'.

(4) See a report in *Les Mondes*, **31** (28 August 1873): 721–2, on Th. Belpaire, 'Note sur le second principe de la thermodynamique', *Bulletins de l'Académie Royale des Sciences ... de Belgique*, **34** (1872): 509–26, and on comments on Belpaire's paper by François Folie, *ibid.*: 448–51.

(5) See the reports in *Les Mondes*, **31** (28 August 1873): 720, 729–33.

(6) See Number 471, and Tait's comment in his card of 31 July 1873 (Number 474 note (3)).

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BRITISH ASSOCIATION PAPER ON GEOMETRICAL OPTICS

[SEPTEMBER 1873]

From the *Report of the British Association for 1873*(1)

ON THE RELATION OF GEOMETRICAL OPTICS TO OTHER BRANCHES OF MATHEMATICS AND PHYSICS

The author said that the elementary part of optics was often set before the student in a form which was at once repulsive to the mathematician, unmeaning to the physical inquirer, and useless to the practical optician. The mathematician looked for precision, and found approximation; the physicist expected unity in the science, and found a great gulf between geometrical and physical optics; the optician found that if he had to design a microscope, he was expected to combine the analytical power of a Gauss with the computative skill of a Glaisher(2) before he could make head or tail of the formulæ. The author maintained that elementary optics might be made attractive to the mathematician by showing that the correlation between the object and the image is not only an example, but the fundamental type of that principle of duality which was the leading idea of modern geometry. The object and image were homographic figures, such that every straight line or ray in the one was represented by a straight line or ray in the other. The relations between pairs of figures of that kind formed an important part of the geometry of position, an excellent treatise on which had been brought out by M. Theodor Reye.(3) To the physicist he would exhibit the unity of the science, by adopting Hamilton's characteristic function as explained in his papers on systems of rays,(4) and using it in the most elementary form from the very beginning of the subject, leading at once to the undulatory theory of light.(5) At the same time the practical optician would learn what were the

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(1) *Report of the Forty-third Meeting of the British Association for the Advancement of Science; held at Bradford in September 1873* (London, 1874), part 2: 38–9.

(2) James Whitbread Lee Glaisher, Trinity 1867, second wrangler 1871, Fellow 1871 (Venn), who had written the 'Report of the Committee...on mathematical tables', in the *Report of the Forty-third Meeting of the British Association*: 1–175.

(3) Theodor Reye, *Die Geometrie der Lage*, 2 vols. (Leipzig, 1866–8). There is a copy in Maxwell's Library (Cavendish Laboratory). Maxwell is alluding to projective geometry.

(4) W. R. Hamilton, 'Theory of systems of rays', *Transactions of the Royal Irish Academy*, **15** (1827): 69–74; *ibid.*, **16** part 1 (1830): 2–62; *ibid.*, **16** part 2 (1831): 93–125; *ibid.*, **17** (1832): 1–44.

(5) See Number 482 notes (8) and (9).

cardinal points of an optical instrument, and would be able to determine them without taking the instrument to pieces. Helmholtz(6) and Listing(7) had pointed out the advantages of the method to the oculist; and Beck(8) had recently placed some of the elementary points in a clear light. Casorati(9) had also exemplified some of the advantages of the method of homographic figures in elementary optics; but though Gauss, the modern founder of that method,(10) and several others, had made honourable mention of the name of Roger Cotes,(11) and of that theorem(12) with respect to which Newton said that 'if Mr. Cotes had lived, we should have known something',(13) no one seemed to have suspected that it would form the meeting-point of all the three methods of treating the science of optics.

(6) Hermann Helmholtz, *Handbuch der physiologischen Optik* (Leipzig, 1867): 35–64.

(7) Johann Benedict Listing, 'Ueber einige merkwürdige Punkte in Linsen und Linsensystemen', *Ann. Phys.*, **129** (1866): 466–72.

(8) A. Beck, 'Die Fundamentaleigenschaften der Linsensysteme in geometrischer Darstellung', *Zeitschrift für Mathematik und Physik*, **18** (1873): 588–600.

(9) Felice Casorati, 'Ricerche e considerazioni sugli strumenti ottici', *Rendiconti dell' Instituto Lombardo di Scienze e Lettere*, ser. 2, **5** (1872): 179–92.

(10) Carl Friedrich Gauss, 'Dioptrische Untersuchungen', *Abhandlungen der Mathematischen Classe der Königlichen Gesellschaft der Wissenschaften zu Göttingen*, **1** (1843): 1–34.

(11) Gauss makes no mention of Cotes; but see Volume I: 334, esp. note (3).

(12) See Number 482 note (10).

(13) Maxwell here misremembers the context of Newton's (rather improbable) judgment on Cotes as reported by Robert Smith, Cotes' cousin. The circumstances are described by Joseph Edleston in his *Correspondence of Sir Isaac Newton and Professor Cotes* (London, 1851): lxxvii.

ON THE EFFECT OF GRAVITY ON THE TEMPERATURE OF A COLUMN OF GAS: REPLY TO FRANCIS GUTHRIE[1]

OCTOBER 1873[2]

From *Nature* (23 October 1873)[3]

ON THE EQUILIBRIUM OF TEMPERATURE OF A GASEOUS COLUMN SUBJECTED TO GRAVITY[4]

Since reading Principal Guthrie's first letter on this subject (vol. viii, p. 67),[5] I have thought of several ways of investigating the equilibrium of

(1) A letter from Francis Guthrie (see Number 457 note (1)), headed 'On the equilibrium of temperature of a gaseous column subject to gravity', was published in *Nature*, **8** (9 October 1873): 486. Guthrie stated his argument in the following form: 'From Mr. Clerk-Maxwell's reply to my note on this subject which appeared in your columns a short time since, it would appear that he does not profess so much fully to explain the difficulty suggested by me as to show that it is capable of explanation, referring your readers to his other works for further information. I would not, therefore, have troubled you further on the subject had it not occurred to me on reading Mr. Maxwell's letter that I could state the case in such a way as to render clearly apparent the grounds for taking different views on this point. / Let a vertical column of gas, subject to gravity and in a state of equilibrium as to pressure and temperature, be divided by a horizontal plane P into two parts, A above and B below. / In the time Δt let a mass M_1 of particles pass in their free course from A to B, and a mass M_2 from B to A. / Let the portion of A from which the particles composing M_1 proceed be called the upper stratum, and the corresponding part of B the lower stratum, then the following consequences may be deduced:– / 1. From the equilibrium of density $M_1 = M_2$. / 2. From the equilibrium of temperature the amounts of work in M_1 and M_2 while passing through P are equal. / 3. From the effect of gravity the work in M_1 while in A reckoning from the commencement of the free course of each particle composing M_1, is less than at P, while that in M_2 is greater. / 4. Whence it follows that of the two equal masses M_1 and M_2 in the upper and lower strata respectively M_1 contains less work than M_2. / 5. The work in M_1 while in the upper stratum reckoned as before, is the same as that of any other equal average mass in that stratum, and the same is the case also of M_2. / 6. The average amounts of work in equal masses in the two strata, and the consequent temperatures of the strata are unequal, the lower stratum having the higher temperature. / I suppose Mr. Maxwell would deny the truth of statement (5). I presume he would argue as follows:– / "Of all the particles in the lower stratum which in the time Δt have at the commencement of their free course a velocity and direction such as would take them through P, gravity in selecting those which compose M_2 excludes those whose velocities are insufficient to overcome the effects of their weights, while in forming M_1 particles of low velocities are selected (included?), which, but for the effects of gravity, would not have cut P in their free courses, consequently the particles in M_1 have an average velocity less than that of the upper stratum from which they come, while the particles of M_2 have a greater average velocity than that of the lower stratum, and consequently the inequality of the average velocity of the particles in the two strata cannot be inferred from the

temperature in a gas acted on by gravity. One of these is to investigate the condition of the column as to density when the temperature is constant, and to show that when this is fulfilled the column also fulfils the condition that there shall be no upward or downward transmission of energy; or, in fact, of any other function of the masses and velocities of the molecules. But a far more direct and general method was suggested to me by the investigation of Dr. Ludwig Boltzmann* on the final distribution of energy in a finite system of elastic bodies.(6) A sketch of this method as applied to the simpler case of a number of molecules so great that it may be treated as infinite, will be found on p. 535.(7) Principal Guthrie's second letter (vol. viii, p. 486)(8) is especially valuable as stating his case in the form of distinct propositions, every one of which, except the fifth, is incontrovertible. He has himself pointed out that it is here that we differ, and that this difference may ultimately be traced to a difference in our doctrine as to the distribution of velocity among the molecules in any given portion of the gas. He assumes, as Clausius, at least in

* Studien über das gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten. Von Dr. Ludwig Boltzmann. Sitzb. d. Akad. d. Wissensch, October 8, 1868 (Vienna).(6)

inequality of the average velocities of the particles composing M_1 and M_2 while in those strata." / This argument, therefore assumes the theory that in a given mass of uniform temperature there are particles moving with every velocity from nothing upwards to a certain limit, and mixed in certain proportions. That this is actually Mr. Maxwell's view I own I might have remembered, but I suppose I overlooked it from an impression in my own mind that the molecular motion was to be regarded as being of a planetary (or in the case of gases a cometary) nature. That in masses of the same temperature velocities were to be regarded as practically uniform, except in so far as affected by the distance of the particles apart, and that the so-called impacts of particles were more properly to be regarded as perihelion passages of bodies moving among each other in hyperbolic orbits. / If this view is the more accurate one, then obviously the argument which I have assumed that Mr. Maxwell would use, falls to the gound. / Is there no possibility of testing the nature of the thermal equilibrium of a column of still air? The result would at any rate throw an unexpected light on the nature of molecular motion.'

(2) See notes (1) and (3).

(3) *Nature*, **8** (23 October 1873): 527–8; printed in the *Report of the Forty-third Meeting of the British Association for the Advancement of Science; held at Bradford in September 1873* (London, 1874), part 2: 29–30, where it is printed as a preliminary to the paper cited in note (7).

(4) The title under which Maxwell's letter to *Nature* was published.

(5) Number 457 note (1).

(6) See Number 472 note (12).

(7) Read: 537. The paper is titled 'On the final state of a system of molecules subject to forces of any kind', *Nature*, **8** (1873): 537–8, and was printed in the *Report of the Forty-third Meeting of the British Association*, part 2: 30–2 (= *Scientific Papers*, **2**: 351–4). See Numbers 472 and 473.

(8) See note (1).

his earlier investigations, did, that the velocities of all the molecules are equal,[9] whereas I hold, as I first stated in the *Phil. Mag.* for Jan. 1860, that they are distributed according to the same law as errors of observation are distributed according to the received theory of such errors.[10]

It is easy to show that if the velocities are all equal at any instant they will become unequal as soon as encounters of any kind, whether collisions or 'perihelion passages'[11] take place. The demonstration of the actual law of distribution was given by me in an improved form in my paper on the Dynamical Theory of Gases, 'Phil. Trans.' 1866, and *Phil. Mag.* 1867,[12] and the far more elaborate investigation of Boltzmann has led him to the same result. I am greatly indebted to Boltzmann for the method used in the latter part of the sketch of the general investigation (see p. 535)[13] which was communicated in a condensed form to the British Association on Sept. 20, 1873.[14]

J. Clerk Maxwell[15]

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(9) R. Clausius, 'Ueber die Art der Bewegung, welche wir Wärme nennen', *Ann. Phys.*, **100** (1857): 353–80; Clausius, 'Ueber die mittlere Länge der Wege...', *ibid.*, **105** (1858): 239–58.

(10) J. C. Maxwell, 'Illustrations of the dynamical theory of gases. Part I. On the motions and collisions of perfectly elastic spheres', *Phil. Mag.*, ser. 4, **19** (1860): 19–32 (= *Scientific Papers*, **1**: 377–91).

(11) Guthrie's phrase: see note (1).

(12) *Phil. Trans.*, **157** (1867): 49–88 (= *Scientific Papers*, **2**: 26–78); and *Phil. Mag.*, ser. 4, **35** (1868): 129–45, 185–217.

(13) See note (7).

(14) See note (7).

(15) Guthrie responded in a letter dated 7 February 1874, published in *Nature*, **10** (18 June 1874): 123, with Maxwell's rejoinder (to be published in Volume III).

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LETTER TO JOHN WILLIAM STRUTT, LORD RAYLEIGH

22 NOVEMBER 1873

From the original in private possession(1)

11 Scroope Terrace
Cambridge
22 Nov 1873

Dear Lord Rayleigh

Your letter of Nov 17 quite accounts for the observed transparency of any gas.(2) With respect to the composition of vibrations from sources irregularly distributed if each vibration is absolutely independent of the others as to phase, then at a distance so great that the ratio of the distances of different sources may be neglected, the energy of radiation is the sum of the energies due to each source separately.

But the case is different with respect to the undulations excited by a system of plane waves when we consider the propagation of the secondary waves in the same direction as the primary wave. For then the time at which a secondary wave reaches a given point will differ only by a certain fraction of a wave-period from the time at which the primary waves reaches the same point because both waves travel at the same rate except close to the source of the secondary wave.

Hence the theory of the direct ray requires special consideration.

But in any case its intensity is diminished only by the energy of the scattered radiation together with that employed in heating the medium.

Your theorems on vibrations are first rate. Will you send me a separate copy if you can spare one (I have that in the Math Societys Trans).(3)

I made use of your dissipation function(4) in lecture today in proving the existence of a system of conjugate harmònic solutions in every problem on the

(1) Rayleigh Papers, Terling Place, Terling, Essex.

(2) See Maxwell's letter to Rayleigh of 28 August 1873 (Number 476). In his paper 'On the transmission of light through an atmosphere containing small particles in suspension, and on the origin of the blue of the sky', *Phil. Mag.*, ser. 5, **47** (1899): 375–84, esp. 376n, Rayleigh observed: 'So far as I remember, my argument was of a general character only.'

(3) J. W. Strutt (Lord Rayleigh), 'Some general theorems relating to vibrations', *Proceedings of the London Mathematical Society*, **4** (1873): 357–68; see Maxwell's referee's report (Number 460). See also Lord Rayleigh, 'On the vibrations of approximately simple systems', *Phil. Mag.*, ser. 4, **46** (1873): 357–61; and 'On the fundamental modes of a vibrating system', *ibid.*: 434–9.

(4) Strutt, 'Some general theorems relating to vibrations': 364; and see Numbers 460, esp. notes (11) and (12), and 461.

conduction of heat (or of electricity when electromagnetic induction may be neglected) and that any given initial state of the system may be expressed as a sum of a set of harmonic solutions.(5)

(5) George Howard Darwin recorded Maxwell's argument in notes on his lecture of 22 November 1873; these form part of Darwin's notes on 'Maxwell's lectures/Oct term 1873' (ULC DAR. 210. 22). Darwin jotted pencilled notes at the lecture, subsequently elaborating them in a written-out expansion on the facing pages of his notebook. This latter version is reproduced here. '*Nov 22* / There is an analogy between electricity & heat which may be utilised for working out & understanding the problems in both sciences. / Electricity at high pot^{l} is worth more for the perform^{ce} of mechan. work & heat at high temp. has the same properties. / The pot^{l} of a body has a simple expression, but the temp. is not so simple & ∴ in this respect heat is more complic^{d} than el^{y}. / Fourier's method consists in the cutting of the body into several parts, such that the heating and cooling of these parts may be considered sep^{y} fr. one a$\overline{\text{nor}}$. The capac. of a body for heat is the qu^{y} of heat requ^{d} to raise its temp 1°. That for elec^{y} is the qu^{y} of elec^{y} required to raise its pot^{l} by unity. This latter depends on the pot^{l} of all the surrounding bodies, hence in this respect the electrical problem is more complicated. / If the pot^{l} of all the bodies of a system be given – the electrostat. energy of the system is a homog^{s} quadr. f^{n} of the pot^{ls} of the sev^{l} parts. Since in heat each body is indep^{t} of all the others, in heat this expression consists only of squares. When products appear in expressions of this nature it shows that the sev^{l} parts react on one a$\overline{\text{nor}}$. The arrangement of the solution so that there shall be no products is equiv^{t} to finding the geom^{l} solution of the diff^{l} eq^{ns}, since the gen^{l} solution (in linear eq^{ns}?) is the sum of the part$\overline{\text{ar}}$ solutions each × plied by a const – i e we have to find a no. of indep^{t} solutions wh. do not react on one a$\overline{\text{nor}}$. / The energy of a system may be repres^{ted} as / $V = \frac{1}{2}\sum_r \sum_s a_{sr} p_s p_r$ – there are n terms if there are 1, 2, 3 … r … s … n bodies p is the electrical pot^{l} & a_{sr} is a const. In elect^{y} $a_{sr} = a_{rs}$. Since in heat there are no products $s = r$ & there are only n terms $\frac{1}{2}\sum_r a_r p_r^2$ – but the expression no longer means the energy of the system. / If there be a conductor (of no capacity in the case of heat i e $\frac{\text{width}}{\text{length}}$ v. small) joining the two bodies at pot^{ls} (or temp^{s}) p_r & p_s. Then $p_r - p_s$ is the electromot. force (or diff. of temp^{s}) & $K_{rs}(p_r - p_s)$ is the flow of the elec^{y} (or heat). / The work done by the electr^{y} is the flow × diff. of pot^{ls}. / But in heat flow × diff. of temp^{s} does not repres^{t} the work done. The work done is some func. of this expression. / There is a complete analogy betw. this & the flow of fluid betw. 2 vessels, where flow × diff. of press^{s} is the work done by the fluid in passing from one vessel to the $\overline{\text{or}}$. Thus the work done by the elec. in flowing from the body r to $s = K_{rs}(p_r - p_s)^2$. / We may suppose that conductors are arranged betw. every body two & two – some of these cond^{rs} might however be suppressed without disturbing the condition (why). / Then $F = \sum_r \sum_s K_{rs}(p_r - p_s)^2$. / In the case of heat we may sum for all values of r & s since all terms for diff^{t} values of r & s go out – (how? is it from the ppties of Laplace's coeff^{ts}). / The presence of electrokinetic energy (i e induced currents) is supposed excluded; hence we must have our conductors well apart – bad short & narrow conductors. / In the case of stat. el^{y} the presence of induced currents is imperceptible. We must suppose the connections betw. the sev^{l} bodies to be made with cotton thread & not with wires (why?) – tho' a needle won't be deflected by the flow in a thread. / It is possible to find a new set of variables to replace the p's such that all the terms with products disappear (i.e. refer to ppal axes in case of surfs of 2nd order). $q = \sum(ap)$ where a is const. Then we sh^{d} get / $V = \frac{1}{2}\sum(B_u q_u^2)$ or the energy of the system /

Do you know any pure mathematical proof that any two homogeneous quadratic functions which cannot become negative can be expressed in the form of the sums of squares by one, and only one linear transformation of the variables?

When is your book on Acoustics coming out? I am afraid that every effort you make towards finishing it will only turn up fresh materials which it would be wrong not to insert.

I am getting more light on Geometrical Optics. The geometry of the subject is the geometry of position, the theory of perspective and of homographic figures apart from all considerations of magnitude.[6]

The calculation of the subject is all founded on Hamiltons characteristic function[7] which, for an instrument symmetrical about an axis, has the form[8]

$$V = V_0 + \mu_1 z_1 + \mu_2 z_2 + \frac{1}{2}\frac{\mu_1(z_2-\alpha_2)(x_1^2+\eta_1^2)+\mu_2(z_1-\alpha_1)(x_2^2+\eta_2^2)-2\phi\mu_1\mu_2(x_1x_2+\eta_1\eta_2)}{(z_1-\alpha_1)(z_2-\alpha_2)-\phi^2\mu_1\mu_2}$$

where $z_1 = \alpha_1$ gives the first principal focus

$z_2 = \alpha_2$ . . . . second . . . .

$f_1 = \mu_1\phi$ first principal focal length

$f_2 = \mu_2\phi$ second . . . .[9]

$F = \frac{1}{2}\sum(C_u q_u^2)$ or the rate of dissipation. / Then clearly $F = -\frac{dV}{dt}$ / $\therefore \frac{dF}{dq_u} = \frac{d^2V}{dq_u\,dt}$ / $\therefore C_u \dot{q}_u = -B_u\frac{dq_u}{dt}$ $\therefore q_u = Q_u e^{(C_u/B_u)t}$. / ($B_u$ will not be a f$^{\text{n}}$ of time if we suppose the theory stated right tho' I missed the argum$^{\text{t}}$ on this part.) / ($\frac{C_u}{B_u}$ is called the modulus. In the case of vibr$\overline{\text{ons}}$ we sh$^{\text{d}}$ have $C_q = -\frac{dq^2}{dt^2}$. If the vibr$\overline{\text{ons}}$ are of the decaying type it w$^{\text{d}}$ be $\frac{dq^2}{dt^2}+A\frac{dq}{dt}+Bq = 0$.) / Thus the actual state can be analysed into a number of independent states each of wh. decays indeptly according to the above law. This may be done in only one way – & might be called a harmonic distribution. The various terms may fitly be called conjugate. / $F = \sum_r\sum_s K_{rs}(p_r-p_s)^2 = K_{rs}(p_r-p_s)(p_r-p_s)$ / $= \sum\sum$ flow × electromot force. / ↙ I don't understand but it is an analogy with Lapl. fns.'

(6) See Number 480.

(7) See Number 480 note (4).

(8) See also Maxwell's paper 'On the relation of geometrical optics to other parts of mathematics and physics', *Proc. Camb. Phil. Soc.*, **2** (1874): 338–40 (= *Scientific Papers*, **2**: 391–2).

(9) For further discussion see Maxwell's paper 'On Hamilton's characteristic function for a narrow beam of light', *Proceedings of the London Mathematical Society*, **6** (1875): 182–90, esp. 189–90 (= *Scientific Papers*, **2**: 389–90).

The denominator divided by f_1 or f_2 is Cotes' or rather Smith's 'apparent distance'(10) or $\dfrac{\text{real}}{\text{angular}}$ diameter. When it is zero, z_1 & z_2 are 'conjugate' foci.

For a telescope the fraction is

$$\frac{\mu_1^2 x_1^2 + m^2 \mu_2^2 x_2^2 + 2m\mu_1 \mu_2 x_1 x_2}{(z_1-\alpha_1)\,\mu_1 + (z_2-\alpha_2)\,m^2\mu_2}$$

where m is the angular magnifying power.(11)

In all cases z_1 & z_2 are measured *from* the instrument.

Figure 482,1

I have got the most general form of the fraction with ten arbitrary constants with a geometrical method of finding the focal lines of the emergent pencil when those of the incident pencil are given but except in the case of pencils passing through a spectroscope they are too complicated.(12)

The Cavendish Laboratory will be evacuated by the contractors before Christmas. The lecture room is in action already.

Yours very truly
J. Clerk Maxwell

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(10) On Cotes' theorem, and 'apparent distance', see Robert Smith, *A Compleat System of Optics*, 2 vols. (Cambridge, 1738), 2: ('Remarks') 76–8; and see Volume I: 391n. For Maxwell's further discussion see his papers 'On the relation of geometrical optics to other parts of mathematics and physics': 339–40, and 'On Hamilton's characteristic function': 190 (= *Scientific Papers*, 2: 392, 390).

(11) See also Maxwell, 'On Hamilton's characteristic function': 190 (= *Scientific Papers*, 2: 390).

(12) As discussed in his paper 'On Hamilton's characteristic function'.

LETTER TO PETER GUTHRIE TAIT

1 DECEMBER 1873

From the original in the University Library, Cambridge(1)

Natural Sciences Tripos(2) 1 Dec 1873

O. T. For the flow of a liquid in a tube,(3) axis z

$$\mu\left(\frac{d^2w}{dx^2}+\frac{d^2w}{dy^2}\right)=\frac{dp}{dz}.^{(4)} \tag{1}$$

Surface condition $$\mu\frac{dw}{d\nu}=\lambda w^{(5)} \tag{2}$$

where ν is the normal drawn towards the liquid.(6)

When the curvature is small, (2) is equivalent to supposing the walls removed back by $\frac{\mu}{\lambda}$ and then λ made ∞ or $w=0$. For glass & water by Helmholtz & Pietrowski $\frac{\mu}{\lambda}=0$.(7)

If so, and if the value of w is $C\left(1-\frac{x^2}{a^2}-\frac{y^2}{b^2}\right)$, $2\mu C\left(\frac{1}{a^2}+\frac{1}{b^2}\right)+\frac{dp}{dz}=0$ which gives C. If not, you may write

$$w=A+Br^2+C_2r^2\cos 2\phi+C_4r^4\cos 4\phi+\&c^{(8)}$$

where $x=ar\cos\theta$ and $y=br\sin\theta$ and then $2\mu B\left(\frac{1}{a^2}+\frac{1}{b^2}\right)+\frac{dp}{dz}=0$ and you satisfy (2) the best way you can when $r=1$.

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(1) ULC Add. MSS 7655, I, b/62. Previously published (in part) in Knott, *Life of Tait*: 114–16.

(2) See Number 488.

(3) See P. G. Tait, 'On the flow of water through fine tubes', *Proc. Roy. Soc. Edinb.*, **8** (1873): 208–9, read 1 December 1873.

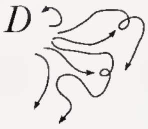
(4) Following Hermann Helmholtz and G. von Piotrowski, 'Über Reibung tropfbarer Flüssigkeiten', *Wiener Berichte*, **40**, Abtheilung II (1860): 607–58, on 652. Here μ is the coefficient of viscosity, w the component of velocity along the axis of the tube z, and p the pressure.

(5) Following Helmholtz and Piotrowski, 'Über Reibung tropfbarer Flüssigkeiten': 653, where λ is the 'Gleitungscoefficient', the coefficient of slipping of the liquid at the surface of the tube.

(6) Compare a similar discussion by Horace Lamb, *A Treatise on the Mathematical Theory of the Motion of Fluids* (Cambridge, 1879): 223–4.

(7) Helmholtz and Piotrowski, 'Über Reibung tropfbarer Flüssigkeiten': 651.

(8) A, B and C are constants; and using polar coordinates $r^2=x^2+y^2$; see Helmholtz and Piotrowski, 'Über Reibung tropfbarer Flüssigkeiten': 652.

As to Ampère[9] – of course you may lay on d_1 (anything) when d_1 is with respect to the element of a circuit. Have you studied H^2 on the potential of two elements?[10] or Bertrand who, with original bosh of his own,[11] rushes against the thicker bosches of H^2's buckler?[12] and says that H^2 believes in a force which does not diminish with the distance[13] so that the reason why Ampère or H^2 or Bertrand observe peculiar effects is because some philosopher in α Centauri happens to be completing a circuit $XQq\,D$ [14] as I am surrounded by Naturals and cannot give references.

In introducing 4^{ions},[15] do so by blast of trumpet & tuck of drum. Why should $V.\alpha\beta\gamma$[16] come in sneaking without having his style & titles proclaimed by a fugleman. Why, even . should be treated with due respect and we should be informed whether he is attractive or repulsive.

What do you think of 'Space-variation' as the name of Nabla?[17]

It is only lately, under the conduct of Professor Willard Gibbs[18] that I have been led to recant an error which I had imbibed from your $\Theta\Delta^{\text{cs}}$[19]

(9) See Tait's 'Note on the various possible expressions for the force exerted by an element of one linear conductor on an element of another', *Proc. Roy. Soc. Edinb.*, **8** (1873): 220–8, read 1 December 1873. On Ampère's formula for the force law between two infinitesimal line elements carrying currents see Number 430 note (5).

(10) Hermann Helmholtz, 'Ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper', *Journal für die reine und angewandte Mathematik*, **72** (1870): 57–128, on 76.

(11) Joseph Bertrand, 'Examen de la loi proposée par M. Helmholtz pour représenter l'action de deux éléments de courant', *Comptes Rendus*, **77** (1873): 1049–54.

(12) Hermann Helmholtz, 'Vergleich des Ampère'schen und Neumann'schen Gesetzes für die elektrodynamischen Kräfte', *Monatsberichte der Königlich Preuss. Akademie der Wissenschaften zu Berlin* (1873): 91–104.

(13) Bertrand, 'Examen de la loi proposée par M. Helmholtz': 1054.

(14) Read: excuse details.

(15) P. Kelland and P. G. Tait, *Introduction to Quaternions, with numerous examples* (London, 1873); see Number 485.

(16) The vector of the product of three vectors; see Kelland and Tait, *Introduction to Quaternions*: 156.

(17) See Number 348 esp. note (3).

(18) Josiah Willard Gibbs, 'Graphical methods in the thermodynamics of fluids', *Transactions of the Connecticut Academy of Arts and Sciences*, **2** (1873): 309–42, esp. 310n; 'The term *entropy*, it will be observed, is here used in accordance with the original suggestion of Clausius, and not in the sense in which it has been employed by Professor Tait and others after his suggestion. The same quantity has been called by Professor Rankine the *Thermo-dynamic function*.'

(19) See P. G. Tait, *Sketch of Thermodynamics* (Edinburgh, 1868): 100, 29; 'It is very desirable to have a word to express the *Availability* for work of the heat in a given magazine; a term for that possession, the waste of which is called the *Dissipation*. Unfortunately the excellent word *Entropy*, which Clausius has introduced in this connexion, is applied by him to the negative of the idea we most naturally wish to express.... [Thus] we shall...use the excellent term Entropy in the

namely that the entropy of Clausius is *unavailable energy* while that of T′ is available energy.[20] The entropy of Clausius is neither the one nor the other it is only Rankines Thermodynamic function[21] and if we compare the vocabulary

Thermodynamic Function	Entropy (Clausius)[22]
Entropy (Tait)	Available Energy

I think we shall prefer the 2nd column. Available Energy there is none in a system of uniform temperature and pressure.

I have also great respect for the elder of those celebrated acrobats, Virial and Ergal,[23] the Bounding Brothers of Bonn.[24] Virial came out in my paper on Frames R.S.E. 1870[25] under the form $\sum Rr = 0$[26] where there is no motion. Where there is motion the time-average of $\frac{1}{2}\sum Rr$ = time-average of $\frac{1}{2}\sum Mv^2$ where R is positive for attraction.[27]

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opposite sense to that in which Clausius has employed it, – viz., so that the *Entropy of the Universe tends to Zero*, which is Thomson's theory of dissipation [of available energy]'. See also William Thomson, 'On a universal tendency in nature to the dissipation of mechanical energy', *Proc. Roy. Soc. Edinb.*, **3** (1852): 139–42 (= *Math. & Phys. Papers*, **1**: 511–14).

(20) J. Clerk Maxwell, *Theory of Heat* (London, 1871): 186; 'Clausius has called the remainder of the energy, which cannot be converted into work, the Entropy of the System. We shall find it more convenient to adopt the suggestion of Professor Tait, and give the name of Entropy to the part which can be converted into mechanical work.'

(21) W. J. M. Rankine, 'On the geometrical representation of the expansive action of heat, and the theory of thermo-dynamic engines', *Phil. Trans.*, **144** (1854): 115–75. In his *Sketch of Thermodynamics*: 29, Tait had himself noted that the 'Aequivalenzwerth of Clausius is nearly identical with the Thermodynamic Function of Rankine'. In his paper 'Ueber eine veränderte Form des zweiten Hauptsatzes der mechanischen Wärmetheorie', *Ann. Phys.*, **93** (1854): 481–506, on 494, Clausius had introduced the concept of 'Aequivalenzwerth' for the quantity he subsequently termed 'Entropie'.

(22) R. Clausius, 'Ueber verschiedene für die Anwendung bequeme Formen der Hauptgleichungen der mechanischen Wärmetheorie', *Ann. Phys.*, **125** (1865): 353–400, esp. 390, 400. He used this term 'nach dem griechischen Worte *ἡ τροπή*, die Verwandlung, die *Entropie* des Körpers zu nennen'; hence '1) Die Energie der Welt ist constant. 2) Die Entropie der Welt strebt einem Maximum zu'.

(23) Clausius' concepts of 'virial' and 'ergal'; see his paper 'Ueber einen auf die Wärme anwendbaren mechanischen Satz', *Ann. Phys.*, **141** (1870): 124–30. See Number 356, esp. note (3).

(24) Clausius had been appointed director of the Bonn physics institute in 1869; see C. Jungnickel and R. McCormmach, *Intellectual Mastery of Nature. Theoretical Physics from Ohm to Einstein*, 2 vols. (Chicago/London, 1986), **2**: 79–82.

(25) J. Clerk Maxwell, 'On reciprocal figures, frames and diagrams of forces', *Trans. Roy. Soc. Edinb.*, **26** (1870): 1–40, esp. 13 (= *Scientific Papers*, **2**: 175–6). See Number 356 note (4).

(26) R is the force between two mass points at a distance r.

(27) See Number 356 note (3).

But it is rare sport to see those learned Germans contending for the priority of the discovery that the 2nd law of $\Theta\Delta^{cs}$ is the Hamiltonsche Princip,(28) when all the while they *assume* that the temperature of a body is but another name for the vis viva of one of its molecules, a thing which was suggested by the labours of Gay Lussac Dulong &c but first deduced from dynamical statistical considerations by $\frac{dp}{dt}$.(29) The Hamiltonsche Princip,(30) the while, soars along in a region unvexed by statistical considerations while the German Icari flap their waxen wings in nephelococcygia,(31) amid those cloudy forms which the ignorance and finitude of human science have invested with the incommunicable attributes of the invisible Queen of heaven.

Dictum of K & T′ concerning 3 points, p 160.(32)
If these perps intersect in G, the three points A, B, G will be in one plane.

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(28) This comment was probably prompted by publication in the *Philosophical Magazine* of a paper by C. Szily, 'On Hamilton's dynamic principle in thermodynamics', *Phil. Mag.*, ser. 4, **46** (1873): 426–34, esp. 434; 'What in thermodynamics we call the second proposition, is in dynamics no other than Hamilton's principle, the identical principle which has already found manifold application in several branches of mathematical physics.' This paper was translated from Szily's 'Das dynamische Princip von Hamilton in der Thermodynamik', *Ann. Phys.*, **146** (1872): 74–86. This was Szily's second intervention in the controversy between Boltzmann and Clausius over the reduction of the second law of thermodynamics to mechanical principles: see Szily, 'Das Hamilton'sche Princip und der zweite Hauptsatz der mechanischen Wärmetheorie', *Ann. Phys.*, **145** (1872): 295–302. Though perhaps prompted by Szily's paper, Maxwell's comment bears generally on the priority controversy between Boltzmann and Clausius over the reduction of the second law of thermodynamics to a mechanical theorem. See Ludwig Boltzmann, 'Über die mechanische Bedeutung des zweiten Hauptsatzes der Wärmetheorie', *Wiener Berichte*, **53**, Abtheilung II (1866): 195–220; Rudolf Clausius, 'Ueber die Zurückführung des zweiten Hauptsatzes der mechanischen Wärmetheorie auf allgemeine mechanische Principien', *Ann. Phys.*, **142** (1871): 433–61; Boltzmann, 'Zur Priorität der Auffindung der Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und dem Principe der kleinsten Wirkung', *Ann. Phys.*, **143** (1871): 211–30; Clausius, 'Bemerkungen zu der Prioritätsreclamation des Hrn. Boltzmann', *Ann. Phys.*, **144** (1871): 265–74; and Clausius, 'Ueber den Zusammenhang des zweiten Hauptsatzes der mechanischen Wärmetheorie mit dem Hamilton'schen Princip', *Ann. Phys.*, **146** (1872): 585–91 (a response to Szily's first paper).

(29) See Number 377 para. 11 and notes (16), (17) and (18) for further discussion.

(30) There is a treatment of Hamilton's principle of 'varying action' in Thomson and Tait, *Natural Philosophy*: 231–41, to which Szily made reference in his paper 'On Hamilton's dynamic principle in thermodynamics': 428.

(31) Aristophanes, *The Birds*, line 817; 'Νεφελοκοκκυγιαν', rendered as 'Cuckoocloudland' in *The Birds of Aristophanes*, (trans.) H. F. Cary (London, 1824): 76.

(32) See Kelland and Tait, *Introduction to Quaternions*: 160; 'To find the condition that the perpendiculars from the angles of a tetrahedron on the opposite faces shall intersect one another... the condition that all three perpendiculars shall meet in a point is that the sum of all the squares of each pair of opposite edges shall be the same.'

Inference $\left(\text{by a new logical formula invented by } \frac{dp}{dt}\right)$. 'All planes pass through O'.

General Exercise. Interpret every 4ion expression in literary geometrical language, e.g. express in neat set terms the result of $\frac{\beta}{\alpha}.\gamma.$[33]

$\frac{dp}{dt}$

(33) β/α is a 'quotient or fraction', where α and β are unit vectors.

FROM A LETTER TO WILLIAM GRYLLS ADAMS[1]

3 DECEMBER 1873

From Campbell and Garnett, *Life of Maxwell*[2]

Natural Science Tripos
[Cambridge]
3 December 1873

I got Professor Guthrie's[3] circular some time ago.[4] I do not approve of the plan of a physical society considered as an instrument for the improvement of natural knowledge. If it is to publish papers on physical subjects which would not find their place in the transactions of existing societies, or in scientific journals, I think the progress towards dissolution will be very rapid. But if there is sufficient liveliness and leisure among persons interested in experiments to maintain a series of stated meetings to show experiments, and talk about them as some of the Ray Club[5] do here, then I wish them all joy; only the manners and customs of London, and the distances at which people live from any convenient centre, are very much against the vitality of such sociability.

To make the meeting a dinner supplies that solid ground to which the formers of societies must trust if they would build for aye. A dinner has the advantage over mere scientific communications, that it can always be had when certain conditions are satisfied, and that no one can doubt its existence. On the other hand, it completely excludes any scientific matter which cannot be expressed in the form of conversation with your two chance neighbours, or else by a formal speech on your legs; and during its whole continuance it reduces the Society to the form of a closed curve, the elements of which are incapable of changing their relative position.

For the evolution of science by societies the main requisite is the perfect freedom of communication between each member and any one of the others who may act as a reagent.

The gaseous condition is exemplified in the soiree, where the members rush about confusedly, and the only communication is during a collision, which in some instances may be prolonged by button-holing.

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(1) See Number 256 note (4).   (2) *Life of Maxwell*: 384–5.

(3) Frederick Guthrie: see Number 442 note (1).

(4) See J. H. Gladstone's presidential report to the Physical Society of London, read 13 February 1875, recording that a 'circular which Prof. Guthrie addressed in 1873 to the leading physicists elicited a gratifying number of replies'; see *Proceedings of the Physical Society of London*, **1** (1874–5).   (5) See Volume I: 314n.

The opposite condition, the crystalline, is shown in the lecture, where the members sit in rows, while science flows in an uninterrupted stream from a source which we take as the origin. This is radiation of science.

Conduction takes place along the series of members seated round a dinner table, and fixed there for several hours, with flowers in the middle to prevent any cross currents.

The condition most favourable to life is an intermediate plastic or colloïdal condition, where the order of business is (1) Greetings and confused talk; (2) A short communication from one who has something to say and to show; (3) Remarks on the communication addressed to the Chair, introducing matters irrelevant to the communication but interesting to the members; (4) This lets each member see who is interested in his special hobby, and who is likely to help him; and leads to (5) Confused conversation and examination of objects on the table.

I have not indicated how this programme is to be combined with eating. It is more easily carried out in a small town than in London, and more easily in Faraday's young days (see his life by B. Jones)[(6)] than now. It might answer in some London district where there happen to be several clubbable senior men who could attract the juniors from a distance.

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(6) Henry Bence Jones, *The Life and Letters of Faraday*, 2 vols. (London, 1870).

ON QUATERNIONS

DECEMBER 1873[(1)]

From *Nature* (25 December 1873)[(2)]

QUATERNIONS

A mathematician is one who endeavours to secure the greatest possible consistency in his thoughts and statements, by guiding the process of his reasoning into those well-worn tracks by which we pass from one relation among quantities to an equivalent relation. He who has kept his mind always in those paths which have never led him or anyone else to an inconsistent result, and has traversed them so often that the act of passage has become rather automatic than voluntary, is, and knows himself to be, an accomplished mathematician. The very important part played by calculation in modern mathematics and physics has led to the development of the popular idea of a mathematician as a calculator, far more expert, indeed, than any banker's clerk, but of course immeasurably inferior, both in resources and in accuracy, to what the 'analytical engine' will be, if the late Mr. Babbage's design should ever be carried into execution.

But although much of the routine work of a mathematician is calculation, his proper work – that which constitutes him a mathematician – is the invention of methods. He is always inventing methods, some of them of no great value except for some purpose of his own; others, which shorten the labour of calculation, are eagerly adopted by all calculators. But the methods on which the mathematician is content to hang his reputation are generally those which he fancies will save him and all who come after him the labour of thinking about what has cost himself so much thought.

Now Quaternions, or the doctrine of Vectors, is a mathematical method, but it is a method of thinking, and not, at least for the present generation, a method of saving thought. It does not, like some more popular mathematical methods, encourage the hope that mathematicians may give their minds a holiday, by transferring all their work to their pens. It calls upon us at every step to form a mental image of the geometrical features represented by the symbols, so that in studying geometry by this method we have our minds

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(1) See the references to Kelland and Tait's *Introduction to Quaternions* (London, 1873) in his letter to Tait of 1 December 1873 (Number 483).

(2) Published (unsigned) in *Nature*, **9** (1873): 137–8. The review can however be confidently attributed to Maxwell's authorship: the *Nature* archives confirm this attribution.

engaged with geometrical ideas, and are not permitted to fancy ourselves geometers when we are only arithmeticians.

This demand for thought – for the continued construction of mental representations – is enough to account for the slow progress of the method among adult mathematicians. Two courses, however, are open to the cultivators of Quaternions: they may show how easily the principles of the method are acquired by those whose minds are still fresh, and in so doing they may prepare the way for the triumph of Quaternions in the next generation; or they may apply the method to those problems which the science of the day presents to us, and show how easily it arrives at those solutions which have been already expressed in ordinary mathematical language, and how it brings within our reach other problems, which the ordinary methods have hitherto abstained from attacking.

Sir W. R. Hamilton, when treating of the elements of the subject, was apt to become so fascinated by the metaphysical aspects of the method,[(3)] that the mind of his disciple became impressed with the profundity, rather than the simplicity of his doctrines. Professors Kelland and Tait in the opening chapter (II.)[4] of their recently published work* have, we think, successfully avoided this element of discouragement. They tell us at once what a vector is, and how to add vectors, and they do this in a way which is quite as intelligible to those who are just beginning to learn geometry as to the most expert mathematician.

The subject, like all other subjects, becomes more intricate as the student advances in it; but at the same time his ideas are becoming clearer and more firmly established as he works out the numerous examples and exercises which are placed before him.

The technical terms of the method – Scalar, Vector, Tensor, Versor[(5)] – are introduced in their proper places, and their meaning is sufficiently illustrated to the beginner by the examples which he is expected to work out.

* 'Introduction to Quaternions, with numerous Examples.' By P. Kelland, F.R.S., formerly Fellow of Queen's College, Cambridge; and P. G. Tait, formerly Fellow of St. Peter's College, Cambridge; Professors in the Department of Mathematics in the University of Edinburgh. (Macmillan, 1873).

(3) In the 'Preface' to his *Lectures on Quaternions* (Dublin, 1853): 1–64, esp. 1–3, Hamilton describes 'Algebra as the SCIENCE OF PURE TIME', referring to his paper 'Theory of conjugate functions, or algebraic couples; with a preliminary and elementary essay on algebra as the science of pure time', *Transactions of the Royal Irish Academy*, **17** (1837): 293–422, esp. 293–7. In his *Lectures on Quaternions*: 2–3n he makes explicit reference to 'passages in Kant's Criticism of the Pure Reason, which appeared to justify the expectation that it should be *possible* to construct, *à priori*, a Science of Time, as well as a Science of Space'.

(4) Chapter II, 'Vector addition and subtraction', in Kelland and Tait, *Introduction to Quaternions*: 6–31.

(5) See Number 353 note (9).

The pride of the accomplished mathematician, however (for whom this book is not written), might have been somewhat mollified if somewhere in the book a few pages had been devoted to explaining to him the differences between the Quaternion methods and those which he has spent his life in mastering, and of which he has now become the slave. He is apt to be startled by finding that when one vector is multiplied into another at right angles to it, the product is still a vector, but at right angles to both. His only idea of a vector had been that of a line, and he had expected that when one vector was multiplied into another the result would be something of a different kind from a line, such, for instance, as a surface. Now if it had been pointed out to him in the chapter on vector multiplication that a surface is a vector, he would be saved from a painful mental shock, for a mathematician is as sensitive about 'dimensions' as an English schoolboy is about 'quantities'.

The fact is, that even in the purely geometrical applications of the Quaternion method we meet with three different kinds of directed quantities: the vector proper, which represents transference from A to B; the area or 'aperture', which is always understood to have a positive and a negative aspect, according to the direction in which it is swept out by the generating vector; and the versor, which represents turning round an axis.

The Quaternion ideas of these three quantities differ from the old ideas of the line, the surface, and the angle only by giving more prominence to the fact that each of them has a determinate *direction* as well as a determinate magnitude. When Euclid tells us to draw a line A B, he supposes it to be done by the motion of a point from A to B or from B to A. But when the line is once generated he makes no distinction between the results of these two operations, which, on Hamilton's system, are each the opposite of the other.

Surfaces also, according to Euclid, are generated by the motion of lines, so that the idea of motion is an old one, and we have only to take special note of the direction of the motion in order to raise Euclid's idea to the level of Hamilton's.

With respect to angles, Euclid appears to treat them as if they arose from the fortuitous concourse of right lines; but the unsatisfactory nature of this mode of treatment is shown by the fact that in all modern books on trigonometry an angle is represented as generated by motion round an axis in a definite direction.

There are thus three geometrical quantities having direction, and the more than magical power of the method of Quaternions resides in the spell by which these three orders of quantities are brought under the sway of the same system of operators.

The secret of this spell is twofold, and is symbolised by the vine-tendril and the mason's rule and square. The tendril of the vine teaches us the relation which must be maintained between the positive direction of translation along a line and the positive direction of rotation about that line. When we have not a vine-tendril to guide us, a corkscrew will do as well, or we may use a hop-tendril, provided we look at it not directly, but by reflexion in a mirror.[6]

The mason's rule teaches us that the symbol, as written on paper, is not a real line, but a mere injunction, commanding us to measure out in a certain direction a vector of a length so many times that of the rule. Without the rule the symbol would have no definite meaning. Thus the rule is the unit of the Quaternion system, while the square *reminds* us that the right angle is the unit versor.

The doctrine of the unit is a necessary part of every exact science, but in Quaternions the application of the same operators to versors, vectors, and areas is utterly unintelligible without a clear understanding of the function of the unit in the science of measurement.

Whether, however, it is better to insinuate the true doctrine into the mind of the student by a graduated series of exercises, or to inculcate it upon him at once by dogmatic statements, is a question which can only be determined by the experience of a new generation, who shall have been born with the extraspatial unit ever present to their consciousness, and whose thoughts, guided by the vine-tendril along the Quaternion path, shall turn always to the right hand, and never to the left.

Prof. Kelland tells us in the preface to the work to which we have alluded that, whereas Sir W. R. Hamilton and Prof. Tait have written treatises on Quaternions for mathematicians, the time has come when it behoves some one to write for those who desire to become mathematicians. Whatever, therefore, advanced mathematicians may think of this book, they ought to reserve their judgment as to its difficulty till they have ascertained how it is assimilated by those for whom it is written – those in whom the desire to become mathematicians has not yet become alloyed with the consciousness that they are mathematicians. For while Prof. Kelland – as he has elsewhere told us – finds but little difficulty in teaching the elements of the doctrine of Vectors to his junior classes, Hamilton himself, the great master of the spell, when addressing mathematicians of established reputation, found, for his Quaternions, but few to praise and fewer still to love.

Prof. Kelland, by the clearness and orderliness of his statements, and by boldly getting rid of everything which is unnecessarily abstruse, has done more than any other man towards rendering the subject easy to the student,

(6) See Numbers 370 and 371.

and reconciling even the case-hardened mathematician to the new method, as applied to geometrical questions of old-established truth.

The other aspect of Quaternions, as a method which every mathematician *must* learn in order to deal with the questions which the progress of physics brings every day into greater prominence, is hinted at by Prof. Tait in the last chapter of the book.(7) He there introduces us to the linear and vector function of the first degree under its kinematical aspect of a homogeneous strain. The importance of functions of this kind may be gathered from the fact that a knowledge of their properties supplies the key to the theory of the stresses as well as the strains in solid bodies, and to that of the conduction of heat and electricity in bodies whose properties are different in different directions, to the phenomena exhibited by crystals in the magnetic field,(8) to the thermo-electric properties of crystals, and to other sets of natural phenomena, one or more of which the scientific progress of every year brings before us.

But as we believe that Prof. Tait is about to bring out a new edition of his treatise on Quaternions, in which this higher aspect of the subject will be brought more prominently forward, we reserve our remarks on Quaternions as an instrument of physical research till we have the subject presented to us by Prof. Tait in a form which adequately represents its latest developments.(9)

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(7) Chapter X, 'Vector equations of the first degree', in Kelland and Tait, *Introduction to Quaternions*: 180–208.

(8) See Numbers 441 and 468.

(9) P. G. Tait, *An Elementary Treatise on Quaternions* (Cambridge, ${}_2$1873): 228–88 (Chap. XI, 'Physical Applications').

LETTER TO HERBERT SPENCER[1]

5 DECEMBER 1873

From the original in the University of London Library[2]

11 Scroope Terrace
Cambridge
5 Dec 1873

Dear Sir[3]

I do not remember the particulars of what I said to Prof. Clifford[4] about nebular condensation. The occasion of it was I think a passage in an old edition of your First Principles,[5] and having since then made a little more acquaintance with your works I regarded it merely as a temporary phase of

(1) Herbert Spencer, philosopher and sociologist (*DNB*).

(2) Herbert Spencer MS 791/92, University of London Library.

(3) In reply to a letter from Spencer of 4 December 1873: 'Sometime this year, Prof. Clifford named to me a criticism you passed upon a certain hypothesis of mine respecting the process of nebular concentration, as tending to produce a hollow liquid spheroid during its closing stages. The moment he named to me this criticism, I saw that I had made a mistake. The process of condensation from the vapourous to the liquid form, I had at first considered in the case of Saturn's rings, where a precipitation into a denser form, recurring at the equatorial portion of the concentrating spheroid, might produce a ring that would maintain its place; supposing the concentration to occur when the centripetal and centrifugal forces were balanced. And I had inadvertently carried this conception to the case of other planets, where there could be no such balance of forces. / There has since occurred to me another hypothesis respecting the mode of condensation and the resulting structure. I have discussed this with my friends Tyndall, Hirst and Clifford; and, while not committing themselves to it, they do not raise any objections against it. Prof. Clifford, however, expressing great faith in your intuitive insight into physical processes, recommended me to obtain, if possible, your opinion respecting the tenability of this hypothesis. / I write to ask whether, after the close of the Cambridge term, you are likely to be in London; and whether, in that case, you could afford me half-an-hour's conversation; and further to ask whether, if you are not coming to London, you could grant me the same favour were I to come to Cambridge before the term ends. / I enclose the outline, in proof, of a speculation on another physical question, respecting which, also, I [...]' (ULC Add. MSS 7655, II/74). On Thomas Archer Hirst see Number 369 note (6). On Spencer's interpretation of Laplace's nebular hypothesis, and his correspondence with Maxwell, see David Duncan, *The Life and Letters of Herbert Spencer* (London, 1908): 428–31.

(4) William Kingdon Clifford: see Number 280 note (7).

(5) On Spencer's interpretation of the nebular hypothesis see his *First Principles* (London, 1862): 362–7. Maxwell may have noted Spencer's discussion of Saturn's rings in illustration of his theory 'that a state of homogeneity is one of unstable equilibrium'; see *First Principles*: 366–7, and also Spencer's letter of 4 December 1873 (note (3)). For Maxwell's comments on the nebular hypothesis in relation to Saturn's rings see Volume I: 443.

the process of evolution which you have been carrying on within your own mind.

Mathematicians, by guiding their thoughts always along the same tracks, have converted the field of thought into a kind of railway system and are apt to neglect cross-country speculations.

It is very seldom that any man who tries to form a system can prevent his system from forming round him and closing him in before he is 40. Hence the wisdom of putting in some ingredient to check crystallization and keep the system in a colloidal condition. Candle-makers I believe use arsenic for this purpose. In psychological matters 'mental suspense' is often recommended but this is generally interpreted as 'acidia' or want of interest.

But you seem to be able to retard the crystallization of parts of your system without stopping the process of evolution of the whole, and I therefore attach much more importance to the general scheme than to particular statements.

With respect to electricity(6) and its relation to molecular motions of various kinds you should look at Professor Challis' speculations as he inclines to a theory of that kind.(7)

You quote me as denying that Electricity is a form of energy,(8) but you give, as an example, an electrified system, the energy of which is the main subject of calculation in my book.

I say that the electrification of a system is a form of energy, just as the-being-wound-up of a watch spring is, but electricity, that is the quantity which we deal with when we compare the charges of electrified bodies, is not of the nature of energy, but of the nature of body, just as the watch spring is.

It is one of the factors of energy, the other being electric potential. It cannot therefore of itself be energy.

But heat, of itself, that is the quantity of heat which will melt so much ice, is energy.

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(6) Presumably the subject of Spencer's supplementary query mentioned in his letter of 4 December (note (3)). The proof of a paper 'What is electricity?', revised and published in Spencer's *Essays: Scientific, Political, and Speculative*, 3 vols (London, 1858–74), **3**: 191–215, is among the Maxwell MSS in ULC Add. MSS 7655, V, c/51. According to Spencer, 'electricity results from the mutual disturbance of unlike molecular motions' (*Essays*, **3**: 195).

(7) James Challis, *An Essay on the Mathematical Principles of Physics* (Cambridge, 1873). See Maxwell's review, 'Challis's "Mathematical Principles of Physics"', *Nature*, **8** (1873): 279–80 (= *Scientific Papers*, **2**: 338–42), critical of Challis' speculations. See also Number 475, and Volume I: 693n.

(8) The proof of Spencer's 'What is electricity?' (see note (6)) includes galleys of a postscript to the text of the essay (as first published in 1864). There is a statement – which was deleted from the text as published in *Essays*, **3**: 203–11 – of 'the paradox enunciated by Prof. Clerk Maxwell; namely, that electricity cannot be classed as a form of energy'.

My peculiar doctrine about electricity, that is the 'matter of electricity', is that it fills all space, so that every cubic foot of space is at all times equally full of it. Every displacement of this medium is therefore in a circuit, as in the case of an incompressible fluid. If this displacement occurs in a non-conductor or 'dielectric' it requires electromotive force to produce it, and it reacts with an equal electromotive force, just as a watch spring requires a force to wind it up and then exerts force on the wheels when the watch is not going.

In a conductor, the electromotive force decays rapidly, so that a continuous current is produced, as if a man should keep winding his watch when it is going, or when the balance wheel is removed, and the watch is running down at great speed.

The reasons for such a doctrine are given in different parts of my book and are referred to in the index under 'Displacement'.

In column Three of the slips you sent me and in the 2nd paragraph (on residual discharge) there is a very graphic description of phenomena which resemble real phenomena in all but one, and that a most significant, respect.[(9)] First read Faradays experimental researches from (1169) to (1178).[(10)] (This is a most remarkable passage, one that has hardly ever been appreciated, and which Faraday himself understood only in his highest scientific moods.) Then on residual discharge (1234) &c.[(11)]

The facts are as follows. Take a plate of spermaceti gutta percha &c. Let A B be metallic electrodes to be connected at pleasure with a machine. Electrify $A+$ (or $B-$, there is no difference) for some time. Then discharge both A and B completely and simultaneously, and insulate them again. In a little A will be found $+$ and B $-$ but feebler than at first. This may be repeated several times, the residual charges becoming always feebler.

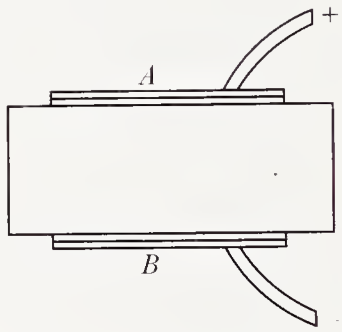

Figure 486,1

(9) In the postscript to his 'What is electricity?' (see notes (6) and (8)) Spencer discusses electric charge in terms of his notion that 'electricity results from the mutual disturbance of unlike molecule motions'. He explains 'residual discharge' in terms of 'perturbations in the layers of molecules'. He adds: 'We have, too, a complete explanation of the truth, insisted on by Faraday, that there can be no charge of one kind of electricity obtained, without a corresponding charge of the opposite kind. For... no wave of molecular perturbations of the nature described, can be produced, without there being simultaneously produced an exactly-equal counter-wave.' He slightly amended the final sentence, here quoted from the proof, in the published text (*Essays*, **3**: 210). In response to comments by Maxwell and others, Spencer appended a supplementary postscript (*Essays*, **3**: 211–15).

(10) Faraday, *Electricity*, **1**: 364–7 ('On the absolute charge of matter').

(11) Faraday, *Electricity*, **1**: 387.

Now take a case apparently similar. Hold a thick plate of metal before the fire till one side is hotter than the other. Then take it away and pour cold water over the heated side till it is cooled to the same temperature as the other.

Then leave the plate to itself for a while. The side which was formerly next the fire will again become hotter than the other, just as the plate of gutta percha became again electrified in the first case.

The thermal phenomenon is explained by the return of the heat which had been conducted a certain distance into the plate. The electrical phenomenon has often been explained in the same way, but not correctly.

If you heat a body and then cool its surface to the atmospheric temperature and then leave it alone, the heat will work its way outwards and warm the surface again.

But if you charge a body ever so highly with electricity over its whole outer surface and keep it charged ever so long, and if you then instantaneously discharge the outer surface and again insulate it, no new electrification will appear. You cannot cause electricity to become absorbed in the substance of the body so as to produce no effect outside at the instant of discharge, and afterwards to creep out of the molecules in which it was latent as heat does.(12)

Observe that I do not say that these phenomena are inconsistent with your theory. I only say that they are very important and significant, and that though you expressly refer to Faraday's dogma in the next paragraph(13) I think it probable that fresh thought on the subject would lead you to modify some expressions in the paragraph on residual charge, so as to exclude from the mind of the reader the idea that electricity is absorbed after the manner of heat.

With respect to the vibrations of molecules I know that you do not believe that a molecule can of itself dance up and down with nothing to dance on, but as I observe that you are always improving your phraseology I shall lay before you my notions on the nomenclature of molecular motions.

(1) I would confine the word *vibration* to those internal motions of a molecule which alter the distances between the parts of which it is composed. The simpler the molecule the smaller the number and the greater the definiteness of the modes of harmonic vibration. Every system has a set of harmonic modes of vibration but whether it actually vibrates and how the

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(12) See Maxwell, *Treatise*, **1**: 50–2 (§§52–4).

(13) See note (9).

motion is divided among these modes depends on the circumstances which set it in motion.

(2) The molecule may also *rotate* about any axis, constant or variable, without altering the distances between its parts. There is nothing which defines the period of such rotation. It varies at every encounter with another molecule.

(3) Next comes the motion of the centre of gravity which is in itself of course perfectly definite, but as regards human knowledge it consists of two parts, visible and invisible.

The *visible* velocity is that of the centre of gravity of the group of molecules, consisting of millions which we deal with as an observable portion of the substance.

(4) The other component, that by which the actual velocity of an individual molecule differs from the mean velocity of the group, is called the invisible motion, or more expressively, the motion of *agitation* of the molecule.

Our knowledge of (2) and (4) is *statistical* only – there is nothing definite in any other sense than the death-rate of a city is definite.

In (1) the periods are definite but the amplitudes are not. (1) (2) & (4) are connected together and the total energy of each rises and falls with the temperature. (3) alone is independent of the others.

It is only in gases that the vibrations of the molecules have definite periods because it is only in gases that a molecule is ever so far from its neighbours as to form a vibrating system by itself like a tuning fork. In solids liquids and even in gases when highly compressed there is so much interaction between the molecules that the vibrating system has a far greater variety of periods and besides this the relative motion of the molecules introduces continual changes in the connexions of the system and therefore in its periods so that the final result is what appears to us complete irregularity of vibration. Hence there are no bright lines in the spectra of hot solids and liquids, and even in gases the spectrum becomes continuous when the gas is dense.

Of course all this has nothing to do with the vibrations (which may or may not be of regular periods) which arise when waves such as sound waves are propagated through a medium consisting of molecules. These vibrations are far slower and on a far larger scale than the vibrations of the constituents of a molecule. The motions belong to class (3) and may be treated as if the medium were continuous (not molecular) without any risk of error.

All these distinctions of motion into regular and irregular &c arise from the limitation of our bodily and mental faculties. If the molecular theory is true and if we could see or trace the motion of each molecule we could not longer distinguish motion into visible and invisible, regular and irregular, motion of masses and heat. We do not say that a cannon ball is hot because it is moving rapidly for we follow it in thought with our thermometer and find it cold. We say that a battle is hot when there are many such balls flying about and when we think of the battle, not of one ball at a time.

I had not intended to send you this letter (which was written at odd times during the examination of papers) but I find I have not time to reduce it to a reasonable form for a day or two so I must leave it as it is.

Yours faithfully
J. CLERK MAXWELL

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LETTER TO HERBERT SPENCER

17 DECEMBER 1873

From the original in the University of London Library(1)

11 Scroope Terrace
Cambridge
17 December 1873

Dear Sir

The reason for which I use the word agitation to distinguish the local motion of a molecule in relation to its neighbours is that I think with you that the word agitation conveys in a small degree, if at all, the notion of rhythm.(2)

If motion is said to be rhythmic when the path is, on the whole, as much in one direction as in the opposite, then all motion is rhythmic when it is confined within a small region of space.

But if as I understand the word rhythmic, it implies not only alternation, but regularity and periodicity, then the word agitation excludes the notion of rhythm, which was what I meant it to do.

In the writings and words of many persons who are trying to conceive of heat in a body as a motion of its parts, I can trace a tendency to attribute some kind of regularity to the motion. They have picked up their ideas of motion confined to narrow limits chiefly from their study of the vibrations of elastic bodies (like tuning forks) and the motion of the particles of a medium during the propagation of a wave. The popularization of true notions on these matters has been the great work of the early part of this century.

But these motions, at least in those cases which are explained in books, are periodic and regular, that is one vibration is exactly like another.

They have also the property that they can be conveyed away from the vibrating body and passed on, in their entirety, to other bodies, as in the propagation of a sound wave which leaves the air behind it at rest.

But the motion called heat is not propagated but only diffused, that is, it cannot be passed from hand to hand so as to make the receiver hot while leaving the giver cold. It can only be communicated from those who have much to those who have less.

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(1) Herbert Spencer MSS 791/93, University of London Library.

(2) In his reply to this letter, dated 30 December 1873 (ULC Add. MSS 7655, II/76; printed in *Molecules and Gases*: 160–2), Spencer wrote: 'I had no intention when I made my passing comment on the word *agitation*, of drawing from you a second letter.', apparently referring to his (missing) reply to Maxwell's letter of 5 December (Number 486). On Spencer's concept of 'the rhythm of motion', his doctrine that 'rhythm results wherever there is a conflict of force not in equilibrium', see his *First Principles* (London, 1862): 317–34, esp. 317.

The explanation of this *diffusion* lies in the *irregularity* of that motion of agitation which we call heat. Heat (physically speaking) is something which a body cannot possess without being hot. Now you will find mention made in books of something called Radiant Heat which is further explained as a regular system of undulations in an æther.

But I say that Radiant Heat is not heat at all, for the medium which transmits it is not hot while it is in it, and though it is in motion, this motion is entirely passed on to the next portion of the medium.

The case is like that of a fire which drives a steam-engine which drives a band which drives a turning lathe which has a piece of wood chucked on it which gets hot by friction.

The ultimate source of this heat is the fire, and one of the media of transmission is the band, but the band does not become hot in virtue of its transmitting energy.

Similarly the transparent media transmit radiation without becoming hot.

A great scientific desideratum is a set of words of *little* meaning – words which mean no more than that a thing belongs to a very large class. Such words are much wanted in the undulatory theory of light in order to express fully what is proved by experiment without connoting anything which is a mere hypothesis. Hamiltons word Vector, signifying a directed quantity without specifying whether it is a displacement, a rotation, a velocity, a magnetization &c is exceedingly useful in such a statement. See arts 816 & 821 of my book on Electricity.(3)

Yours truly
J. Clerk Maxwell

(3) Maxwell, *Treatise*, 2: 404, 407–8.

EXAMINER'S REPORT ON THE NATURAL SCIENCES TRIPOS (PHYSICS) 1873

DECEMBER 1873[1]

From the original in the University Library, Cambridge[2]

NATURAL SCIENCES TRIPOS 1873

PHYSICS

Of the twenty three candidates, seventeen obtained marks for their answers to questions in Physics. The knowledge of the subject as shown by these answers was in a good many cases of a very unsatisfactory kind, arising partly from an unintelligent use of popular text books and partly from a familiarity with the appearance of instruments without any knowledge of the principles on which their action depends.

Several of the candidates however, and in particular M^r^ Davies of St. Johns[3] sent up answers which showed that Experimental Physics, treated without the higher mathematics, may be learned in a sound & scientific manner.

A certain amount of knowledge of the first principles of physics ought I think to be required of candidates during the first three days, as necessary in every branch of Natural Science.

During the last three days,[4] the object of the Examiner should be to give credit to those who can show a practical as well as a theoretical knowledge of the methods and results of physical research. The questions in the last three days should therefore be so arranged as to give full occupation to the real student of physics during the hours of examination while they afford no chance of making marks to the man of hearsay information.

JAMES CLERK MAXWELL

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(1) See Numbers 483 and 486 for reference to the examination.

(2) ULC, Natural Sciences Tripos Mark Book, Cambridge University Archives, Min. VIII.56, 41^v^. There is a draft in ULC Add. MSS 7655, V, k/10.

(3) John Paget Davies, St John's 1870 (Venn).

(4) According to the regulations for the examination for the Natural Sciences Tripos, in 'the last six papers the questions shall take a wider range...and some of the questions shall have special reference to the Philosophy and History of those subjects'; see *The Cambridge University Calendar for the Year 1873* (Cambridge, 1873): 34–5.

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THE EQUATION OF CONTINUITY AND PHYSICAL ANALOGY

1873[1]

From the original in the University Library, Cambridge[2]

NOTES ON M^r^ APPLETON'S DISSERTATION[3]

On p. 4 it would be more general in the equation of continuity instead of $K\left(\frac{dV}{dz}+\frac{d^2V}{dz^2}\delta z\right)$ to write $K\frac{dV}{dz}+\frac{d}{dz}\left(K\frac{dV}{dz}\right)\delta z$.

The first equation then becomes

$$CD\frac{dV}{dt}=\frac{d}{dx}\left(K\frac{dV}{dx}\right)+\frac{d}{dy}\left(K\frac{dV}{dy}\right)+\frac{d}{dz}\left(K\frac{dV}{dz}\right)$$

which is the general equation of the flow of heat in an isotropic but not necessarily homogeneous solid within which there are no sources or sinks.[4] C, D, and K are functions of whatever the state of the body at the point $x\,y\,z$ depends on such as

the chemical nature of the substance

its physical state as determined by its state of aggregation as solid liquid or gaseous, powdered, spongy or dense &c and also on its temperature.

In so far as pressure affects the state of aggregation, C D & K are functions of pressure, but we have no reason to believe that pressure affects C, D, K in any other way. Magnetization may also affect these quantities. On the other hand, C, D, K are not functions of anything which does not affect the physical state of the body directly as for instance

1 Flow of heat through the element does not affect it.

2 Flow of electricity through a conducting element does not directly affect it, only by raising the temperature.

3 Electric and Magnetic potentials, being mere artificial concepts, cannot affect the physical state of a body.

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(1) This date is conjectural. See note (3).

(2) ULC Add. MSS 7655, V, i/6.

(3) Probably for a Trinity College Fellowship: Richard Appleton, Trinity 1867, Fellow 1873 (Venn).

(4) See Number 339 esp. note (3). K is the conductivity of the body for heat, C the heat capacity per unit mass, and D the density of the body.

General note on the analogies of Heat Electrostatics Elektrokinetics and Magnetic Induction.

(1) V in the equation represents –
Temperature Electric Potential and Magnetic Potential. Of these Temperature is a physical state of a body and therefore its properties may be functions of Temperature.

Electric and Magnetic Potentials are not physical states. They are reckoned from a purely arbitrary zero, and a change of potential does not affect any property of the body.

(2) $\frac{dV}{dx}$ represents –

(α) The component flow of heat across a surface normal to x.(5)

We have no reason to believe that the physical state of any body is affected by the flow of heat across it. In fact we have every reason to believe that the flow of heat is going on in all directions in a body of uniform temperature, and that what we call a flow of heat is only a preponderant flow in one direction.

(β) The electromotive force at a point in a dielectric.(6)

If, as I suppose, this produces a state of strain in the dielectric, the physical properties of the dielectric may vary with its value from the state of complete freedom from polarization to that at which disruptive discharge occurs. No such variation of properties has as yet been experimentally demonstrated, but there is no reason against it.

(γ) The electromotive force at a point in a conductor.(7)

Here the state of strain, if it exists in a conductor is continually decaying and breaking down. We have abundance of experimental evidence that Ohms law is true to a great degree of accuracy, and therefore the conductivity of a body is not affected by the intensity of the electromotive force acting on it, or by the strength of the current produced. (If the current raises the temperature, of course the rise of temperature, and not the current itself, increases the resistance.)

(δ) The magnetizing force at a point in a ferro- or diamagnetic body.(8)

Magnetization is a change in the physical state of a body. It has been shown by Thomson to alter the physical properties of the body as regards

(5) [William Thomson,] 'On the uniform motion of heat in homogeneous solid bodies, and its connection with the mathematical theory of electricity', *Camb. Math. J.*, **3** (1842): 71–84 (= *Electrostatics and Magnetism*: 1–14).

(6) See the *Treatise*, **1**: 384 (§332).

(7) See the *Treatise*, **1**: 345 (§297).

(8) See the *Treatise*, **2**: 21 (§395).

conduction of electricity &c,[9] and we know that the magnetic permeability varies with the magnetization.[10]

(3) K represents

α thermal conductivity
β specific inductive capacity
γ electric conductivity
δ magnetic permeability.[11]

All these are independent of

Electric and magnetic potential
Flow of heat and of electricity but depend upon Temperature, electric displacement & magnetization.

(4) $CD = 0$ in the case of electricity.[12]

p. 14 It should be distinctly stated that Thomson was the first to point out the analogy between the mathematical theories of heat & electricity[13] and this ought to be done early in the Dissertation.
p. 35 The description of electrolysis here given is much older than Clausius. Clausius introduced the doctrine that in an electrolyte in its ordinary state the ions are often dissociated by the effect of the collisions of the molecules and that it is when they are in this state that the electromotive force guides their motion.[14]
p. 36 strain instead of stress
stress = distribution of force, strain = distribution of displacement.[15]
p. 37 and to be itself rendered impervious to electricity.
p. 38 Sp. ind. cap. certainly does not vary with potential it may vary with temperature and with electromotive force.
p. 41 doubtful analogy between 1st law of motion and conduction of heat without expenditure of energy
better analogy the diffusion of gases or of black & white balls in a bag.
p. 47 changing the temperature of a standard body.

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(9) See Thomson's discussion of 'Magnetic permeability, and analogues in electro-static induction, conduction of heat, and fluid motion', in his *Electrostatics and Magnetism*: 482–6.
(10) See the *Treatise*, **2**: 51 (§428).
(11) See the *Treatise*, **2**: 237 (§618).
(12) Yielding Laplace's equation for points in space where there is no electrification: see the *Treatise*, **1**: 86, 99, 115, 384 (§§83, 96, 102, 332).
(13) See note (5).
(14) See Number 478 esp. note (6).
(15) See Number 206 esp. note (7).

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REPORT ON A PAPER BY OSMOND FISHER ON THE ELEVATION OF MOUNTAINS

circa DECEMBER 1873[1]

From the original in the University Library, Cambridge[2]

REPORT ON M^r FISHERS PAPER ON THE ELEVATION OF MOUNTAINS BY LATERAL PRESSURE[3]

The aim of this paper is to investigate the numerical data of the lateral contraction of the earths crust since it became solid in order to compare it with the amount of crumpling exhibited by the strata which have been elevated into mountain ranges.

The investigations given in the paper are of several different kinds. The first relates to the volumes of mountains above and of valleys below a certain datum level, defined as the present surface of the earth, supposing that during its shrinking the crust had yielded freely to lateral compression without either crumpling up or in any way increasing its vertical thickness.

The equation derived from these considerations appears consistent with this definition of the datum level. But it does not seem to me to be of any use in relation to the aim of the paper. It only tells us that the volume of the crust has remained the same since it has become cold in spite of upheavals and subsidences. But what we want is a measure of the amount of disturbance and this is not to be got by an expression in which hollows are negative and heights positive but by one in which both are positive. For instance the expression might be that of the potential energy of the present distribution of matter which might be derived from it by levelling up and down to a

(1) This dating is tentative. A minute of a meeting of the Council of the Cambridge Philosophical Society, dated 1 December 1873, records: 'Mr O. Fisher's (second) paper "On the elevation of mountains by lateral pressure" to be read this evening, was ordered to be referred.' ('Council Minute Book, 1871–85', Scientific Periodicals Library, Cambridge). Fisher had previously published a paper 'On the elevation of mountains by lateral pressure, its cause, and the amount of it, with a speculation on the origin of volcanic action', *Trans. Camb. Phil. Soc.*, **11** (1871): 489–506.

(2) ULC Add. MSS 7655, V, i/5.

(3) See Osmond Fisher, 'On the inequalities of the earth's surface viewed in connection with the secular cooling', *Trans. Camb. Phil. Soc.*, **12** (1875): 414–33. A minute of a meeting of the Council of the Cambridge Philosophical Society, dated 8 February 1875, records: 'It was agreed that Mr O. Fisher's paper "On the elevation of mountain chains by compression" be printed in the Society's Transactions.' ('Minute Book').

spheroidal surface of the same volume as the actual earth. (This is not M^r Fishers datum level.)

Thus $\sum$ (Mass of elevated portion × height of centre of gravity of do:)

$+\sum$ Mass of earth displaced from hollows × depth of c.g. of hollows would give a quantity *essentially* $+^{ve}$ representing the energy of the present distribution of mountains and valleys.

But what is really wanted is a numerical estimate of the actual amount of crumpling of the crust, that is to say an estimate of the original area of what is now a square mile of crust derived from observation of the contortions of the strata and the distortions of fossils therein.

Such an estimate is referred to in the paper; it is derived from Prof Ramsays restored sections(4) and gives linear compression between $\frac{1}{13}$ and $\frac{1}{21}$.(5)

The valuable part of the paper is the comparison of this estimate with the linear shrinking of the surface due to the cooling of the earth.

I have not had time to go through the calculations but on a rough estimate the results do not seem to be affected by any important error and they show that to assume a linear shrinking of $\frac{1}{13}$ or even $\frac{1}{21}$ due to cooling of hot rocks since the surface became solid would be very extravagant.

The author suggests other causes of shrinkage besides loss of heat namely the escape of water.(6) It is probable or rather certain that water-substance if it exists at great depths under great pressure and at high temperture is neither a gas nor a liquid being above its critical point.(7)

In this state substances are easily dissolved in it, not however so much on account of greater tendency to combine with water as on account of a greater tendency of their own to dissipation. At still higher temperatures the water-substance becomes itself dissociated into oxygen and hydrogen but it does not follow that the dissolved substances will be precipitated. The 'magma' may be all the more complete the higher the temperature, because though the bonds of affinity have fallen away, the prison walls prevent the elements from escaping.

But of all the unknown regions of the universe the most unsafe to reason about is that which is under our feet.

On the whole, I consider the value of the paper to lie in the comparison

(4) A. C. Ramsay, 'On the denudation of South Wales and the adjacent counties of England', *Memoires of the Geological Survey of Great Britain*, **1** (1846): 297–335.

(5) As stated by Fisher, 'On the inequalities of the earth's surface': 428.

(6) The passages in Maxwell's report, from 'It is probable…' to '…that which is under our feet', were quoted by Fisher in 'On the inequalities of the earth's surface': 431n, with the appended comment: 'The following remarks… were received from a quarter which disposes me to place a great reliance on them.'

(7) See Number 381 note (3).

between the observed crumpling of the strata and the theoretical shrinking of the crust by cooling and in the conclusion that if the shrinkage was similar to that of melted rocks and slags it would not be sufficient to account for the observed crumpling. There is also a suggestion as to the shrinkage by escape of water the objections to which so far as they are stated in the paper I do not think of great moment considering the slowness of diffusion through a thickness equal to that of the earths crust.

I have not been able to see the value of the first part of the paper, namely that which involves the 'datum line' equation. Unless I become convinced by further explanation, I should say that this part of the paper, if printed, would greatly detract from the value of the whole.[8]

J. Clerk Maxwell

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(8) But see Fisher, 'On the inequalities of the earth's surface': 415–19, where discussion of the datum line is included.

APPENDIX

§1 The following letters, which are not extant as autograph manuscripts, have been abbreviated from the versions printed in the *Life of Maxwell*.

(1) Letter to Charles Hope Cay
5 January 1865 (Number 240).

(2) Letter to Charles Benjamin Tayler
2 February 1866 (Number 256).

§2 The following letters printed in extract in the *Life of Maxwell*, have not been reproduced.

(1) Letter to Lewis Campbell
22 November 1864 (*Life of Maxwell*: 340).

(2) Letters to Charles Hope Cay
18 November 1863, 14 October 1865 (*Life of Maxwell*: 337–8, 343–4).

(3) Letters to Katherine Mary Clerk Maxwell
22 June 1864, 23 June 1864, 26 June 1864, 28 June 1864, December 1873 (*Life of Maxwell*: 338–40, 387).

§3 Letters written to Maxwell
Locations of the letters and details (where appropriate) of their citation and reproduction in this volume are given. Many of these letters – notably those from G. G. Stokes, P. G. Tait and William Thomson – have been reproduced *in extenso*. Letters which have been reproduced in abbreviated form are marked * below, those merely cited are marked †.

(1) Letters from George Biddell Airy
   (1) 12 November 1868, Royal Greenwich Observatory Archive, ULC, Airy Papers 6/5, 411R–V; * Number 314 note (4).
   (2) 14 October 1872, ULC, Airy Papers 6/259, 203R–V; Number 424 note (2).
   (3) 26 October 1872, ULC, Airy Papers 6/259, 205R; Number 426 note (2).
   (4) 29 October 1872, ULC, Airy Papers 6/259, 210R–V; * Number 426 note (7).

(2) Letter from John Aitken
6 March 1873, ULC Add. MSS 7655, II/70.

(3) Letter from Jane Barnard
20 June 1871, ULC Add. MSS 7655, II/47.

(4) Letters from William Benson
  (1) May 1870, ULC Add. MSS 7655, II/37; † Number 341 note (6).
  (2) 13 March 1871, ULC Add. MSS 7655, II/43; † Number 358 note (3).

(5) Letter from Edward William Blore
13 February 1871, ULC Add. MSS 7655, II/38A; Number 357 note (3).

(6) Letter from George Phillips Bond
9 July 1863, Bond MSS, Harvard University Archives UAV. 630.6; * Number 217 notes (3), (4), (6) and (14).

(7) Letter from Robert E. Branston
21 October 1867, ULC Add. MSS 7655, II/27.

(8) Letter from Lewis Campbell
4 July 1872, ULC Add. MSS 7655, II/58.

(9) Letter from Arthur Cayley
20 April 1868, ULC Add. MSS 7655, II/29; Number 320 note (3).

(10) Letter from Robert Bellamy Clifton
22 November 1871, ULC Add. MSS 7655, II/53.

(11) Letter from Alexander Crum Brown
4 September 1873, ULC Add. MSS 7655, II/73; * Number 478 notes (12) and (24).

(12) Letter from V. Dwelshauvers-Dery
12 May 1872, ULC Add. MSS 7655, II/57.

(13) Letters from Joseph David Everett
  (1) 19 July 1872, ULC Add. MSS 7655, II/60; † Number 341 note (19).
  (2) 26 July 1872, ULC Add. MSS 7655, II/61; * Number 341 note (19).

(14) Letter from James David Forbes
4 June 1864, ULC Add. MSS 7655, II/22.

(15) Letter from William Francis
11 July 1873, Henry Augustus Rowland Papers MS. 6, Milton S. Eisenhower Library, The Johns Hopkins University, Baltimore; Number 467 note (2).

(16) Letter from George Griffith
27 July 1873, ULC Add. MSS 7655, II/72; † Number 470 note (25) and * Numbers 474 note (13) and 478 note (2).

(17) Letter from G. A. Hirn
25 November 1872, ULC Add. MSS 7655, II/68; † Number 426 note (7).

(18) Letters from Charles Hockin
 (1) 15 May 1868, ULC Add. MSS 7655, II/30; * Number 289 note (8).
 (2) 27 July 1868, ULC Add. MSS 7655, II/31; * Number 297 notes (6) and (9).
 (3) 11 March 1870, ULC Add. MSS 7655, II/34; † Number 297 note (6) and * Number 378 note (9).

(19) Letters from Fleeming Jenkin
 (1) 10 January 1868, ULC Add. MSS 7655, II/28; Number 287 note (8).
 (2) 28 October 1871, ULC Add. MSS 7655, II/51; † Number 385 note (10).

(20) Letter from James Prescott Joule
n.d. [June 1871], ULC Add. MSS 7655, II/49; * Number 339 note (15).

(21) Letter from William Longman
20 June 1871, ULC Add. MSS 7655, II/48; Number 381 note (2).

(22) Letter from Arthur Luke
6 October 1868, ULC Add. MSS 7655, II/24.

(23) Letters from Cecil James Monro
 (1) 2 June 1870, Greater London Record Office, Acc. 1063/2105; * Number 341 notes (2) and (20).
 (2) 3 March 1871, GLRO, Acc. 1063/2106, 2109b, 2109c; * Number 359 notes (2) and (7).
 (3) 9 March 1871, GLRO, Acc. 1063/2107; * Number 359 notes (2), (14) and (15).
 (4) 21 March 1871, GLRO, Acc. 1063/2108; * Numbers 359 note (15) and 363 notes (2), (3) and (4).
 (5) 10 September 1871, GLRO, Acc. 1063/2109a; * Number 359 note (8).

(24) Letter from E. J. Nanson
5 December 1873, ULC Add. MSS 7655, II/75.

(25) Letter from George E. Preece
7 March 1873, ULC Add. MSS 7655, V, i/12.

(26) Letter from Bartholomew Price
4 January 1871, ULC Add. MSS 7656, P 659; * Introduction note (90) and * Number 367 note (3).

(27) Letters from Herbert Spencer
 (1) 4 December 1873, ULC Add. MSS 7655, II/74; Number 486 note (3).

(2) 30 December 1873, ULC Add. MSS 7655, II/76; * Number 487 note (2).

(28) Letters from George Gabriel Stokes

(1) 16 February 1871, ULC Add. MSS 7655, II/40; Number 357 note (3).

(2) 18 February 1871, ULC Add. MSS 7655, II/41; Number 357 note (4).

(3) 23 February 1871, ULC Add. MSS 7655, II/42; Number 357 note (5).

(4) 14 March 1871, ULC Add. MSS 7655, II/44; Number 358 note (11).

(29) Letter from John William Strutt, Lord Rayleigh

14 February 1871, typed copy in private possession; * Numbers 355 notes (9), (10) and (12) and 357 note (3), and † Number 358 note (6).

(30) Letter from William Swan

11 May 1872, ULC Add. MSS 7655, II/56.

(31) Letters and postcards from Peter Guthrie Tait

(1) n.d. [June 1865], ULC Add. MSS 7655, I, a/1; Number 249 note (2).

(2) 6 April 1866, ULC Add. MSS 7655, I, a/2; Number 262 note (15).

(3) 27 November 1867, ULC Add. MSS 7655, I, a/3; Number 276 note (2).

(4) 6 December 1867, ULC Add. MSS 7655, I, a/4; Number 277 note (2).

(5) 13 December 1867, ULC Add. MSS 7655, I, a/5; Number 277 note (22).

(6) 1 February 1871, ULC Add. MSS 7655, I, a/7; Number 353 note (11).

(7) 17 February 1871, ULC Add. MSS 7655, I, a/8; Number 356 note (7).

(8) 31 March 1871, ULC Add. MSS 7655, I, a/9; Number 365 note (5).

(9) 5 April 1871, ULC Add. MSS 7655, I, a/10; Number 366 note (5).

(10) 9 May 1871, ULC Add. MSS 7655, I, a/11; Number 368 note (6).

(11) 13 May 1871, ULC Add. MSS 7655, I, a/12; Number 369 note (8).

(12) 14 May 1871, ULC Add. MSS 7655, I, a/13; Number 371 note (8).

(13) 1 June 1871, ULC Add. MSS 7655, I, a/14; Number 373 note (5).
(14) 5 June 1871, ULC Add. MSS 7655, I, a/15; Number 375 note (2).
(15) 7 June 1871, ULC Add. MSS 7655, I, a/16; Number 375 note (2).
(16) 13 June 1871, ULC Add. MSS 7655, I, a/17; Number 376 note (2).
(17) 14 June 1871, ULC Add. MSS 7655, I, a/18; Number 378 note (11).
(18) 20 June 1871, ULC Add. MSS 7655, I, a/20; Number 378 note (11).
(19) 9 July 1871, ULC Add. MSS 7655, I, a/21; Number 380 note (2).
(20) 10 November 1871, ULC Add. MSS 7655, I, a/22; Number 389 note (14).
(21) 2 January 1872, ULC Add. MSS 7655, I, a/23; Number 398 note (10).
(22) 6 January 1872, ULC Add. MSS 7655, I, a/25; Number 399 note (8).
(23) n.d. [January 1872], ULC Add. MSS 7655, I, a/55; Number 403 note (8).
(24) 25 January 1872, ULC Add. MSS 7655, I, a/26; Number 401 note (7).
(25) 11 June 1872, ULC Add. MSS 7655, I, a/27; Number 413 note (3).
(26) 28 June 1872, ULC Add. MSS 7655, I, a/28; Number 414 note (2).
(27) 13 July 1872, ULC Add. MSS 7655, I, a/29; Number 417 note (2).
(28) 15 July 1872, ULC Add. MSS 7655, I, a/30; Number 417 note (4).
(29) 16 July 1872, ULC Add. MSS 7655, I, a/31; Number 417 note (3).
(30) 17 July 1872, ULC Add. MSS 7655, I, a/32; Number 417 note (3).
(31) 19 July 1872, ULC Add. MSS 7655, I, a/33; Number 417 note (3).
(32) 30 June 1873, ULC Add. MSS 7655, I, a/34; Number 465 note (2).
(33) 7 July 1873, ULC Add. MSS 7655, I, a/35; Number 465 note (2).

(34) 31 July 1873, ULC Add. MSS 7655, I, a/36; Number 474 note (3).

(32) Letters from James Thomson

(1) 21 July 1871, James Thomson Papers MS 13/22b, The Queen's University of Belfast Library; Number 382 note (2).

(2) 11 January 1873, James Thomson Papers MS 13/22e, Queen's University Library; * Number 421 notes (3) and (12).

(33) Letters and postcards from Sir William Thomson

(1) 14 December 1868, ULC Add. MSS 7655, II/32; Number 316 note (4).

(2) 11 January 1871, ULC Add. MSS 7655, I, a/6; Number 350 note (14).

(3) 17 June 1871, ULC Add. MSS 7655, I, a/19; Number 377 note (1).

(4) 4 January 1872, ULC Add. MSS 7655, I, a/24; Number 393 note (7).

(5) 24 August 1872, ULC Add. MSS 7655, II/62; Number 420 note (8).

(34) Letter from Coutts Trotter
20 April [1871], ULC Add. MSS 7655, II/46; * Number 361 note (2).

(35) Letter from Robert Tucker
8 April 1870, ULC Add. MSS 7655, II/35; Number 311 note (11).

(36) Letter from John Tyndall
'Monday', *Life of Maxwell*: 381.

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