Stefano Bordoni

Crossing the boundaries between matter and energy

Integration between discrete and continuous theoretical models in late nineteenth century British electromagnetism

Università degli Studi di Pavia

La Goliardica Pavese

Viale Golgi, 2 - 27100 Pavia
Tel. (0382) 529570-525709 - Fax 423140
www. lagoliardicapavese.it
e-mail: info@lagoliardicapavese.it

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"We have seen that in some of its properties Radiant Matter is as material as this table, whilst in other properties it almost assumes the character of Radiant Energy. We have actually touched the border land where Matter and Force seem to merge into one another, …. I venture to think that the greatest scientific problems of the future will find their solution in this Border Land, …."

Crookes W. 1879, p. 91

"An immediate fact is at once comprehensible. Its fruits may become evident in the shortest time, such as the various applications of the Röntgen rays and the utilisation of the Hertz waves in wireless telegraphy.

The battle which the theories have to fight is, however, an infinitely wearisome one; indeed, it seems as if certain disputed questions which existed from the beginning will live as long as the science."

Boltzmann L. 1904, p. 592

"The notion that matter has intrinsic powers, and conversely that active influences and forces are in some sense substantial, was never entirely absent from Greek thought, and although apparently banished in seventeenth-century science, it may be said to have returned, heavily disguised, in contemporary physics."

Hesse M.B. 1961, p. 38

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Stefano Bordoni

Foreword

The present study takes into account electromagnetic theories developed in Great Britain in the late nineteenth century, after Maxwell published his mature theory. I have confined my research to the years starting from 1881 to the early 1890s. In 1881, the second edition of Maxwell's *Treatise* appeared, the first chapters revised by Maxwell himself. In 1893 and 1894, J.J. Thomson and J. Larmor published new important theoretical contributions to the interpretation of electromagnetic phenomena. In particular, I have focused on the debate about two basic entities of physics: matter and energy. Those contributions led to important transformations in the concepts of matter and energy and, moreover, brought the two concepts closer to each other. The expression "crossing the boundaries", in the title page, can be interpreted in two ways. First, both matter and energy underwent a conceptual change, wherein matter underwent a sort of dematerialisation and energy underwent a sort of materialisation. Second, some theoretical models broke the borderline between continuous and discrete models, both for matter and energy. That theoretical debate had as a background the problematic link and the floating boundaries between mechanics and electromagnetism, as well as the floating boundaries between the tradition of mathematical physics and emerging theoretical physics. In addition, in the backstage of late nineteenth century electromagnetism, other boundaries were involved: between a macroscopic description and a microscopic description in terms of invisible entities, as well as between the traditional models of contiguous action and at-a-distance-action.

The concept of *energy* had only recently divided by the concept of *force* and the distinction between *kinetic* and *potential* energy recently stated. Nevertheless it was just in the context of electromagnetic theory, in the late nineteenth century, that that distinction was challenged. The concepts of *matter* and *mass*, although coming from a long-lasting tradition and having already experienced a long-lasting debate, were challenged as well.

Early in the 1890s, J.J Thomson and J. Larmor outlined some theoretical conceptions involving a complex interplay between the constitution of matter and the nature of energy. They tried to realise an integration between discrete models and continuous models, both for matter and energy. Although several historians have been concerned with late XIX century electromagnetic theories, I realised that a deeper investigation into the years around 1890 was required, in order to better appreciate the originality of J.J. Thomson and Larmor's theoretical models. I have faced and rephrased traditional questions and interpretations, but I have also put forward new interpretations.

I hope I have managed to perform a double, demanding task: unfolding some underestimated contributions to theoretical physics, and showing how relevant those contributions actually were for the emergence of theoretical physics. My interpretation of that emergence takes into account both institutionalised German and non-institutionalised British traditions. I have pointed out what I consider to be the hallmark of late nineteenth century theoretical physics, namely the awareness of the importance of conceptual models and the wide independence of those models from the other aspects, both mathematical and empirical, of a physical theory. That kind of independence would not have been appreciated a few decades before and was no longer appreciated in the subsequent decades.

In the complex landscape of British and Continental theoretical physics, I do not find completely reliable the historiografical thesis of a competition between a mechanical and an electromagnetic world-view. I hope I have managed to show that such a sharp distinction does not suit late nineteenth century British electromagnetic theories, in particular J.J. Thomson and Larmor's theories.

I have modified in part the received view concerning Poynting, Heaviside, J.J. Thomson and Larmor's electromagnetic theories. With regard to Poynting, I find that his model of energy transfer, as devised in 1885, cannot be looked upon as a mere accomplishment of Maxwell's theory, for it challenged, at least potentially, the continuous character of electromagnetic actions. Heaviside's *macroscopic* theory of electromagnetic phenomena, although troubled by the inability to account for electric conduction, which is well-known to historians, showed an unexpected fruitfulness. Following the analogy with the electromagnetic field, Heaviside managed to outline a not well-known *field-theory* of gravitation.

I have shown the theoretical relevance of Larmor's attempts to unify *conduction* currents and *displacement* currents, as a result of a unified representation of matter and electromagnetic energy. In particular, I have emphasised J.J. Thomson's attempt to outline, in 1893, a discrete and molecular model for electromagnetic radiation. This attempt was not an isolated and odd hypothesis: it could rely on a theoretical background and it was later developed in 1903, in order to account for X-rays scattering by matter.

Furthermore, I have pointed out J.J. Thomson and Larmor's commitment to meta-theoretical and methodological issues. Notwithstanding the different features of their specific theoretical models, both of them explicitly faced the conceptual tension between a mathematical-phenomenological approach to physics and an approach emphasising visualisations, interpretations and provisional models. The latter was just a specific hallmark of the emerging theoretical physics: that hallmark can help us to understand what late nineteenth century theoretical physics really was.

In the present study, I am trying to go beyond the general ascertainment that aether was the unifying concept of late nineteenth century Victorian science: I am inquiring into the different theories, in order to appreciate their specific features and their differences. I find too general the conception of a "Victorian ether theory": I see many *theories* rather than a single *theory*. Moreover, I do not find convincing the claim that those theories were "the embodiment of an extreme option within the mechanical world view". I believe that J.J. Thomson and Larmor undertook a meaningful project of unification, which went beyond pure electromagnetic or pure mechanical world-views.[1]

From the historiographical point of view, I have shown that a better comprehension of the conceptual links between late nineteenth century and early twentieth century theoretical physics requires that different levels of scientific enterprise be taken into account. We should distinguish between the specific features of a theory, on the one hand, and the more general and long-term conceptions, or *conceptual streams*, converging on it, on the other hand.[2] I hope I have managed to show that the above distinction can help to better understand both elements of continuity and discontinuity in the transition between two subsequent historical stages. In particular, I claim that the separate appraisal of specific theoretical features, general conceptual streams and meta-theoretical or methodological commitments can help the context of the so-called Einstenian *revolution* to be better appreciated.

The structure of the book consists of an *Introduction*, "The Landscape", followed by two *Parts*, "Macroscopic models" and "Microscopic models"; at

[1] On the identification of late nineteenth century British physics with a *mechanical* world-view, see Siegel D.M. 1981, p. 263. Harman's appraisal, "Larmor […] developed a theory of an ethereal plenum to unify the electromagnetic and mechanical properties of the ether", seems to me more suitable. See Harman P.M. 1982, pp. 149-50. As Noakes stated, "the notion of a coherent 'Cambridge School' also breaks down on closer analysis of the views of genuine Cambridge physicists". See Noakes R. 2005, p. 420.

[2] I think that the *conceptual streams* I refer to in the present book can satisfy two basic features of Renn's historical epistemology: first, they cross "the borders between science and its context" and, second, they represent "long-range" elements in the history of science. See Renn J. 1996, p. 6; see also p. 2.

the end, an *Afterword*, "A theoretical heritage". In the *Introduction,* I have defined the theoretical context: I have put forward many queries, both theoretical and methodological, emerged from scientific debates taking place in the late nineteenth century. The two *Parts* deal with a detailed analysis of some primary sources, concerning British scientists actively involved in the making up of a *systematic* electromagnetic theory. In the *Afterword*, I have shown the deep conceptual links between theoretical physics of the late nineteenth century and theoretical physics of the early twentieth century.

The *Introduction* points at two targets: first, displaying the theoretical debate taking place in late nineteenth century physics, as emerged from a selection of primary sources; second, undertaking a critical dialogue with the secondary sources committed to the interpretations of those theoretical debates. As I have tried to account for, only at the end of the nineteenth century, theoretical physics was acknowledged, although not everywhere and also in a very problematic way, as a specific *practice* in physics. As part of the emergence of theoretical physics, a sharp debate on aims, methods and principles of physics spread throughout Great Britain and the Continent, giving rise to different representations of the physical world, as well as different ways of conceiving physical knowledge and scientific practice. In this context, I have tried to show how questionable is the identification and the comparison among the so-called physical *world-views*: mechanical, electromagnetic, thermodynamic and energetic world-views.

Part I deals with macroscopic theoretical models of matter and energy in British electromagnetic theories. Starting from Maxwell and his conception of energy embedded in aether or ordinary dielectrics, we encounter the hard concept of "electric displacement", imagined as an elastic reaction of the medium to electric forces. Some years later, J. H. Poynting tried to shift the attention from Maxwell's *electric displacement* to the actual transfer of energy through the medium, at the expense of going back to Faraday's conceptual tools, namely electric and magnetic tubes of force. The new concept of energy flux received an enthusiastic appraisal from some physicists, like O. Lodge, who imagined energy travelling through space and time just like matter. This kind of interpretation of energy seemed questionable and even misleading to others who, like O. Heaviside, refused any substantialisation of energy. He singled out some specific features of Maxwell's theory, namely the medium, the energy and the *fields*, and tried to build a consistent and *purified* theory, getting rid of other conceptual tools, like potentials and Lagrange's equations. He also tried to outline a field theory of gravitation, making use of the same model of energy, spread throughout a medium devoid of any microscopic, invisible structure.

Part II, "Microscopic models", focuses on theoretical models of matter and energy developed by J.J. Thomson and J. Larmor, in particular on their efforts to explain electromagnetic phenomena in terms of actions taking place at a microscopic, invisible level. From a very general point of view, their theories show similar theoretical features; in contrast, their specific features are quite different. They had in common the use of potentials and a Lagrangian approach; moreover, they tried to undertake a dialogue with Continental theoretical models. As to more specific features, J.J. Thomson shared Poynting's attempt to replace Maxwell's force and displacement with Faraday's tubes of force, and tried to represent electromagnetic actions as a discrete set of unit tubes of force connecting unit elements of matter and electric charge. J. Larmor tried to represent matter, electric charge and electromagnetic actions as dynamical structures in a continuous aether, pursuing the aim of a great unification. More specifically, Thomson suggested a bundle of tubes of force as a discrete model for electromagnetic radiation, and tried to integrate it with Maxwell's continuous model of radiation. Larmor suggested the *electron* as a discrete microscopic structure of matter, and tried to integrate it with the continuous structure of Maxwell's electromagnetic aether.

In particular I have inquired into the "thema-antithema" or "the thematic couple" of discreteness and continuity in late nineteenth century British electromagnetism. Both J.J. Thomson and Larmor tried to realize a deep integration between continuity and discreteness in their representations of matter and energy. They tried to realize a deep integration between long-lasting conceptual streams. That commitment to integration represents an element of continuity, linking British electromagnetism to early twentieth century theoretical physics, in particular Einstein's 1905 theories about matter and energy.

If the *Introduction* deals with late nineteenth century scientific context of those theories, the *Afterword* deals with the comparison between those theories and early twentieth century theoretical physics. I have introduced the expression *conceptual streams*, where the word "stream" evinces the stress I put on their dynamic and long-lasting character. I therefore call *conceptual stream* the history of the occurrences of a given *theme*, or *conceptual model*, or *theoretical model*, in the history of science.[3]

I have confined myself to published texts, even though, in some cases, not wholly analysed. The content of knowledge stored in published documents

[3] On "theme" or "themata" see Holton's books, in particular Holton G. 1973, pp. 11 and 13.

was wider, more interesting and open to new interpretations than I expected at the beginning of my research. Besides papers published in specialised scientific reviews, I have taken into account also treatises, short papers or communications in more popular scientific reviews (for instance *Nature*, the "weekly illustrated journal of science"), as well as communications and addresses to scientific meetings. I think that a statement such as "textbooks do not themselves form part of the active research front" does not suit the end of the nineteenth century, although it suits, in part, more recent science. The above quotation echoes a similar distinction stated by Kuhn between "extraordinary science" implemented in scientific papers and "normal science" implemented in textbooks. It has already been noticed that some advanced textbooks, written in the last decades of that century, exhibited new conceptions and were part of the active research front.[4]

According to the old distinction between *internal* and *external* history of science, I would have invented a fragment of *internal* history of science. Nevertheless I have realised that the specific features of late nineteenth century theories could be dissociated neither from general conceptions on nature, nor from methodological and pedagogical commitments. I have tried to throw light on wide-ranging and long-term scientific concepts emerging from those specific physical theories. Just for this reason, the present book displays general conceptions and speculations beside equations, mathematical deductions and other technicalities. I have not dared to split up just what Maxwell, Poynting, Heaviside, Lodge, FitzGerald, J.J. Thomson, Larmor, … as well as Planck, Mach, Helm, Boltzmann, Hertz, … had tried firmly to unite. I claim that the awareness of a problematic link and of a relative independence between the conceptual models and the mathematical structure of a physical theory was just one of the specific characteristics of theoretical physics in the late nineteenth century.

In the last decades, many historians have offered several accounts and interpretations on British electromagnetism in the late nineteenth century. This proliferation of interpretations shows us how problematic the attainment of a certain degree of *objectivity* actually is. The fact is that history cannot exist without historians. They are endowed with their own criteria of selection of data and their intellectual frameworks. In a recent book, C. Smith blamed history of science made "*with the benefit of hindsight*". I definitely agree, even though the query appears to me quite complex. Just as we are living after the events we are committed to

[4] On the distinction between textbooks and active research, see Kuhn T.S. 1962, in T.S. 1996, pp. 136-7, and Kragh H. 1987, p. 126. On the re-evaluation of advanced textbooks, see, for instance, Bevilacqua F. 1985, p. 542.

describe, we can single out from the past what we consider noteworthy. We are the "omnipresent narrator" who cannot be excluded from history.[5] In short, I think that every history is a historical reconstruction, and that a reconstruction cannot be *objective* but can be *reliable*, even though, unfortunately, this *reliability* cannot be further explained.[6] Although no history can be completely reliable, I expect that historians are able to appreciate whether a given history is more reliable or *sophisticated* than another.[7] In this context, I find interesting some remarks expressed by J. Roger about the concept of "historical objectivity", in the 1980s: a non-trivial objectivity is attained when a historian is able to collect and enlighten facts and documents in such a reliable way that those facts and documents could even be used by other historians in order to support a different interpretation.[8]

Finally, I consider the present study intrinsically provisional and not complete. On the other hand, both scientific research and historical research are intrinsically provisional and not complete, however different from each other scientific *methods* and *historical* methods are supposed to be.[9]

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[5] See Smith C. 1998, p. 12.

[6] It seems to me that S. Shapin, in his book on the so-called *Scientific Revolution*, made similar remarks. See Shapin S. 1996, p. 10: "selection is a necessary feature of *any* historical story, and there can be no such thing as definitive or exhaustive history, […] there is inevitably something of 'us' in the stories we tell about the past. This is the historian's predicament, and it is foolish to think there is some method, however well intentioned, that can extricate us from this predicaments"

[7] I am indebted to E. Giannetto and S. D'Agostino for informal talks on this subject; nevertheless, the responsibility for the above sentences is exclusively mine.

[8] Roger J. 1984, p. 299.

[9] In reality, science is intrinsically historical and a historical inquiry allows us to better appreciate what science *really* is.

Introduction: *THE LANDSCAPE*

1. Different models for matter, energy and interactions

In the last page of his *Treatise on Electricity and Magnetism*, Maxwell synthesized the difference between the theoretical model of at-a-distance action and the theoretical model of contiguous action: the keystone of the comparison was the transfer of energy. The question was: "If something is transmitted from one particle to another at a distance, what is its condition after it has left the one particle and before it has reached the other?" In other words, "how are we to conceive this energy as existing in a point of space, coinciding neither with the one particle nor with the other?" Because the two volumes of *Treatise* had been devoted to the development of a theory of electromagnetic phenomena from the point of view of contiguous action, the answer could be nothing else but a conception of energy transferred through a medium. The stress was actually on the medium and on the time of propagation: "whenever energy is transmitted from one body to another in time, there must be a medium or substance in which the energy exists after it leaves one body and before it reaches the other …".[1] In chapter XI of the second volume, he had shown the mathematical equivalence between the representation of electrostatic and electrokinetic energy as placed in electrified bodies and electric currents, and the representation of energy as spread throughout the medium, aether or dielectric matter as well. The first representation stemmed from the model of at-a-distance action between charged bodies or electric currents; the second representation was consistent with the model of contiguous action. Maxwell was aware that the two representations of energy could be, at the same time, equivalent from the mathematical point of view but sharply different from the conceptual point of view.[2] As he claimed in his subsequent booklet *Matter and Motion*, energy was embedded in aether or matter, and it could be transferred through either aether or matter; conversely, the properties of matter could be appreciated just through the transformations of energy.[3]

The mathematical expressions corresponding to the first representation consisted in some integrals involving "the components of the electromagnetic momentum" of a circuit, "the components of the density of the current at any point of the conducting circuit", and "the charge of electricity at a place". The mathematical expressions corresponding to the second

[1] Maxwell J.C. 1881, pp. 448-9.

[2] Maxwell J.C. 1881, p. 251. For other details, see chapters 6 and 7 of the present book.

[3] Maxwell J.C. 1878, pp. 164-5.

representation consisted in other integrals, involving the "magnetic force", the "magnetic induction", the "electric displacement" and the "electromotive force". Following the already established distinction between potential and kinetic energy, Maxwell identified electrostatic energy with potential energy and magnetic-electrokinetic energy with kinetic energy.[4]

It is worth mentioning that the mathematical equivalence can be appreciated only in the case of steady currents: in the general electromagnetic case, the two components do not preserve a separate meaning and we have to take into account the concept of an electromagnetic field without any specific distinction between kinetic and potential components. Indeed, that distinction between two components, at least in the sharp way stated by Helmholtz, had already been challenged by some Continental theories. In Weber's electrodynamics, for instance, there was an *electrokinetic* potential depending on velocity.[5] Besides, that mathematical equivalence, in some sense, hid the specific features of Maxwell's field theory. This is a very interesting issue: as some historians have remarked, in his *Treatise*, Maxwell did not develop the specific features of his theory. Even when he tried a Lagrangian approach to electricity and magnetism, he took into account only "closed currents in well defined curvilinear conducting circuits". In other words, he applied *dynamical* methods "precisely to the case *not* characteristic of what we think of as 'Maxwell's theory'".[6]

Some queries concerning electromagnetic energy cast light on the close relationship between matter and energy in the context of Maxwell's electromagnetic theory. He introduced new specific conceptions for matter and energy, and the new conceptions were tightly interwoven with the planning of a systematic electromagnetic theory. More specifically, the nature of the link between matter and energy was tightly connected to the nature of electromagnetic actions.

The models of matter could rely on a tradition as long-lasting as that of the models of action. On the contrary, the models of energy were an

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[4] Maxwell J.C. 1881, pp. 248-51. The word *energy* and the distinction between *kinetic* and *potential* had only recently been established: see, for instance, Elkana Y. 1974, p. 118, and Harman P.M. 1982, p. 59.

[5] Helmholtz had criticised Weber's electrodynamics as he thought it represented a violation of his Principle of conservation of energy. For a detailed account, see Bevilacqua 1983, pp. 122-33, in particular p. 123.

[6] Stein H. 1981, p. 311-2. The same concept had been pointed out by Hirosige: see Hirosige T. 1969, p. 192-3. Contrary to what we would expect, "[t]he whole first volume of Maxwell's *Treatise* deals with electrostatics and steady currents, and only a limited number of pages of the second volume deal with the theory of the electromagnetic field". See D'Agostino S. 2000a, p. 117. See also Bevilacqua F. 1983, p. 27 and 150.

offspring of the last decades of the nineteenth century. Moreover, for some years around the middle of the nineteenth century, the concept of energy overlapped with the concept of force.[7]

In general, since the eighteenth century, the conceptual clash between models of matter had been deeply connected to the corresponding clash between models of action. In other words, within a certain approximation, we can put discreteness of matter and action at a distance on one hand, and continuity of matter and contiguous action on the other hand. The model of forces acting at a distance between couples of atomic bodies was in competition with the model of actions propagating through an all-pervading continuous medium.[8] Actually, in Maxwell's electromagnetism and Hertz's electrodynamics, contiguous action matched up to the theoretical model of continuous media; in Weber's and Helmholtz's electrodynamics, action at a distance matched up to the theoretical model of a discrete structure of media. As I will discuss in the following pages, Hertz rejected the model of forces at a distance and the atomic model was associated to that rejection, for "in tracing back phenomena to force we are compelled to turn our attention continually to atoms and molecules".[9] At the beginning of the twentieth century, the existence of another meaningful link concerning the models of action was suggested by J.T. Merz in his *History*, which is of particular interest to us as it bears direct witness to the transformations which took place in the late nineteenth century. Merz pointed out that the theoretical model of action at a distance, or what he called the "astronomical view", was built following the analogy with "physical astronomy". The conceptual model of contiguous action was built following the analogy with other physical sciences or "processes of emanation, of a gradual spreading out, of a flow or conduction".[10] In other words, there was a definite

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[7] That overlap was widely analysed by Y. Elkana. See Elkana Y. 1974, chapters II and V.

[8] Voltaire offered a famous and synthetic portrait of the competition between the two representations of matter and interactions: see Arouet F.M. 1733, pp. 109-10, quoted in Hesse M. 1961, pp. 169-70. Recently D'Agostino put Maxwell's and Hertz's "pure-field theories", stressing "continuity and locality", on the one hand, and Weber and Helmholtz's "discreteness and distant action" on the other. See D'Agostino S. 2000b, p. 398.

[9] Hertz H. 1894, in Hertz H. 1956, p. 17. See also p. 14, wherein it stated that, up to the middle of the nineteenth century, the ultimate aim of physical science "was apparently to explain natural phenomena by tracing them back to innumerable actions-at-a-distance between the atoms of matter". D'Agostino remarked: "… since he had abolished the concept of force from among the foundational concepts of his mechanics, atoms and molecules found no place there as well. He believed that atoms and atomic forces were among the basic features of an action-at-a-distance theory such as Weber's." (D'Agostino S. 2000b, p. 404)

[10] Merz J.T. 1912, pp. 79. Merz was "an Englishmen who received a rigorous education in Germany in mathematics, physics, philosophy and theology" (Cahan D. 2003, p. 5). At the

connection between the two models of action and some sections of physical sciences.

At first sight, within a certain approximation, Merz's reconstruction of the emergence of models of action in physical sciences in the nineteenth century seems quite reliable. In the first class, we could put the action at a distance and the discreteness of matter, both associated to mechanics. In the second class, we can put the contiguous action and the continuity of every medium, both associated to the emergence of new theories involving optics, electricity and magnetism. Nevertheless, on more careful examination, two flaws appear. First, contiguous action cannot be put in connection univocally with a given field of physical sciences: early British electromagnetism was associated to contiguous action while Continental electrodynamics was associated to action at a distance. In some way, both models can be traced back to the tradition of mechanics. At-a-distance action had connections with celestial mechanics whereas contiguous action had connections with the mechanics of continuous. Second, in the last decades of the nineteenth century, two *new* fields of physics, the electromagnetic theory and thermodynamics, underwent a theoretical shift from continuous models to discrete models. To sum up, I find the link between models of matter and models of action definitely more convincing than the link between models of action and sections of physical sciences. At the same time, I will show that, in the late nineteenth century, even the former link was challenged.

In any case, both at-a-distance and contiguous action models belonged to long-lasting traditions. Merz associated contiguous action to a conceptual tradition going from Huygens to Euler and then to Maxwell. In the second half of the twentieth century, M. Hesse tried to outline an even longer tradition, going from the ancient Greeks to the twentieth century. She traced back the basic features of the conceptual model of contiguous action to Stoic philosophy.[11] Furthermore, she noted that contiguous action split into two different representations, depending on the microscopic representations associated to the macroscopic continuous structure of the medium. Hesse remarked that when Maxwell denied the existence of forces acting "at sensible distances", he let the reader imagine two ways of

turn of the twentieth century, he wrote *A History of European Thought in the Nineteenth Century*, in two volumes.

[11] See Merz J.T. 1912, pp. 7-8, footnote 2, and Hesse M. 1961, pp. 76 and 281. In order to show the long-term effects of the debate on the models of action, she reported that, in the middle of the twentieth century, two distinguished physicists, Wheeler and Feynman, still claimed that the "field" representation, namely contiguous action, and action-at-a-distance representation "should be intertranslatable", and should be looked upon as complementary aspects in our description of nature.

conceiving the model of contiguous action. In the case of a medium endowed with an intrinsic continuity, the action can actually propagate with continuity even at microscopic level. In the case of a medium endowed with a microscopic discrete structure, the action is only macroscopically contiguous but microscopically at a distance.[12]

Finally, we cannot forget that, besides the better-known theoretical traditions of at-a-distance action and contiguous action, there was also a third tradition, namely the model of *retarded potentials* or the model of *at-a-distance delayed action*. Gauss in 1845, Riemann in 1858, the Danish scientist L. Lorenz in 1867, C. Neumann in 1868 and Clausius in 1876, all tried to realize an integration between some features of action at a distance and some features of contiguous action; in particular, they devised an electromagnetic action at a distance, but propagating in a finite time. The most significant results were achieved by L. Lorenz in 1867 and C. Neumann in 1868.[13]

Following Jammer's classical book on the history of the concept of mass, the emergence of the concept of inertial mass was quite a linear process or "a gradual development which started with Johannes Kepler and terminated with Leonhard Euler", whose "rudimentary sources can be traced back to the Neoplatonic idea of the inertia and inactivity of matter as opposed to the vitality and spontaneity of mind".[14] That development was indeed not so gradual, involving some conceptual discontinuities, and it is questionable whether it may be really qualified as a development or, rather, as an emergence and re-emergence of some basic models. Moreover, besides the supposed conceptual stream going from Kepler to Euler, there were other conceptual streams, as Jammer himself showed in some detail. The models of matter and the concepts of mass had always been quite problematic, even in so-called *classical physics*. Jammer pointed out several concepts of mass emerging from Newton's *Principia*: first, a specific quantity corresponding to bulk multiplied by density, second, a *vis insita* looked upon as *resistance* to accelerated motion, third, a *vis insita* looked upon as *natural disposition* to not accelerated motion, fourth, a receptor of gravitational actions, and, finally, as a source of gravitational actions. Mass was subsequently defined in terms of velocity, making use of the conservation of linear momentum in a collision, or in terms of acceleration. In the late nineteenth century Hertz

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[12] Hesse M. 1961, pp. 207-8.

[13] See Hesse M. 1961, p. 221, and Bevilacqua F. 1983, pp. 99-108.

[14] Jammer M. 1961, p. 5.

criticized Newton's double concept of inertia, namely inertia as mass and inertia as force.[15]

Two meaningful steps in the history of the concept of mass are linked to some developments in late nineteenth century electromagnetism. The first step had already been pointed out by Merz: Maxwell's conception of dielectric "somewhat obliterated the clear distinction between empty space and space filled with insulating matter, such as air".[16] The fact is that, in Maxwell's electromagnetic theory, *empty space* was not empty but filled with aether. The second step was undertaken in 1881 by J.J. Thomson: a part, at least, of the inertia of an electrically charged body had an electromagnetic nature. Starting from Crookes' experiments on electric discharges in vacuum-tubes, J.J. Thomson inquired into "the force existing between two electrified bodies" as well as "the magnetic force produced by such a moving body", in order to attain both "a test of the theory" and "a guide to future experiments". He first took into account the case of "a charged sphere moving through an unlimited space filled with a medium of specific inductive capacity K". Then he wrote down a series of theoretical deductions, based on Maxwell's concept of *electric displacement*: its variation in a dielectric produces "effects analogous to those produced by ordinary currents flowing through conductors".[17] Besides this, there was an assumption concerning energy: "a field in which electric currents exist is a seat of energy". As a consequence, "the motion of the charged sphere has developed energy" and the production of this amount of energy has an effect on the moving sphere: it "must experience a resistance as it moves through the dielectric". Differently from the electric current in conductors, Maxwell's *displacement current* was not expected to undergo a sort of "dissipation of energy through the medium". In this case, the *resistance* should be of a different kind: "it must be equivalent to an increase in the mass of the charged moving sphere". The amount of this *electromagnetic* increase of the mass was $(4/15)\mu e^2/a$, where μ was the coefficient of magnetic permeability, q the charge on the sphere and a the radius of the moving sphere. In terms of both electric and magnetic properties (K and μ) of the medium, this expression transforms into $(4/15)\mu K^2 V^2/a$, where V was "the potential of the sphere".[18] Subsequently, in 1889, Heaviside came to the expression $2q^2/3ac^2$,

[15] Jammer m. 1961, pp. 70-1 and p. 125; see also, p. 90 (in particular on Saint-Venant), pp. 92-100 (in particular on Mach), and p. 225 (in particular on Hertz). On Hertz's criticism see the following pages.
[16] Merz J. T. 1912, p. 194.
[17] Thomson J.J. 1881, pp. 229-30.
[18] Thomson J.J. 1881, pp. 230-34.

where q and a were charge and radius of a conducting sphere and c was the velocity of electromagnetic waves.[19]

P.G. Tait, in a book published in 1885, *Properties of Matter*, which was "the introduction to the course of Natural Philosophy in Edinburgh University", showed how problematic the concept of matter still was. He reported a list of nine different definitions singled out from a large collection, and pointed out "the mutual incompatibility of certain pairs of these definitions". Even more problematic or "probably beyond the range of human intelligence" appeared to Tait the query about "the ultimate nature of matter": he thought that we should confine ourselves only to inquiring into the *structure* of matter. He claimed that the most interesting theory on this structure was W. Thomson's "hypothesis of *Vortex Atoms*". It seemed "of a perfectly unique, self-contained character", although the "hard *Atom*" of Democritus and Leucippus "survives (as at least an unrefuted, though a very improbable hypothesis) to this day". Nevertheless W. Thomson's theory of atoms as the "rotating part of a fluid which fills all space" failed in explaining inertia: indeed, Tait claimed, the inertia of matter was not explained, but assumed as a consequence of the inertia of the fluid.[20]

W. Thomson (later Lord Kelvin) is one of the characters in the background of the present research. The other chief character in the background, H. von Helmholtz, in 1858 had published some theoretical researches about vortex filaments and vortex rings, which could permanently settle down in a perfect fluid. In 1867, Thomson dared to identify vortex rings with atoms of ordinary matter and developed the corresponding model[21]. Some decades ago,

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[19] This is the expression nowadays accepted by physicists. See Heaviside O. 1889a, pp. 325-6. See also Jammer M. 1961, pp. 138-41.

[20] Tait P.G. 1885, pp. 12-16, 19 and 21. See p. 21: "The fluid, whatever it be, must have inertia:- that is one of the indispensable postulates of v. Helmholtz investigation; and the great primary objection to Thomson's theory is, that it explains matter only by the help of something else which, though it is not what we call matter, must possess what we consider to be one of the most distinctive properties of matter." P.G. Tait, Maxwell's friend and close scientific correspondent, held the chair of natural philosophy at the University of Edinburgh from the 1860s to the end of the century.

[21] W. Thomson held the chair of natural philosophy at the University of Glasgow from the early 1850s to his retirement. In 1842, he had shown the mathematical equivalence between theories of thermal phenomena and electrostatic phenomena. See Thomson W. 1842, in Thomson W. 1872, pp. 1-14. For an account of W. Thomson's theories, see Siegel D.M. 1981, pp. 241-2, and Darrigol O. 2000, pp. 77, 114-15, 117, 133. A short account of Helmholtz's results can be found in Siegel D.M. 1981, pp. 255-6. In 1883, J.J. Thomson tried to apply W. Thomson's model to the kinetic theory of gases: "The pressure of a gas is one of the first things a kinetic theory of gases has to explain. Sir William Thomson gives the following explanation of the pressure of a gas on the vortex atom theory …". See Thomson J.J. 1883, p. 109. Giusti Doran pointed out that W. Thomson got involved in

Giusti Doran remarked that the conception of matter as a specific state of motion in an all-pervading medium echoed Leibniz's former conceptions, and she stressed the importance of this conceptual link between Leibniz and W. Thomson. Furthermore she looked upon W. Thomson's theory of matter as "the basis of a unified field theory of matter" or "the modern conception of space", which "both belonged to and subverted the mechanical philosophy". I find this thesis quite suggestive, even though whirling motions taking place in a medium seem to me a model of matter somehow different from Leibniz's conception of a matter endowed with a sort of intrinsic power.[22] Nevertheless, Thomson himself, in a *Friday Evening Lecture* held in 1860 before the Royal Institution, explicitly associated his theoretical model to Leibniz's conceptions. Moreover, he pointed out another meaningful link between the theoretical model of matter as a dynamical structure in a continuous medium, on the one hand, and the theoretical model of contiguous action, on the other hand. He claimed that the "belief in atoms and in vacuum" had to be looked upon "as a thing of the past", and that "we can no longer regard electric and magnetic fluids attracting or repelling at a distance as realities". According to W. Thomson, this was just the conception "against which Leibnitz so earnestly contented in his memorable correspondence with Dr. Samuel Clarke".[23]

Merz pointed out that, "the vortex-atom theory has marked an epoch in the history of thought", as being "the most advanced chapter in the kinetic theory of matter, the most exalted glimpse into the mechanical view of nature". Nevertheless, it suffered "two fundamental difficulties", namely the unexplained origin of both inertial and gravitational properties of matter.[24] I agree with Merz on the importance he gave to that model of atom, in the contexts of both history of physics and more general history of scientific thought. This importance is actually underestimated in the received view of

circulatory structures in the aether two years before Helmholtz's paper on vortex rings. See Giusti Doran B. 1975, p. 189.

[22] See Giusti Doran B. 1975, pp. 140 and 142, footnote 7. Leibniz's "monad" was the basic entity in nature, and it was a dynamical entity. It would undergo transformations under the effect of an "internal principle" ("un *principe interne*"): it would be the seat of actions and connections ("une pluralité d'affections et de rapports"). Every monad would be influenced by every action taking place in every side of universe ("tout corps se ressent de tout ce qui se fait dans l'univers"). Nothing is passive or idle in the universe ("il n'y a rien d'inculte, de stérile, de mort dans l'univers"). See statements 10, 11, 13, 15, 21, 61 and 69 in Leibniz's *La Monadologie*.

[23] Thomson W. 1860, in Thomson W. 1872, p. 224.

[24] Merz J.T. 1912, pp. 62 and 64-6. According to Merz, the vortex-atom theory, in the context of British natural philosophy, represented a sort of revenge of Descartes on Newton. See p. 62, footnote 1.

the history of science, perhaps because of a misleading retrospective attitude.[25] The fact is that the model suffered from many difficulties and was criticized both in Great Britain and on the Continent. As Kragh pointed out, the criticism concerned the purely mathematical character of the perfect fluid, and the unknown cause which made motion emerge from that perfect fluid. I find quite convincing Kragh's claim that "the theory explained too much – and therefore too little". In the end, scientists lost interest in it "not primarily because it disagreed with empirical data but rather because of its lack of progress."[26] Thomson himself, in the already quoted 1860 lecture, expressed the belief that "electricity in itself is to be understood as not an accident, but an essence of matter": the vortex-atom model, at that stage, could not account for that supposed fundamental property of matter. Although the model was abandoned in the 1890s even by its author, the conceptual stream survived and found a new implementation in Larmor's electron. Whereas Kragh sees "if only indirectly, a kind of revival" I see a subsequent stage in a long-lasting conceptual stream.[27]

In this context, it is worth mentioning Maxwell's passages in support of the theoretical model of atom as a hydrodynamical ring. In the 1875 edition of *Encyclopaedia Britannica*, he stated that, although the "small hard body imagined by Lucretius, and adopted by Newton, was invented for the express purpose of accounting for the permanence of the properties of bodies", it failed "to account for the vibrations of a molecule as revealed by the spectroscope". On the contrary, "the vortex ring of Helmholtz, imagined as the true form of atom by Thomson, satisfies more of the conditions than any atom hitherto imagined". He found that the main satisfactory feature of the model was its "permanent" and, at the same time, pliable structure.[28] At the same time, Giusti Doran reminded us that, before 1875, Maxwell did not trust in a dynamical model of matter. In 1873, in a paper published in *Nature*, he wondered whether matter could be infinitely divisible. He stated that

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[25] Apart from B. Giusti Doran and H. Kragh, as far as I know.

[26] Kragh H. 2002, pp. 88-9, 92 and 95.

[27] Kragh acknowledged that, although Larmor's electrons "emerged on the ruins of the vortex atoms, so to speak, the two concepts had much in common". This claim is consistent with his interest in inquiring into "the heritage of the vortex atom, that is, certain traces or similarities to it that can still be found in modern physics". See Kragh H. 2002, pp. 34 and 71.

[28] See Maxwell 1875, in Maxwell 1890, vol. II, pp. 470-1: "In the first place, it is quantitatively permanent, as regards its volume and its strength, - two independent quantities. It is also qualitatively permanent, as regards its degree of implication, whether 'knottedness' on itself or 'linkedness' with other vortex rings. At the same time, it is capable of infinite changes of form, and may execute vibrations of different periods, as we know molecules do."

"[a]ccording to Democritus and the atomic school, we must answer in the negative" and that the answer was common to "the atomic doctrine of Democritus, Epicurus, and Lucretius, and, I may add, of your lecturer".[29]

W. Thomson's kinetic model of matter can be placed alongside a conceptual tradition wherein matter is not a fundamental entity but is derived from some kind of dynamism. M. Hesse identified a conceptual stream or "a physical picture in which force is more fundamental than matter" with the ideal line connecting, in the chronological order, Leibniz, Boscovich, Kant and Faraday.[30] This can be accepted provided that we acknowledge that this common conceptual stream went through scientific theories and natural philosophies quite different from each other. Historians have always found it difficult to give a definite interpretation of Leibniz's concept of mass, because of some changes intervening in the subsequent stages of his philosophical and scientific system. Nevertheless we can say that, in the final stage of that system, mass became a dynamical entity, endowed with an active power. In some way, matter became force or, in other words, first came force and then matter.[31] The impenetrability of matter was a dynamical effect, the consequence of a repulsive action. As Jammer stated, "Inertia as the principle of the continuation of motion" was looked upon as the effect of an action coming from within the body. Furthermore, "inertia as the principle of resistance" to moving forces "must be of the same category as these, that is, it must be a force." I disagree with Jammer's claim that Leibniz's concept of force "was not very fruitful and productive for the advancement of theoretical physics".[32] I find that a general conceptual stream, which involved Leibniz and then W. Thomson, found interesting implementations in Larmor and J.J. Thomson's theories, and then re-emerged in the twentieth century.

In the 1890s, on the Continent, W. Ostwald, a distinguished physicist-chemist (one of the main upholders of *energetism*, and later Nobel Prize winner), starting from a different methodological perspective, advocated the exclusion of matter from the list of fundamental physical entities: "the concept of matter, which has become indefinite and contradictory, has to be

[29] Maxwell J.C. 1873, p. 437. See also Giusti Doran B. 1975, p. 192: "Thomson was isolated during the early years of his vortex-atom theory when he opposed the Lucretian atom and challenged the foundations of the kinetic theory of gases. As late as 1873 Maxwell agreed with Democritus, Epicurus, and Lucretius' view …"

[30] Hesse M. 1961, p. 166.

[31] See footnote 22. For other details, see Jammer M. 1961, pp. 76-80.

[32] Jammer M. 1957, pp. 161-2 and 187.

replaced by the concept of energy".[33] In the same decade, Hertz took the opposite way: in Hertz's physics, forces were replaced by hidden masses and by their hidden motions. He also criticized Newton's dualistic conception of force: it was both an action on a given body, as expressed by the first two laws, and a relationship between two bodies, as expressed by the third law.[34] In the case of a stone tied to a string and moving along a circle, Hertz criticized the interpretation in terms of centrifugal forces balancing or opposing centripetal ones. He wondered what exactly was the physical meaning of those supposed centrifugal forces: were they "anything else than the inertia of the stone?" In this case, he added, why should we take "the effect of inertia twice into account, firstly as mass, secondly as force?". Moreover, forces were assumed to be the *causes* of a change in uniform motions, whereas the so-called centrifugal forces were looked upon as the *effects* of non-inertial motions. Hertz was dissatisfied with that clash between causes and effects and stated that "centrifugal force is not a force at all". His criticism became even sharper when he took into account a body at rest, although imagined as submitted to the action of a great number of forces. On "a piece of iron resting upon a table" many kind of forces are supposed to act: "every atom of the iron" should experience the gravitational attractions of "every other atom of the universe", as well as electric attractions or repulsions, magnetic forces and "various kinds of molecular forces". Hertz found quite strange that "all the forces are so adjusted amongst each other that the effect of the whole lot is zero". What we actually see is only the effect of that supposed sum of forces: "in spite of

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[33] Ostwald W. 1896, pp. 159-60: "Ihren schärfsten Ausdruck hat dies erkenntnisstheoretische Postulat durch meinen Hinweis erhalten, dass der unbestimmt und wider spruchsvoll gewordene Begriff der Materie durch den der Energie zu ersetzen ist, da nur auf solchem Wege die Uebereinstimmung zwischen dem, was wir durch unsere Formeln zum Ausdruck bringen, und dem, wovon wir zu reden pflegen, hergestellt werden kann." See also McCormmach R. and Jungnickel C. 1986, Vol. 2, p. 220, and Harman P.M. 1982, p. 147. W. Ostwald held the sole German chair of physical chemistry, at the University of Leipzig, from 1887 until his retirement.

[34] Hertz H. 1894, in Hertz H. 1956, p. 6: "The force spoken of in the definition and in the first two laws acts upon a body in one definite direction. The sense of the third law is that forces always connect two bodies, and are directed from the first to the second as well as from the second to the first. It seems to me that the conception of force assumed and created in us by the third law on the one hand, and the first two laws on the other hand, are slightly different. This slight difference may be enough to produce the logical obscurity of which the consequences are manifest in the above example." Miller pointed out an interesting analogy among Hertz's concealed masses, Newton's concealed forces and energetists' concealed energies. See Miller A.I. 1984, p. 78. H. Hertz held the chair of physics at the Technische Hochschule in Karlsruhe from 1885 to 1889. There he undertook his researches on electromagnetic waves. Then he moved to Bonn.

thousand existing causes of motion, no motion takes place". He looked for "other representations" of mechanics, "more closely conformable to the things which have to be represented", where the concept of force was banned.[35]

Even the rejection of forces could rely on a tradition. In the 1870s, G.R. Kirchhoff, from 1875 colleague of Helmholtz in Berlin, rejected forces. His mathematical physics was based only on the concepts of space, time and mass, all of them assumed as not problematic. In his *Lectures on some Recent Advances in Physical Science*, held in 1874 spring and published in 1876, Tait stated that, in physical science, the *status* of energy is far more important than the *status* of force. Tait considered energy as a primary physical entity and force as a secondary entity derived from the first: "the so-called force in any direction is merely the rate of transference, or of transformation, of energy per unit of length for displacement in that direction". Moreover, force "has not necessarily objective reality any more than has Velocity or Position". Even though force still seemed a "very useful" idea, the advance of science would probably have banished it, just like "Caloric and Phlogiston" or even "Electric Fluid". Nevertheless, Tait acknowledged that in 1876 there was still a conceptual misunderstanding of the concept of energy and a corresponding misuse of the word "force" in the British scientific community.[36] In the "Preface to second edition" of his *Lectures*, Tait found that the concept of "force" deserved some attention and was "of great importance at the present time", even though within some decades "it will probably have lost all but a mere antiquarian interest".[37] The last chapter of the book consisted of a lecture held at Glasgow in 1876, before the British Association, and was devoted to the concept of force. He criticized the ambiguous use of the word "force", and made use of sharp statements like "there is probably no such *thing* as force at all!", "it is, in fact, merely a convenient expression for a certain 'rate'", or "a good deal of the confusion about Force is due to Leibniz".[38] That search for clarity and correctness was tightly connected to a conception of science as a strictly

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[35] Hertz H. 1894, in Hertz H. 1956, pp. 6, 13-14 and 25.

[36] Tait P.G. 1876, pp. 16-7. See p. 17, footnote 1: "Great confusion has been introduced into many modern British works by a double use of the word Force. It is employed, without qualification, sometimes in the sense of force proper …, sometimes in the sense of energy." On the pliability of the concept of *Kraft* introduced by Helmholtz in his 1847 *Erhaltung der Kraft*, see Elkana Y. 1974, chapters V and VII.

[37] Tait P.G. 1876, p. xiv.

[38] Tait P.G. 1876, p. 41.

monogamous marriage between experiment and mathematics, without any conceptual or theoretical component.[39]

The same interpretation of *force* as an abstract concept, and *energy* as the real concept was shared by Merz in his historical reconstruction. He pointed out the emergence of a specific need for "the creation of a new vocabulary", in the second half of the nineteenth century. In his chronological reconstruction, he emphasised "the introduction of the term 'work' by Clausius in 1850, and of "the term 'energy' by William Thomson, who adopted it from Young in the year 1852". He emphasized the innovative nature of the concept of energy, comparable with Darwin's evolutionism as to importance: it required that "the older text-books … had to be rewritten".[40]

In his 1876 *Lectures*, Tait stated that the Principle of the conservation of energy could be expressed in terms of a mutual balance between two kind of energy: "energy of position, or *Potential Energy*, on one side, and "energy of motion or *Kinetic Energy*", on the other side. That balance required that "the amount of potential energy lost in every stage of the operation is precisely equal to the amount of kinetic energy gained".[41] Potential energy was qualified by Tait as a "dormant, or passive, form" of energy, while kinetic energy was qualified as "active". Nevertheless, whereas the concept "of kinetic energy is a very simple one", the concept of potential energy "is not by any means so easy or direct". That two bodies, when taken away from each other, should experience their potential energy to be increased, appeared to Tait "still one of the most obscure problems in physics". The theory of LeSage, who pursued the "hopeful attempt … to explain the mechanism of Gravitation" by the collision of "ultramundane Corpuscles",

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[39] Tait P.G. 1876, p. 342: "Nothing can be learned as to the physical world save by observation and experiment, or by mathematical deductions from data so obtained."

[40] Merz J. T. 1912, pp. 115 and 136. See also p. 116: "It is now being recognised more and more that the word 'force' applies only to a mathematical abstraction, whereas the word 'energy' or 'power to perform work', applies to a real quantity;…" He distinguished three different aspects in the emergence of the new entity. See p. 137: "The first philosophical generalisation were given by Mohr and Mayer; the first mathematical treatment was given by Helmholtz; the first satisfactory experimental verification by Joule, during the second quarter of the century." He credited Poncelet with having introduced the term mechanical work in 1829, and having stated that "the inertia of matter transforms work into vis viva and vis viva into work". See pp. 98-101. Tait also credited Young with having been the first to use "the term ENERGY to signify the power of doing work". See Tait P.G. 1876, p. 358. On the use of the word "energy in Young's *Lectures on Natural Philosophy*, see also Elkana Y. 1974, p. 25.

[41] Tait P.G. 1876, pp. 18-9. On the expressions *kinetic* and *potential* energy Merz reported that W. Thomson distinguished between *dynamical* and *statical* energy and then, in 1853, Rankine introduced "the terms actual (or sensible) energy and potential (or latent) energy", soon followed by Thomson himself. See Merz J.T. 1912, pp. 138-9.

appeared to Tait as the most clever and fruitful solution to the puzzle of potential energy. Theories of the same kind, Tait wrote, "will probably lead us to regard all kinds of energy as ultimately Kinetic".[42]

He stressed the "objectivity" of energy, the fact that it "possesses to the full as high a claim to objective reality as matter possesses", even though it is "by no means so tangible". He mentioned the similarity between matter and energy and expressed the "grand principle of Conservation of Energy" in terms of "portion(s) of energy". Those portions could not "be put out of existence" or "brought into existence, by any process at our command". The principle of "invariability of the quantity of energy in the universe" was looked upon by Tait as "a companion statement to that of invariability of the quantity of matter". Moreover, "so far as we yet know", matter undergoes only preservation while energy undergoes both preservation and transformation: in other words, matter cannot be "transmuted from one kind to another". According to Tait, the future of matter would have been decided by gravitation, and the future of energy by a law of dissipation.[43]

In his 1885 book on matter, Tait introduced a new couple of fundamental entities in physics: among the first statements of the introductory chapter we read: "In the physical universe there are but two classes of things, MATTER and ENERGY".[44] Among the different kinds of matter, Tait listed air and water, as well as luminiferous aether. Among the different kinds of energy he listed waves and heat, as well as electric currents. He pointed out that they were all "examples of energy associated with matter". In many passages he emphasised what he considered the keystone of physics: the deep link between matter and energy. More specifically, he stated that "Energy is never found except in association with matter" and probably "energy will ultimately be found … to depend upon motion of matter". Nevertheless, this symmetry between matter and energy was broken by two elements: matter consists of "parts which preserve their identity" while energy "cannot be identified"; in addition, matter "is simply passive" or "*inert*" while energy "is perpetually undergoing transformations". In that conceptual context we find a sharp statement against action at a distance,

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[42] Tait P.G. 1876, pp. 358-9 and 362. On LeSage's theory, see Jammer M. 1957, pp. 192-4; see also the reference in chapter 12 of the present book.

[43] Tait P.G. 1876, p. 18. On the future of the universe, see pp. 20-1: "Thus, so far as we can as yet determine, in the far distant future of the universe the quantities of matter and energy will remain absolutely as they now are – the matter unchanged alike in quantity and quality, but collected together under the influence of its mutual gravitation, so that there remains no potential energy of detached portions of matter; the energy also unchanged in quantity, but entirely transformed in quality to the low form of uniformly diffused heat."

[44] Tait P.G. 1885, p. v and p. 2.

which was qualified as "a very old but most pernicious heresy, of which much more than traces still exist among certain schools, even of physicists".[45]

In 1887, when he held the chair of "theoretical physics" at Kiel University, Planck wrote a treatise on the conservation of energy. Three elements appeared tightly connected: the interpretation of electromagnetic phenomena, the interpretation of the conservation of energy and the choice between the theoretical model of contiguous action and the theoretical model of at-a-distance action. The latter appeared to Planck as the more general, for it could take into account the whole universe. Actually, in the action-at-a-distance model, the force acting on a given body can be considered as the sum of all forces exerted by whatever distant source of the universe. On the contrary, contiguous action had a narrower scope but it appeared to Planck more suitable in order to explain electromagnetic phenomena. He decided to explore the consequences of contiguous action, even from the methodological point of view. He tried to combine contiguous action with the conservation of energy and found for this combination the name "infinitesimal theory". That *infinitesimal* approach involved all physics: every action on an infinitesimal volume could be transmitted, in a finite time, through the surface surrounding it.[46] Energy, electromagnetic or not, could be interpreted as something similar to matter. Not only could energy neither be created nor destroyed, but it could not disappear from a given place and instantaneously appear in another distant place. Energy could flow through the boundaries of a volume, just as matter did. The principle of conservation of energy became closely linked to specific ways of transfer of energy. According to this conception or "infinitesimal theory, energy, like matter, can change its place only with continuity through time". The energy of a material system could be represented as a series of units or elements: "every definite element approaches its place and just there can be found". In brief, Planck claimed that the infinitesimal theory corresponded to the following conception: "the energy of the whole system can be looked upon as the sum of the energies of every single system".[47] The conception of

[45] Tait P.G. 1885, pp. 3-6.

[46] Planck M. 1887, pp. 244: "Wenn die Infinitesimaltheorie sich also bestätigt, so ist damit zugleich ein neues allgemeines Naturgesetz erwiesen, nämlich das Gesetz, dass alle Veränderungen, die in und an irgend einem materiellen Element vor sich gehen, vollständig bestimmt sind durch die augenblicklichen Vorgänge innerhalb und an der Grenze des Elements. Er versteht sich, daß dieser Satz tief hineingreift in das Wesen und die Wirkungsweise aller Naturkräfte."

[47] Planck M. 1887, pp. 245: "Nach der Infinitesimaltheorie dagegen kann Energie, wie Materie, nur stetig mit der Zeit ihren Ort verändern. Die in einem geschlossenen Raum befindliche Energie kann vermehrt oder vermindert werden nur durch solche äußere Wirkungen, die durch physikalische Vorgänge in der Grenzfläche des Raumes vermittelt

"elements" of energy travelling through space and time was an important contribution to the scientific debate in the late nineteenth century. Moreover, that conception helps us to better understand the conceptual roots of the theoretical researches Planck subsequently undertook on the electromagnetic and thermodynamic properties of radiation.[48]

Just how widespread the acknowledgement of a conceptual link between matter and energy was in the late nineteenth century is shown by Poincaré's representation of electromagnetic energy as something flowing as "a fictitious fluid". In 1900, what actually prevented Poincaré from the complete identification with "a real fluid" was the fact that "this fluid is not indestructible".[49]

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werden, man kann also auch hier von einem Hindurchgehen der Energie durch diese Fläche reden. Dann läßt sich die Energie eines materiellen Systems stets in Elemente zerlegen, deren jedes einem bestimmten materiellen Element zukommt und in diesem ihren Platz findet …"

[48] Even recently, in a historical survey of the "light-quantum hypothesis, S. Brush pointed out that Planck's 1900 assumption of "an integer number of energy elements was only a mathematical device". Although I agree with Brush and "Kuhn and other historians" on "the evidence that Planck in 1900 did not propose physical quantisation of electromagnetic radiation", I do not find any evidence that Planck actually had previously refused the physical concept of "energy elements", or that "energy elements" were beyond his theoretical horizon. Planck's 1887 treatise shows many clues as to his commitment to look for new models for the transfer of energy. That general theoretical commitment is however consistent with his subsequent refusal of Einstein's 1905 specific theoretical model of quantisation. See Brush S. 2007, pp. 212-14. See also the *Afterword*, at the end of the present book.

[49] Poincaré H. 1900a, p. 468: "Nous pouvons regarder l'énergie électromagnétique comme un fluide fictive … qui se déplace dans l'espace conformément aux lois de Poynting. Seulement il faut admettre que ce fluide n'est pas indestructible et que dans l'élément de volume $d\tau$ il s'en détruit pendant l'unité de temps une quantité … ; c'est ce qui empêche que nous puissions assimiler tout à fait dans nos raisonnements notre fluide fictif à un fluide réel."

2. On the emergence of theoretical physics

The chief characters in the story to be told in the present book were committed to both advanced mathematics and the most speculative side of natural philosophy: they were *theoretical physicists*. At this stage it would be useful to clarify the concept itself of *theoretical physics*. Some years ago, McCormmach and Jungnickel traced the history of the settlement of the first chairs of theoretical physics in the last decades of the nineteenth century. They explored German speaking countries and other neighbouring countries to a certain extent influenced by German cultural traditions.[1] I claim that, although in a less formal way, a tradition of theoretical physics was emerging even in Great Britain in the second half of the nineteenth century. Theoretical physics emerged from the awareness that a theory is a representation of the real world, not a mere description. A theory entailed an interpretation, an intellectual choice and this led to a more sophisticated relationship between the theories and the phenomena they were supposed to account for.[2] The choice between the conceptual model of contiguous actions and the conceptual model of at-a-distance actions was an instance of theoretical choice. Theoretical physics emerged together with the awareness that physical research had to take into account, in a conscious way, not only logical and mathematical aspects or technical and experimental aspects. There was another aspect, neither formal nor empirical; sometimes, the difference between two theories could be really found in that conceptual aspect. The main hallmark of theoretical physics was the awareness that different conceptual models were legitimated to account for the same class of phenomena. In my opinion, Helmholtz's 1870 paper on the comparison between Continental and British electromagnetic theories, as well as the last chapter of Maxwell's *Treatise,* were the first instances of the emergence of theoretical physics.[3] In the first case, even though the comparison took place mainly on the grounds of mathematical physics and experimental physics, Helmholtz acknowledged the existence of different conceptual

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[1] See McCormmach R. and Jungnickel C. 1986, vol. 2, p. 33. From the general historical point of view, it is worth mentioning that the institutionalisation of theoretical physics was contemporary with German political unification and the contribution of physics to the development of German industry. See McCormmach R. and Jungnickel C. 1986, vol. 2, p. 2.

[2] See D'Agostino S. 2000a, p. xi: "It is true that theoretical physics was mainly a creation of turn-of-the century German physics, where it received full institutional recognition, but it is also undeniable that outstanding physicists in other European countries, namely Ampère, Fourier and Maxwell, also had an important part in creation." I agree with D'Agostino on Maxwell, much less on Ampère and Fourier.

[3] For a detailed account of Helmholtz's paper, see Darrigol O. 1993, pp. 232-9.

models in competition, which tried to explain the same class of phenomena. More explicitly theoretical was the comparison between physical theories put forward by Maxwell in 1873. Not only did he explicitly acknowledge the existence of different conceptual models in different theories, but the comparison among them took place mainly on conceptual ground rather than on mathematical and experimental grounds.

A recently published book shows how problematic the interpretation of the emergence of theoretical physics really is. The title, *Masters of theory*, is supposed to make reference to theoretical physics but the subtitle, *Cambridge and the Rise of Mathematical Physics*, suggests that the book deals with mathematical physics. The fact that the first line of the *Preface* presents the book as offering "a new account of the rise of modern mathematical physics" is consistent with the latter interpretation. Nevertheless, in the next pages, the author makes reference to all aspects of the life of those mathematicians that "were also implicated in the making of the modern theoretician".[4] It seems that *mathematical physics* has the same meaning of *theoretical physics*, and *mathematician* and *theoretician* refer to the same people. In the first chapter, the author complained that "mathematically formulated theories have continued … to be regarded as the province of the history of ideas". Then he pointed out the "distinction, in Western society between the terms 'theory' and 'practice'", or the "distinction between theoretical and experimental works". The opposition to practical or experimental activity led to an identification between mathematical physics and theoretical physics.[5] In a subsequent page, three occurrences of "theoretical work" are used with the same meaning of "mathematics and natural philosophy", and of "philosophical principles and mathematical methods". Some questions, concerning generally history of science, arise. Confining ourselves to the nineteenth century, was theoretical physics then looked upon as equivalent to mathematical physics? How can that supposed equivalence match with the not so slightly different equivalence between theoretical physics, on the one hand, and the integration between mathematics and natural philosophy, on the other? Can we accept the representation of physics of the late nineteenth century as a threefold entity, composed of experimental physics, mathematical physics and theoretical physics, being the last acknowledged and institutionalised just at that time? If a detailed inquiry into the history of nineteenth century physics, as Warwick's book really is, raises this kind of questions, it means that some definitions are really not so definite and have to be handled

[4] Warwick A. 2003, pp. ix-x.
[5] Warwick A. 2003, p. 12.

with care. In Germany, where the institutionalisation of theoretical physics occurred first, the creation of extraordinary professorships for theoretical physics, mainly in Prussian universities, underwent a certain number of ambiguities. The authoritative study of McCormmach and Jungnickel deployed some of the ambiguities which accompanied that institutionalisation. The first professorships, they stated, "were created solely to support the ordinary professor of physics, not to acknowledge a new speciality". Moreover, those university positions "were planned as transitional positions for young physicists", as the first step towards a career in experimental physics. It seems that sometimes "theoretical physics" was looked upon as physics presented in a more sophisticated and complete way, including mathematical subtleties. In other words, theoretical physics as advanced physics or, simply, mathematical physics. Candidates were expected to show their skills in both the experimental and *theoretical* sides of physics, just as well as candidates to experimental positions were expected to. In some universities (Kiel for instance), on the contrary, "theoretical physics was recognized as a necessary speciality", endowed with a specific characteristic, "as a link between, and an enrichment of, mathematics on the one hand and the natural sciences on the other".[6] In some way, this last feature actually supports the conception of theoretical physics as the integration of advanced mathematical physics and the tradition of natural philosophy. Nevertheless, in order to show how complex the emergence of *theoretical* physics in Germany was, the authors remarked that the appointment of Planck to theoretical physics at Kiel in 1885 implied that he "agreed to teach all of mathematical physics and, if necessary, to help out in experimental physics". Even more puzzling was the situation in some technical institutes, where the teaching "of 'applied' or 'technical' physics fell to the teachers of theoretical physics"; the authors specified that this happened "at several universities and technical institutes". [7]

The German institutional framework described by McCormach and Jungnickel shows how difficult a reliable historical reconstruction of the meaning of theoretical physics in the late nineteenth century is. The question is: are we able to single out one or more elements, specific enough but also general enough, in order to qualify European theoretical physics? What were those original element, emerging in the last decades of the nineteenth century, which, even though in a puzzling way, here and there appeared and then became clearly identifiable?

[6] McCormmach R. and Jungnickel C. 1986, vol. 2, pp. 33 and 41-3.
[7] McCormmach R. and Jungnickel C. 1986, vol. 2, pp. 48 and 55-6.

First of all, we know that neither Maxwell nor Helmholtz had ever held a chair in theoretical physics, as well as subsequently neither Larmor nor J.J. Thomson had.[8] In the period under consideration, we have to distinguish between the institutionalisation of *theoretical physics* in German universities and *theoretical physics* as actually practiced in some European countries. At the same time, we have to acknowledge that a precise chronological link actually exists: even though the German institutionalisation of theoretical physics cannot be identified with its actual practice, they emerged beside each other, almost in the same decades. It seems that the institutionalisation of the chairs of theoretical physics was the result of a convergence of several and even contradictory elements: among them, the demand for the acknowledgement of a new attitude towards physical sciences.

I find that the first hallmark of the emergence of *theoretical physics* was a specific awareness: different conceptual models can explain the same class of phenomena and, sometimes, by means of the same mathematical tools. In this sense I look upon theoretical physics as the more sophisticated heir, in the deeply modified context of a new professionalized physics, of the most speculative side of *natural philosophy*. The second hallmark was the awareness of the independence of conceptual models, the more speculative side of *natural philosophy*, from the mathematical structures and the empirical content of a physical theory. Late nineteenth century theoretical physics made it evident that the conceptual models embedded in a theory are part of that theory and contribute to qualify it. At the same time, those models could be detached from the theory and the remaining part of the theory, namely the empirical content and the formal structure, preserved an autonomous meaning, independent from the models tightly connected to it. In some way, the latter had to be looked upon as *another* theory.[9]

I agree with Warwick's historical reconstruction of the emergence of what we at present call *mathematical physics*: the process took place in the eighteenth century, and was characterised by uncertain boundaries between mathematics and physics. Those who practised *mathematical physics* were qualified as mathematicians and "made little distinction between the physical problems they were trying to solve and the mathematical techniques they

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[8] See Bevilacqua F. 1995, p. 15: "the professionalisation of the new discipline, … was largely a German novelty. Only H.A. Lorentz chair in theoretical physics at Leiden in 1877 can be seen as part of this same trend. J.C. Maxwell's 1871 appointment at Cambridge was in experimental physics. Italy had to wait until Enrico Fermi's chair in 1926 for a position in theoretical physics." See also Giannetto E. 1995, p. 8.

[9] Some decades ago M. Hesse pointed out the close relationship among models, *intelligibility* and *understanding* of a physical theory. See Hesse M. 1961, pp. 27-8.

employed".[10] The case of Cambridge University in the second half of the nineteenth century is interesting for the comprehension of the relationship between mathematical physics and experimental physics. The Mathematical Tripos (final mathematical examination) had first been established in the 1760s, stemming from the new "emphasis on mathematics and related natural philosophical subjects". By the end of the eighteenth century, "mixed mathematics" dominated the examination and "Book I of Newton's *Principia* had become the absolute pinnacle of elite undergraduate studies".[11] The Natural Science Tripos was first held in 1851 and "covered chemistry, mineralogy, geology, comparative anatomy, physiology and botany"; advanced mathematics was excluded from it. Wilson pointed out that both Mathematical Tripos and Natural Science Tripos dealt with physics but neither the first nor the latter succeeded in combining a satisfactory amount of experimental physics and mathematical physics in order "to produce the ideal physicist". Actually Maxwell, Rayleigh, J.J. Thomson and others were unsatisfied with the existence of two different training in physics, both outmoded in some respect. Nevertheless the two different trainings mirrored the long-lasting distinction between mathematical physics, or applied mathematics, on the one hand, and natural philosophy, on the other. It was J.J. Thomson who, around 1890, after some unsuccessful attempts, managed to introduce more experimental physics in Mathematical Tripos and more mathematics in Natural Science Tripos. I find that theoretical physics emerged after the full mathematisation and together with the first wide systematisations of *Baconian* sciences, as for instance sciences of heat and electricity.[12]

To sum up, I look upon *theoretical physics* as the re-emergence and the transformation, in a quite different context, of the speculative side of *natural philosophy* tradition. The different context, as I will show in a next section, was that of an accomplished professionalisation of scientific practice: that context required that a theoretical physicist was accustomed to advanced mathematical physics. Therefore the third hallmark of the emergence of theoretical physics was a new sophisticated alliance between

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[10] Warwick A. 2003, p. 29.

[11] Warwick A. 2003, pp. 56 and 58.

[12] Wilson D.B. 1982, pp. 325-7 and 349-56. I find that Buchwald and Hong's claim, "German theoretical physics was born along with such new fields as electrodynamics, physical optics, thermodynamics and statistical physics", is too vague. I find that what we call *electrodynamics* and *physical optics* (when associated to the names of Ampère and Fresnel) emerged quite before the emergence of theoretical physics, in whatever sense theoretical physics is considered, either as institutionalisation or actual practice. See Buchwald J.Z. and S. Hong 2003, pp. 168-9.

the more speculative side of natural philosophy and advanced mathematical physics, an alliance wherein a high degree of independence was allowed.[13]

Holton also pointed out the emergence of a "duality" in science, consisting in "clear and prescribed types of concepts", on the one hand, and "free licence of creativity", on the other hand. He thought that "the dilemma is resolved … by distinguishing two very different activities", namely "private science" and "public science".[14] Nevertheless, the distinction between *public* and *private* does not suit late nineteenth century physics. The emergence of theoretical physics corresponded to a public debate on different theoretical models or themes, taking place within the boundaries of public institutions of physics. This specific feature of late nineteenth century physics has been pointed out by different historians, both in early and in late twentieth century. Merz interpreted that feature as a "tendency of purely scientific thought of the century to lead up to philosophical problems". Kragh spoke in terms of "a spirit of scientific speculation"; I agree with the stress he put on that outstanding speculative commitment, which "would have been considered reckless twenty years earlier and was considered reckless twenty years later".[15]

H. Poincaré, one of the chief protagonists of late nineteenth century physics, made meaningful remarks on the new methodological attitude. In 1888, in his *Leçons sur la Théorie Mathématique de la lumière,* he pointed out that "many optical theories […] are available in order to explain optical phenomena and they all are plausible". He noted that "most of Fresnel's results are transferred without modifications to the electromagnetic theory of light", and that it was worthwhile taking into account such plurality of theories.[16] In 1890, in his *Élecricité et Optique*, he remarked that two

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[13] See, for instance, Giannetto E. 1995, p. 4: "… in those years and researches it was experienced the impossibility, and hence the breakdown, of a univocal determination and foundation of physics from experiments and mathematics." I found in Giannetto's paper two basic elements of my historical sketch: the deep roots of theoretical physics in the tradition of natural philosophy and the relative independence from mathematical physics.

[14] Holton G. 1973, p. 387. See also the *Appendix* at the end of this book.

[15] Merz J.T. 1912, p. 199, and Kragh H. 1996, p. 62. For more recent accounts, see Pont J.-C. 2007, p. XXII: in the last decades of the nineteenth century, "[l]a rupture avec le réalisme naïf des années 1850 est brutale." The acknowledgement of a plurality of conceptual models was the consequence of a new awareness: no theory "peut prétendre à une définitive exclusivité, puisque aucune ne peut se prévaloir de décrire véritablement le monde." (Lacki J. 2007, p. 248)

[16] Poincaré H. 1888, pp. II, III and 2. See, in particular p. II: "Il serait dangereux de se borner à l'une d'elles; on risquerait ainsi d'éprouver à son endroit une confiance aveugle et par conséquent trompeuse. Il faut donc les étudier toutes et c'est la comparaison qui peut surtout être instructive." See also p. 2: "[La] théorie électro-magnétique conduit aux même

theories can be conceptually in contradiction and, at the same time, "useful instruments" for physical research.[17] When he confined himself to mechanical explanations, he stated that infinite mechanical explanations were consistent with a given set of phenomena. In particular, he found that the conceptual model making reference to two electric fluids was equivalent to the model claiming the existence of one electric fluid. Poincaré found that, in some way, meta-theoretical attitude and personal taste was at stake when empirical checks could not help us to distinguish between two different models.[18] In 1892, in a subsequent collection of lessons, *Thermodinamique*, he stressed the historical nature of scientific knowledge: the plurality of theoretical models had both synchronic and diachronic aspects. That plurality of "theoretical models and even metaphysical conceptions" enriched scientific enterprise.[19]

In the same year, Boltzmann published a paper, corresponding to the first essay of his *Populäre Schriften* (subsequently translated as "On the methods of theoretical physics"). He did not say explicitly what theoretical physics was or should have been, but gave a historical account of the emergence of some issues looked upon as the hallmark of what he called a new "scientific method". Boltzmann's key word was "model": during the nineteenth century, in mathematics, there has been "the return from purely analytic to constructive methods and illustrations by means of models". Physics, as well as mathematics, saw the emergence of models, even though of different kind: those models played an important role in theoretical physics.[20]

According to Boltzmann, the first stage in the establishment of "a sharply defined method of theoretical physics" corresponded to the models of

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résultats analytiques que la théorie des ondulations de Fresnel; l'interprétation physique des formules seule diffère." See another passage, p. XVII: "J'ai pris le parti d'exposer successivement deux théories complètes, mais entièrement différents." Darrigol has recently pointed out Poincaré's pluralistic and evolutionary conception. See Darrigol O. 2007, p. 223 : "Cette conception plurielle, évolutive, et structurale de la théorie physique rompait avec la tradition française de mathématiques.".

[17] See Poincaré H. 1890, p. VIII: "Deux théories contradictoires peuvent en effet, pourvu qu'on ne le mêle pas, et qu'on n'y cherche pas le fond des choses, être toutes deux d'utiles instruments de recherches, …"

[18] See Poincaré H. 1890, pp. XIV-XV: "Entre toutes ces explications possibles, comment faire un choix pour lequel le secours de l'expérience nous fait défaut? […] Notre choix ne peut donc plus être guidé que par des considérations où la part de l'appréciation personnelle est très grande." Maxwell, in his *Treatise*, had already pointed out that infinite mechanical models could account for "a given species of connexion between the motions of the parts of a system". See Maxwell J.C. 1881, vol. II, p. 428.

[19] See Poincaré H. 1892, p. XIV.

[20] Boltzmann L. 1892, in Boltzmann L. 1974, pp. 5-7.

matter and force worked out by "the great Parisian mathematicians", after the French revolution. The second stage corresponded to the application of microscopic models of matter in motion to explain the internal state of macroscopic bodies at rest; this stage was associated by Boltzmann to the names of Clausius and Maxwell. A further stage corresponded to the introduction of successful models in order to explain "biological forms and phenomena": Darwin's theory, according to Boltzmann, had realized just this kind of conceptual shift from *description* to *explanation*. At the same time, physics underwent a sort of internal *secession*, induced by the then widespread criticism of the concept of force. On the one hand, some physicists, like Kirchhoff and Hertz, "took a turn in the opposite direction", transforming physics "into a descriptive science properly speaking". On the other hand, others "were especially fond of the colourful wrappings of mechanical representation"; in other words, they, like W. Thomson, made use of detailed and expressive models, involving "steel, rubber, glue" and other machinery. Boltzmann saw also an intermediate methodology, wherein physicists made use of mechanical models, "seeing in their own excogitated mechanism not those of nature but merely pictures or analogies".[21] I think that Boltzmann's historical reconstruction is reliable, even though I place the emergence of theoretical physics not at the beginning of the nineteenth century but just in that *secession* placed in the second half of the century. I claim that, from the mainstream of mathematical physics, set up by *Parisian mathematicians*, theoretical physics emerged as the requirement of a new, more sophisticated relationship between conceptual models and mathematical structures. In other words, theoretical physics maintained a meaningful link with mathematical physics but, at the same time, at least to a certain extent, involved a sort of independence between conceptual frameworks and mathematical structures.

It seems to me that this point is well enlightened in the second part of Boltzmann's *Schrift*. He pointed out three elements which contributed to establish the new trend: first, the attempt to attain "illustrative and tangible representations" of mathematical structures, second, the

[21] Boltzmann L. 1892, in Boltzmann L. 1974, pp. 7-8. In 1897, he developed similar conceptions in the first volume of his *Vorlesungen über die Principe der Mechanik.* He though that some "unclarities in the principles of mechanics" derived from "not starting at once with hypothetical mental pictures". He realised that without those pictures or, more in general, without "any hypothetical features", a satisfactory scientific knowledge could not be attained. It was just "the use of pictures" that allowed the scientist to go "beyond an unsimplified memory mark for each separate phenomenon". He thought that mechanics, in particular, required "very special mental pictures from the outset", even though that method appeared as "the very opposite of the modern one". See Boltzmann L. 1897, in Boltzmann L. 1974, pp. 225-8.

acknowledgement that same mathematical pattern or "differential equations hold for the most various phenomena", and, eventually, the acknowledgement that, although equation stemmed from specific conceptual models, they became "more detached from the models". I agree with Boltzmann's claim that the last element is explicitly expressed in the Maxwell's more mature contributions to electromagnetism. At that stage, a sort of independence between the mathematical structure and the conceptual model was realized. The model could fall without dragging down the mathematical theory stemming from that model in its falling.[22]

An instance of the actual practice of dissociating theoretical components from mathematical components in a physical theory is reported by Warwick in his recent book. Routh, one of the most successful among Cambridge's private tutors, used parts of Maxwell's *Treatise* as a textbook, but he taught the electromagnetism there contained "at least implicitly, in the form of an action-at-a-distance theory".[23] This fact shows that mathematical physics could actually be separated from theoretical physics, namely by the conceptual models giving meaning just to that mathematical component of the theory. As a consequence, mathematical and theoretical components could be independently accepted and taught. In some way, mathematical structures showed to be endowed with a meaning in themselves: that meaning could survive the rejection of the theoretical models they had stemmed from.

In this context the case of Italy is interesting: in the late nineteenth century, the existence of theoretical physics was acknowledged neither at the institutional level nor at the methodological level. There, mathematicians, following the tradition of applied mathematics, dealt with electromagnetic theories and developed sophisticated mathematical models for elastic or pseudo-elastic actions taking place in Euclidean or not-Euclidean spaces filled with aether. The theories outlined by Beltrami and Padova were the offspring of mathematicians deeply interested in physics; however, from the institutional point of view, they were definitely mathematicians rather than physicists.[24]

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[22] Boltzmann L. 1892, in Boltzmann L. 1974, pp. 9-11. See p. 11: "… the old hypotheses could be upheld only so long as everything went well; but now the occasional lack of agreement was no longer harmful, for one cannot reproach a mere analogy for being lame in some respects. […] In the end, philosophy generalised Maxwell's ideas to the point of maintaining that knowledge itself is nothing else than the finding of analogies."

[23] Striking enough, he "made no reference at all in his lecture notes to the field-theoretic approach adopted by Maxwell …, nor did he discuss the electromagnetic theory of light". (Warwick A. 2003, p. 307)

[24] See Neri D. and Tazzioli R. 1994, pp. 21-31.

In the course of the nineteenth century, scientists had lost the widespread firm belief that physics consisted of mechanics *plus* some other surrounding topics which were waiting to be brought into the boundaries of mechanics. Those topics involved light, heat, electricity, magnetism and the inner structure of matter.[25] As McCormmach and Jungnickel noted in their authoritative study, "[t]he relationship between these theories and mechanics was a subject of widespread debate at the turn of the century". The late nineteenth century "was a time of intense questioning of the foundations of physics" and one of the most exciting queries was "the possibility – and the desiderability – of the extension of mechanical modes of explanation throughout physics".[26] The fact is that some concepts seemed to have a peculiar nature, in some way independent from any possible mechanical explanation. In particular we could mention the concept of *entropy*, arising from thermodynamics, and the concept of *field*, arising from the electromagnetic theories of British tradition.[27] The different sections of physics, namely mechanics, electromagnetism, thermodynamics, and the recent concept of energy became the centre of attraction for corresponding attempts at unifying physics. Thus historians have spoken of different physical *world-views* emerging from theoretical physics of the late nineteenth century: a mechanical, an electromagnetic, a thermodynamic and an energetic world-view. There were attempts to found all physics on entities and concepts stemming from mechanics, or electromagnetism, or thermodynamics, or energy.[28] In reality, in the last decade, in the German

[25] Fourier's mathematical theory of heat was probably the first step. See Fourier J. 1822, pp. ii-iii: "Mais quelle que soit l'étendue des théories mécaniques, elles ne s'appliquent point aux effets de la chaleur. Ils composent un ordre spécial de phénomènes qui ne peuvent s'expliquer par les principes du mouvement et de l'équilibre." See also p. xi: "Les équations différentielles de la propagation de la chaleur expriment les conditions les plus générales, et ramènent les questions physiques à des problèmes d'analyse pure, ce qui est proprement l'objet de la théorie. Elles ne sont pas moins rigoureusement démontrées que les équations générales de l'équilibre et du mouvement."

[26] McCormmach R. and Jungnickel C. 1986, vol. 2, p. 212.

[27] See Renn J. and v. Rauchhaupt U. 2005, pp. 31-2. Merz credited Faraday with having been the first to introduce the words "dielectric" and "magnetic field" (1845), and W. Thomson with having been the first (1851) "to introduce the term 'field' and 'lines of force' into mathematical literature, adopting them from Faraday". (Merz J.T. 1912, p. 68, footnote 3, p. 70, and p. 73, footnote 1). On the origin of the word *field*, see Harman P.M. 1982, p. 72, and Darrigol O. 2000, p. 98.

[28] See McCormmach R. and Jungnickel C. 1986, vol. 2, chapter 25. Boltzmann, for instance, for a short time, had been interested in the theoretical perspective of energetism. See McCormmach R. and Jungnickel C. 1986, vol. 2, pp. 219-20, and Bevilacqua F. 1995, pp. 29-30. On the existence of three "world picture" (mechanical, electrodynamic and thermodynamic) see also Renn J. 2006, in Renn J. (ed.) 2006, Vol. 1, p. 42. I will discuss the *world-views* in the next chapter.

scientific context, explicit attempts to devise a world-view were developed and published. We could mention Helm's thick 1898 book *Die Energetik nach ihrer geschichtlichen Entwicklung*, a history of the concept of energy from the point of view of energetism. On the side of the electromagnetic world-view, we could mention Wien's 1900 paper, "Ueber die Möglichkeit einer elektromagnetischen Begründung der Mechanik", where he tried to found mechanics on electromagnetism. In Hertz's 1894 *Die Prinzipien der Mechanik (In neuem Zusammenhange dargestellt)*, the old mechanical world-view was transformed into a daring attempt to re-build physics on a geometrical-kinematical basis.

Theoretical physics also strengthened its identity in the course of those debates on different world-views: in truth, the debates involved neither experimental physics nor mathematical physics. Queries involving foundations and methods of physics were credited with having the same importance as devising new experiments or developing new mathematical tools. J. Renn analysed and interpreted in an original way the relationships among the emerging theoretical physics, the debate on physical world-views and the then established sections of physics: mechanics, electrodynamics or electromagnetism, and thermodynamics. Not only did the three sections or disciplines give rise to different and competing world-views but, on the borderline between each couple of disciplines, typical queries emerged. These "borderline problems" emerged when a definite class of phenomena was expected to be explained by two different disciplines. An instance of borderline problem was the electrodynamics of moving bodies: the mechanical approach did not match the corresponding electromagnetic approach.[29]

However, the adjective *mechanical* requires some specifications; in particular, the distinction between a mechanical and a dynamical approach should be specified. It seems to me that Buchwald managed to outline important aspects of the difference, as intended at the end of the nineteenth century, between *dynamical system* and *mechanical model*. In a mechanical approach, a specific architecture of the system was devised and the machinery underlying this architecture had to be specified. The Englishman O. Lodge and the Irishman G.F. FitzGerald developed detailed mechanical models for the electromagnetic field and for electromagnetic actions taking place in conductors and dielectrics (see, for instance, Lodge's

[29] According to Renn, the borderline problems were like an intellectual engine, which made theoretical physics develop. See Renn J. and Rynasiewicz R. 2005, p. 32, and Renn J. 2006, in Renn J. (ed.) 2006, Vol. 1, pp. 30, 32 and 43. On the conceptual difference between *electrodynamics* and *electromagnetic phenomena* see chapter 5 of the present *Introduction*.

1889 successful book *Modern Views on Electricity*). That methodological attitude led Duhem to express his famous remark: "we had imagined we would have entered the quiet and tidy room of deductive thought: now we realise we have entered a factory".[30] In a dynamical approach, a specific architecture did not have to be devised; only an energy function (with kinetic and potential components) had to be specified. Starting from the energy function, the equation of Lagrange followed. The generalised co-ordinates and the corresponding velocities were not necessarily associated to parts of a mechanical system. For instance, in papers written before 1864, Maxwell had displayed detailed mechanical models, whereas afterwards he turned his attention to a more general dynamical approach. In particular, in British electromagnetic theories following that approach, and put forward after Maxwell's death, every process was associated to a specific energy contribution. Hamilton's principle required that the time integral of the difference between kinetic and potential energy, after having taken into account all energy contributions, had a minimum.[31]

The adjectives *mechanical* and *dynamical* experienced a plurality of interpretations.[32] The former could rely on a long-lasting tradition, wherein it was identified with the two conceptual pillars of matter and motion. Nevertheless, in 1865, in his outstanding paper on the foundation of an electromagnetic theory, Maxwell had associated the two pillars to the adjective *dynamical*: he stated that his theory "may be called a *Dynamical* Theory, because it assumes that in that space there is matter in motion, by which the observed electromagnetic phenomena are produced".[33] At the same time, the word *dynamism* was associated to a more radical *mechanicism*, wherein matter consisted of a specific state of motion, or a dynamical structure, in an all-pervading medium. In the second half of the nineteenth century, this last conception had been developed in Great Britain and his followers could find in it a fascinating opportunity to unify physics. An

[30] Duhem P. 1906, p. 111: "… nous pensions entrer dans la demeure paisible et soigneusement ordonnée de la raison déductive; nous nous trouvons dans une usine". Duhem's criticism was part of a more general comparison, where French "esprit de Descartes" was opposed to British "faculté imaginative de Bacon" or, in general, "raison" faced "imagination". See Duhem P. 1906, pp. 105 and 115. See also Darrigol O. 2000, p. 188.

[31] Buchwald described the dynamical approach to physics as based on two *postulates*: "all processes can be exhaustively described in terms of the energy changes they effected", and "these changes are governed by Hamilton's principle". See Buchwald J.Z. 1985c, p. 226. See also Buchwald J.Z. 1985c, p. 226, Buchwald J.Z. 1985a, pp. 20-21, and Siegel D.M. 1981, p. 259.

[32] Topper, for instance, claimed that J.J. Thomson's use of the words "mechanical" and "dynamical" was "not always consistent"; see Topper D.R. 1980, p. 38.

[33] Maxwell J.C. 1865, p. 460.

extreme dynamical conception was professed by FitzGerald in the 1890s: in 1896 he wondered whether "motion be … the objective aspect of thought".[34] The fact is that both scientists, in the second half of the nineteenth century, and historians, in the twentieth century, have associated different meanings to the adjectives *mechanical* and *dynamical*. W. Thomson, for instance, in 1852, thought that "it is convenient … to divide *stores* of mechanical energy in two classes – *statical* and *dynamical*": the adjective *statical* referred to forces and the adjective *dynamical* referred to motion. A body placed at a given height or an electrified body were instances of "mechanical energy of the statical kind", whereas matter in motion, light and radiant heat were instances of "mechanical energy of the dynamical kind".[35] In any case, the adjective *mechanical* seemed to have a different meaning from the adjective *dynamical*. In 1900, Larmor, in the Preface of his *Aether and Matter*, in a footnote specified that, throughout his essay, "the term *mechanical* is used in antithesis to *molecular*". Mechanics was considered a section of Dynamics, for the former referred only to macroscopic models, while the latter referred to both macroscopic and microscopic models: "*mechanics* is the dynamics of matter in bulk", he wrote, "in contrast with molecular dynamics".[36]

Merz offered another perspective and wrote: "it was proposed to make the term dynamics the general term which embraces kinetics and statics as subdivisions, and to reserve the word 'mechanics' for the science of machines". This choice was consistent with the tendency to call *mechanical* the resort to machinery, in order to represent and explain physical phenomena.[37] These different interpretations about the meaning of the two

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[34] See FitzGerald G.F. 1896, p. 441. That passage was not an isolated remark: in 1890 he had already surmised a closed link between matter, motion and thought. See FitzGerald G.F. 1890, in FitzGerald G.F. 1902, p. 276: "It was stated that what seemed a possible theory of ether and matter what that space was full of such infinite vortices in every direction, … This hypothesis explains the differences in Nature as differences of motion. If it be true, ether, matter, gold, air, wood, brains, are but different motions. Where alone we can know what motion in itself is – that is, in our own brains – we know nothing but thought. Can we resist the conclusion that all motion is thought?" G.F. FitzGerald held the chair of Natural and Experimental Philosophy in Dublin (Trinity College). On the query of the "twin" chair held by FitzGerald, see O'Hara J.G. and Pricha W. 1987, p. 134 (reference 60).

[35] Thomson W. 1852, in Thomson W. 1882, p. 511. On this specific issue, see Siegel D.M. 1981, p. 245; on the different meanings of "mechanical" explanations, see Harman P.M. 1982, p. 9.

[36] Larmor J. 1900, p. xiii, footnote.

[37] Merz J. T. 1912, p. 144; it is worth noting that this semantic shift experienced by the adjective *mechanical* restored in some way its ancient (before the so-called *Scientific Revolution*) meaning.

adjectives *mechanical* and *dynamical* show us how questionable is the attribution of a definite meaning to the foundations of the so-called mechanical world-view.

3. On the physical world-views

According to McCormmach and Jungnickel, the landscape of late nineteenth century theoretical physics, was interpreted in terms of a competition among "mechanical", "electromagnetic" and "energetic" foundations. This historiographical framework is doubtless useful as a first approximation. At the same time it is worth mentioning a different interpretation, that of S. Abiko. He classed "European physical traditions around the turn of the century" in two sets: he named them "a chemico-thermal tradition" and a "particle-dynamical tradition".[1] This is an original and interesting framework, even though it appears to me less convincing than McCormmach and Jungnickel's. The fact is that both classifications are unsuitable when applied, in particular, to British tradition. For instance, confining myself to theoretical researches stemming from late nineteenth century electromagnetism, I find in J.J. Thomson traces of a physical-chemical tradition but not traces of a chemical-thermal tradition in its stricter sense. We can find traces of a dynamical tradition, even though not exclusively associated to a "particle" conception. At the same time, I find in J. Larmor traces of a mechanical foundation alongside traces of an electromagnetic foundation. As I will show in *Part II* of the present book, J.J. Thomson and Larmor tried to make discrete and continuous models live together, both for matter and energy. The fact is that the historical interpretation of late nineteenth century world-views deserves further specifications.

I would like to start from the incipit of McCormmach's 1970 authoritative paper on Lorentz and the "electromagnetic view of nature": he claimed that "an electromagnetic view of nature was announced at the beginning of the twentieth century".[2] He found that the problematic relationship between mechanics and electrodynamics could be traced back to around the middle of the nineteenth century, in the tradition of Continental theories. He remarked that in Weber, Clausius, Riemann and Carl Neumann's electrodynamic theories, there were "a number of radical departures from the conventional mechanical viewpoint". Those conceptual innovations concerned "finite propagation of electric forces", "violation of Newton's law of action and reaction", an "upper limit on the possible relative velocity of particles", "a new concept of energy conservation" and some kind of "a velocity-dependent apparent mass for electric particles". According to McCormmach, those conceptions were inherited by subsequent British

[1] Abiko S. 2003, p. 211.
[2] McCormmach R. 1970a, p. 459.

electromagnetic theories: in some way, those "fragmentary ideas" transformed into the outline of "an expressly non-Newtonian dynamics". Eventually, "the electron and relativity theory" established "a new era".[3]

Although I find not completely convincing the last part of the genealogy, I find it correct to point out that the overcoming of mechanics can be traced back to action-at-a-distance tradition as well as to contiguous action tradition. McCormmach referred specifically to German physics, in particular to Weber's electrodynamics: he saw in it an attempt to devise "an electrical view of nature", "recasting the law of gravitation according to his electrical model". He identified a theoretical stream starting from the Italian Mossotti and arriving at the Dutch Lorentz through the German Weber and Zöllner. He thought that, in Lorentz's view, Continental theories "needed only to be revised to incorporate the field concept and the electrical nature of light". McCormmach looked upon "Lorentz electron theory" as one of the paths leading to "the anticipation of a purely electromagnetic understanding of physical reality".[4]

Apart from the questionable concept of "anticipation", I criticise McCormach's thesis, for I find in Lorentz a definite attempt to integrate a mechanical with an electromagnetic representation, rather than a "pure" electromagnetic representation. Lorentz was actually indebted to Continental theories: he refused action at a distance but accepted molecular representation of electricity. Nevertheless, it is questionable whether Lorentz's late nineteenth century theories led, in some way, to an electromagnetic world-view. His electrodynamics, as displayed in his 1892 and 1895 essays, as well as in his 1899 short paper, preserved the footprint of a double mechanical-electromagnetic character. His five equations can be classed in two subsets: four equations involved the electromagnetic fields and the fifth echoed Continental electrodynamic equations, containing a static term and a term depending on velocity. Even Lorentz's 1904 paper, displaying his more mature electrodynamics, preserved the same double character. I find that only his 1900 paper on gravitation can be meaningfully

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[3] McCormmach R. 1970a, p. 472.

[4] McCormmach R. 1970a, pp. 459, 462 and 472. Mossotti accounted for gravitation in terms of electric forces. He imagined a particle of matter as composed by "two opposite electric atoms", and he assumed that "the attraction between two such ponderable particles is greater than their repulsion". See McCormmach R. 1970a, p. 476. See also pp. 471-2 for the relationship between electrodynamics and "the mechanical view of nature", in the middle of the nineteenth century.

associated to an electromagnetic world-view or, more specifically, to an attempt to unify forces on an electric basis.[5]

In 1892 H.A. Lorentz published in *Archives Néerlandaises* a thick paper, "*La théorie électromagnétique de Maxwell et son application aux corps mouvants*", which filled about two hundred pages of the journal. He avowed that he was accomplishing Maxwell's project of unification between optics and electromagnetism. In the first lines of the paper, he made explicit reference to the intervening medium in the interpretation of electric currents. The interest in Maxwell's theory had spread on the Continent after Hertz's reinterpretation. Lorentz looked upon that reinterpretation as an interesting simplification and clarification, even though at the price of skipping important theoretical features.[6]

The first chapter of the paper was devoted to the electrodynamics of bodies at rest, the second and the short third to the electrodynamics of bodies which were supposed to drag aether, and the fourth to a theory of electric particles in motion without any aether drag. I would like to concentrate on some less known parts of Lorentz's paper, concerning the link between mechanical and electromagnetic representations of matter, aether and electricity. In the first chapter, he outlined a theoretical model quite similar to that put forward by Poincaré in 1890: he assumed that all substances, aether included, were embedded in an incompressible fluid, whose displacements corresponded to electric actions. In dielectrics, "particles of this fluid" could only swing around steady positions when excited by external forces. On the contrary, in conductors, those particles were in equilibrium everywhere and were actually displaced by applied

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[5] See the first lines of Lorentz H.A. 1900a, in *Collected Papers* 5, p. 198: "Les grands progrès qu'on a faits, pendant les dernières dizaines d'années, dans la connaissance du mécanisme des phénomènes électriques et magnétiques, nous engagent plus que jamais à nous demander si, de même que ces actions, la pesanteur, ou l'attraction universelle, ne peut pas être considérée comme une conséquence de certains changements dans l'état de l'éther. Et d'abord, il emporte d'examiner si l'on peut arriver à une explication de la gravité en se bornant aux conceptions … que l'éther peut être le siège de deux changements d'état qui existent dans un champ électrique et dans un champ magnétique, et qui satisfont aux équations électromagnétiques bien connues."

[6] Lorentz H.A. 1892a, p. 364. See also pp. 367-368: "Il y a une différence essentielle entre la méthode de M. *Hertz* et celle de *Maxwell*. M. Hertz ne s'occupe guère d'un rapprochement entre les actions électromagnétiques et les lois de la mécanique ordinaire. Il se contente d'une description succincte et claire, indépendante de toute idée préconçue sur ce qui se passe dans le champ électromagnétique. Inutile de dire que cette méthode a ses avantages. Cependant, on est toujours tenté de revenir aux explications mécaniques. C'est pourquoi il m'a semblé utile d'appliquer directement au cas le plus général la méthode dont Maxwell a donné l'exemple dans son étude des circuits linéaires."

forces.[7] Although the electric fluid filled all space, "other kinds of matter existed", either as "atomic structures mutually passing through", or as "different implementations of the same substance". In short, Lorentz first assumed the existence of "ponderable matter", then aether together with "a substance able to retain electricity", and eventually "material points charged by electromagnetic motions", which had not to be identified with the electric fluid itself. He was aware of the oddness of the model, and thought that any attempt to better specify it would have led him to "useless speculations". Therefore he decided to confine himself to "a provisional analysis", which could subsequently be replaced by "a better developed theory".[8] When inquiring into changing electric currents emerging from the discharge of a condenser, Lorentz imagined machinery representing the "elasticity excited in the dielectric layer" inside the condenser. The gears of that machinery corresponded to the electric fluid and to the substance which was the seat of electromagnetic actions. A mechanical model, echoing Maxwell's former models and more recent British models, was on the stage. At the end of the chapter, Lorentz remarked that his model led to equations which were "essentially the same of Maxwell, Heaviside and Hertz".[9]

In the second chapter, starting from Hertz's hypothesis of aether completely dragged by bodies in motion, he developed quite a different model, still leading, once again, to Hertz's equations. He classed physical phenomena in "two, well definite sets": on the one hand, "electric phenomena", on the other hand "motion of matter".[10] If the former were submitted to an electromagnetic *view*, the latter were submitted to a mechanical *view*. Indeed, the first chapters of the paper, when taken into account as a whole, appear as a collection of different theoretical models and different points of view. In particular, Lorentz represented energy as Maxwell did, but represented matter following a plurality of models. With

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[7] Lorentz H.A. 1892a, pp. 391-392.

[8] Lorentz H.A. 1892a, pp. 393-394. See, in particular, p. 394: "J'indiquerai par M à la fois la matière pondérable et la substance qui retient l'électricité contenue dans l'éther, par N la matière qui est le siège des mouvements électromagnétiques. (…) Pour fixer les idées je supposerai que la matière M est immobile et qu'elle ne fait point partie du système auquel nous avons appliqué le principe de *d'Alambert*. Ce système est donc composé du fluide électrique et de la matière N."

[9] Lorentz H.A. 1892a, pp. 400 and 407. See p. 400: "On pourrait comparer ce dernier [le fluide électrique] à une tige dentée qui se déplace en sens longitudinal, et la matière N à une roue dentée s'engrenant avec cette tige; en effet, une résistance quelconque, qui s'oppose à un mouvement donné de ces organes, ne les amènera pas instantanément au repos; il faudra pour cela un temps d'autant plus long que la masse de la roue est plus considérable."

[10] Lorentz H.A. 1892a, pp. 409 and 420.

regard to methodological strategy, it is worth noticing that Lorentz took the liberty of devising models and applying each of them to a suitable set of phenomena. As already noted, this was a specific hallmark of late nineteenth century theoretical physics. There was an attempt to devise *constructive* and *substantialist* models, in order to inquire into the intimate nature and structure of physical entities, far beyond the mathematical laws ruling them.

The fourth chapter contains Lorentz's freshest insights and is well-known to historians. It is known, in particular, that the new model displayed in that chapter led to integrate Fresnel's optical theory with the optical consequences of Maxwell's electromagnetic theory. He imagined "ponderable matter as completely pervious to aether" and containing "a large number of little particles endowed with positive or negative electricity": electromagnetic phenomena were interpreted as "arising from the displacement of such particles". Electric charge was interpreted as an excess of a given kind of particles, electric current as "a true stream of particles" and Maxwell's electric displacement as an actual "departure from their balance centre".[11] According to Lorentz, his model of electrified particles, so close to Continental tradition, could not overshadow the contiguous action model: electromagnetic actions had to propagate through aether, in a finite time. He thought that "the footprint of Maxwell's principles" consisted in the prominence of aether: rather than forces between electrified particles he saw contiguous actions propagating from particles to aether and, conversely, contiguous actions propagating from aether to electrified particles.[12] Indeed, he aimed at the integration between "Maxwell's theory" and "former conceptions", wherein a complex interplay between three *substances* and two interactions was involved. He made use of mechanical and electromagnetic interactions, on the one hand, and matter, aether and microscopic electrified particles, on the other. From the point of view of interactions, mechanical ones could affect both ordinary matter and microscopic electrified particles; electromagnetic ones could affect both aether and microscopic electrified particles. Mechanical interactions could not affect aether, just as pure electromagnetic actions

[11] Lorentz H.A. 1892a, pp. 432-433.

[12] Lorentz H.A. 1892a, pp. 433-434: "… dans la nouvelle forme que je vais lui donner, la théorie de Maxwell se rapproche des anciennes idées […] aussi la valeur de la force, à un certain moment, n'est elle pas déterminée par les vitesses et les accélérations que les petits corps possèdent à ce même instant ; elle dérive plutôt des mouvements qui ont déjà eu lieu. En termes généraux, on peut dire que les phénomènes excités dans l'éther par le mouvement d'une particule électrisée se propagent avec une vitesse égale à celle de la lumière. On revient donc à une idée que *Gauss* énonça déjà en 1845 et suivant laquelle les actions électrodynamiques demanderaient un certain temps pour se propager de la particule agissante à la particule qui en subit les effets."

could not affect ordinary matter. From the point of view of the three physical *substances*, matter could interact with microscopic electrified particles and the latter could interact with aether; matter and aether could not interact with each other. A *mechanical* framework met an *electromagnetic* framework.

In a subsequent essay published in 1895, he still put forward his 1892 theoretical model involving the above three *substances*. He explicitly claimed his commitment to integrate different theoretical models, specifically "the core of Maxwell's theory" with "former electrical theory", namely Clausius and Weber's Continental tradition. Essentially the same model was displayed in a short paper he published in 1899: his electrodynamics was founded on aether, "electrons" and ponderable matter, and involved both electromagnetic and mechanical forces.[13] In brief, I find that, in the 1890s, Lorentz could not be associated to a whatsoever "pure electromagnetic world view". The fact is that McCormmach's statements on the first page of his paper are toned down by other remarks to be found in the following pages, where he states, for instance, that forces acting on electric particles "can also be derived by mechanical reasoning from the hypotheses". On the same page, he acknowledged that "mechanical principles were most useful" in the derivation of such forces. Similar remarks are deployed in the conclusive section of the paper. McCormmach remarked that "Lorentz's 1892 theory had deliberate elements of mechanical construction" even though "it was by no means a purely mechanical theory". However, Lorentz's "electrical charge and electromagnetic ether were both avowedly nonmechanical entities". To sum up, he acknowledged the double nature, both mechanical and electromagnetic, of microscopic electric charges: they were "in part mechanical bodies to which the laws of motion apply", even though "the charges attached to these bodies remain, however, unexplained".[14]

With regard to Lorentz's 1899 paper, McCormmach explicitly noted "a tension which always existed in Lorentz's theory". In particular, he spoke of

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[13] Lorentz H.A. 1895, in *Collected Papers 5*, pp. 2-8, in particular p. 8: "Ueberhaupt liegt in den Annahmen, die ich einführe, in gewissem Sinne eine Rückkehr zu der älteren Electricitätstheorie. Der Kern der Maxwell'schen Anschauungen geht damit nicht verloren, aber es ist nicht leugnen, dass man mit der Annahme von Ionen nicht mehr weit entfernt ist von den electrischen Theilchen, mit denen man früher operirte. In gewissen einfachen Fällen tritt dies besonders hervor." See also Lorentz H.A. 1899, in *Collected Papers 5*, pp. 139-40: "J'introduirai donc d'abord les vecteurs $\boldsymbol{d}$ et $\boldsymbol{H}$, que l'on appelle le déplacement diélectrique et la force magnétique; en outre je représenterai par ρ la densité de la charge de la matière pondérable, par $\boldsymbol{v}$ sa vitesse, et par $\boldsymbol{F}$ la force agissant sur cette matière par unité de charge."

[14] McCormmach R. 1970a, pp. 463, 466 and 493.

"a dual foundation" of his "conception of the electromagnetic world". On the one hand, there were forces, "computed with the aid of Maxwell's continuous fields"; on the other hand, there was "the motion of the electrons", which "followed the mass-point laws of Newtonian mechanics". It seems to me that the existence of those two conceptual pillars in Lorentz's theories shows a complex interplay between mechanical and electromagnetic world-views, rather than a *pure* electromagnetic world-view.[15]

McCormmach looked upon the mechanical world-view as a very general scheme, including both discrete and continuous models of matter, as well as both action at a distance and contiguous action. On the contrary, he found that the "electromagnetic view of nature" was based upon three specific hypotheses: first, "the only physical reality are the electromagnetic ether and electric particles", second, "all laws of nature are reducible to properties of the ether" and, third, those properties "are defined by the electromagnetic field equations".[16] It seems to me quite questionable that Lorentz, in the 1890s, can be associated to those sharp hypotheses. In addition, none of them was an exclusive feature of an electromagnetic world-view. Even Hertz established the primacy of the electromagnetic fields and did not try to explain them mechanically. He relied on aether and on electromagnetic equations in aether, but his name is associated to his sophisticated recasting of mechanics and to a mechanical world-view, rather than to an electromagnetic world-view. This is further evidence of the approximate nature of labels such as *mechanical* or *electromagnetic* world-views.

As I have already pointed out, in Lorentz's electrodynamics, as deployed in his papers throughout the 1890s and the early twentieth century, there were at least three *substances*: ordinary matter, electrons and aether.[17] As McCormmach himself acknowledged, ordinary matter followed the laws of mechanics, whereas aether and electric charge "were basic principles, and he did not liken them to anything else". Because of that mix of mechanical and

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[15] McCormmach R. 1970a, p. 474.

[16] McCormmach identified the "simplest version of the electromagnetic view" with theories (Larmor's, for instance) which assumed "that electric particles are merely structures in the ether", namely that "the ether is the sole reality". See McCormmach R. 1970a, p. 459.

[17] See, once again, Lorentz H.A. 1892, p. 432, where he put forward his leading theoretical model: "Il m'a semblé utile de développer une théorie des phénomènes électromagnétiques basée sur l'idée d'une matière pondérable parfaitement perméable a l'éther et pouvant se déplacer sans communiquer à ce dernier le moindre mouvement. […] Il suffira, dans ces applications, d'admettre que tous les corps pondérables contiennent une multitude de petites particules à charges positives ou négatives et que les phénomènes électriques sont produits par le déplacement de ces particules.".

electromagnetic features, he thought that Lorentz had not accomplished a pure electromagnetic world-view but he had only called it forth. He wrote that "others responded to the problem areas Lorentz's theory made central to physics"; "others" modified Lorentz's theories, in order to "exclude all nonelectromagnetic elements". It was just "others" who "extrapolated" Lorentz's theories "into a vision of the future universal physics".[18] According to McCormmach, the "others" were some scientists, both experimentalists, like Kaufmann, and theoreticians, like Abraham, who, early in the twentieth century, undertook a research programme devoted to ascertaining the following theory: electron mass has an electromagnetic nature, ordinary matter is made of electrons, chemical classifications involved suitable arrangements of electrons, and gravitation could actually be reduced to electric interactions.[19] I agree with this specific interpretation, for I find that pursuing an electromagnetic world-view was a project not so widespread in the European scientific community and chronologically limited to the first decade of the twentieth century. As far as I know, an explicit claim of an electromagnetic world-view, or the outline of an electromagnetic foundation of mechanics and gravitation, appeared in scientific literature only at the dawn of the twentieth century. In 1895 Lorentz had simply assumed that, probably, intermolecular forces were akin to electromagnetic actions with regard to the way of propagation.[20] The most interesting feature of late nineteenth century theoretical physics was the pursuit of an integration between different *views*, rather than the reduction of all physics to a single, particular view.

In 1975, Giusti Doran opened her essay claiming that the "electromagnetic view of nature" should be regarded as the "greatest disjuncture since the seventeenth-century Newtonian synthesis". She added that the "revolution … germinated and matured in Britain during the nineteenth century" and "culminated in the final decades". According to Giusti Doran, the *revolution* consisted of a "conscious rejection of the mechanical concepts of atom, void and force, in favour of the plenum and a field-theoretic notion of matter". That meaningful and underestimated events took place in physics of the late nineteenth century seems to me noteworthy, even though I cannot agree with the choice of qualifying them as an *electromagnetic view of nature*. I think that the more relevant event was the attempt to integrate an

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[18] McCormmach R. 1970a, pp. 460 and 463.

[19] McCormmach R. 1970a, p. 481.

[20] See, for instance, Wien W. 1900a, Lorentz H.A. 1900, and Abraham M. 1905. See Lorentz H.A. 1895, pp. 122: "… dass auch die Molekularkräfte, ähnlich wie wir es gegenwärtig von den electrischen und magnetischen Kräften bestimmt behaupten können, durch den Aether vermittelt werden."

electromagnetic theory with a theory of matter and, more in general, the attempt to integrate mechanics with electromagnetism.[21]

In a recent debate, S. Katzir and S. Seth put forward two different sets of features to identify the so-called "electromagnetic world view": this shows that those specific features are still questionable. Katzir emphasised three elements: first, "all (inertial) mass is of electromagnetic origin", second, "all forces are of electromagnetic origin", and, third, "the forces and the mass should be able to explain all phenomena". Seth emphasised three other elements: first, "a distaste for microphysical mechanical modelling", second, "the belief that physical realities are electromagnetic in origin", and, third, "a programmatic commitment to problems whose solution promised to secure a universal physics based on electromagnetic laws and concepts".[22] Some decades ago, Jammer emphasised the connection between the concept of electromagnetic inertia and the electromagnetic world-view. Nevertheless, the concept of an electromagnetic inertia concerned specifically electrically charged particles, whereas the more general conception of an electromagnetic world-view corresponded to the attempt to derive all physics from electromagnetism. I think that the connection between *electromagnetic* inertia and *electromagnetic* world-view cannot be transformed into an identification: as Katzir has recently stated, the first was "more common" than the latter.[23] I find that the interplay between *mechanic* tradition and electromagnetic theories was quite complex. We have already seen that what we call *electromagnetic* inertia is attributed to J.J. Thomson: an electrically charged sphere moving through aether would experience an apparent increase in its mass. Topper noticed that the *electromagnetic* inertia was explained by Thomson making use of dynamical actions taking place in the aether. In some way, what we look upon as an electromagnetic interpretation of inertia, at a more fundamental level could be looked upon as a mechanical interpretation of electromagnetism. In other words, traditional inertia could be explained as an electromagnetic effect but that electromagnetic effect, in its turn, was explained as a mechanical interaction between the sphere and the aether.[24]

A historiographical framework in terms of world-views had already been put forward by Merz, at the beginning of the twentieth century. He had envisaged a diachronic, rather than synchronic, scheme, which consisted of

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[21] Giusti Doran B. 1975, pp. 134-5.

[22] Katzir S. 2005, p. 189, and Seth S. 2005, p. 195.

[23] Jammer M. 1961, pp. 142-4, and Katzir S. 2005, p. 189.

[24] See Topper D.R. 1980, pp. 40-1; with regard to the different interpretations given by historians, swinging from a Thomson *mechanically* oriented to a Thomson *electromagnetically* oriented, see pp. 51-2.

three subsequent stages. His account started from an "astronomical view of nature", corresponding to a *Newtonian* conceptual model of force, then took into account a "kinetic view of nature", corresponding to a conceptual model based on matter and motion, and, eventually, described a "physical view of nature", corresponding to a conceptual model based on energy. According to Merz, the second stage, a kinetic world-view emerged first in optics, then in thermology and, eventually, in electricity and magnetism: it was triggered by the discovery that radiant heat had much in common with ordinary light. The nineteenth century "dream of an ultimate kinetic explanation or interpretation of all natural phenomena" appeared rooted in the "successful development of the undulatory theory of light" of the first decades of the century. Then it had been strengthened by other theoretical developments taking place in the last decades: the kinetic theory of gases, W. Thomson's model of vortex atom, and Maxwell's electromagnetic theory. Merz interpreted those different developments of physics as part of the same trend; they had in common a kinetic representation of nature.[25] They all assumed that "the supposed static properties of matter could be explained by different modes of motion": microscopic *kinetic* models could explain different macroscopic effects. He acknowledged that the *kinetic* view did not succeed in explaining gravitation, even though it gave rise to a new conception of matter. Indeed, the kinetic view transformed and unified the representations of matter: at a microscopic level, for instance, the observed basic differences among solid, liquid and gaseous bodies disappeared.[26]

Merz attributed the emergence of the subsequent "physical view of nature", or the new foundation of physics on the concept of energy, mainly to the "Scotch school of natural philosophy": among its members, W. Thomson and Maxwell.[27] Therefore, in Merz's historical framework, both W. Thomson and Maxwell had an outstanding role in the emergence of both *kinetic* and *physical* views of nature. This framework seems in general unsatisfactory, because the last two world-views overlap to a large extent. At the same time, this specific aspect prevents us from establishing a too tight correspondence between every important scientist and a specific world-view. I think that every interpretation given in terms of physical world-views, or views of nature, suffers an excess of simplification and can be useful only as a rough approximation.

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[25] Merz J.T. 1912, pp. 34-6 and 104-5, in particular footnote 2. It is worth noting that, in Chapter VI of his *History*, "On the kinetic or mechanical view of nature", *kinetic* and *mechanical* were used with the same meaning.
[26] Merz J. T. 1912, pp. 56, 89 and 160.
[27] Merz J. T. 1912, p. 141.

Starting from the conceptual model of Faraday lines of force, though submitted to a deep reinterpretation, Maxwell had begun to build his electromagnetic theory. From his first papers till the *Treatise* and beyond, we could follow the history of a theoretical effort to bridge the gap between two sections of physical sciences: mechanics and electromagnetism. I have already mentioned, in the last lines of his *Treatise*, Maxwell's focus on "the conception of a medium in which the propagation takes place" and on the medium, which "ought to occupy a prominent place in our investigation". He pointed out what he considered as a future task for physicists: "to endeavour to construct a mental representation of all the details of its action". Although such an effort had always been his "constant aim in this treatise", he considered the task not yet accomplished.[28] In reality, in the late nineteenth century, part of the then not so large community of physicists were involved with researches at the boundaries between mechanics and electromagnetism. In the British community, I could mention the experiments performed by O. Lodge, between 1890 and 1893, on the supposed aether drag.[29] Another problem, both theoretical and technical, placed on the borderline between mechanics and electromagnetism was *unipolar induction*, namely the electric current stemming from the rotation of a magnet around its own axis. Theoretical physicists and engineers, both in Great Britain and on the Continent, wondered whether the magnetic lines of force did rotate together with the magnet or not.[30] The debate concerned not only the kinematic behaviour of lines of force but also the nature of lines of force themselves. Furthermore, not only unipolar machines affected the theoretical debate on the problematic link between electromagnetism and mechanics, but they affected, at least to the same extent, technology and industry.

Faraday had taken into account many different arrangements of rotating magnets and conductors. Starting from 1831, in his *Diary* and in his *Experimental Researches in Electricity,* he recorded many experiments performed with magnets and electric circuits rotating around each other. When interpreted in terms of lines of force, his experiments suggested that

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[28] Maxwell J.C. 1881, vol. 2, p. 449.

[29] Differently from Buchwald, I do not think that, apart from few theoretical physicists like Larmor and Lorentz, the community of physicists was not interested in problems at the borderline between mechanics and electromagnetism. See Buchwald J.Z. 1985a, p. xi. Harman's appraisal is quite different. See Harman P.M. 1982, p. 116. On Lodge's experiments, see Hunt B.J. 1991, p. 192.

[30] On the widespread debate in the second half of the nineteenth century, see Miller A.I. 1981, in particular pp. 155-71. On the link between theoretical researches and practical applications, see Miler A.I. 1984, p. 108.

the electric current arose from the rotation of the magnet around its own lines of force.[31] In terms of the *seat* of the electromotive force emerging from the rotation between wire and magnet, two opposite interpretations were at stake. If the lines of force rotated together with the magnet, they would *cut* the wire: as a consequence, the *seat* of the electromotive force was placed in the wire. If the lines of force did not rotate together with the magnet, they could not *cut* the wire and therefore the wire could not be the *seat* of the electromotive force: the *seat* of the force would be in the magnet itself, which would be *cut* by its own lines of force.[32]

In 1841, the query of the *seat* of electromagnetic actions had been taken into account by a scientist supporting a different theoretical framework: W. Weber published a paper in *Annalen der Physik* under the title "Unipolare Induction". Indeed, in the late nineteenth century, the query of unipolar induction raised important theoretical debates. In a paper published in 1890 in *Annalen der Physik* and devoted to electromagnetic equations for moving bodies, Hertz considered unipolar induction as an instance of "electric force produced by a convective motion of magnetism". He started from the general hypothesis that "ether which is hypothetically assumed to exist in the interior of ponderable matter only moves with it". As a consequence, "the absolute motion of a rigid system of bodies has no effect upon any internal electromagnetic processes whatever in it". Matter, in its motion, "carries with it the lines of force", although the concept itself of *lines of force* was questionable, for they "simply represent a symbol for special conditions of matter". In a sharper way, he stated that "speaking of an independent motion of such conditions" had "no meaning".[33] In 1895, Larmor devoted some passages to unipolar induction in the second part of his series of papers "A Dynamical Theory of the Electric and Luminiferous Medium". He explained the phenomenon in terms of his recently established "electron". The electrons contained in the magnet "moved across the magnetic field of the aether" and experienced a force producing "an electromotive force along the revolving magnet". In particular, this force gave rise to an "electric separation by drifting the positive ions towards the axis and in the direction

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[31] See, for instance, Faraday M. 1831, in Faraday M. 1932, vol. I, p. 402; Faraday M. 1855, in Faraday M. 1965, vol. III, pp. 336-7.

[32] In a passage dated July 1851, to be found in the fifth volume of his *Diaries*, Faraday remarked that, when "the magnet is still and the wire is moving, it seems unlikely that the current should be generated any where else than in the moving wire". The interpretation was more complex in the case of the moving magnet, and he wondered: "when the magnet is moving, where is the current then generated? Do the lines of force revolve with the magnet or do they not?" See Faraday M. 1851, in Faraday M. 1934, vol. V, pp. 397-8.

[33] Hertz H. 1890b, in Hertz H. 1962, p. 242, 246, 250 and 255.

of the length of the magnet one way, and negative ions the opposite way".[34] In 1900, Poincaré published a paper on unipolar induction, in a journal devoted to technicians, even though the content was more theoretically than technically oriented. On the first page, Poincaré stated that the observed magnetic field could not change because of a symmetrical rotation of the magnet. In other words, the observed magnetic field could not be affected by that kind of motion. In the end, on the motion of lines of force, he concluded that, both in closed and in open circuits, "the query cannot be solved, for it has no meaning".[35] Hertz and Poincaré had in common a limited trust in lines of force, looked upon as mere mental representations of doubtful usefulness. Nevertheless, this was not a general trend in the last years of the nineteenth century. As I will show in some detail in the following chapters, some British scientists, like Poynting and J.J. Thomson, dissatisfied with Maxwell's concept of "displacement current", found in lines of force or, better, in *tubes of force*, the way to attain a more concrete or *physical* representation of electromagnetic actions.[36]

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[34] Larmor J. 1895, p. 727.

[35] Poincaré H. 1900b, p. 41 : "… cet aimant peut être fixe ou tourner autour de son axe; dans tous les cas le champ magnétique dû à cet aimant est invariable; car à cause de la forme symétrique de l'aimant, la rotation de l'aimant ne peut rien changer à ce champ". See also p. 53: "la question ne peut être résolue parce qu'elle n'a pas de sens".

[36] See Poynting J.H. 1885, pp. 277-8, and Thomson J.J. 1891, pp. 149-50. See chapters 8 and 12 of the present book.

4. Professionalisation and methodological debates

In the 1870s, Darwin's theory gave rise to a debate on the age of the Sun. On the one hand, Darwin and the geologists needed centuries of millions of years to account for the slow processes of evolution; on the other hand, W. Thomson and other physicists claimed that the Sun could have been *living* only for some decades of millions of years. Physicists could rely only on the then known physical effects to explain the energy processes taking place in the Earth and in the Sun, which were interpreted as thermal engines undergoing the laws of thermodynamics. In the first editions of Darwin's *Origin of Species* there are many passages concerning the immense time interval required in order to allow the natural selection to act. In the chapter "Imperfection of the geological record" and in the last chapter of the book, Darwin pointed out how difficult it was even to imagine the slowness of transformations and the amount of time required.[1] There is no reference to W. Thomson in the book, nor does his name appear in the *Index*, in the last pages of the book. On the contrary, after the sixth edition, Darwin added a new chapter, "Miscellaneous objections to the theory of natural selection", and took into account Thomson's objections. The index shows two references to Thomson, the first in the chapter on geological records, and the second in the conclusive chapter. In the first, Darwin acknowledged how problematic an estimate of the world's lifetime was, and how uncertain the hypotheses on the first stages of that lifetime were.[2] In the second reference, Darwin remarked once again that scientists had not enough knowledge "to speculate with safety on its past duration" and, in particular, that "we do not know at what rate species change". In any case, that quarrel, involving even ideological and theological queries, can also be interpreted as a clue of the growing gap between physical sciences and other sciences.[3] This was consistent with the process of specialisation and

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[1] Darwin C. 1860, pp. 282-8 and p. 482: "The mind cannot possibly grasp the full meaning of the term of a hundred million years; it cannot add up and perceive the full effects of many slight variations, accumulated during an almost infinite number of generations."

[2] Darwin C. 1958, p. 309: " Here we encounter a formidable objection; for it seems doubtful whether the earth, in a fit state for the habitation of living creatures, has lasted long enough. Sir William Thomson concludes that the consolidation of the crust can hardly have occurred less than or more than 400 millions years ago, but probably not less than 98 or more than 200 million years. […] It is, however, probable, as Sir William Thomson insists, that the world at a very early period was subjected to more rapid and violent changes in its physical conditions than those now occurring; ….". For some detail on the origin of Sun heat in W. Thomson's speculations, see Harman P.H. 1982, p. 67.

[3] Darwin C. 1958, p. 431. On some scientific and religious issues involved in that debate, see Kjærgaard P.C. 2002, p. 255. I find worth quoting J.J. Thomson's report on W. Thomson's

professionalisation of the different sciences, which took place in the last decades of the nineteenth century.[4] The different sciences required specific methods and specific conceptual tools; they were becoming so different from each other that communication was becoming more and more difficult.

We have to notice that the words *scientist* and *physicist* were introduced in England around the middle of the nineteenth century, even though they did not have much success among those who practised what we nowadays call *science* or *physics*. This is well known to historians: in 1964, S. Ross showed how many debates, even philological ones, took place, in the second half of the nineteenth century, on the choice of those two words. In the British context, *scientist* and *physicists* appeared as foreign and unpleasant qualifications, not so convenient and honourable for men of science. The word *scientist* echoed trades and business; men of science felt that the word "degradated their labours of love to a drudgery for profit or salary".[5] Also H. Kragh reminded us that, before the end of the nineteenth century, "the profession of scientist did not really exist": a researcher was called "savant, natural philosopher, man of science, virtuoso, …". What we now call *physics* stemmed from the tree of natural philosophy as a professional discipline endowed with "methods that markedly distinguished it from astronomy, chemistry and mathematics". D. Cahan pointed out the emergence, in the last decades of the nineteenth century, of definite boundaries between science and other intellectual activities, as well as the emergence of definite boundaries among the different sciences. Furthermore he argued that "there was no identifiable scientific community before the early nineteenth century."[6] I agree only in part with Cahan, for even in the seventeenth century, the members of the Royal Society or the correspondents of M.

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commitment to the determination of the Age of the Earth: "he told me that before the discovery of radium had made some of his assumptions untenable, he regarded his work on the Age of the Earth as the most important of all." The core of his calculation was "the loss of heat by the earth due to the radiation from its surface of the heat coming up by conduction from the warmer parts below". The result he attained required that "the time between now and the solidification of the earth's crust could not be very much greater than 100 million years". See Thomson J.J. 1936, pp. 420-21.

[4] Astronomy was an exception: it was already a profession. I am indebted to A. Gualandi for this specification.

[5] Ross S. 1964, p. 66. Among other interesting quotations, Ross reported Faraday and W. Thomson's reactions: see pp. 72-3. The noun/adjective *scientifique*, in France, and the noun *Naturwissenschaftler*, in Germany, raised a similar debate. See, for instance, Harman P.M. 1985a, p. 2.

[6] Kragh H. 1987, p. 25 and Cahan D. 2003, p. 11 (see also p. 4). See also Kjærgaard P.C. 2002, pp. 260 and 281, and Buchwald J.Z. and Sungook Hong 2003, p. 165 (for an account of the historiographical debate concerning the emergence of *Physics*, see pp. 166-7).

Mersenne could be qualified as a *scientific community*. The fact is that, in the course of the nineteenth century, the meaning of *scientific community* changed, because of the emergence of definite boundaries among different sciences and because of the transformation of scientific practice into a job. At the beginning of the twentieth century, Merz remarked that the process of specialisation concerning different sciences was soon followed by a process of sub-specialisation. In 1887, for instance, Arrhenius' researches led to the official birth of physical chemistry. The same year saw the publication of the first scientific journal specifically devoted to that field of chemistry, the German *Zeitschrift für physikalische Chemie*.[7]

In the context of British Universities, what we now call "physics" was taught under the name of two different subjects: "natural philosophy" in Scottish Universities and "mixed mathematics" at Cambridge.[8] In 1848, the Cambridge Mathematical Tripos underwent a first reform, in order to include mathematical physics, though neither electricity nor heat were taken into account. In the 1870s, another tradition emerged: the experimental physics of the Cavendish Laboratory. Maxwell, who was committed to the direction of this laboratory, proposed that also electricity and magnetism were taught, and introduced them in the Mathematical Tripos. Subsequently, J.J. Thomson, in its turn director of the Cavendish Laboratory, reformed the Natural Science Tripos, qualifying them in a more mathematical way. During the last decade of the nineteenth century, the training of those who we nowadays qualify as physicists passed slowly from the Mathematical Tripos to the Natural Science Tripos. Around the end of the century, Larmor took care of the former and J.J. Thomson of the latter. However, in the last decades of nineteenth century, at Cambridge University, the importance of physics grew in both Tripos.[9]

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[7] See Merz J. T. 1912, pp. 165-6. Arrhenius claimed that both electrical and chemical effects took place in salt dissociation. How startling Arrhenius conjecture appeared to contemporary scientists was showed by J.J. Thomson in his *Recollections*: that a dilute solution of *KCl* contained positive ions of potassium and negative ions of chlorine seemed quite strange, for "potassium itself is so violently acted upon by water that a piece of the metal thrown on water burst into flame". The assumption that an atom of potassium "would not be acted upon in the water seemed as reasonable as to suppose that a man could escape getting wet by diving into the sea". See Thomson J.J. 1936, pp. 389-90.

[8] According to D.B. Wilson, what we name *British physics* should be considered as the interplay between Scottish tradition and Cambridge tradition. See Wilson D.B. 1985, pp. 12-3. See also Harman P.M. 1985a, p. 4.

[9] See Warwick A. 2003, pp. 102, 111 and 218, Darrigol O. 2000, pp. 166 and 343, Harman P.M. 1985a, pp. 2, 10 and 11, and Harman P.M. 1985b, p. 207. Consistently with the widespread trend towards specialisation, in the context of Natural Science Tripos, "in 1882 the Board of Natural Science Studies was divided into the Special Board for Physics and

The process of divergence, which took place among sciences, concerned physics as well: we have already taken into account the setting up of different sections in the field of physics and the emergence of different foundations or *world-views*. Moreover, that process affected even the methods and aims of physics and took place mainly in German speaking countries. Methodological debates did not resemble previous philosophical debates on the nature and boundaries of natural knowledge. They emerged in close connection with actual researches undertaken in the fields of physical sciences (and physiology); they were placed within the boundaries of the actual scientific debate. As Cassirer noticed half a century ago, in the first decades of modern age, science had fought over its own existence. In the late nineteenth century, conflict and competition were brought inside the boundaries of science, in particular inside the boundaries of physics.[10] In 1876, G. Kirchhoff, after he was appointed to the chairs of mathematical physics and then theoretical physics in Berlin, published the first volume of his four volume masterpiece, *Vorlesungen über mathematische Physik*. In the introduction, he claimed that physics, in particular mechanics, could not aspire to the *explanation* of the physical world, but had to confine itself to mere *description* of phenomena. Scientists had to confine themselves to "how phenomena take place, without inquiring into their causes". He claimed he was only interested in a pure description, based on the concepts of "space, time and matter" and carried out by means of "pure mathematics".[11] As we have already seen, the concept of force was considered as an auxiliary concept, devoid of any deep physical meaning: in no way could it be associated to the concept of cause.[12] E. Mach went much further, exposing physics to a deep analysis, which was at the same time logical, conceptual and historical. He claimed that every class of phenomena could undergo a

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Chemistry and the Special Board for Biology and Geology". See Wilson D.B. 1982, pp. 338-40 and 347.

[10] Cassirer E. 1950, p. 84: "When Mach or Planck, Boltzmann or Ostwald, Poincaré or Duhem are asked what a physical theory is and what it can accomplish we receive not only different but contradictory answers, and it is clear that we are witnessing more than a change in the purpose and intent of investigation."

[11] Kirchhoff G. 1877, "Vorrede", p. III: "Aus diesem Grunde stelle ich es als die Aufgabe der Mechanik hin, die in der Natur vor sich gehenden Bewegungen zu beschreiben, und zwar vollständig und auf die einfachste Weise zu *beschreiben*. Ich will damit sagen, dass es sich nur darum handeln soll, anzugeben, *welches* die Erscheinungen sind, die stattfinden, nicht aber darum, ihre *Ursachen* zu ermitteln. Wenn man hiervon ausgeht und die Vorstellungen von Raum, Zeit und Materie voraussetzt, so gelangt man durch rein mathematische Betrachtungen zu den allgemeinen Gleichungen der Mechanik."

[12] Kirchhoff G. 1877, "Vorrede", p. IV: "... die Einführung der Kräfte hier nur ein Mittel bildet, um die Ausdrucksweise zu vereinfachen, um nämlich in kurzen Worten Gleichungen auszudrücken, ..."

plurality of explanations and, in addition, that explanations had changed over time (and will change over time), in the course of the history of science. In 1872, in his first important book, *Die Geschichte und die Wurzel der Satzes von der Erhaltung der Arbeit*, he stressed the importance of history in scientific knowledge: "even though we could learn from history nothing else than the variability of points of view", he wrote, "this would be really precious". The physical knowledge is historical in its nature and, at the same time, it involves a plurality of interpretations, as well as every kind of knowledge. Mach thought that history helped us look upon science as "something neither static nor complete".[13]

According to Mach, physical researches had to follow a phenomenological approach: physics had to deal with phenomena and relationships among phenomena. We can only understand "phenomena by means of other phenomena": he claimed that even the description of physical events in terms of space and time was, in the end, a description in terms of optical devices and astronomical rotations.[14] That concept was repeatedly stressed by Mach in different times and different books, papers and lectures. For Mach, science was a tool able to bring in our mind some kind of order: this order was nothing else than knowledge itself or, better, the process of knowledge. The main property of science was its usefulness for the mind of researchers: by means of that order, or "economy of thought", they become able to save themselves time and intellectual efforts.[15] Nevertheless, *economy* did not mean a sort of synthetic collection of phenomena and laws, in accordance to a trivial phenomenology; it required an intellectual performance and a search for connections. Scientists had to "find, then, what remains unaltered in the phenomena of nature, to discover the elements thereof and the mode of their interconnection and

[13] Mach E. 1872, in Mach E. 1909, p. 3: "In der That, wenn man aus der Geschichte nicht lernen würde, als die Verhänerlichkeit der Ansichten, so wäre sie schon unbezahlbar. Von der Wissenschaft gilt mehr als von irgend einem andern Ding das Heraklit'sche Wort: 'Mann kann nicht zweimal in denselben Fluss steigen.' Die Versuche den schönen Augenblick durch Lehrbücher festzuhalten, sind stets vergebliche gewesen. Man gewöhne sich also bei Zeiten daran, dass die Wissenschaft unfertig, veränderlich sei. Wer nur eine Ansicht oder eine Form einer Ansicht kennt, glaubt nicht, dass je eine andere dagewesen, glaubt nicht, dass je eine andere kommen wird, der zweifelt nicht, der prüft nicht."

[14] Mach E. 1872, in Mach E. 1909, p. 35: "Das gegenwärtige Streben der Physik geht dahin, jede Erscheinung als Functionen anderer Erscheinungen und gewisser Raum – und Zeitlagen darzustellen. Denken wir uns nun die Raum – und Zeitlagen in den betreffenden Gleichungen in der oben gedachten Weise ersetzt, so erhalten wir einfach jede Erscheinung als Function anderer Erscheinungen."

[15] This methodological precept can be found in Mach's *Mechanics*. See Mach E. 1883, in Mach E. 1960, p. xxiii and p. 7.

interdependence". *Economy* involved some kind of theoretical activity, devoted to "make the waiting for new experiences unnecessary". Mach considered science deeply committed to unification; one of its aims was "discovering methods of describing the greatest possible number of different objects at once and in the most concise manner".[16]

Mach participated to the debate on the foundations of physics in an original way: he thought that mechanics was neither the starting point of physics nor its general framework. What we call mechanics was nothing else but the last link in a chain of experiences put in some order by our laws; as a consequence, he stated, "purely mechanical phenomena do not exist". Experiences and sensations concerned physiology; physiology, in its turn, dealt with chemical, thermal and electric phenomena, rather than mechanical. In brief, "purely mechanical phenomena, accordingly, are abstractions, made, either intentionally or from necessity, for facilitating our comprehension of things".[17] We could say that physiology came before physics and, in physics, electromagnetic and thermal phenomena came before mechanical explanations.

The relationship between scientific knowledge and the wide set of human experiences attracted the attention of other *fin de siècle* scientists. Planck, who did not share Mach's epistemology and subsequently had a sharp debate with him, wondered whether thermodynamics should be founded on mechanics or on experience.[18] He chose a "more inductive approach" which, he claimed, corresponded "to the present state of science". Nevertheless, that approach did not exclude a theoretical foundation: thermodynamics could be based on mechanical foundation or on electromagnetic foundation as

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[16] Mach E. 1883, in Mach E. 1960, pp. 7-8.

[17] Mach E. 1883, in Mach E. 1960, p. 596: "The production of mutual accelerations in masses is, to all appearances, a purely dynamical phenomenon. But with these dynamical results are always associated thermal, magnetic, electrical, and chemical phenomena, and the former are always modified in proportion as the latter are asserted. On the other hand, thermal, magnetic, electrical, and chemical conditions also can produce motions." See also p. 612: "Processes, thus, that in appearance are purely mechanical, are, in addition, to their evident mechanical features, always physiological, and, consequently, also electrical, chemical, and so forth."

[18] Planck's paper (the text of a 1908 public lecture held at Leiden University), published in 1909, Mach's answer, published in 1910, and Planck's subsequent paper, published in the same year, all appeared in *Physikalische Zeitschrift*. See *Pys. Zeit.* 1910, XI, pp. 599-606, and 1186-90. See also "From the Preface to the first edition - April 1897", in Planck M. 1945, p. viii: "A third treatment of Thermodynamics has hitherto proved to be the most fruitful. This method is distinct from the other two, in that it does not advance the mechanical theory of heat, but, keeping aloof from definite assumptions as to its nature, starts direct from a few very general empirical facts, mainly the two fundamental principles of Thermodynamics."

well. Which set of concepts or entities were more primitive seemed to Planck not so important: the most significant step was, in any case, the achievement of a real unification in the comprehension of nature.[19] The principle of Conservation of Energy appeared as the natural candidate for the unification of physics. Nevertheless, there was a debate also around the concept of energy: that debate did not concern essentially empirical or mathematical aspects, but the comparison between two different theoretical models. On the one hand, the more traditional description of phenomena by means of space and time, equations of motion and geometrical paths. On the other hand, only processes, transformations of energy and the corresponding numerical accounts. The second theoretical model was then known with the name "energetism". Another query concerned energy itself: on the one hand, it could be imagined as a sort of substance, endowed with autonomous existence with regard to material bodies; on the other, it could be imagined as a sort of property or relation among material bodies. In Germany the debate was quite sharp, mainly around 1895, when the energetists were the chief characters of the annual conference of German scientists and physicians held in Lübeck.[20] One of those characters, G. Helm, in a book published in 1898, pointed out the relevant features of the more radical energetism. He considered energetics as the physical approach "*capable to a much greater degree than the old theories of adapting itself directly to our experiences*". His conception of "general theoretical physics" was so strict than it could accept "neither atoms nor energy nor any other such concept, but only those *experiences* which are immediately derived from groups of observations". Although energy was the key concept, he refused "to attribute substantial existence to energy", for he saw in it "a dubious departure from the original clarity of Robert Mayer's views".[21] As already remarked, another important supporter of energetism was W. Ostwald. He spoke against the mechanical world-view and against the atomic models of matter: he claimed a strict phenomenological approach. At the beginning, even Boltzmann was interested in the new theoretical turn: he was among the organizers of the Lübeck conference, and contributed to the choice of the

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[19] Planck M. 1897, in Planck M. 1945, p. ix: "This last, more inductive treatment [...] cannot be considered as final, however, but may have in time to yield to a mechanical, or perhaps an electro-magnetic theory. Although it may be of advantage for a time to consider the activities of nature – Heat, Motion, Electricity, etc. – as different in quality, and to suppress the question as to their common nature, still our aspiration after a uniform theory of nature, on a mechanical basis or otherwise, which has derived such powerful encouragement from the discovery of the principle of conservation of energy, can never be permanently repressed."

[20] See McCormmach R. and Jungnickel C. 1986, vol. 2, p. 220, and Cassirer E. 1950, pp. 96-7.

[21] Helm G. 1898, p. 362; English edition: Helm G. 1992, p. 401.

subject. Nevertheless, he could not share that sharp methodological commitment and opposed Ostwald and Helm's theses. The debate continued after the conference, through the pages of *Annalen der Physik*, between 1895 and 1896.[22] Although Planck was interested in a phenomenological foundation of thermodynamics, in a paper published in 1896, he raised many objections to energetism: among them, the missing distinction between reversible and irreversible phenomena.[23] In addition, he thought that energetism had no heuristic power and it had reduced itself to an abstract speculation.

In the same year, from the British Isles, FitzGerald, Professor at Trinity College, Dublin, qualified Ostwald's energetics as "unphilosophical as well as unscientific". FitzGerald emphasised the positive role of hypotheses and conceptual models in scientific enterprise; in other words, he emphasised the scientific value of theoretical physics compared to mathematical phenomenology, which denied those conceptual components. He thought that the scientist needed much more than a "dry catalogue" of facts: he needed, for instance, "a theory of gravitation" as well as "a hypothesis of natural selection".[24] An extreme commitment to phenomenology, he stated, would lead us to reject even the hypothesis that men are able to communicate to each other: "if he rejects all hypotheses, why not this?", FitzGerald asked Ostwald. A point of strong opposition was "the unexplained constitution of an ether" and, in general, the mechanical models applied in both optics and electromagnetism. FitzGerald claimed the fruitfulness of theoretical models, in particular mechanical models, and addressed his sharp criticism to the core of energetism. He depicted it as a regressive methodology, which made use of energy in the same way as, in the previous century, natural philosophers had made use of lists of subtle fluids, which had to be continuously updated.[25] FitzGerald did not go so far as to criticize his British colleagues, but we can remark that this kind of criticism well suited British physicists and their attempt to represent new electromagnetic effects by means of new mathematical expressions for energy added to the Lagrangian of the physical system under consideration.

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[22] See Planck M. 1896, Helm G. 1895a, Ostwald W. 1896, and Boltzmann L. 1896a. See also McCormmach R. and Jungnickel C. 1986, vol. 2, pp. 219-20. For a short account of the complex conceptual net involving Boltzmann, Ostwald and Planck's approaches to thermodynamics see Harman P.M. 1982, pp. 147-8.

[23] See Planck M. 1896, pp. 76-7: "Vor Allem hat die Energetik die Verschleierung des principiellen Gegensatzes zwischen reversibeln und irreversibeln Processen verschuldet, an dessen Herausbereitung und weiterer Vertiefung nach meiner Ueberzeugung jeder Fortschritt der Thermodynamik und der Verwandtschaftslehre geknüpft ist."

[24] FitzGerald F.G. 1896, p. 441.

[25] FitzGerald F.G. 1896, p. 441-2.

With regard to aether, its existence was not, in general, questioned, but its role in the representation of the physical world was twofold: as a primitive universal substratum, on the one hand, or as a medium among other media, on the other. The complex interplay between physical theories and general philosophical issues led some scientists to qualify the first representation as anti-materialistic. The fact is that, after the polemical address of Ostwald, in 1895 at Lübeck, devising mechanical models was censured as *materialistic* by the upholders of energetism, and their opponents censured energetism as *metaphysical*.[26] Nevertheless, I think that the debate involving Ostwald and Fitzgerald cannot be translated in terms of *materialism* and *idealism*. If FitzGerald is qualified as an idealist or anti-materialist because of his *aethereal* world-view, on the contrary, his dynamical structures of aether, akin to W. Thomson's vortex atom or Larmor's electron, were qualified as materialistic machinery by a German anti-materialist like Ostwald. I think that Kragh has offered a good synthesis of the debate between Ostwald and Fitzgerald when he states that "FitzGerald agreed with Ostwald's anti-materialism, but, referring to vortex atom, denied that it implied anti-mechanism".[27]

However, energy had become a pivotal concept in all sections of physics. In Maxwell's theory, the interpretation of energy was closely linked to the interpretation on the nature of electromagnetic actions: the theoretical model of energy which Maxwell preferred was a consequence of the theoretical model of contiguous action which he supported. Afterwards, Poynting put electromagnetic energy in the foreground: invisible, transversal streams of energy were interpreted as the cause of visible electric currents.[28] We have seen that some British physicists, like O. Lodge, emphasised the *substantialisation* of energy, namely the conception of energy akin to matter. In some way, energy, like matter, could spread through space and time: conservation of energy corresponded to the process of transfer from place to place in a finite time. The attention was turned to the propagating entity, namely energy, rather than to the medium through which the propagation took place, namely aether. Nevertheless, this did not cause the medium to be faded into the background.[29] The substantialisation of energy was criticized by other British scientists, like O. Heaviside, as well

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[26] See Merz J.T. 1912, p. 186, and Kragh H. 1996, pp. 64 and 67.

[27] Kragh H. 1996, p. 85.

[28] See Poynting J.H. 1884, Poynting J.H. 1885a, and chapter 8 of the present book.

[29] I do not think that Poynting's substantialisation of energy opened the way to the desubstantialisation of aether. I think that the complex interplay among electromagnetic energy, aether and Faraday's tubes of force in Poynting and J.J. Thomson's theories led to a different kind of substantialisation of aether rather than an actual desubstantialisation.

as by German scientists like Helmholtz and Hertz.[30] In a section on the conservation of energy, in a paper devoted to electromagnetic equations for bodies at rest, Hertz expressed his scepticism: about energy, he wrote, "there appears to me to be much doubt as to what significance can be attached to its localisation and the following of it from point to point". Nevertheless, that conception was warmly received in Germany by a young assistant of Helmholtz, W. Wien, and subsequently widely discussed by a *Privatdozent* of Karlsruhe University, G. Mie.[31]

As I have already pointed out, in Planck's 1887 treatise on the Principle of conservation of energy, localisation and *individualisation* of energy were as fundamental as its conservation. The theoretical model of streams of energy was not affected by the different hypotheses on the nature of the medium supporting that stream. According to Planck, a theory on the transfer of energy could dissociate its lot from the lot of whatever theory of aether. As he stated, "the fact that aether does not behave like solid, liquid or gaseous matter does not cause any difficulty to the infinitesimal theory".[32] This indifference can be easily understood if we think that the process of substantialisation, subdivision into elements and transfer of energy decreased the importance of the medium of propagation. The last passages of Planck's book focused once again on his "infinitesimal theory": the general theoretical model of actions propagating with continuity through both space and time seemed to Planck the new horizon of physics.[33]

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[30] See Hertz. H. 1890a, in Hertz H. 1962, p. 220: "Considerations of this kind have not been yet been successfully applied to the simplest cases of transference of energy in ordinary mechanics; and hence it is still an open question whether, and to what extent, the conception of energy admits of being treated in this manner." Hertz displayed an interesting mechanical example in "Supplementary notes", at the end of the book (Hertz. H. 1890, in Hertz H. 1962, pp. 276-7). For a detailed analysis of this example, see Buchwald 1985a, pp. 41-3.

[31] See Wien W. 1892a and Mie G. 1898. See also McCormmach R. and Jungnickel C. 1986, vol. 2, p. 224.

[32] Planck M. 1887, pp. 245-6: "Und zwar ist es offenbar zunächst von grö�ter Wichtigkeit, dass Wesen dieser Theorie vollkommen zu trennen von allen Hypothesen, mit denen man der Anschauung zu Hilfe kommt, die aber mit der Theorie an und für sich nichts zu thun haben. Die Schwierigkeiten, welche dabei unserem Vorstellungsvermögen erwachsen können, kommen durchaus nicht in Betracht; dass z. B. der Äther sich nicht so verhält wie einer der uns bekannten festen, flüssigen oder gasförmigen Körper, ist ein Umstand, welcher der Infinitesimaltheorie nicht die mindeste Verlegenheit bereitet." On Planck's "Inifinitesimaltheorie" see chapter 1 of the present book.

[33] Planck M. 1887, p. 247: "Dann findet jede Erscheinung ihre vollständige Erklärung in den räumlich und zeitlich unmittelbar benachbarten Umständen, und alle endlichen Processe setzen sich aus Infinitesimalwirkungen zusammen."

Generally speaking, the methodological tension between phenomenological (or empirical, or *inductive*) approaches and theoretical approaches was one of the main features of the debate which took place at the end of the nineteenth century.[34] Those who practised a phenomenological approach opposed mechanical models, but mechanics, the target of the sharpest criticism of phenomenologists, was not abandoned: some scientists did not relinquish their claim to a mechanical approach to physics. Helmholtz, the *dean* of German physics, and W. Thomson, the *dean* of British physics, pursued some kind of general mechanical view. In particular, W. Thomson pointed out the necessity of mechanical models for an actual comprehension of physical processes. Electromagnetic phenomena, for instance, seemed to him too abstract and obscure without the help of detailed mechanical models. The electromagnetic explanation of light, in particular, seemed to him unsatisfactory: "I want to understand light as well as I can", he said in 1884, "without introducing things that we understand even less of".[35]

After ten years, there was a very sophisticated theoretical attempt to rebuild a mechanical foundation of physics: Hertz's 1894 *Die Principien der Mechanik*. In the "Preface", he described his methodology and the general aim of the book. His main task was the reduction of all physics to a generalised new mechanics. Fundamental laws and concepts of mechanics had to be clarified, in order to rebuild a reliable theoretical framework, where "the ideas of force and the other fundamental ideas of mechanics appear stripped of the last remnant of obscurity". He was not principally interested in mathematical details: what he considered new and more interesting in his reconstruction of mechanics was "the logical and philosophical aspect of the matter".[36] As in the case of his electromagnetic theory, he tried to find a balance between a formalistic or mathematical approach and a more sophisticated theoretical approach. On the one hand, he set up a theory by means of definitions, theorems and differential equations. On the other

[34] Half a century ago, Cassirer pointed out the risk intrinsically connected to every phenomenology: a drift towards physiology and psychology. See Cassirer E. 1950, p. 101.

[35] See the well-known passage of W. Thomson's 1884 *Baltimore Lectures*, in its original version, reprinted in Thomson W. 1987, p. 206: "I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model I understand it. As long as I cannot make a mechanical model all the way through I cannot understand; and that is why I cannot get the electromagnetic theory. Hence I cannot grasp the electromagnetic theory of light. I firmly believe in an electro-magnetic theory of light, and that when we understand electricity and magnetism and light, we shall see them all together as part of a whole." There is a slightly different quotation in Cassirer 1950, p. 115: the fact is that the version published in 1904 by the Cambridge University Press contains subsequent alterations.

[36] Hertz H. 1894, in Hertz H. 1956, "Author's Preface".

hand, he acknowledged that a theory required a conceptual representation, or a rational *invention*, in order to be put in correspondence with nature. A theory is a good representation, he stated, when the relationships among the abstract symbols of the representation correspond to the relationships among the real entities associated to those symbols. More in detail, in Hertz methodology, the make-up of a theory required five steps. First, scientists "form for ourselves images or symbols of external objects ". Second, they work out some consequences from these *images*. Third, they translate the observed effects into *images*. Fourth, they check whether "the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured". In other words, they check the mutual consistency between the outcomes of the second and third steps, namely the consistency of the whole theoretical process. Fifth, they have to undertake another kind of control: the "conformity" or "uniformity between nature and our thought".[37] The methodological perspective of Hertz was quite different from Mach's. For the latter, physical laws had the function of summarising a certain set of empirical information. For Hertz, the relationship between a theory and the corresponding empirical information was more complex and definitely more indirect. Theory represented a sort of intermediary between experiences and mathematical laws: laws match experiences only at the end of a rational process.[38]

The new mechanics Hertz put forward was based only on space, time and mass: we have already noted that forces were banned, for he considered *force* as a puzzling concept. Nevertheless, he proposed to replace puzzling forces by perhaps an even more puzzling concept: hidden masses and motions (*verborgene Massen*, *verborgene Bewegungen*). To better understand Hertz's conceptual strategy, the adjective "hidden" should be submitted to a deeper analysis. Here I am confining myself to some passages of a D'Agostino study, where he highlights two different meanings of "hidden": either invisible, although actually existing in the physical world, or existing in our mind, in our representation of the physical world. The second meaning displays a more

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[37] Hertz H. 1894 in Hertz H. 1956, p. 1.

[38] It seems to me that Cassirer correctly pointed out the mark of Kant's philosophy, and the philosophical distance between Mach and Hertz. See Cassirer E. 1950, p. 106: "The fundamental concepts of physics, according to Mach, are the product of and the passive impressions left by the effects of objects upon the sense organs, whereas for Hertz they are the expression of a highly complex intellectual process – a process in which theorizing holds full sway in order to attain to its goal through experience and therein to find confirmation or justification. Accordingly Hertz held fast to the possibility and necessity of a 'pure natural science' in the sense of Kant – an idea that Mach and the phenomenalistic physics which he represented could only reject with horror."

Kantian *flavour*.[39] As far as I know, Hertz himself did not clarify the matter in a satisfactory way. However, this strategy seemed not so odd to Hertz, for he claimed that thermodynamics and electromagnetic theories had realised that kind of conceptual shift from forces to hidden motions. The motion of invisible atoms in the kinetic theory of heat, and electromagnetic stresses in the aether seemed to him instances of that shift. Helmholtz and W. Thomson, he stated, had applied the same strategy.[40]

In the end, physics was reduced to mechanics and mechanics was reduced to geometry and kinematics. This new physics appeared in accordance with the theoretical model of contiguous action. He thought that the alliance between that conceptual model and a physical-geometrical representation of actions had shown to be particularly useful in electromagnetic phenomena. Those phenomena represented the field "in which the decisive battle between these different fundamental assumptions of mechanics must be fought out". Hertz hoped that the success of recent electromagnetic theories could be reiterated in the field of mechanics.[41] The axiomatic framework built by Hertz emphasised the logical consistency of the system, but the book lacked in detailed examples and convincing applications. We can presume that this was due to Hertz's untimely death. However Helmholtz, in his preface to the book, pointed out that Hertz had undertaken an ambitious task, had pursued it in an original way, but he had not accomplished it.[42] Hertz's book was definitely the most sophisticated and enthusiastic attempt to achieve a mechanical representation of nature in the late nineteenth century; at the same time, it appeared to contemporaries as a *too late*

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[39] D'Agostino S. 2000a, pp. 194-5.

[40] Hertz H. 1894, in Hertz H. 1956, p. 26: "The forces connected with heat have been traced back with certainty to the concealed motions of tangible masse. Through Maxwell's labours the supposition that electro-magnetic forces are due to the motion of concealed masses as become almost a conviction."

[41] Hertz H. 1894, in Hertz H. 1956, p. 41: "… the assumption of invariable distance-forces only yields a first approximation to the truth; a case which has already arisen in the sphere of electric and magnetic forces. […] a second approximation to the truth can be attained by tracing back the supposed actions-at-a-distance to motions in all-pervading medium whose smallest parts are subjected to rigid connections; a case which also seems to be nearly realised in the same sphere."

[42] Helmholtz H. 1894, in Hertz H. 1956, "Preface by H. von Helmholtz", without page number: "He has chosen as his starting-point that of the oldest mechanical theories, namely, conception that all mechanical processes go on as if the connections between the various parts which act upon each other were fixed. […] Unfortunately he has not given examples illustrating the manner in which he supposed such hypothetical mechanism to act; to explain even the simplest cases of physical forces on these lines will clearly require much scientific insight and imaginative power."

contribution, realised when an updated version of mechanics was looked upon as an outmoded subject of research.

5. The complex interplay between words and concepts

At this point, a semantic specification could be useful: I am using the expression *electromagnetic theory* instead of *electrodynamics*, for it appears more suitable for the British scientific context. French expressions corresponding to *electrodynamics* or *electrodynamic phenomena* had been introduced by Ampère in the eighteenth section of his essay "Théorie (mathématique) des phénomènes électro-dynamiques"[1], just in opposition to electromagnetic phenomena. According to Ampère, electromagnetism involved necessarily magnets in themselves, whereas electrodynamics involved electric currents, and also explained the nature of magnets. That electrodynamics was preferred to and more fundamental than an electromagnetic theory was consistent with Ampère's conceptual framework, wherein magnetism was nothing else but an electrodynamic effect. "I think I have to qualify them as *electrodynamic phenomena*", Ampère claimed, "just for the phenomena I am dealing with stem from electricity in motion".[2] In Maxwell's *Treatise*, electrodynamics or, better, *electrokinematics*, the title of part II, had become less general than *electromagnetism*, the title of the conclusive part IV, whose scope included electric, magnetic as well as optical phenomena. In addition, the word *electromagnetism* was associated to the model of contiguous action, a model more sympathetic with the conception of an autonomous existence of electric and magnetic actions, even in places far from electric charges and electric currents. The word *Elektrodynamik* was used in the German scientific context, at least until Einstein's 1905 famous paper "Zur Elektrodynamik bewegter Körper". In the late nineteenth century and later, the choice between electrodynamics and electromagnetism was more theoretical than empirical. It did not depend mainly on phenomena in themselves but rather on their interpretation: it dealt with different conceptual models in the tradition of natural philosophy.[3]

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[1] The adjective "mathématique" appeared in the essay published in 1827 in *Mémoires de l'Académie des Sciences*, but did not appear in the volume published by Didot in 1826. The two versions only differ in the content of some notes in the Appendix.

[2] Ampère A.M. 1826, in Ampère A.M. 1887, pp. 114-5: "C'est parce que les phénomènes dont il est ici question ne peuvent être produits que par l'électricité en mouvement, que j'ai cru devoir les désigner sous la dénomination de *phénomènes électro-dynamiques* ; celle de *phénomènes électromagnétiques*, qu'on leur avait donnée jusqu'alors, convenait bien tant qu'il ne s'agissait que de l'action découverte par M. Oersted entre un aimant et un *courant électrique* ; mais elle ne pouvait plus présenter qu'une idée fausse depuis que j'avais trouvé qu'on produisait des phénomènes du même genre sans *aimant* et par la seule action mutuelle de deux *courants électriques*." See also Darrigol O. 2000, p. vii.

[3] I share D'Agostino's historical reconstruction. See D'Agostino S. 2000a, p. 7: "Adopting the spirit of Ampère's works, Gauss and Weber translated the French into *Elektrodynamik*

Maxwell's electromagnetic theory had left some queries without a definite answer. Among them, the nature of the electric charge and electric conduction, but also some magneto-optic effects and optics in moving bodies. Another question concerned the actual production and detection of the expected electromagnetic waves. Around 1880, their production and detection appeared questionable even for British physicists who were very close to Maxwell's conceptions. Nevertheless, at the end of the decade, in the first months of 1888, independently from Hertz, Lodge had produced and detected some kind of electromagnetic waves.[4] The experiments Hertz realised, between the autumn of 1886 and 1888, by means of electric oscillators, showed that electromagnetic waves propagated with finite speed, that oscillations were perpendicular to the direction of propagation and that some typical optical effects, like reflection, refraction and interference could be observed.[5] British scientists were quite surprised when they realised that a German had managed to perform the most systematic experiments on a British theory. In a short paper, published in 1893 in *Nature* but not signed ("almost certainly by Fitzgerald", claims Hunt; see Hunt B.J. 1991, p. 199), the reviewer of Hertz's *Untersuchungen über die Ausbreitung der Electrischen Kraft* stated that Hertz's research "well deserves the best attention of all interested in the greatest scientific advance of the last quarter of the nineteenth century". Hertz could join the list of the main contributors to science: "thermodynamics founded by Carnot and Clausius, conservation of energy by Joule, bacteriology by Pasteur, the origin of species by Darwin, and the functions of the ether by Faraday, Maxwell and Hertz".[6]

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and Helmholtz and Hertz also used this German term in their reinterpretation of Maxwell's ideas. Conversely, the term *electromagnetism* appears to have originated with Ørsted and, extensively used in Faraday's and Maxwell's work, remained the standard word in the Anglo-Saxon tradition. Following the German tradition, Lorentz and Einstein used *Elektrodynamik* while most others theoretical physicists of the twentieth century preferred the English translation of the term, *electrodynamics*. "

[4] Hertz himself acknowledged the importance of Lodge's experimental investigations, and FitzGerald's theoretical predictions. See Hertz H. 1892, in Hertz H. 1962, p. 3. A short account of British contributions can be found in W.Thomson's *Preface* to Hertz's *Electric waves,* the English translation of his *Untersuchungen über die Ausbreitung der Electrischen Kraft*. See Thomson W. 1892, in Hertz H. 1962, p. xiv. Lodge and S.P. Thomson's experiments, and FitzGerald's theoretical researches before 1887, on the track of some kind of electromagnetic perturbations, are discussed in O'Hara J.G. and Pricha W. 1987, pp. 11-14. See also Susskind C. 1964, Hunt B.J. 1991, pp. 33-45, 146-53, and 210, and Darrigol O. 2000, p. 205. On the repetitions and improvements of Hertz's experiments in Britain and Ireland, see O'Hara J.G. and Pricha W. 1987, pp. 14-20.

[5] Hertz H. 1892, in Hertz H. 1962, pp. 1-20.

[6] FitzGerald F. G. (supposed) 1893, p. 539.

Indeed, the so-called *discovery* of electromagnetic waves, foreseen by Maxwell's theory, called into play a complex conceptual transition. Hertz himself, the scientist who is credited to have performed the most convincing experimental corroborations of Maxwell's field theory, at the beginning did not share the basic assumption of that theory. Maxwell's theory was not Hertz's starting point but his end-point, through a conceptual path spanning from 1887 to 1888. As W. Thomson accurately noted in the *Preface* to Hertz's *Electric waves,* the English translation of his *Untersuchungen*, Hertz "began his electrical researches in a problem happily put before him thirteen years ago by Professor von Helmholtz". He had undertaken the experiments on "the relation between electromagnetic forces and dielectric polarisation of insulators, without, in the first place, any idea of discovering a progressive propagation of those forces through space".[7] Hertz himself, in the "theoretical" section of his *Introduction* to *Electric Waves*, acknowledged that "notwithstanding the greatest admiration for Maxwell's mathematical conceptions, … it was not possible for me to be guided in my experiments directly by Maxwell's book". He had started from "Helmholtz's work, as indeed may plainly be seen from the manner in which the experiments are set forth".[8] In the context of the emergence of theoretical physics, in particular the connection between that emergence and the tradition of mathematical physics, it is worth mentioning Hertz' qualification of Maxwell's conceptions as "mathematical".

In Berlin, Hertz had collaborated with Helmholtz, who, starting from 1870, had published some papers devoted to a systematic reconstruction of electrodynamics. He had published a general law for the electrodynamic potential between two elements of electric current, which depended on a free parameter. When that parameter assumed the values +1, -1 e zero, the general law *reproduced* mathematically the theories of F.E. Neumann, W. Weber and J.C. Maxwell. Helmholtz's approach is quite interesting, for it represented a procedure of translation between different theories, in particular theories belonging to the different conceptual models of contiguous action and action at a distance.[9] The distinction between the

[7] Thomson W. 1893b, p. xiv. It seems to me that Buchwald managed to synthetically express the specific hallmark of Hertz's experimental enterprise: "it was designed either to show that something does not occur or else to find something new that was not required by the kind of physical scheme that Hertz deployed". See Buchwald J.Z. 1994, p. xiii.

[8] Hertz H. 1892, in Hertz H. 1962, p. 20. See also Doncel M.G. 1991, p. 1.

[9] For a more detailed analysis of that law, see Wise N.M. 1981, p. 298, Bevilacqua F. 1983, pp. 111-115, and Darrigol O. 1993, p. 233. Darrigol remarked that Helmholtz had been the first scientist to undertake a deep theoretical analysis of Maxwell's theory before Maxwell's *Treatise* was published: see Darrigol O. 2000, p. 225. I have already pointed out

mathematical match and the theoretical mismatch should be stressed: from the theoretical point of view, Helmholtz's translation of Maxwell's theory was quite different from Maxwell's theory.

In the already quoted *Introduction*, Hertz described the intellectual path which led him towards Maxwell's theoretical model.[10] He opened the "theoretical" part with the question: "what is that we call the Faraday-Maxwell theory?" Hertz was aware of having not managed to completely catch the physical meaning of some of Maxwell's propositions; nor has he managed to offer a consistent and unified summary of the whole theory. This sounds not so odd, when we consider that, in order to perform his experiments, he had not been inspired directly by Maxwell's original texts, but by Helmholtz's theoretical framework. Moreover, Hertz acknowledged that Maxwell's theory, as interpreted in Helmholtz's framework, suffered a sort of conceptual misunderstanding.[11] In other words, Maxwell's theory was deeply embedded in the theoretical model of contiguous action and could not merely be replaced by a theory which was mathematically equivalent but conceptually different. Nevertheless, in the following lines, Hertz was tempted to simplify the confrontation with Maxwell's theory: for such a theory was too hard to be conceptually identified in a simple way, he was led to reduce Maxwell's theory to the corresponding set of equations. This reduction was achieved by two steps: in the first, he overlapped Maxwell's theory, Helmholtz's interpretation of Maxwell's theory and his own reconstruction of Maxwell's theory. In the second step, corresponding to a passage frequently quoted, he identified the common part of the three previous entities with Maxwell's equations. In other words, the difficulties in the interpretation of Maxwell's theory led Hertz to give up taking into account the specific theoretical features of that theory.[12]

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that, besides action at-a-distance theories and contiguous action theories, there were the so-called theories of "retarded potentials". See Darrigol O. 2000, pp. 211-13.

[10] Doncel compared Hertz' Laboratory Notes (*Versuchsprotokolle*) with letters and diaries, and found some faults in previous chronological reconstructions of experiments and corresponding interpretations. See Doncel M.G. 1991, in particular p. 6.

[11] Hertz H. 1892, in Hertz H. 1962, p. 20: "But unfortunately, in the special limiting case of Helmholtz's theory which leads to Maxwell's equations, and to which the experiments pointed, the physical basis of Helmholtz's theory disappears, as indeed it always does, as soon as action at-a-distance is disregarded."

[12] Hertz H. 1892, in Hertz H. 1962, p. 21: "Thus the representation of the theory in Maxwell's own work, its representation as a limiting case of Helmholtz's theory, and its representation in the present dissertations – however different in form – have substantially the same inner significance. This common significance of the different modes of representation (and others can certainly be found) appears to me to be the undying part of Maxwell's work. This, and not Maxwell's peculiar conceptions or methods, would I

Those steps were in sharp contrast with the previous acknowledgement of the importance of conceptual components in a theory. Moreover, they were in contrast even with the analysis of the different conceptual models of contemporary electromagnetic theories which Hertz himself undertook in the following pages. That contradiction was pointed out in the already quoted short paper published in *Nature* in 1893: the remarks of the author were quite sharp. He accused Hertz with seeming "to look upon Maxwell's theory as a series of Maxwell's equations" and claimed that "Maxwell has done much more than produce a series of equations that represent electromagnetic actions". The essential point concerned energy: in Maxwell's theory, "energy is stored in the ether by stresses working on strains". Any description of the theory which did not take into account that specific feature, the author wrote, "is a very incomplete representation of Maxwell's theory".[13]

It is quite strange indeed, that Hertz, immediately after the above-mentioned statements, began to analyse the different conceptual model emerged in the recent history of electromagnetism. The attempt to find the precise place for Maxwell's theory in that classification appears even stranger. He wondered "wherein lies, in my opinion, the especial difficulty of Maxwell's own representation", and he explicitly stated that he could not "agree with the oft-stated opinion that this difficulty is of a mathematical nature".[14] In some way Hertz returned to the previous, more sophisticated appreciation of physical theories: equations are only one component of a theory. The theoretical representations of the invisible processes, which tried to explain the visible phenomena, had also to be taken into account. Hertz deployed four theoretical models: the first consisted of traditional action at a distance, and the second corresponded to the so-called "potential theory", where "an acting body is still both the seat and the source of the force". The third took into account the polarisation of the medium and "is represented by Helmholtz's theory". Nevertheless, that model could be further split into two sub-models (say 3a and 3b), according to the relative weight given to "an influence due to direct action-at-a-distance, and an influence due to the intervening medium". In the limiting case (3b), when

designate as „Maxwell's theory". To the question, „What is Maxwell's theory?", I know of no shorter and more definite answer than the following: - Maxwell's theory is Maxwell's system of equations. Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case of Maxwell's theory; every which leads to different equations, and therefore to different possible phenomena, is a different theory."

[13] FitzGerald F.G. (supposed) 1893, p. 539.

[14] Hertz H. 1892, in Hertz H. 1962, pp. 21-22: "Perhaps it may be of service to many of my colleagues if I here briefly explain the fundamental conceptions of the three representations of Maxwell's theory to which I have already referred."

polarisation overwhelmed action at a distance, "the whole of the energy" was in the medium. According to Hertz, this case resembled Maxwell's theory, but, he claimed, the resemblance was misleading. Maxwell's theory corresponded to a fourth model, where actions at a distance had to be definitely denied. In Hertz's interpretation of Maxwell's theoretical model, if we could extract matter and aether from a certain region between the plates of an electrically charged condenser, in that region we would not find any electric action.[15] Nevertheless, if the fourth model was the actual conception of Maxwell, some specific passages of Maxwell's writings, Hertz remarked, did not seem in accordance with that conception. Hertz thought that what we call *Maxwell's theory* was the result of a long-lasting intellectual process: every conception preserved some *footprints* of previous, different conceptions. According to Hertz's interpretation, Maxwell's theory would contain its own history.[16] Emerging from this stratification of interpretations, the concept of electricity appeared to Hertz to be a questionable concept: for instance, in Maxwell's *Treatise*, this word assumed at least two different meanings. In some cases, electricity seemed "a quantity which can be either positive or negative, and which forms the starting-point of distance-forces (or what appear to be such)". In other cases, it was associated to "that hypothetical fluid from which no distance-forces (not even apparent ones) can proceed, and the amount of which in any given space must, under all circumstances, be a positive quantity".[17]

After having stressed the differences among the various theoretical models, in the last passages of his *Introduction*, Hertz came back to the previously claimed overlap between Maxwell's theory and Maxwell's equations. This shows us how strong the methodological tension between a theoretical

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[15] The detailed analysis of the different theoretical models, when applied to a charged condenser, can be found in Hertz H. 1892, in Hertz H. 1962, pp. 22-26. On the differences among those models, I mention once again FitzGerald's (supposed) summary, which appeared in *Nature*, in 1893. According to the author of the paper, what would distinguish the second from the third theoretical model would be the extension of electric polarisation from dielectric matter to aether. See FitzGerald G.F. (supposed) 1893, p. 538. For a detailed account of Hertz's analysis, see Bevilacqua F. 1983, pp. 216, 203-5. Some decades ago, M. Hesse pointed out the existence of "a contradiction between the directions of polarisation of particles of aether in 3b and 4", for "the aether is no longer pictured as analogous in this respect to actual dielectrics". See Hesse M. 1961, p. 214, footnote 1.

[16] See Hertz H. 1892, in Hertz H. 1962, p. 27: "Now, when Maxwell composed his great treatise, the accumulated hypotheses of this earlier mode of conception no longer suited him, or else he discovered contradictions in them, and so abandoned them. But he did not eliminate them completely; quite a number of expressions remained which were derived from his earlier ideas."

[17] Hertz H. 1892, in Hertz H. 1962, p. 27; on Maxwell's conception of electric charge, see chapter 7 of the present book.

approach and a mathematical-phenomenological approach really was, and how problematic the acknowledgment of theoretical physics as a specific way of practising physics really was. Some of Hertz's methodological hesitations also show the objective difficulty in classifying Maxwell's theory, as well as the more general difficulty in identifying all aspects neither empirical nor mathematical of a physical theory.

In the 1880s and 1890s, similar theoretical and methodological queries emerged in connection with the interpretation of cathode rays. *Cathode rays*, *Kathodestrahlen* and *Rayons cathodiques* were the words associated to a set of visible phenomena involving light, heat and electrification, whose explanation involved supposedly invisible particles or supposedly invisible processes taking place through aether.[18]

In the first chapter of his Treatise, Maxwell had mentioned the "electrical discharge through rare gases" as a phenomenon which could "probably throw great light on the nature of electricity as well a son the nature of gases and of the medium pervading space". Nevertheless, in spite of those expectations, he thought that, in the 1870s, the phenomenon was "outside the domain of the mathematical theory of electricity".[19] In the same decade, W. Crokees, skilful experimentalist and lecturer, and editor of the journal *Chemical News*, was able to produce a vacuum of the order of one millionth of an atmosphere in vacuum tubes. In 1879, in a paper published in that journal, he made reference to Faraday's expression "Radiant Matter", in order to point out the four states of matter, namely solid, liquid, gaseous, and "radiant". He found that "decreasing the number of molecules in a given space and lengthening their mean free path" led to phenomena so distinct "from anything which occurs in air or gas at the ordinary tension" that we must assume the existence of "Matter in a Fourth state or condition". According to Crookes, the highly exhausted tube allowed scientists to perceive the passage from matter considered as a *continuum* to matter considered as a collection of *discrete* units, and to "contemplate the

[18] As already noted in a previous chapter, in the second half of the nineteenth century, in the context of British physics, aether or some kind of medium had shown its usefulness not only in optics and electromagnetism but also in the dynamical theory of matter. As Buchwald and Warwick recently stated, aether, "the catholic underpinning of all nature", is nowadays disappeared from the landscape of physical sciences; on the contrary, what nowadays physicists consider as the *rightful* heir of rays, the modern *electron*, is one of the most outstanding components of that landscape. See Buchwald J. Z. and Warwick A. 2001, pp. 1-2.

[19] Maxwell J.C. 1881, pp. 57-8.

molecules individually".[20] He looked upon cathode rays as material (molecular) rays, and in a controversial experiment he thought he had managed to show that "the molecular stream from the negative pole is able to move any light object in front of it". That radiant matter consisted of a stream of "negatively electrified" molecules, and a magnet could bend that stream, as showed by other experiments he systematically undertook.[21] A specific feature of molecular rays was their relative independence from the different kind of gas which originally filled the tube; he noted that "the phenomena of phosphorescence, shadows, magnetic deflection, &c., are identical, only they commence at different pressures". That specific and unusual feature dealt with the double nature of cathode rays, which shared some properties of matter but also some properties of energy. In other words, Crookes surmised that the rays assumed the double nature of "Radiant Matter" and "Radiant Energy". He imagined that the boundary between matter and energy, where "Matter and Force seem to merge into one another", echoed in some way the boundary between "Known and Unknown", where the solution of "the greatest scientific problems of the future" were presumably involved.[22]

In the 1890s, the number of experiments on rays and discoveries of new rays grew up; were those rays particles or streams of energy? Hertz, for example, thought that they could be reduced to some kind of perturbations through aether. Experiments on cathode rays appeared to W. Thomson as a tool to inquire into the questionable relationship between aether and ponderable matter. Hertz, in 1892, performed some experiments with electric fields superimposed onto the paths of cathode rays: he observed no deflection. He then realised that the rays could not consist of charged particles; they seemed instead some kind of electromagnetic waves

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[20] Crookes W. 1897, pp. 90-92. See p. 92: "In these highly exhausted vessels the molecules of the gaseous residue are able to dart across the tubes with comparatively few collisions, and radiating from the pole with enormous velocity, they assume properties so novel and so characteristic as to entirely justify the application of the term borrowed from Faraday, that of *Radiant Matter*."

[21] Crookes W. 1879, pp. 106, 126 and 128. He put forward a very simple mechanical model. See p. 127: "The molecules shot from the negative pole may be likened to a discharge of iron bullets from a mitrailleuse, and the magnet beneath will represent the earth curving the trajectory of the shot by gravitation." On Maxwell's criticism about the supposed mechanical effects of cathode rays, see, for instance, Harman P.M. 1998, p. 182.

[22] Crookes W. 1879, pp. 130-31. He made reference to a "Border Land" where "Ultimate Realities" probably lay, and where his interests in both physical sciences and *psychical* researches could hopefully converge. On Crookes' commitment to *psychism* and *spiritualism*, see, Oppenheim J. 1985, pp. 338-54.

propagating through aether.[23] P. Lenard, who had worked together with Hertz since 1891, went on with Hertz's project of research. At the end of 1892, he built a cathode tube endowed with a window able to let the rays go outside the tube, in order to check their behaviour independently from the discharge process. The task was still the same: to inquire into the nature of cathode rays. In 1894, Lenard published two papers, wherein he claimed that the nature of rays was that of processes in the aether ("Vorgänge im Aether"). In the first paper, he focused on their power of crossing thin sheets of metal, as well as on their propagation in a region of space free from matter. His theoretical framework led him to perform experiments in order to check three fundamental issues: the similarity between cathode rays and electromagnetic radiation of various wavelengths, the connection between the rays and aether and, eventually, the different permeability of different kinds of matter with regard to rays. He wondered whether these rays corresponded to "processes taking place through matter or through aether", a question already solved "for sound and light". For "the empty space is not hindrance to the propagation of rays", he concluded that they "have to be acknowledged as processes through aether".[24] Then he found an important difference between them and electromagnetic radiation of short wavelengths: when crossed by the former, ordinary matter showed its discrete structure. The rays involved "processes of so extraordinary subtlety that dimensions of molecular order have to be taken into account".[25] In 1894, in his second paper, Lenard drew attention to a possible magnetic deflexion of the rays. Provided that a deflection was actually detected, how could that deflexion be interpreted? Was it a clue to their corpuscular nature, or the phenomenon could still be explained in terms of processes in the aether?[26] According to Lenard, the agreement between the behaviour of cathode rays and the behaviour of electric currents was misleading: magnetic

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[23] Thomson W. 1893a, p. 389: "If a first step towards understanding the relations between ether and ponderable matter is to be made, it seems to me that the most hopeful foundation for it is knowledge derived from experiment on electricity at high vacuum." See also Smith G. E. 2001, p. 28. For a more detailed analysis of Hertz's experiments, see Falconer I. 1987, p. 244.

[24] Lenard P. 1894a, pp. 225-227: "Vom besondere Interesse ist die Möglichkeit, die Strahlen in ein vollständiges Vacuum treten zu lassen, in welchem sie bekanntlich nicht erzeugt werden könnten; die Möglichkeit also, mit ihnen denselben Fundamentalversuch auszuführen, der für den Schall, für das Licht entschieden hat, ob dieselben Vorgänge in der Materie sind oder Vorgänge im Aether. Wie man sehen wird, ist der luftleere Raum kein Hindernis für die Ausbreitung der Strahlen. Sie durchziehen ihn mit grosser Intensität auf meterlangen Strecken; sie geben sich somit als Vorgänge im Aether zu erkennen."

[25] Lenard P. 1894a, pp. 266-267.

[26] Lenard P. 1894b, p. 23.

fields produced distortions in the aether which, in their turn, influenced the motion of rays in some indirect way. He concluded that "according to Hertz's experiments, the deflection of cathode rays is not an effect of the magnet on the rays themselves, but rather an effect of the magnet on the intervening medium".[27]

A year later, this interpretation was overturned by J. Perrin, after a series of experiments. The fact was that experiments in themselves could not be conclusive, for every result could be interpreted in different ways, as in the case of magnetic deflection. Perrin also devised his experiments in connection with definite hypotheses. He compared the hypothesis of Hertz and Lenard with the hypothesis of Crookes and J.J. Thomson: he chose the latter, namely "matter negatively charged and in motion with great velocity". He explicitly stated that his experiments were suggested by "this last hypothesis".[28] Perrin used a cylinder, placed inside the cathode tube, in order to trap the rays in a sort of Faraday's cage and detect their supposed electric charge. In fact the cylinder showed a negative electric charge only after the rays had entered in it. Provided that the whole electric charge of the system had to be conserved, Perrin looked for the corresponding positive charge and was able to detect it, after having trapped a supposed reverse flux of positive rays in an analogous cylindrical Faraday's cage placed beyond the perforated cathode. He found that this positive charge was approximately of the same amount of the negative: this was consistent with his hypothesis that the tube was the seat of two opposite fluxes of electricity.[29] In the last passages of his paper, Perrin stated that experimental results did not match "the theory which identifies the cathode rays with ultra-violet rays". On the contrary, they were consistent with the hypothesis that cathode rays were a kind of "material radiation", and were electrically charged; probably they stemmed from the breaking and subsequent spreading out of ordinary matter contained, at low pressure, inside the tube. He thought that "near the cathode, the electric field is

[27] Lenard P. 1894b, pp. 32-33: "Die Ablenkung der Kathodenstrahlen ist nach Hertz' Versuchen nicht eine Wirkung des Magneten auf die Strahlen selbst, sondern eine Wirkung desselben auf das durchstrahlte Medium; die Strahlen breiten sich anders aus im magnetisierten Medium als im nicht magnetisierten. Denn wirkten Kräfte zwischen dem Magneten und den Strahlen selbst, so müsste auch der Magnet, beweglich gemacht, durch die Kathodenstrahlen abgelenkt werden, was nicht der Fall ist."

[28] Perrin J. 1895, p. 1131: "Cette dernière hypothèse m'a suggéré quelques expériences que je vais résumer sans m'inquiéter, pour le moment, de rechercher si elle rend compte de tous les faits jusqu'à présent connus, et si elle peut seule en rendre compte. Ses partisans admettent que les rayons cathodiques sont chargés négativement ; à ma connaissance, on n'a pas constaté cette électrisation ; j'ai d'abord tenté de vérifier si elle existe, ou non."

[29] Perrin J. 1895, p. 1132.

strong enough to break into fragments, or *ions*, some molecule of the gas left in the tube".[30]

In the same year, W.C. Röntgen, then at Würzburg University, found another kind of rays, sent forth by matter when hit by cathode rays. The new rays (named X-rays) were able, in their turn, to deeply travel across matter.[31] Although he was not able to detect any "evidence of refraction of these rays in passing from one medium into another", he claimed that "the reflection of X-rays from the above named metals is proved". In his attempt to interpret the new rays, he made reference to Lenard's explanation of cathode rays: he thought that they were "phenomena of the ether". Nevertheless, there was an important difference between them, for the substances analysed were "more transparent to X-rays than to cathode rays". Moreover, he had noticed that the new rays could not be deflected by a magnet, even making use of "very intense fields"; indeed, that deflection appeared to him as "a characteristic property of the cathode rays". However, from the conceptual and linguistic point of view, the name "rays" seemed to him not questionable, for he had observed "the entirely regular formation of shadows" when bodies of different shapes were put in the path of "the agent which proceeds from the wall of the discharge-apparatus". Then he put forward another surmise, that X-rays were similar to ultra-violet light, but he had tried "in many ways to detect interference phenomena … without success"; neither could they "be polarized by any of the ordinary methods". Eventually, at the end of the paper, he put forward another conjecture, that X-rays were "longitudinal vibrations in the ether", a conjecture which, he cautiously noticed, waited for "further confirmation".[32]

In 1897, in a subsequent paper, Röntgen reported on experiments inquiring into the interaction between rays and matter: he realised that matter struck by radiation could send forth other radiation, but he was not able to decide

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[30] Perrin J. 1895, p. 1133: "L'ensemble de ces résultats ne paraît pas facilement conciliable avec la théorie qui fait des rayons cathodiques une lumière ultra-violette. Ils s'accordent bien au contraire avec la théorie qui en fait un rayonnement matériel et qu'on pourrait, me semble-t-il, énoncer actuellement ainsi: Au voisinage de la cathode, le champ électrique est assez intense pour briser en morceaux, *en ions*, certaines des molécules du gaz restant. Les ions négatifs partent vers la région où le potentiel croît, acquièrent une vitesse considérable et forment les rayons cathodiques; …."

[31] He noticed that, during the discharge of a cathode tube "covered with thin, black card-board", there was "a bright illumination of a paper screen covered with barium platinum-cyanide, placed in the vicinity of the induction-coil". He discovered that, in general, "all bodies are transparent to this agent, though to very different degrees": for substances of the same thickness, this kind of transparency depended on their density. See Röntgen W.C. 1895, in Röntgen W.C. 1981, pp. 3-5.

[32] Röntgen W.C. 1895, in Röntgen W.C. 1981, pp. 9-13.

"whether the rays emitted by a body which is receiving radiation are of the same kind as those which are incident".[33] In the scientific community, the query about the nature of cathode rays and X-rays appeared still as an unsolved query, but in German universities the hypothesis that both of them consisted of some kind of electromagnetic radiation, or process in/through the aether, was definitely looked upon as the most reliable.

In 1896, the French H. Becquerel had found a new kind of rays spread by Radium and Uranium salts: even the nature of those rays appeared questionable. In 1899, in a "Note" in *Comptes Rendus* on the influence of a magnetic field on "rays sent forth by radio-active substances", he remarked that their nature and some features of their emission "were still a mystery of great interest". Nevertheless, he thought that, from the experiments he had performed, those rays were quite similar to cathode rays.[34]

From subsequent experimental researches of J.J. Thomson, W. Kaufmann and E. Wiechert, performed from 1897 onwards, the theoretical model of cathode rays as particles emerged reinforced. Nevertheless another element emerged, an element which, up to then, no theory had taken into account: a strong asymmetry between elementary *positive* and *negative* electric charges. Neither Lorentz nor Larmor had found theoretical reason to introduce such a basic difference in the ratio between mass and electric charge of positive and negative charges.[35]

The complex interaction between experimental and theoretical physics was mirrored by the different words introduced in order to qualify the new entities. In J.J. Thomson's papers, cathode rays were interpreted and named as "particles", "corpuscles", "ions", "primordial atoms" and "carriers" (of electric charge). Only after a deep process of reinterpretation, the scientific community identified them with Lorentz's "ions" and Larmor's "electrons".[36] Different linguistic choices corresponded to different

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[33] Röntgen W.C. 1897, in Röntgen W.C. 1981, p. 23. He imagined a large family of rays and a sort of continuity inside it; in particular, he imagined a sort of missing link between X-rays and cathode rays, namely a kind of rays "which form, so far as absorption is concerned, the link between one kind of rays and the other." See Röntgen W.C. 1897, in Röntgen W.C. 1981, pp. 35-6.

[34] Becquerel H. 1899, p. 1001: "Tous ces faits montrent que le rayonnement du radium se rapproche considérablement des rayons cathodiques; […] toutefois le fait de leur émission continue et sans affaiblissement notable, par des substances non électrisées, n'en reste pas moins, jusqu'ici, un mystère d'un grand intérêt." For other details, see, for instance, Falconer I. 1987, p. 249.

[35] See Buchwald J.Z. and Warwick A. 2001, p. 3; see also Smith G.E. 2001, p. 24.

[36] See Thomson J.J. 1897, pp. 294, 296, 310, 311 and 313, Thomson J.J. 1898b, p. 528, and Thomson J.J. 1899b, pp. 547-8. As N. Robotti remarked, "in 1897 there was no *a priori* reason for seeing the *corpuscle* and the 'electron' as one". The fact is that "*electron* on the

theoretical models. In addition, in Larmor's theory, the electron, represented as a dynamical *knot* of aether, was a *particle* in quite a peculiar sense. This fact reminds us that the history of science is also a history of words which change their meaning. Darrigol distinguished two different theoretical approaches to the new particle. He claimed that, for J.J. Thomson, "the new particle was the fundamental building block of all matter", while for other physicists, "it was a materialization of the quantum of electric charge". In this "dual exploitation of the new particle" Darrigol saw the existence of two different traditions concerning "the rising physics of ions". More specifically, on the one hand, there was the tradition "founded by Schuster and Thomson", which "focused on electric conduction in electrolytes and gases, and on the structure of matter". On the other hand, there was that "of Lorentz, Larmor and Wiechert", which "sought to improve Maxwell's synthesis of optics and electromagnetism". It seems to me that this "dual exploitation" was not so sharp: Larmor and J.J. Thomson were committed to the explanation of both the structure of matter and the structure of electric charge and electromagnetic field.[37]

There was actually a resistance to identify J.J. Thomson "corpuscle", stemming from cathode tubes, with Larmor's "electron". The inertia of Larmor's electron was found questionable by Larmor himself, although essentially electromagnetic in its nature: his conception was not so distant from J.J. Thomson's. The latter, from 1881 to the end of the century, had considered the nature of charged particles inertia as an open question. In 1899, he wondered "whether the mass of the negative atom is entirely due to its charge" and stated that "[w]e have no means yet of knowing whether or not the mass of the negative ion is of electrical origin".[38]

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one hand and *corpuscle* on the other started out and continued to seem two entities not necessarily connected". (Robotti N. 1996, p. 274)

[37] See Darrigol O. 2000, p. 313. On the complex interplay between theoretical models and experiments, Kragh remarked that "Thomson's corpuscle was seen as different from the Lorentz-Larmor electron" during "a brief period of confusion" in the last years of the century. I see meaningful theoretical differences between Larmor's *electrons* and Lorentz's *ions* and *electrons*. J.J. Thomson *corpuscles* were a third theoretical entity, even though those "charges of negative electricity carried by particles of matter" resembled more Lorentz's *electrons* than Larmor's *electrons*. In my view, the supposed *confusion* corresponded to a competition among different theoretical models. See Kragh H. 1996, p. 69, and Thomson J.J. 1897, p. 302.

[38] Thomson J.J. 1899b, p. 563. On J.J. Thomson's appraisal of Larmor's electron, Darrigol remarked that the former "shared Heaviside's criticism of the electron qua singularity"; on the contrary, "FitzGerald and Lodge … approved the project of reducing electrons to singularities in a dynamical ether." Warwick reported that the scientists of Cavendish Laboratory, who had worked with J.J. Thomson, and were involved in the researches on

If the nature of the link between matter and electricity, or between inertial mass and electric charge, appeared questionable, the concept itself of electric charge appeared questionable as well. The expressions *electric charge* and *electric current* had different meanings in British and Continental theories. Maxwell's leading conception of electric charge was hard to understand for Continental physicists. The concept was taken into account in various sections of *Treatise*: although Maxwell made use of different representations, the representation he preferred was that of electric charge as the effect of a discontinuous distribution of "electric displacement". According to this interpretation, the electric current was not represented as a stream of electrified particles but as the effect of continuous and unfruitful attempts to offer resilience to electric tensions by the structure of the conductor. In a more radical way, Heaviside interpreted the electric current as an effect associated to magnetic force, so reversing the relationship which pictured electric current as the cause and magnetism as the effect.[39] Larmor and Lorentz's subsequent theories changed once again those theoretical models, trying to bridge the gap between British and Continental traditions.

Finally, with regard to the history of words which changed their meaning in the course of the late nineteenth century, we must quote the expression *Maxwell's equations*. Nowadays this expression denotes something different from the equations Maxwell actually wrote: Maxwell's *Treatise* contained neither what physicists now call *Maxwell's equations* nor any suggestion in order to produce or detect electromagnetic waves. What we now call *Maxwell's equations* is the result of some mathematical rearrangements and many subsequent theoretical transformations. From the mathematical point of view, they are quite similar to the equations Heaviside wrote after Maxwell's death, when he translated some of the equations appearing in Maxwell's *Treatise* into a vector notation. When taking into account their conceptual context, Heaviside's equations were the result of the theoretical effort to get rid of electrostatic and electrodynamic potentials. Equations involving only *fields of force* represented the alternative to Lagrangian and Hamiltonian *dynamical* methods, which Heaviside sharply distrusted. Subsequently Hertz and then Lorentz re-wrote and re-interpreted them; in 1905 they were further re-interpreted by Einstein. He divorced the equations from aether, so handing over to us equations quite close to the

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"the negatively charged, subatomic particles" or "corpuscles", were not at ease with "Larmor's massless electrons." (Darrigol 2000, p. 343, and Warwick A. 2003, p. 349) I find questionable whether Larmor's electrons were purely "massless". See chapter 17 of the present book.

[39] See chapters 6, 7 and 10 of the present book. See also Darrigol O. 1993, pp. 210-11.

original pattern with regard to their form, but quite distant with regard to their meaning.[40] Indeed, some years before, in 1900, another German physicist, E. Cohn, had envisaged electromagnetic fields propagating through space, without any resort to aether: "we will avoid to speak of aether", he explicitly stated. As a consequence, he excluded "every molecular hypothesis, both mechanical and electrical, as well as every mechanical interpretation of electromagnetic processes" and decided to give up "all the consequences which can follow from such hypotheses".[41]

In the end, in order to show how complex and branched the history of aether was, we must mention Einstein's lecture held at Leiden in honour of Lorentz in 1920, wherein he criticized his former exclusion of aether from the landscape of physics and hinted at a new kind of aether devoid of every kinematical property.[42]

[40] See chapters 10 and 11 of the present book. See also Darrigol O. 1993, p. 213. On the conceptual roots of Heaviside's theoretical distrust of potentials, see Darrigol O. 2000, p. 207. It is worth mentioning that, in 1905, the more recent aether theories appeared *revolutionary*, whereas Einstein's electrodynamics could appear *conservative*, as it was associated to an old-fashioned mechanical world-view. See, for instance, Harman P.M. 1982, p. 154.

[41] Cohn E. 1900, p. 30: "Daneben noch von einem „Aether" zu sprechen, werden wir vermeiden. Wir schliessen nach dem Gesagten jede mechanische oder elektrische Molecularhypothese ebenso, wie jede mechanische Deutung elektromagnetischer Vorgänge aus, und verzichten damit auf alle Folgerungen, welche nur aus solchen Hypothesen fliessen können. Unsere Absicht bei diesem Vorgehen ist, zu untersuchen, wie weit man den Thatsachen der Erfahrung mit einem Mindestmaass theoretischer Annahmen gerecht werden kann." See also Darrigol O. 2000, pp. 260-1. In the 1890s, Ostwald sharply criticised the reality of aether. For some details on the role of aether in German scientific literature, see Kostro L. 2000, pp. 19-24.

[42] Einstein claimed that "according to the general theory of relativity space is endowed with physical qualities; in this sense, therefore, there exists an ether". He imagined a *gravitational* aether involving the intimate nature of space and time, although it could not consist of "parts which may be tracked through time". Einstein A. 1920, in Einstein A. 2002, pp. 176 and 181-2. Kostro emphasized the passage from 1905 to 1919, when "the ether is resurrected in the general theory of relativity". See, in particular, Kostro L. 2000, p. 2, where some passages of Einstein's *Morgan Manuscript* are explicitly quoted. See the *Afterword*, at the end of the present book.

Part I: *MACROSCOPIC MODELS*

6. Matter, electric charge and energy in Maxwell's Treatise.

Needless to say, a history of British electromagnetism in the late nineteenth century cannot underestimate Maxwell's more mature contribution to electromagnetism, namely his *Treatise on Electricity and Magnetism*, first published in 1873. I am taking into account the 1881 edition of Maxwell's *Treatise*, for before his death (1879) he managed to revise the first nine chapters of the new edition.[1] There are improvements and corrections with regard to the 1873 edition but the "Preface" preserves the old date 1873, suggesting that the statements there contained were considered by Maxwell himself as worth-while in 1879 as they were in 1873.

In the first words of the "Preface", Maxwell explicitly stated that his main task was to make mechanics and electromagnetism match.[2] A second important issue was the connection between electromagnetism and other sections of physics: mechanics, of course, but also thermodynamics, optics and chemistry. His electromagnetic theory seemed to him the best intellectual device then available to understand the physical world; the manifold connections between electromagnetism and other sections of physics suggested that primacy.[3]

We are faced with the main steps of Maxwell's scientific program: the first step consisted of inquiring into electromagnetic phenomena. Only after that, scientists could fruitfully investigate on the boundaries between electromagnetism and other sections of physical knowledge. The special relationship between mechanics and electromagnetism involved a sort of division of labour: an electromagnetic theory was more promising with regard to the comprehension of nature, but mechanics offered the most steady and

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[1] See Niven W.D. 1881, "Preface to the second edition" in Maxwell J.C. 1881, vol. I, p. xv.

[2] See Maxwell J.C. 1881, vol. I, p vi: "Having thus obtained the data for a mathematical theory of electromagnetism, and having shewn how this theory may be applied to the calculation of phenomena, I shall endeavour to place in as clear a light as I can the relation between the mathematical form of this theory and that of the fundamental science of Dynamics, in order that we may be in some degree prepared to determine the kind of dynamical phenomena among which we are to look for illustrations or explanations of the electromagnetic phenomena."

[3] See Maxwell J.C. 1881, vol. I, p vii: "The internal relations of the different branches of the science which we have to study are more numerous and complex than those of any other science hitherto developed. Its external relations, on the one hand to dynamics, and on the other to heat, light chemical action and the constitution of bodies, seem to indicate the special importance of electrical science as an aid to the interpretation of nature. It appears to me, therefore, that the study of electromagnetism in all its extent has now become of the first importance as a means of promoting the progress of science."

reliable mathematical framework for physical theories. Maxwell called "dynamics" that traditional, formal framework; only in that sense he pursued the *reduction* of electromagnetism to dynamics.[4]

The last pages of Maxwell's *Preface* deal with Faraday's theories. He stated that his *Treatise* was an attempt to mathematically dress the fertile conceptions of Faraday: the general conceptual model of contiguous action and the specific model of *lines of force*. Scholars have already debated on the theoretical connection between Faraday and Maxwell and I do not wish to enter into the quarrel. The fact is that Maxwell claimed this intellectual genealogy, and the claim is repeatedly stressed in many pages of his *Preface*[5].

Now we have to enter the *Treatise* and focus on matter, energy and other entities linked in some way to them, often in a very problematic way. The first chapter, "Description of phenomena", of the first part, "Electrostatics", offers a phenomenological approach to electricity. Maxwell faced some questions concerning basic electric entities: electric charge, electric field, line of force, electric energy, …. Just how problematic the concept of electric charge was, is clearly expressed by Maxwell himself: was charge a substance?

> "While admitting electricity, as we have now done, to the rank of a physical quantity, we must not too hastily assume that it is, or is not, a form of energy, or that it belongs to any known category of physical quantities. All that we have hitherto proved is that it cannot be created or annihilated, so that if the total quantity of electricity within a closed surface is increased or diminished, the increase or diminution must have passed in or out through the closed surface."[6]

This last property was true for matter and for the total energy of a system, but it was not true for specific kinds of energy considered in themselves: heat, for instance does not undergo a conservation law. At first, Maxwell compared electric charge to energy. When taking into account

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[4] See Maxwell J.C. 1881, vol. I, p vii: "Finally, some progress has been made in the reduction of electromagnetism to a dynamical science, by shewing that no electromagnetic phenomena is contradictory to the supposition that it depends on purely dynamical action." On the different meanings of the adjectives "mechanical" and "dynamical", see chapter 2 of the present book.

[5] See Maxwell J.C. 1881, vol. I, pp. x, xi, xiii and xiv. On the complex conceptual link between Maxwell and Faraday see, for instance, Siegel D.M. 1981, pp. 239-46.

[6] Maxwell J.C. 1881, vol. I, p. 37.

energy from the theoretical point of view of action at a distance, a body outside a close surface would be allowed to exchange energy instantaneously with a body within it. Things would go differently according to the point of view of contiguous action: we could follow the passage of energy, in or out, through a surface over time, as we do in the case of matter. There was also a different problem, concerning the dimensional properties of physical entities: among many different possibilities, energy can be reckoned multiplying electric charge by electric potential. Electric charge being only one component of the product, it could not have the same physical dimensions of energy, the result of that product.[7]

Some problems also emerged from the identification of electric charge with matter: the widespread conception of electricity as a fluid, in particular the model of two fluids which can compensate with each other, could not match whatever model of matter. The process of electrification would consist of the transfer of positive or negative electricity from one body to another. In that conceptual framework, "electric charge" would be the excess or the lack of some kind of "electric charge" stored in the matter. Therefore the expression "electric charge" would have two different meanings, corresponding, in Maxwell's words, to two different adjectives: "free charge" in the first case, and "combined charge" (or "fixed", or "latent") in the second. In many senses the word "fluid" seemed to Maxwell unsuitable for both kinds of charge.[8]

Maxwell's reference to "men of science who are not natural philosophers" points out the theoretical character of his inquiry into the meaning of "electric charge", and points out the effort to go beyond both the empirical and mathematical aspects of physics. According to Maxwell, not only would the amount of free electricity be allowed to change, but even the combined electricity would be. In fact, if we want to transfer an amount X of positive charge from A to B, there are many possibilities: X of positive electricity from A to B, or X of negative electricity from B to A, or, for instance (among many other combinations) X/2 of positive electricity from A to B together with X/2 of negative electricity from B to A. As a consequence the body A

[7] See Maxwell J.C. 1881, vol. I, p. 37.

[8] See Maxwell J.C. 1881, vol. I, pp. 38-39: "In most expositions of this theory the two electricities are called `Fluid`, because they are capable of being transferred from one body to another, and are, within conducting bodies, extremely mobile. The other properties of fluids, such as their inertia, weight and elasticity, are not attributed to them by those who have used the theory for merely mathematical purposes; but the use of the word Fluid has been apt to mislead the vulgar, including many men of science who are not natural philosophers, and who have seized on the word Fluid as the only term in the statement of the theory which seemed intelligible to them."

could lose X, X/2 or nothing of its combined electricity: there was not any law of *conservation* for the combined electricity.[9] Maxwell thought that the hypothesis of two fluids was so unfit that he did not take into account the possibility of a general law of conservation for the sum of free and combined electricity. Indeed, he had taken into account that hypothesis but had immediately rejected it, because it was considered as a mathematical trick devoid of physical meaning. It is worth noting that the above remarks make sense in a theoretical framework where electricity was not associated to specific elements of matter: on this specific issue, Maxwell's criticism was quite sharp.

> "But to those who cannot use the word Fluid without thinking of a substance it is difficult to conceive how the combination of the two fluids can have no properties at all, so that the addition of more or less of the combination to a body shall not in any way effect it, either by increasing its mass or its weight, or altering some of its other properties. Hence it has been supposed by some, that in every process of electrification exactly equal quantities of the two fluids are transferred in opposite directions, so that the total quantity of the two fluids in any body taken together remains always the same. By this new law they `contrive to save appearances`, forgetting that there would have been no need of the law except to reconcile the `two fluids` theory with facts and to prevent it from predicting non-existent phenomena."[10]

According to Maxwell, neither could the one-fluid theory better explain the behaviour of electrification. Making reference to previous theories, he assumed that one of the fluids, for instance the negative one, was equivalent to ordinary matter and the other was endowed with a particular property: each part of it would repel the other parts in accordance with the Coulombian law of the inverse square of distance. What sounded questionable was the fact that a similar property applied to matter: in that model, particles of matter "are supposed to repel each other and attract those of electricity". This was in blatant contradiction with the well-known attractive property of gravitation and required a complex balance of effects in order "to account for the attraction of gravitation". Moreover, the one-fluid theory, just like the two-fluids theory, did not solve another query: the transfer of electric fluid should produce a corresponding, measurable change

[9] See Maxwell J.C. 1881, vol. I, p. 39.
[10] Maxwell J.C. 1881, vol. I, pp. 39-40.

in the mass or weight of bodies. In the end Maxwell appears disappointed by these theories and eager to escape from what he considered to be a theoretical trap. He would have followed another way to test the nature of electric charge: the effects it produced on surrounding media.[11]

Once again he made reference to his intellectual anchor: "the essential character of the mode of investigation pursued by Faraday in his *Experimental Researches*". He took into account two classes of physical entities: Forces, Fields, Electromotive forces, Potentials and Lines of force, on one hand, and Capacity of a conductor, Resistance and Specific Inductive Capacity, on the other. If the entities of the second class could be qualified as properties of matter, the entities of the first class could not be so easily qualified. Among the above concepts, "electric field", the keystone of his theory, deserved some specifications.

> "The Electric Field is the portion of space in the neighbourhood of electrified bodies, considered with reference to electric phenomena. It may be occupied by air or other bodies, or it may be a so-called vacuum, from which we have withdrawn every substance which we can act upon with the means at our disposal."[12]

The adjective "so-called" reminds us that, according to Maxwell, *vacuum* was not empty. There were good reasons for it: how could space be empty and, at the same time, be the potential seat for something like energy? Indeed, Maxwell could choose between two different conceptions: the field as a portion of space (as Maxwell literally stated) or something happening inside it. In the first case, it would be very difficult to find a region of space really empty; in the second case what is happening should be further explained. Choosing the first answer Maxwell seems to have solved (or perhaps skipped over) two problems by means of a single sentence: he dismissed the *empty* vacuum and avoided specifying what an electric field really is.[13]

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[11] See Maxwell J.C. 1881, vol. I, pp. 40-41. See, in particular, p. 41: "In the present treatise I propose, at different stages of investigation, to test the different theories in the light of additional classes of phenomena. For my own part, I look for additional light on the nature of electricity from a study of what takes place in the space intervening between the electrified bodies."

[12] Maxwell J.C. 1881, vol. I, p. 45.

[13] On *vacuum* as a dielectric, see Maxwell J.C. 1881, p. 51: "A vacuum, that is to say, that which remains in a vessel after we have removed everything which we can remove from it, is therefore an insulator of very great electric strength."

In Maxwell's theory, the electric strength is the higher value of electromotive force bearable by a body just before a disruptive discharge takes place. If vacuum has got some electric property at the highest degree, it is not empty; it is quite similar to a substance. In Maxwell's theoretical framework, the attribution of substantiality appeared unsuitable for electric charge but not so unsuitable for vacuum. Maxwell's "vacuum", the universal medium, was endowed with some properties typical of matter: among them, inertia and elasticity. Were these properties similar to the corresponding properties of ordinary matter? Historians have long debated on this query: some of them have claimed that Maxwell's medium did not have mechanical properties. The subject matter is quite demanding: I think that Maxwell's medium is, at least to a certain extent, a mechanical medium. The fact is that, as I have shown in the *Introduction*, the landscape of late nineteenth century British electromagnetism offers different meanings associated to the adjective *mechanical*. Furthermore, it seems to me that attempts to devise a non-mechanical aether were performed later on, from the middle of the 1890s to the first years of the twentieth century.[14]

Looking for other remarks on matter and energy in the context of Maxwell's theory, in the first chapter, in the last section, after the phenomenological part, we find some theoretical notes under the title "Plan of this Treatise". Once again Maxwell pointed out that better results could be achieved by a contiguous action model rather than by action-at-a-distance model. In the latter, he wrote, "we may determine the law of the action, but we can go no further in speculating on its cause"; in the former, "we are led to inquire into the nature of that action in each part of the medium". In other words, the theoretical model of contiguous action would allow us to understand the nature of forces only observing their effects on interposed matter. Forces spread their actions through matter, and consequently matter became sensitive to forces, therefore entering a new, excited state.[15]

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[14] On the thesis of a "nonmechanical" medium, see, for instance, Giusti Doran B. 1975, p. 206. Maxwell's physical *world* consisted essentially of matter and motion, and interactions propagated through space at finite speed. As he looked upon "electric fields" as some kind of tensions propagating with continuity from a given region of space-matter to another region of space-matter, I see in Maxwell traces of a renewed *Cartesian* tradition. Obviously, every label appears, at a finer inquiry, as exceedingly simplified. See the last passages of this chapter.

[15] Maxwell J.C. 1881, vol. I, p. 59. With reference to W. Thomson's theorem, Maxwell pointed out the deep connection among matter, energy and fields. See Maxwell J.C. 1881, vol. I, p. 60.

Energy had to be stored inside matter and matter underwent mechanical stresses, "as in the familiar instances of the action of one body on another by means of the tension of a rope or the pressure of a rod". Another reference to Faraday appears at this point, in particular to Faraday's lines of force. In Maxwell's theoretical model, stresses, lines of force, matter, electromotive intensity (field) and energy are deeply connected to each other. The amount of the stresses corresponded to the energy density already reckoned.

> "The nature of this stress is, as Faraday pointed out (Exp. Res., series xi, 1297), a tension along the lines of force combined with an equal pressure in all directions at right angles to these lines. The magnitude of these stresses is proportional to the energy of the electrification per unit of volume, or, in other words, to the square of the resultant electromotive intensity multiplied by the specific inductive capacity of the medium."[16]

Energy transformed matter, and lines of force were the mark or the effect of that transformation; lines of force appear as bridges (in a conceptual way if not in a material way) between matter and energy. This view is repeatedly stated: the concept of electric tension (belonging to the tradition of electric theories since the eighteenth century) is made equivalent to stresses through the medium, still represented as tensions in a rope. In many ways, in these pages, the links among lines of force, tension and field intensity are stressed.[17]

At this point Maxwell explicitly introduced his readers to "another step", in order to form an idea "of the nature of the electric polarization of the dielectric medium." Electric polarisation was qualified as the state of dielectric matter when it experiences an electromotive force: electromotive intensity would produce what Maxwell called an "electrical displacement", and polarisation would be the effect of that displacement. The concept of electric *displacement* is fundamental in Maxwell's theory and scholars have inquired into it in many ways. I would like to quote some original passages and add some personal remarks.

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[16] Maxwell J.C. 1881, vol. I, p. 60.

[17] See Maxwell J.C. 1881, vol. I, p. 61: "Along the lines of force there is tension, and perpendicular to them there is pressure, the numerical magnitude of these forces being equal, and each proportional to the square of the resultant intensity at the point."

> "When the electromotive force acts on a conducting medium, it produces a current through it, but if the medium is a non-conductor or dielectric, the current cannot flow through the medium, but the electricity is displayed within the medium in the direction of the electromotive intensity, the extent of this displacement depending on the magnitude of the electromotive intensity, so that if the electromotive intensity increases or diminishes, the electric displacement increases and diminishes in the same ratio."[18]

There is a linear relationship between the electromotive force ***E*** and the electric displacement ***D***, corresponding to the well-known equation $\mathbf{D} = \varepsilon \mathbf{E}$. But this equation, written in this way does not allow us to appreciate Maxwell's theoretical view. To all appearances it sounds right, for ***E*** is the cause and ***D*** is the effect: an electromotive force induces an electric displacement throughout matter. Nevertheless this representation hides the theoretical view that Maxwell himself was to explain in the following sentences: the medium reacts elastically to the electromotive force in the same way as that of a spring. In this case we have a linear relation between force and displacement, in symbols $\mathbf{F} = -k\mathbf{x}$, where the minus represents the vector character of the relation. According to this theoretical framework, the relation between electromotive force and displacement could be written as $E = kD$, corresponding to $\mathbf{E} = \frac{1}{\varepsilon}\mathbf{D}$. That this was not a mere mathematical quibble is showed by the following passage.

> "The analogy between the action of electromotive force in producing electric displacement and of ordinary mechanical force in producing the displacement of an elastic body is so obvious that I have ventured to call the ratio of the electromotive intensity to the corresponding electric displacement the *coefficient of electric elasticity* of the medium. This coefficient is different in different media, and varies inversely as the specific inductive capacity of each medium."[19]

In a short and sharp passage, Maxwell linked electric charge to polarisation: electricity was considered as a peculiar state of matter, when

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[18] Maxwell J.C. 1881, vol. I, pp. 61-62. The concept of electric *displacement* has been widely analysed in Buchwald J.Z. 1885a, Hunt B.J. 1991, Darrigol O. 1995, Darrigol O. 2000, ...

[19] Maxwell J.C. 1881, vol. I, p. 62. On the mathematical symbols "+" and "-" in the above equations, see Darrigol O. 2000, p. 162 ("Maxwell's well-known plus-minus dyslexia").

matter is the seat of electric (elastic) energy.[20] Electric charge was connected to displacement in a way we could translate mathematically as $D = dQ/da$, where *dQ* is the amount of electric charge and *da* is the surface element. The electric charge was the effect of the electric displacement and a measure of it. In other words, the electric charge was the effect of stresses inside matter: the word "charge" suggested the behaviour of a spring or other elastic devices which are *charged*, namely put in a state of tension. Even electric currents were connected to the *electric displacement*: what Continental scholars imagined as the transfer of some kind of substance over time, Maxwell imagined as the variation of the *electric displacement* over time.[21]

This close connection between electric charge and electric displacement is pointed out in another passage, in the next page, where a slightly different quantity, the "whole displacement", is introduced. We could easily be deceived by the word "whole displacement" if we did not consider it and the simple "displacement" as two different quantities. If the latter is nothing else but the usual vector *displacement* **D**, the former can be translated into the integral displacement $\iint \mathbf{D} \cdot d\mathbf{a}$. In Maxwell's theory, this second *displacement* corresponds to electric charge.[22]

If we introduce this new quantity "whole displacement" $\Delta = \mathbf{D} \cdot d\mathbf{a}$, the relation $D = dQ/da$ can be written as $\Delta = dQ$: Maxwell's choice of the letter *E* for the "whole displacement" appears a bit misleading, not only with regard to modern symbols, but even with regard to the symbols he used in his *Treatise*. Apart from misleading symbols, the concept is repeated in the

[20] See Maxwell J.C. 1881, vol. I, p. 62: " The amount of the displacement is measured by the quantity of electricity which crosses unit of area, while the displacement increases from zero to its actual amount. This, therefore, is the measure of the electric polarisation."

[21] See Maxwell J.C. 1881, vol. I, p. 62: "The variations of electric displacement evidently constitute electric currents. These currents, however, can only exist during the variation of the displacement, and therefore, since the displacement cannot exceed a certain value without causing disruptive discharge, they cannot be continued indefinitely in the same direction, like the currents through conductors."

[22] See Maxwell J.C. 1881, vol. I, pp. 62-63: "If a charge *e* is uniformly distributed over the surface of a sphere, the resultant force at any point of the medium surrounding the sphere is numerically equal to the charge *e* divided by the square of the distance from the centre of the sphere. This resultant force, according to our theory, is accompanied by a displacement of electricity in a direction outwards from the sphere. If we now draw a concentric spherical surface of radius *r*, the whole displacement, *E*, through this surface will be proportional to the resultant force multiplied by the area of the spherical surface. But the resultant force is directly as the charge *e* and inversely as the square of the radius, while the area of the surface is directly as the square of the radius. Hence the whole displacement, *E*, is proportional to the charge *e*, and is independent of the radius."

following short sentence (with Maxwell's own italics): "*The displacement outwards through any spherical surface concentric with the sphere is equal to the charge on the sphere*". The same concept appears once more in the next page, wherein the mathematical aspect of the relation between electric charge and electric displacement is pointed out: "the surface integral of the displacement taken over the surface will be equal to the charge on the conductor within".[23]

To better depict what electric displacement was, Maxwell took into account a condenser consisting of two conducting plates *A* and *B*, and some dielectric interposed. He also imagined a quantity *Q* of electricity already conveyed by a conducting wire *W* from plate *B* to plate *A*. As a consequence, an electromotive force would arise in the dielectric, directed from *A* to *B*. This electromotive force would be followed by an electric displacement, and the amount of displacement crossing an imaginary surface dividing the dielectric in two layers would be just *Q*. Two consequences followed from that model: first, electric charge is flowing both through the conducting wire and through the dielectric; second, the whole electric circuit is a closed circuit. In such a way the usual distinction between conductors and dielectrics was overcome.

> "Every case of charge or discharge may therefore be considered as a motion in a closed circuit, such that at every section of the circuit the same quantity of electricity crosses in the same time, and this is the case, not only in the voltaic circuit where it has always been recognised, but in those cases in which electricity has been generally supposed to be accumulated in certain places.
>
> (...) We are thus led to a very remarkable consequence of the theory we are examining, namely, that the motions of electricity are like those of an *incompressible* fluid, so as the total quantity within an imaginary fixed close surface remains always the same. This result appears at first sight in direct contradiction to the fact that we can charge a conductor and then introduce it into the

[23] Maxwell J.C. 1881, vol. I, pp. 63-4. Trying to synthesise Maxwell's passages, and making use of the equivalence between volume-integrals and surface-integrals, we can write

$$\iiint \rho\, dv = \iint \mathbf{D} \cdot da\,; \quad \iiint \rho\, dv = \iiint div(\mathbf{D}) \cdot dv\,; \quad \rho = div(\mathbf{D}).$$

From the dimensional point of view, the left member of the last equation is a charge density, and the right member is a displacement divided by a length. It follows that the displacement is a charge density multiplied by a length. In this sense it is reasonable to consider it as an *electric displacement*. See Maxwell J.C. 1881, vol. I, pp. 82-84.

> closed space, and so alter the quantity of electricity within that space."[24]

Despite the choice of making use of the word "fluid", Maxwell's *incompressible fluid* is more akin to an amount of tension, or a chain of tensions, than to an amount of substance. Two pages further on, in a list summarising the "peculiar features of the theory", the same concept of fluid reappears, together with the remarkable conclusion that all electric currents are close currents.[25]

In such a picture, if dielectrics corresponded to media answering elastically to a stirring electromotive force, conductors corresponded to overstressed springs, unable to offer an elastic resistance to the electric force. Rather than representing dielectrics as bad conductors or no conductors at all, Maxwell's represented conductors as very bad dielectrics, or dielectrics whose elasticity have been almost completely wasted.[26] In Maxwell's theoretical model, it was as if each part of a conductor tried to offer an elastic resistance to the electric force but did not manage it; in any contiguous part of the medium these unfruitful attempts would continuously take place and suddenly vanish. Energy is imagined as spread throughout dielectrics: matter would be put in a state of tension, just like a spring. This model is displayed in a list of issues Maxwell wrote in order to qualify his theory.

> "The peculiar features of the theory are:
>
> That the energy of electrification resides in the dielectric medium, whether that medium be solid, liquids or gaseous, dense or rare, or even what is called a vacuum, provided it be still capable of transmitting electrical action.

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[24] Maxwell J.C. 1881, vol. I, p. 64.

[25] See Maxwell J.C. 1881, vol. I, p. 66: "That in every case the motion of electricity is subject to the same condition as that of an incompressible fluid, namely, that at every instant as much must flow out of any given closed surface as flows into it. It follows from this that every electric current must form a closed circuit."

[26] See Maxwell J.C. 1881, vol. I, p. 66: "That whatever electricity may be, and whatever we may understand by the movement of electricity, the phenomenon which we have called electric displacement is a movement of electricity in the same sense as the transference of a definite quantity of electricity through a wire is a movement of electricity, the only difference being that in the dielectric there is a force which we have called electric elasticity which acts against the electric displacement, and forces the electricity back when the electromotive force is removed; whereas in the conducting wire the electric elasticity is continually giving way, …"

> That the energy in any part of the medium is stored up in the form of a state of constraint called electric polarisation, the amount of which depends on the resultant electromotive intensity at the place."[27]

In 1877, while he was revising the first chapters of his *Treatise*, Maxwell published the booklet *Matter and Motion*. In some passages, he stressed the role of energy in physical science. He stated that inquiring into "the various forms of energy", taking into account "the conditions of the transference of energy from one form to another", corresponded to "the whole of the physical science".[28] In the sixth chapter, matter and energy appear so tightly linked to each other that our knowledge of matter can only be mediated by energy. In Maxwell's words, "[a]ll that we know about matter relates to the series of phenomena in which energy is transferred from one portion of matter to another". The transfer of energy through space and time allows us to have a definite perception of both matter and energy.

> "Hence, as we have said, we are acquainted with matter only as that which may have energy communicated to it from other matter, and which may, in its turn, communicate energy to other matter. Energy, on the other hand, we know only as that which in all natural phenomena is continually passing from one portion of matter to another."[29]

Nevertheless, that double link between matter and energy did not lead Maxwell to conceive them as endowed with the same properties: matter could be definitely identified but energy could not. Although he had pointed out that energy "cannot exist except in connection with matter", its physical existence had quite a different character.[30] He thought that we should not

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[27] Maxwell J.C. 1881, vol. I, p. 65. The energy density amounted to $(1/2)\boldsymbol{E}\cdot\boldsymbol{D}$, which corresponded to the expression $(1/2)\varepsilon E^2$.

[28] Maxwell J.C. 1878, p. 168.

[29] Maxwell J.C. 1878, pp. 163-4.

[30] See Maxwell J.C. 1878, pp. 165-6: "We cannot identify a particular portion of energy, or trace it through its transformations. It has no individual existence, such as that which we attribute to particular portions of matter.

The transactions of the material universe appear to be conducted, as it were, on a system of credit. Each transaction consists of the transfer of so much credit or energy from one body to another. This act of transfer is called work. The energy so transferred does not retain any character by which it can be identified when it passes from one form to another."

speak in terms of particles of energy in the same way we speak of particles of matter. The main reason was found in the fact that the amount of energy of a physical system depends on the choice of the reference frame. The kinetic energy "of the parts relative to the centre of mass" can be definitely computed, whilst the kinetic energy of the centre of mass depends on "the body which we select as our origin". In addition, we know only the amount of energy transferred from a fragment of matter to another and not the whole amount of energy. The latter is a *hidden* content of energy: although "[w]e cannot reduce the system to a state in which it has no energy", that energy "remain unperceived to us". Nevertheless, Maxwell claimed, this intrinsic limit to the complete knowledge of energy does not negatively affect the physical knowledge, for "all phenomena depend on the variation of energy, and not on its absolute value".[31]

The Kantian *flavour* of Maxwell's conception of matter echoed the Kantian *flavour* of some remarks Helmholtz had made on the conservation of energy. In 1847, in his essay *Über die Erhaltung der Kraft*, Helmholtz had pointed out the tight link between matter and energy/force ("Kraft" in Helmholtz's conceptual and linguistic framework).[32] In this perspective, it does not sound strange that, in the *Treatise*, the "fundamental dynamical idea of matter" appeared to Maxwell both a physical and a philosophical issue. On the physical ground, matter was "capable by its motion of becoming the recipient of momentum and energy"; on the philosophical ground, that idea of matter appeared tightly "interwoven with our forms of thought".[33]

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[31] Maxwell J.C. 1878, pp. 166-8.

[32] See Helmholtz H. 1847 in Helmholtz H. 1889, p. 5: "Es ist einleuchtend, dass die Begriffe von Materie und Kraft in der Anwendung auf die Natur nie getrennt werden dürfen. [...] Ebenso fehlerhaft ist es, die Materie für etwas Wirkliches, die Kraft für einen blossen Begriff erklären zu wollen, dem nicht Wirkliches entspräche; beides sind vielmehr Abstractionen von dem Wirklichen, in ganz gleicher Art gebildet; wir können ja die Materie eben nur durch ihre Kräfte, nie an sich selbst wahrnehmen." As Elkana pointed out in 1974, at that stage Helmhioltz's *Kraft* was a very pliable concept or "a concept in flux". See Elkana Y. 1974, p. 137. On Kant's influence on Helmholtz, see Elkana Y. 1974, p. 167.

[33] Maxwell J.C. 1881, vol. II, p. 182. See Kant I. 1787, in Kant 1881, p. 271: "Die Substanz im Raume kennen wir nur durch Kräfte, die in demselben wirksam sind, entweder andere dahin zu treiben (Anziehung) oder von Eindringen in hin abzuhalten (Zurückstossung und Undurchdringlichkeit); andere Eigenschaften kennen wir nicht, die den Begriff der Substanz, die im Raum erscheint und die wir Materie nennen, ausmachen." On the influence of Kant on W. Whewell, and on the influence of W.B. Hamilton and W. Whewell on the young Maxwell, see Harman P.M. 1998, pp. 28-36; see pp. 190-94 for Maxwell's dynamical concept of matter.

7. Mathematical physics and theoretical physics.

At the end of Chapter V, "Mechanical action between two electrical systems", in the first part of his *Treatise*, Maxwell specified once again his model in terms of stresses in dielectrics, lines of force, electric charge, electric currents and energy.[1] According to Maxwell, all electromagnetic phenomena follow from polarisation in dielectrics and polarisation is the effect of stresses taking place inside matter. He acknowledged that the existence of stresses required an explanation, and that explanation would have involved a theory of matter. He was aware that the last target had not been reached: he put forward his theory as a not fully accomplished attempt.[2]

In many sentences on the same page he stressed and repeated with the same words the tight relationship between electric charge and electric displacement: the former would correspond to the "total displacement", namely the surface integral of displacement multiplied by the inductive capacity of dielectric. That Maxwell's theory was a theory of dielectric matter is shown by his explanation of the *Leyden jar*, wherein the specific roles of dielectric matter and conducting matter came into play. In the jar, the glass is in contact with both an inner and an outer conducting coat; if the jar is charged and we consider a surface of glass, we find in it two faces charged in the opposite way. But if we consider a surface of glass with one side in contact with the conducting coat, the two opposite charges are not neutralized any more, for the conductor "is incapable of maintaining in itself the inductive state". As a consequence "the surface charge will not be neutralized, but will constitute that apparent charge which is commonly called the Charge of the Conductor."[3] In other words, what in other theories was called "electric charge on a conductor" was looked upon by Maxwell as the effect of an unbalanced tension appearing at the boundaries between dielectrics and conductors. This electric charge would arise from the leaky *electric elasticity* of conductors; the latter would be the kind of matter not able to retain the electric polarisation. The only electric charge left would

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[1] See Maxwell J.C. 1881, vol. I, p. 153: "At every point of the medium there is a state of stress such that there is a tension along the lines of force and pressure in all directions at right angles to these lines, the numerical magnitude of the pressure being equal to that of the tension, and both varying as the square of the resultant force at the point."

[2] See Maxwell J.C. 1881, vol. I, p. 154: "I have not been able to make the next step, namely, to account by mechanical considerations for these stresses in the dielectric. I therefore leave the theory at this point, merely stating what are the other parts of the phenomenon of induction in dielectrics."

[3] Maxwell J.C. 1881, vol. I, p. 155.

be that placed on dielectrics, which are able to offer a long-lasting elastic reaction to electric forces.[4]

The electric charge was a sort of side-effect of polarisation: it was "only the manifestation of a single phenomenon, which we may call Electric Polarisation".[5] Energy was the energy of polarisation, a particular condition of dielectric matter, and energy density corresponded to "tension on unit of area" *p*, in accordance with the following mathematical steps:

$$p = \frac{1}{2}DE = \frac{1}{2}\varepsilon E\,E = \frac{1}{2}\frac{k}{4\pi}E\,E = \frac{k}{8\pi}E^2 = \frac{2\pi}{k}D^2$$

In the last passages of the chapter, Maxwell took into account the electric currents in conductors: currents would be nothing else but the side-effect of the transformation of elastic tensions into heat. The representation of conductors as *bad* dielectrics is pointed out once again: conductors are the seat of a dissipation of energy, from mechanical to thermal.

> "If the medium is not a perfect insulator, the state of constraint, which we call electric polarisation, is continually giving way. The medium yields to the electromotive force, the electric stress is relaxed, and the potential energy of the state of constraint is converted into heat."[6]

According to a correct energetic balance, the potential energy of polarisation continuously transforms both into the kinetic energy of the electric current and into heat associated to it. The temperature of the conductor grows until an equilibrium state, when "as much heat is lost by conduction and radiation from its surface as is generated in the same time by the electric current."[7] Electric current appears as an intermediate state,

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[4] See Maxwell J.C. 1881, vol. I, p. 155: "The charge therefore at the bounding surface of a conductor and the surrounding dielectric, which on the old theory was called the charge of the conductor, must be called in the theory of induction the surface charge of the surrounding dielectric. According to this theory, all charge is the residual effect of the polarisation of the dielectric. This polarisation exists throughout the interior of the substance, but it is there neutralized by the juxtaposition of oppositely charged parts, so that it is only at the surface of the dielectric that the effects of the charge become apparent."

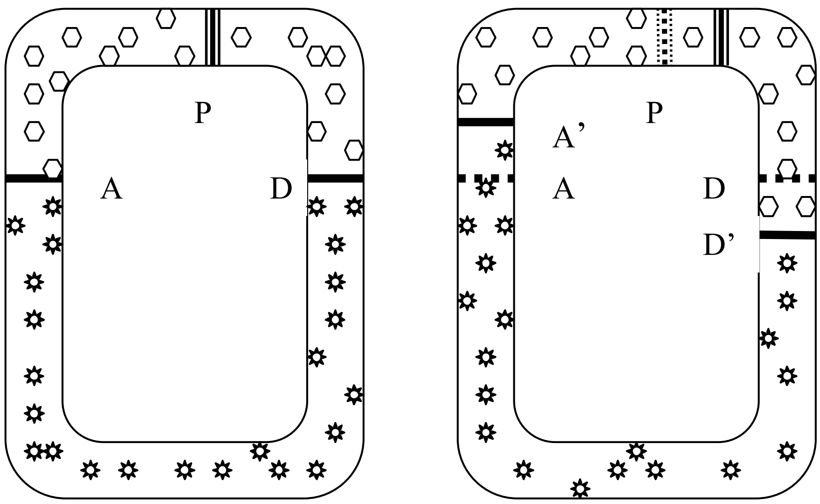
[5] Maxwell J.C. 1881, p. 156.

[6] Maxwell J.C. 1881, vol. I, p. 156.

[7] Maxwell J.C. 1881, vol. I, p. 156.

which allows the transition between electric polarisation and thermal dissipation.

In the second part of *Treatise*, the tenth chapter has the meaningful title "Conduction in Dielectrics". In the last section, "Mechanical Illustration of the Properties of a Dielectric", Maxwell showed a hydrodynamic model of the process of charge and discharge in dielectrics. I am describing a slightly simplified version, which preserves the original meaning and helps to better explain that meaning. A closed rectangular pipe contains mercury in its lower part and water in the upper. A piston *P*, placed inside the upper horizontal branch can push the water towards the right. When the piston is in its equilibrium position *P*, mercury reaches the same level, *A* and *D*, in the vertical branches of the pipe. When we push the piston, water goes down in the right branch and it goes up in the left one: *A'* and *D'* are the new equilibrium positions. In Maxwell's theoretical view, this arrangement would represent dielectric polarisation.[8]



[8] See Maxwell J.C. 1881, vol. I, p. 423: "The excess of water in the tube *D* may be taken to represent a positive charge of electricity on one side of the dielectric, and the excess of mercury in the tube *A* may represent the negative charge on the other side. The excess of pressure in the tube *P* on the side of the piston next *D* will then represent the excess of potential on the positive side of the dielectric."

If the piston were free to move and return to the previous position *P*, we would have the representation of the discharge of a dielectric. The motion of the fluids represents the change of electric displacement over time, which in Maxwell's theory is the "displacement current". The fluids of the model are incompressible and this corresponds to the fact that "there is no real accumulation of electricity at any place". There is no accumulation but only displacement: the model suggests that what we call *accumulation* of electric charge corresponds to nothing more than a displacement of matter together with an increase of potential energy. If the piston were leaky, the pressure would be wasted by the contrary flux of water restoring the original balance: this is the case of a conductor, which cannot endure polarisation.[9] In Maxwell's model, charge and discharge would correspond to retaining or releasing a state of tension, just like a spring: a body is electrically charged in the same way a spring is charged. Electric charge is connected to matter in a dynamical way: it corresponds to the dynamical tension of a substance much more than to a substance in itself.

Maxwell dwelt upon the relationship between matter and electric charge also in the fourth chapter of the second part, "Electrokinematics", a chapter devoted to electrolysis. On the first page, he remarked that he was talking about a subject belonging "quite as much to Chemistry as to Electricity", even though he would have confined himself to the "electrical point of view". He hoped that electrolysis would have helped him to understand "the true nature of electric current", for motions of matter and motions of electricity were both involved: "currents of ordinary matter and currents of electricity" seemed two aspects and "essential parts of the same phenomenon".[10] In brief, electrolysis suggested a deep connection between matter and electricity. A given amount of electric charge was associated to cations and anions; in such a way, a current of ions could be interpreted as a convective electric current.

> "The actual transfer of the ions through the substance of the electrolyte in opposite directions is therefore part of the phenomenon of the conduction of an electric current through an electrolyte. At every point of the electrolyte through which an electric current is passing there are also two opposite material currents of the anion and the cation, which have the same lines of flow with the electric current, and are proportional to it in magnitude.

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[9] See Maxwell J.C. 1881, vol. I, pp. 424-5.
[10] Maxwell J.C. 1881, vol. I, p. 345.

> It is therefore extremely natural to suppose that the currents of the ions are convection currents of electricity. And, in particular, that every molecule of the cation is charged with a certain fixed quantity of positive electricity, which is the same for the molecules of all cations, and that every molecule of the anion is charged with an equal quantity of negative electricity."[11]

Nevertheless Maxwell thought that the "tempting hypothesis" of convective currents of ions would have led "into very difficult ground". One of the consequences was the independence of the given amount of electricity associated to cations from the kind of cations: that amount should have been the same for every kind of molecule. Reasoning in terms of electrochemical equivalents and introducing some "molecular speculations", Maxwell assumed that each molecule, "on being liberated from the state of combination, parts with a charge whose magnitude is 1/N", where N is "the number of molecules in an electrochemical equivalent". This led to the hypothesis of a definite quantity of electricity, which he called "molecular charge"; it could be considered as "the most natural unit of electricity".[12]

Maxwell noted the theoretical gap between the concept of the "electrification of a molecule" and the concept of electricity explained in other parts of his *Treatise*. A strong conceptual tension arose from the conception of charge as a side-effect of strains taking place in a continuous elastic medium, and the different conception of electric charge as discrete units associated to discrete units of matter. Tension took place between a continuous and a discrete model, and between a substantial (*molecular charge*) and a dynamical model (*displacement*). Other questions arose as well. Why should the molecular charge exchanged between a molecule of chlorine and a molecule of zinc be equal to the molecular charge exchanged between a molecule of chlorine and a molecule of copper, knowing that "the electromotive force between chlorine and zinc is much greater than that between chlorine and copper"? In other words, "why should electromotive forces of different intensities produce exactly equal charges?" He found a solution in a pragmatic approach to the subject matter. He assumed the concept of "*one molecule of electricity*" as a useful concept, though it was "out of harmony with the rest of this treatise": it would have allowed him "to state clearly what is known about electrolysis, and to appreciate the

[11] Maxwell J.C. 1881, vol. I, p. 346.
[12] Maxwell J.C. 1881, vol. I, p. 349.

outstanding difficulties".[13] He was outlining a sort of gross draft, provisional and simplified, far from a satisfactory theory to be still developed.[14]

Both a satisfactory theory of electricity and a satisfactory theory of matter seemed to Maxwell still not accomplished; a theory of matter appeared to him even more intricate. The fact is that "every chemical compound is not an electrolyte": matter existed both in a state involving electricity and in a state where electricity seems to be not involved. The structure of matter could not be explained only by electricity: even a satisfactory theory of electricity would not have been able to account for the structure of matter, for "chemical combination is a process of a higher order of complexity than any purely electrical phenomenon".[15] The phenomenon of electrolysis challenged the theoretical model Maxwell had developed on matter, energy and electricity, based on the model of solid dielectrics. Electrolysis suggested a different model for both matter and electricity: he took it seriously into account but he could not rely on it.[16]

In the "Preface" to his *Treatise*, he had placed his trust in theoretical physics and consistent theoretical models. He had asserted that he was satisfied neither by "lecture-room experiments" nor by "mathematical memoirs", both unable to "form a connected system". He wanted to proceed "in a methodical manner" and he appreciated "Faraday's way of conceiving phenomena". He acknowledged the leading role of theoretical physics, where "theoretical speculations" and "physical hypotheses" showed the way to the scientific research. He was aware that Faraday's conception and continental action-at-a-distance conception accounted for the same phenomena, but the

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[13] Maxwell J.C. 1881, vol. I, pp. 349-350.

[14] See Maxwell J.C. 1881, vol. I, p. 351: "This theory of molecular charges may serve as a method by which we may remember a good many facts about electrolysis. It is extremely improbable that when we come to understand the true nature of electrolysis we shall retain in any form the theory of molecular charges, for then we shall have obtained a secure basis on which to form a true theory of electric currents, and so become independent of this provisional theories."

[15] Maxwell J.C. 1881, vol. I, p. 353.

[16] In 1890, Poincaré vividly described the sense of discomfort ("malaise") and mistrust ("défiance") of a French scholar who read Maxwell's *Treatise*. French educated people, accustomed to systematic, logic and precise accounts, found it hard to appreciate a series of "provisional and independent models". See Poincaré H. 1890, pp. V and VIII. See also pp. VII-VIII: "Ainsi, en ouvrant Maxwell, un Français s'attend à y trouver un ensemble théorique aussi logique et aussi précis que l'Optique physique fondée sur l'hypothèse de l'éther; il se prépare ainsi une déception que je voudrai éviter au lecteur en l'avertissant tout de suite de ce qu'il doit chercher dans Maxwell et de ce qu'il n'y saurait trouver. [...] ...; le savant anglais ne cherche pas à construire un édifice unique, définitif et bien ordonné, il semble plutôt qu'il élève un grand nombre de constructions provisoires et indépendantes, entre lesquelles les communications sont difficiles et quelquefois impossibles."

latter was "entirely alien from the way of looking at things which I adopt". He claimed the importance of "a philosophical point of view": on those grounds, he wrote, the "two methods should be compared". Even though both methods "have succeeded in explaining the principal electromagnetic phenomena", they had to be ultimately judged on the grounds of theoretical physics, where "fundamental conceptions (…) as well as most of the secondary conceptions of the quantities concerned" did the difference.[17]

The hypothesis of molecules of electricity could account for observed phenomena and, in addition, it had already been translated into a mathematical theory. Nevertheless, those "theoretical speculations" and "physical hypotheses", which he had referred to in the "Preface", led him to refuse theories empirically and mathematically as reliable as his own. Not only did a good theory have to satisfy the requirements of experimental physics and mathematical physics; it also had to offer a consistent and unified conceptual structure.

Nevertheless, his strong theoretical commitment did not prevent Maxwell from analysing the different features of different conceptual models. For instance, in chapter III of the first part of his *Treatise*, he took into account energy from the point of view of a system of conductors, rather than from the point of view of the medium. He started from a given "quantity of electricity δe" which could be brought "from an infinite distance (or from any place where the potential is zero) to a given part of the system where the potential is V". The work done during that process amounted to $V \cdot \delta e$; that definition of work was, at the same time, a definition of potential. The result of the process was an increase δe in the amount of electric charge the system already possessed. In general, the work done "in producing a given alteration in the charges of the system" could be expressed by a sum of integrals $\sum(\int V \cdot \delta e)$, "where the summation ($\sum$) is to be extended to all parts of the electrified system".[18] Maxwell assumed that originally the system had zero charge and zero potential; subsequently he imagined that "the different portions of the system be charged simultaneously, each at a rate proportional to its final charge". In that case, the electric energy of the system, "expressed in terms of the charges of the different parts of the system and their potentials", was $W = \frac{1}{2} \sum(V \cdot e)$, provided that e be considered as the "final charge" and V the "final potential of any part of the system".[19]

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[17] Maxwell J.C. 1881, vol. I, pp. ix and xii.
[18] Maxwell J.C. 1881, vol. I, p. 96.
[19] Maxwell J.C. 1881, vol. I, p. 97.

In that chapter, the way of conceiving electric energy was quite different from the theoretical model Maxwell had put forward in the first chapter. Here electricity is something whose quantity can be carried from one position to another, something which is attached to bodies; energy is localised in bodies as well. Here the medium was not involved, and the corresponding theoretical model was an alternative to the previous one. Even electrodynamic energy could be imagined as associated to bodies or, better, to electric currents. In the fourth part of *Treatise*, in chapter VI, devoted to the "dynamical theory of electromagnetism", Maxwell noted that electric currents can produce work and the "capacity of doing work is nothing else than energy". Electric currents consisted in a kinetic phenomenon of some kind, whose cause was named "Electromotive Force". Electromotive force had not "to be confounded with ordinary mechanical force", even though work and energy were both "exactly of the same kind" and "measured by the same standards or units". Energy could be transformed in many ways, giving rise to motion, heat or pure electromagnetic actions; in any case, energy was represented as affecting conductors and electric circuits.[20]

In chapter XI of the same part, "On energy and stresses in the electromagnetic field", Maxwell tried to bridge the gap between the two different theoretical models of energy discussed in different parts of his *Treatise*. He started from electrostatic energy expressed in terms of electric charge and electric potential, both localised on bodies; then some mathematical manipulations allowed him to reach the expression of energy given in terms of electric forces and electric displacement, both localised everywhere in the medium. The deductive process showed the mathematical equivalence between otherwise different theoretical models. In the end, not only did Maxwell acknowledge the mathematical equivalence between energy localised in electric currents and energy localised in the medium, but pointed out the deep conceptual difference between the two theoretical models.

> "The electrokinetic energy of the system may therefore be expressed either as an integral to be taken where there are electric currents, or as an integral to be taken over every part of the field in which magnetic force exists. The first integral,

[20] See Maxwell J.C. 1881, vol. II, pp. 196-8. See, in particular, pp. 197-8: "Part of the work done by an electromotive force acting on a conducting circuit is spent in overcoming the resistance of the circuit, and this part of the work is thereby converted into heat. Another part of the work is spent in producing the electromagnetic phenomena observed by Ampère, in which conductors are made to move by electromagnetic forces. The rest of the work is spent in increasing the kinetic energy of the current, and the effects of this part of the action are shewn in the phenomena of the induction of currents observed by Faraday."

> however, is the natural expression of the theory which supposes the currents to act upon each other directly at a distance, while the second is appropriate to the theory which endeavours to explain the action between the currents by means of some intermediate action in the space between them. As in this treatise we have adopted the latter method of investigation, we naturally adopt the second expression as giving the most significant form to the kinetic energy."[21]

The conceptual tension between mathematical physics and theoretical physics was explicitly on the stage. The mathematical aspect of his electromagnetic theory involved two equivalent mathematical representation of energy, corresponding to two different theoretical representations. Both of them were logically consistent but, without any doubt, Maxwell preferred the representation of energy stored in the medium. As I have remarked in the first chapter, in the *Introduction*, the equivalence was really granted only in the limited context of electrostatic phenomena and steady electric currents. That equivalence did not encompass all electromagnetic phenomena: however, this is Maxwell's theoretical heritage.

> "The energy of the field therefore consists of two parts only, the electrostatic or potential energy
>
> $$W = \frac{1}{2}\iiint (P\,f + Q\,g + R\,h)\,dx\,dy\,dz$$
>
> and the electromagnetic or kinetic energy
>
> $$T = \frac{1}{8\pi}\iiint (a\,\alpha + b\,\beta + c\,\gamma)\,dx\,dy\,dz$$ "[22]

We could put Maxwell's theoretical models in a more general perspective, involving the nineteenth century landscape of the history of physics. Analogies between heat and electricity were developed by W. Thomson, around the middle of the nineteenth century in Great Britain.[23] That century

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[21] Maxwell J.C. 1881, vol. II, p. 251.

[22] Maxwell J.C. 1881, vol. II, p. 253. On the "equivalence" between the two models, and the sharp split between *kinetic* and *potential* electromagnetic energy, see chapter 1 of the present book.

[23] See Thomson W. 1845, p. 27: "Corresponding to every problem relative to the distribution of electricity on conductors, or to forces of attraction and repulsion exercised by electrified bodies, there is a problem in the uniform motion of heat which presents the same analytical conditions, and which, therefore, considered mathematically, is the same problem."

had been crossed by the query on the nature of heat: was it a substance or a state of motion, some kind of matter or some kind of energy? The same question appeared suitable even for electricity. With regard to heat, we know that in the second half of the nineteenth century there was a transition from a matter-like conception to an energy-like conception. I venture to imagine that Maxwell could have outlined a conceptual path of the same kind: from a matter-like conception of electricity, as fluid or substance, to an energy-like conception of electricity, as a side-effect of the concentration of *electric-elastic* energy on the surfaces where dielectrics are in contact with conductors.[24]

At the same time, when dealing with specific phenomena, Maxwell made use of other representations: to explain electrolysis, for instance, he resorted to the alternative model of microscopic "ions". This fact led Darrigol to state that "Maxwell integrated some of Ampère's and Weber's atomistics into his own theory". Although I do not find convincing the attribution to Maxwell of that integration, it seems to me that Darrigol has suitably highlighted Maxwell's leading conception of electric charge and electric current in the context of his whole theory. On the one hand, he acknowledged the plurality of Maxwell's conceptions; on the other hand, he singled out a "core" and a "periphery" in that wide field of conceptions. In fact, we find in Maxwell's *Treatise* a general theoretical framework and some auxiliary conceptions, which Maxwell devised in order to explain some specific class of phenomena. I share Darrigol's remark, that the core of Maxwell's electromagnetic theory "was essentially macroscopic": matter and aether were looked upon as "a single continuous medium with variable macroscopic properties (specific inductive capacity, magnetic permeability and conductivity)". At the "periphery" there were other models, specifically devoted to the explanation of magnetisation in matter, electrolysis and the Faraday effect. Those phenomena led Maxwell to acknowledge that a more detailed picture of the connection between aether and matter was really required.[25]

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[24] Sometimes, Maxwell has been associated to a radical *dynamism*. See, for instance, Siegel D.M. 1981, p. 264. This interpretation seems to me too radical: although, after 1875, Maxwell shared W. Thomson's dynamical conception of matter, he did not get involved in projects of great unification on a *dynamical* basis. See also chapter 1 of the present book. It is worth noting that, in the context of British electromagnetic theories, the *substantialization* of energy emerged just after electric charge had been *desubstantialized* in Maxwell's theory.

[25] See Darrigol O. 2000, p. 168 and 174: "He tried three different strategies. For magnetisation, he modified his theory to integrate molecular assumptions; for electrolysis, he proposed a temporary ionic theory that contradicted his general concept of the electric current; for the Faraday effect, his method was essentially based on a phenomenological modification of the optical Lagrangian, although he invoked a deeper molecular mechanism."

According to Maxwell, the effects of the electromotive force could help us to test the substantial model of electricity. If "electricity were a fluid like water", when an electric current starts to flow through a coil, "the coil would at first rotate in the opposite direction". Maxwell concluded that phenomena "of this kind", which "cannot be confounded" with electromagnetic induction, had not been observed. If observed, the phenomenon would lead us to look upon electricity "as a real substance, and we should be able to describe the electric current as a true motion of this substance".[26]

In 1881, two years after Maxwell's death, the conceptual tension between the different models of electricity was well described by Helmholtz in a lecture held before the Royal Institution. He acknowledged that the hypothesis of two electric fluids or electric substances endowed with "opposite qualities" was "a rather complicated and artificial machinery" and that Maxwell's "mathematical language" offered, in a simple and consistent way, "the laws of the phenomena". Nevertheless, beyond "mathematical formulae", he found it hard to explain what "a quantity of electricity" was, as well as to explain "why such a quantity is constant, like that of a substance". The fact was, Helmholtz remarked, that the "old notion of substance" could not necessarily be identified with the notion of matter. In that sense, scientist could preserve the word "substance" for the two kinds of electricity, granted that electricity "cannot be neither generated nor destroyed".[27] Moreover, electrolysis actually challenged Maxwell's desubstantialisation of electric charge: following Faraday's law, Helmholtz stated, "through each section of an electrolytic conductor we have always equivalent electrical and chemical motion". He found a "real relation" between "equivalents of chemical elements" and "equivalent quantities of electricity", even though the existence of chemical atoms of matter "may be hypothetical". Scientists could not rely on a theory explaining "all the facts of chemistry as simply and as consistently as the atomic theory". Helmholtz thought that the latter had to be accepted. As a consequence, the atomic constitution of matter entailed the atomic constitution of electricity.[28]

I also agree with Darrigol on the claim that, in some way, the conceptual tension between macroscopic and microscopic models was "inaugurated" by Maxwell himself. See Darrigol O. 2000, p. 176. On that conceptual tension, see also Harman P.M. 1998, p. 186.

[26] Maxwell J.C. 1881, vol. II, pp. 201-2.

[27] Helmholtz H. 1881, p. 283.

[28] Helmholtz H. 1881, pp. 289-90. See, in particular, p. 290: "If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions,

In the last decade of the century, W. Thomson was still exploring the possible connections among aether, matter and electricity. In 1890 he tried to outline a quite general unified theoretical framework, wherein aether ("a merely ideal substance"), matter, electricity and heat appeared deeply linked to each other. Nevertheless, that unified framework was considered by W. Thomson himself more a speculation than a reliable theoretical model, more a dream than an effective representation. He acknowledged that "the triple alliance, ether, electricity, and ponderable matter" were more "a result of our want of knowledge, and of capacity of imagine beyond the limited present horizon of physical science, than a reality of nature".[29]

In the same decade, some difficulties in explaining conductivity brought J.J. Thomson and Larmor, although in a different way, to develop a new conception, both dynamical and substantial, of electric charge. As I have already shown, since the 1880s, first in Great Britain and then on the Continent, some scientists had begun to conceive a sort of substantialisation of energy.

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which behave like atoms of electricity. As long as it moves about in the electrolytic liquid, each ion remains united with its electric equivalent or equivalents."

[29] See Thomson W. 1889, p. 465: "All of this essentially involves the consideration of ponderable matter permeated by, or embedded in ether, and a tertium quid which we may call electricity, a fluid go-between, serving to transmit force between ponderable matter which we call heat."

Appendix: *The mathematical bridge between two different theoretical models*

In chapter XI of the fourth part of his *Treatise*, Maxwell first wrote energy as

$$W = \frac{1}{2}\sum (e\Psi),$$

"where *e* is the charge of electricity at a place where the electric potential is Ψ and the summation is to be extended to every place where there is electrification".[30] The charge *e* was linked to the electric displacement $\boldsymbol{D}$ by the law of divergence $\rho_e = \nabla \cdot \boldsymbol{D}$, or

$$e = \left(\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz}\right) dx\,dy\,dz\ .$$

The last expression led to a new equation for the energy:

$$W = \frac{1}{2}\iiint \left(\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz}\right)\Psi\, dx\, dy\, dz\,.$$

The integral was extended throughout all space; in a more compact mathematical notation it can be written as $W = \frac{1}{2}\iiint (\nabla \cdot \boldsymbol{D})\Psi dx\,dy\,dz$, and the three terms of the kind $\frac{df}{dx}\Psi$ can be written as $\frac{df}{dx}\Psi = \frac{d}{dx}(f\Psi) - f\frac{d\Psi}{dx}$.

> "Integrating this expression by parts, and remembering that when the distance, *r*, from a given point of a finite electrified system becomes infinite, the potential Ψ becomes an infinitely small quantity of the order r^{-1}, and that *f, g, h* become infinitely small quantities of the order r^{-2}, the expression is reduced to

[30] Maxwell J.C. 1881, vol. II, p. 248.

$$W = -\frac{1}{2}\iiint\left(f\frac{d\Psi}{dx} + g\frac{d\Psi}{dy} + h\frac{d\Psi}{dz}\right)dx\,dy\,dz\,,$$

where the integration is to be extended throughout all space."[31]

The gradient of Ψ is nothing but the electromotive force or, in symbols, $-\nabla\Psi=(P,Q,R)$, so that the energy can be written in terms of the electromotive force and electric displacement:

$$W=\frac{1}{2}\iiint(Pf+Qg+Rh)dx\,dy\,dz\,.$$

The mathematical equivalence had thus been proven: Maxwell pointed out that equivalence and, at the time, did not explicitly side with one or the other of the two theoretical models.[32]

In the subsequent sections, "Magnetic Energy" and "Electrokinetic Energy", Maxwell undertook the same mathematical steps. In the latter of these sections, he started from "the kinetic energy of a system of currents", expressed by

$$T=\frac{1}{2}\sum(pi),$$

where *p* was "the electrokinetic momentum of the circuit". The sum was performed over all the circuits of the system, where currents of intensity *i* flowed. If *(F, G, H)* are the components of the electromagnetic momentum, and *(u, v, w)* are the components of the vector density of electric current, the energy can be expressed by

$$T=\frac{1}{2}\iiint(Fu+Gv+Hw)dx\,dy\,dz\,,$$

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[31] Maxwell J.C. 1881, vol. II, p. 248.

[32] See Maxwell J.C. 1881, vol. II, p. 249: "Hence, the electrostatic energy of the whole field will be the same if we suppose that it resides in every part of the field where electrical force and electrical displacement occur, instead of being confined to the places where free electricity is found."

"where the integration is to be extended to every part of space where there are electric currents".[33] The following step consisted of replacing the current density with the *curl* of the magnetic force, according to the circuital law (the so-called Ampère's law), which we can write, in a more modern notation, as $\nabla \times \boldsymbol{H} = 4\pi \boldsymbol{J}$ or $\nabla \times (\boldsymbol{a}, \boldsymbol{b}, \boldsymbol{g}) = 4\pi (\boldsymbol{u}, \boldsymbol{v}, \boldsymbol{w})$. If $\boldsymbol{A} = (F, G, H)$, the energy can be written in a synthetic way as

$$T = \frac{1}{8\pi} \iiint \boldsymbol{A} \cdot (\nabla \times \boldsymbol{H})\, dx\, dy\, dz \,.$$

> "If we integrate this by parts, and remember that, at a great distance *r* from the system, α, β and γ are of the order of magnitude r^{-3}, we find that when the integration is extended throughout all space, the expression is reduced to
>
> $$T = \frac{1}{8\pi} \iiint \left[\alpha \left(\frac{dH}{dy} - \frac{dG}{dz} \right) + \beta \left(\frac{dF}{dz} - \frac{dH}{dx} \right) + \gamma \left(\frac{dG}{dx} - \frac{dF}{dy} \right) \right] dx\, dy\, dz$$ "[34]

Remembering that $\nabla \times \boldsymbol{A} = \boldsymbol{B}$ or $\nabla \times (\boldsymbol{F}, \boldsymbol{G}, \boldsymbol{H}) = (\boldsymbol{a}, \boldsymbol{b}, \boldsymbol{c})$, Maxwell could write the "kinetic" energy as

$$T = \frac{1}{8\pi} \iiint (a\alpha + b\beta + c\gamma)\, dx\, dy\, dz \,.$$

He specified that "the integration is to be extended throughout every part of space in which magnetic force and magnetic induction have values differing from zero".[35]

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[33] Maxwell J.C. 1881, vol. II, p. 250.
[34] Maxwell J.C. 1881, vol. II, p. 250.
[35] Maxwell J.C. 1881, vol. II, p. 251.

8. *The electromagnetic energy and the structure of aether*

Three years after the 1881 edition of Maxwell's *Treatise*, J.H. Poynting, then professor of physics at the Mason College of Birmingham, published a paper in the *Philosophical Transactions*, "On the Transfer of Energy in the Electromagnetic Field", drawing the attention of the scientific community to a new role for energy in electromagnetic actions.[1] Starting from the phenomenon of electromagnetic induction, he suggested that energy was not carried by electric currents in the direction of the currents themselves, but travelled transversally. Consistently with Maxwell's theoretical conceptions, he imagined that electromagnetic energy had not its seat in the conductors but in the surrounding medium. Moreover, Poynting made some original remarks on the transfer of energy. In the theoretical model of contiguous actions, energy could not skip from one body to another instantaneously, but its transfer needed a given time interval. That concept could be expressed saying that energy possesses some kind of *continuity* with regard to time. Poynting's original contribution was the attribution of continuity to energy even with regard to space. He imagined a flux of energy travelling with continuity through both time and space.[2]

Poynting announced a new law for the transfer of energy and claimed that it was consistent with Maxwell's conception of energy as spread throughout aether or other dielectrics.

> "According to Maxwell's theory, currents consist essentially in a certain distribution of energy on and around a conductor, accompanied by transformation and consequent movement of energy through the field.

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[1] For a short time, before Maxwell's death, Poynting had worked at the Cavendish Laboratory in Cambridge, under the direction of Maxwell himself.

[2] See Poynting J.H. 1884, p. 343: "Formerly a current was regarded as something travelling along a conductor, attention being chiefly directed to the conductor, and energy which appeared at any part of the circuit, if considered at all, was supposed to be conveyed thither through the conductor by the current. But the existence of induced currents and of electromagnetic actions at a distance from a primary circuit from which they draw their energy, has led us, under the guidance of FARADAY and MAXWELL, to look upon the medium surrounding the conductor as playing a very important part in the development of the phenomena. If we believe in the continuity of the motion of energy, that is, if we believe that when it disappears at one point and reappears at another it must have passed through the intervening space, we are forced to conclude that the surrounding medium contains at least part of the energy, and that it is capable of transferring it from point to point."

> Starting with Maxwell's theory, we are naturally led to consider the problem, How does the energy about an electric current pass from point to point – that is, by what path according to what law does it travel from the part of the circuit where it is first recognisable as electric and magnetic to the parts where it is changed into heat or other forms?
>
> The aim of this paper is to prove that there is a general law for the transfer of energy, according to which it moves at any point perpendicularly to the plain containing the lines of electric force and magnetic force, …"[3]

The starting point of Poynting's mathematical deduction was just "the energy of the field", that Maxwell had expressed in terms of electric and magnetic forces or, equivalently, in terms of the electric displacement ***D*** and the magnetic induction ***B***. Therefore the electromagnetic energy corresponded to the sum

$$\frac{K}{8\pi}\iiint\left(P^2+Q^2+R^2\right)dx\,dy\,dz+\frac{\mu}{8\pi}\iiint\left(\alpha^2+\beta^2+\gamma^2\right)dx\,dy\,dz\ ,$$

where the first term was electrostatic and the second electromagnetic: (P, Q, R) are the components of the electric force ***E*** and (α, β, γ) the components of the magnetic force ***H***.[4]

After some pages of mathematical manipulations on the above integral, Poynting arrived at the following equation:

$$\begin{aligned}\frac{K}{4\pi}\iiint\left(P\frac{dP}{dt}+Q\frac{dQ}{dt}+R\frac{dR}{dt}\right)dx\,dy\,dz+\frac{\mu}{4\pi}\iiint\left(\alpha\frac{d\alpha}{dt}+\beta\frac{d\beta}{dt}+\gamma\frac{d\gamma}{dt}\right)dx\,dy\,dz\\ +\iiint\left(X\dot{x}+Y\dot{y}+Z\dot{z}\right)dx\,dy\,dz+\iiint\left(Pp+Qq+Rr\right)dx\,dy\,dz\\ =\frac{1}{4\pi}\iint\left[l\left(\beta R'-\gamma Q'\right)+m\left(\gamma P'-\alpha R'\right)+n\left(\alpha Q'-\beta P'\right)\right]dS\end{aligned}$$

The first two integrals on the left side corresponded to electric and magnetic *power* (time variation of energy) entering the given volume. The

[3] Poynting J.H. 1884, pp. 343-4.
[4] See Poynting J.H. 1884, pp. 345-6.

third term represented "the work done per second by the electromagnetic forces, that is, the energy transformed by the motion of the matter in which current exists", namely mechanical energy transformed in electromagnetic energy. The vector of components (*X*, *Y*, *Z*) represented the electromagnetic force per unit volume, and $(\dot{x}, \dot{y}, \dot{z})$ were the components of velocity. The fourth term represents the waste of electromagnetic energy and subsequent transformation into heat or other kinds of energy. The vector of components (*p*, *q*, *r*) represented the conduction current ***C***: the whole term could be written as $\boldsymbol{E} \bullet \boldsymbol{C}$ in a more modern vector language. According to Poynting, the right side "asserts that this energy comes through the bounding surface" ***dS*** wrapping up the given volume.[5]

This is Poynting's specific contribution, corresponding to the flux of electromagnetic energy. With regard to the terms inside the surface integral, (*l, m, n*) are the direction cosine of the normal to *dS*, (α, β, γ) the components of magnetic force and (*P'*, *Q'*. *R'*) the components of a vector ***E'*** defined by the following relationships:

$$
\begin{aligned}
P' &= -\frac{dF}{dt} - \frac{d\psi}{dx} \\
Q' &= -\frac{dG}{dt} - \frac{d\psi}{dy} . \\
R' &= -\frac{dH}{dt} - \frac{d\psi}{dz}
\end{aligned}
$$

Here (*F*, *G*, *H*) are the components of the vector potential ***A*** and ψ is the electrostatic potential.[6] In vector language, $\boldsymbol{E}' = \dot{\boldsymbol{A}} - \nabla\psi$ and the flux of energy is $\frac{1}{4\pi}\iint [H \times \boldsymbol{E}'] dS$.

In the application of the theory to "a circuit containing a voltaic cell", namely an ordinary electric circuit, Poynting pointed out the process of transformation of energy, and the spatial distribution and direction of the electromagnetic flux of energy. The conducting wire of the circuit was a sort of transformer of energy: it was the seat of the transformation of electromagnetic energy into heat or other forms of energy. Nevertheless he noted that, at least in the case of ordinary (voltaic) circuits, after the

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[5] Poynting J.H. 1884, pp. 346-8.

[6] See Poynting J.H. 1884, p. 347.

transformation, the energy is sent forth from the wire in a form which was still electromagnetic in its most intimate nature. Part of the outgoing electromagnetic energy was a visible electromagnetic radiation; in other words, it consisted of ordinary light.[7]

With the help of his theoretical model of transverse streams of energy travelling throughout the dielectric medium, Poynting attempted to give an explanation of the phenomenon of electromagnetic induction. He dared a new interpretation, even though his re-interpretation shared the main features of Maxwell's general theoretical model: the contiguous action, a continuous model of matter, and energy spread throughout the medium. He thought that Maxwell's model required some other details concerning the behaviour of energy: his specific model integrated Maxwell's theory with a sort of kinematics of energy.

> "It is not so easy to form a mental picture of the movement of energy which takes place when the field is changing and induced currents are created. But we can see in a general way how these currents are accounted for. When there is a steady current in a field there is corresponding to it a definite distribution of energy. If there is a secondary circuit present, so long as the primary current is constant, there is no E.M.I. in the secondary circuit for it is all at the same potential. The energy neither moves into nor out of it, but streams round it somewhat as a current of liquid would stream round a solid obstacle. But if the primary current changes there is a redistribution of the energy in the field. While this takes place there will be a temporary E.M.I. set up in the conducting matter of the secondary circuit, energy will move through it, and some of the energy will there be transformed into heat or work, that is, a current will be induced in the secondary circuit."[8]

Following the same conceptual path, Poynting offered a reinterpretation of the electromagnetic theory of light, in terms of the energy flux. He was able

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[7] See Poynting J.H. 1884, p. 354: "Again, when the only effect in a circuit is the generation of heat, we have energy moving in upon the wire, there undergoing some sort of transformation, and then moving out again as heat or light. If MAXWELL'S theory of light be true, it moves out again still as electric and magnetic energy, but with a definite velocity and intermittent in type. We have in the electric light, for instance, the curious result that energy moves in upon the arc or filament from the surrounding medium, there to be converted into a form which is sent out again, and which, though still the same in kind, is now able to affect our senses."

[8] Poynting J.H. 1884, p. 358.

to link to each other, in a simple way, three properties of radiation: velocity, energy and intensities of fields. In the aether, when maximum velocity of waves is $1/\sqrt{\mu K}$, ***E*** had to be perpendicular to ***H***, and the electric energy had to be equal to magnetic energy.[9]

In the last part of his paper, Poynting pointed out what he looked upon as misconceptions concerning Maxwell's "electric displacement". He thought that the word could suggest the image of something really moving in the direction of the electric force. A good candidate for this *something* in motion would seem just energy, but it was not the case: for this reason, Poynting thought that "our use of the term is somewhat unfortunate".[10] In other words, the concept of "displacement" in itself seemed to Poynting unsuitable for a correct interpretation of Maxwell's theory. The last statements of the paper underscored the primacy of energy in Poynting's specific theoretical model. At the same time, the general theoretical model was still that of Maxwell: electric currents appeared as a sort of side-effect of the transformation of energy, when energy is transferred from dielectrics to conductors.[11]

In 1885, Poynting published another paper in *Philosophical Transaction*, "On the connection between Electric Current and the Electric and Magnetic Inductions in the surrounding field", whose title suggests that electric currents are the main subject matter. Nevertheless, the title is slightly

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[9] See Poynting J.H. 1884, p. 360: "It may be noted that the velocity $1/\sqrt{\mu K}$ is the greatest velocity with which the two energies can be propagated together, and that they must be equal when travelling with this velocity. For if v be the velocity of propagation and ϑ the angle between the two intensities, we have

$$\frac{EH\sin\vartheta}{4\pi v} = \frac{KE^2}{8\pi} + \frac{\mu H^2}{8\pi} \text{ or } v = 2\sin\vartheta \bigg/ \left(\frac{KE}{H} + \frac{\mu H}{E}\right)$$

The greatest value of the numerator is 2 when ϑ is a right angle, and the least value of the denominator is $2\sqrt{\mu K}$, when the two terms are equal to each other and to $\sqrt{\mu K}$.

The maximum value of v is therefore $1/\sqrt{\mu K}$, and occurs when $\vartheta = \frac{\pi}{2}$ and $KE^2 = \mu H^2$."

[10] See Poynting J.H. 1884, pp. 360-361.

[11] See Poynting J.H. 1884, p. 361: "I have therefore given several cases in considerable detail of the application of the mode of transfer of energy in current-bearing circuits according to the law given above, as I think it is necessary that we should realise thoroughly that if we accept MAXWELL'S theory of energy residing in the medium, we must no longer consider a current as something conveying energy along the conductor. A current in a conductor is rather to be regarded as consisting essentially of a convergence of electric and magnetic energy from the medium upon the conductor and its transformation there into other forms. The current through the seat of so-called electromotive force consists essentially of a divergence of energy from the conductor into the medium."

misleading, for the most interesting part of this paper consists of a conceptual shift from electromagnetic energy to Faraday's tubes of force.

The paper begins with a long footnote added some months after the paper was sent. This footnote contains some of Faraday's passages (*Exp. Res*. Vol. 1, § 1659) concerning forces between electric currents, in particular Faraday's criticism of Ampère's interpretation of those forces. Poynting pointed out and put in italics some sentences wherein Faraday attempted to explain the interaction between electric currents in terms of lines of inductive force weakening and fading away, contracting and ultimately disappearing.[12] In some way the quotation enlightens the theoretical keystone of this paper. If the first paper had pointed out the primacy of energy, the second drew attention back to Faraday and his peculiar *hardware*, consisting of tubes of force moving, expanding and collapsing through space. In accordance with this different approach, he repeated the linguistic and conceptual criticism of Maxwell's concept of "electric displacement", which he had expressed in 1884. The same linguistic and conceptual mistrust in Maxwell's *electric displacement* was avowed by FitzGerald in the same year, in a short paper published in *Nature*. FitzGerald expressed dissatisfaction with this word, mainly because it suggested a change of position rather than a change in the structure of the medium; the latter seemed to him closer to those electromagnetic actions described by Maxwell's equations.[13] Poynting claimed that the word "induction" would have been better: in a more suitable symmetric representation, we would have two *inducing* forces or intensities, both electric ($\boldsymbol{E}$) and magnetic ($\boldsymbol{H}$), and two corresponding *induced* vectors $\boldsymbol{D}$ and $\boldsymbol{B}$. However he tried to outline a close association between Maxwell's *fields* and Faraday's tubes of force.

> "If we symbolise the electric and magnetic conditions of the fields by induction tubes running in the directions of the intensities, the tubes being supposed drawn in each case so that the total induction over a cross section is unity, then we have reason to suppose that the electric tubes are continuous except where there are electric

[12] See Poynting J.H. 1885, p. 277.

[13] See FitzGerald G.F. 1885a, p. 5: "It seems much more likely that what he called 'electric displacement' are changes in structure of the elements of the ether, and not actual displacements of the elements. … so that I think the word 'displacement' was unfortunately chosen." Could "displacement" be used as synonymous of "polarization" in Maxwell's theory? Maxwell's *Treatise* did not solve the question. For a detailed analysis of Maxwell's "displacement" and "polarization", see Buchwald J.Z. 1985a, pp. 23-9.

> charges, while the magnetic tubes are probably in all cases continuous and re-entrant."[14]

Poynting's theoretical task was quite demanding: he had to connect Maxwell's fields, Faraday's tubes of force, and his own flux of energy, in a consistent way. We should imagine electric and magnetic "tubes" around conducting wires crossed by an electric current: fluxes of energy would correspond to the motion of these tubes. He acknowledged that he was performing a conceptual change, for induction, just like energy, had to be propagated transversally.

> "In the neighbourhood of a wire containing a current, the electric tubes may in general be taken as parallel to the wire while the magnetic tubes encircle it. The hypothesis I propose is that the tubes move in upon the wire, their places being supplied by fresh tubes sent out from the seat of the so-called electromotive force. The change in the point of view involved in this hypothesis consists chiefly in this, that induction is regarded as being propagated sideways rather that along the tubes or lines of induction. This seems natural if we are correct in supposing that the energy is so propagated, and if we therefore cease to look upon current as merely something travelling along the conductor carrying it, and in its passage affecting the surrounding medium."[15]

I find in this paper two other conceptual shifts: they are not explicitly avowed and I would like to unfold them. First, there is a sort of substantialisation of energy when it is associated to the conceptual model of tubes of force. The semantic choice of Poynting, in favour of "tubes" of force rather than "lines" of force, supported a *substantial* representation of electric tensions rather than a pure *geometrical* representation.[16] That

[14] Poynting J.H. 1885, p. 278.
[15] Poynting J.H. 1885, p. 278; [italics in the paper].
[16] In the conceptual shift from "lines" to "tubes", Duhem found a specific feature of British physics as opposed to Continental physics. See Duhem P. 1906, p. 110 : "Le physicien français ou allemand concevait, dans l'espace qui sépare les deux conducteurs, des lignes de force abstraites, sans épaisseur, sans existence réelle; le physicien anglais va matérialiser ces lignes, les épaissir jusqu'aux dimensions d'un tube qu'il remplira de caoutchouc vulcanisé; à la place d'une famille de ligne de force idéals, concevable seulement par la raison, il aura un paquet de cordes élastiques, visible et tangibles, solidement collées par leurs deux extrémités aux surfaces des deux conducteurs, distendues, cherchant à la fois à se raccourcir et à grossir ; …" Duhem's interpretation (and criticism) suits Poynting far less than other British physicists: in general, Poynting did not make use of detailed mechanical

choice supported a matter-like representation of energy rather than a more abstract representation. Secondly, there is a shift from a continuous conception to a discrete conception of energy. According to Poynting, bundles of discrete tubes of force travelled through the medium, giving rise to the known electromagnetic effects. He was aware that his conceptual shifts concerned the theoretical aspect of electromagnetism and did not affect in any way its empirical features. He was interested in a better explanation of electric currents and all phenomena connected to them: his tubes of force in motion could not undergo a direct experimental check. We can directly observe only material dielectrics, conductors, electric currents and their effects. In addition, he knew he would not have been able to give a detailed account of interactions between tubes of force and aether, or between tubes of force and matter.[17]

Poynting stated that Maxwell's theory could be based on three main "principles" and this statement in itself represented a sort of re-interpretation of the theory. The first principle consisted in "the assumption that energy has position, *i.e.*, that it occupies space". The second and the third principles corresponded to the two circuital laws for electric and magnetic intensities. In Poynting's words, "the line integral of the electric intensity round any closed curve is equal to the rate of decrease of the total magnetic induction through the curve", and "the line integral of the magnetic intensity round any closed curve is equal to $4\pi\times$ current through the curve".[18]

Following the theoretical path going back from Maxwell to Faraday, Poynting suggested replacing the second and third "principles", namely the circuital electromagnetic equations, with corresponding statements in terms of tubes of force. Thus they became:

> "*Whenever electromotive force is produced by change in the magnetic field, or by motion of matter through the field, the E.M.F. per unit length or the electric intensity is equal to the number of tubes of magnetic induction cutting or cut by the unit length per second, …*
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> […]

models or machinery. On this specific feature of Poynting's research, see, for instance, Hunt B.J. 1991, pp. 94-5.

[17] See Poynting J.H. 1885, p. 278: "As we have no means of examining the medium, to observe what goes on there, but have to be content with studying what takes place in conductors bounded by the medium, the hypothesis is at present incapable of verification. Its use, then, can only be justified if it accounts for known facts better than any other hypothesis."

[18] Poynting J.H. 1885, pp. 278-9.

> *Whenever magnetomotive force is produced by change in the electric field, or by motion of matter through the field, the magnetomotive force per unit length is equal to 4π x the number of tubes of electric induction cutting or cut by unit length per second, …*"[19]

Why did Poynting undertake the conceptual path going from calculus and from a mathematical and sophisticated theory back to a non-mathematic theory, back to the kingdom of such peculiar things as tubes or lines or force? If "what is electric charge?" or "what are fields?" were, at that time, very demanding questions, "what are tubes of force?" was demanding as well. Nevertheless tubes of force, more than lines of force, seemed endowed with a specific physical consistency. They appeared as neither matter nor energy: they were too *aethereal* to be matter but too *material* to be only pure energy. In Maxwell's theoretical model, energy was everywhere, wherever space existed. Perhaps Poynting realized that a more definite localisation of energy was required, and tubes of force could offer the physical prop to that localisation.

Another kind of query arises when we look for symmetry between electric and magnetic phenomena. The symmetry is actually realised in pure aether: in a more modern and synthetic vector form the two circuital laws become

$$\nabla \times E = -\dot{H} \qquad \text{and} \qquad \nabla \times H = \dot{D}.$$

In cases of matter or conductors carrying electric currents, circuital laws become

$$\nabla \times E = -\dot{H} \qquad \text{and} \qquad \nabla \times H = \dot{D} + J\ ,$$

where ***J*** is the usual conduction current. The general re-interpretation in terms of tubes of force is theoretically consistent only if the currents are also re-interpreted in terms of lines of force. This is what Poynting actually realised. In 1884 he had already represented electric currents as a sort of side-effect of an energy flux converging upon the wire. Electromagnetic energy underwent a transformation, becoming in part kinetic energy, in part heat and in part new electromagnetic energy irradiated outwards. In that model, electric currents corresponded to the kinetic component of that

[19] Poynting J.H. 1885, pp. 280-81.

proliferation of energies. Now the energy flux could be put forward in a more substantial way by means of tubes of force collapsing toward the wire: in this way, even electric currents corresponded to one of the several effects arising from the motion of tubes of force.[20]

Quite mysterious remained the detailed interaction between tubes of force and conducting matter: did the former dissolve into the latter? Perhaps, suggested Poynting in a footnote, "the induction is not destroyed, but only loses its continuity". The process of the collapse of tubes was described by Poynting in some detail: there was a process suitable for electric tubes and a different process suitable for magnetic tubes.

> "Hence it appears that the energy dissipated per second may be represented as half electric half magnetic, the electric energy being dissipated by the breaking up of the tubes, and their disappearance while the magnetic energy is dissipated by the shortening of the tubes, and their final disappearance by contraction to infinitely small dimensions of the diameters of the rings by which we may represent them. At all points therefore outside and inside the energy crossing any surface may be represented as equally divided between the two kinds."[21]

Despite the reference to "infinitely small dimensions", Poynting's theoretical model was still a macroscopic model, without any reference to microscopic structures, neither for matter nor for fields. Among the phenomena which he tried to explain by means of his theoretical model there was the discharge of a condenser through a conducting wire. A transient electric current flows through the wire whilst the electric field between the plates of the condenser decreases. Poynting's representation realised a theoretical synthesis between Faraday's tubes of force and his own energy flux.[22]

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[20] See Poynting J.H. 1885, pp. 281-282: "The hypothesis proposed as to the nature of the current is that *C* electric induction tubes close in upon the wire per second. The wire is not capable of bearing a continually-increasing induction, and breaks the tubes up, as it were, they energy appearing finally as heat."

[21] Poynting J.H. 1885, p. 284.

[22] See Poynting J.H. 1885, p. 287: "According to the hypothesis here advanced we must suppose the lessening of the induction between the plates – induction being used with the same physical meaning as MAXWELL's displacement – to take place by the divergence outwards of the induction tubes. We may picture them as taking up the position of successive lines of induction further and further away from the space between the plates, their ends always remaining on the plates. They finally converge on the wire, and are then

The last pages of the paper were devoted to "the general equations of the electromagnetic field" or better, to "obtain equations corresponding to and closely resembling those of Maxwell by means of the principle upon which this paper is founded." To sum up, Poynting tried to combine Faraday's specific theoretical model with Maxwell's more sophisticated mathematical framework.[23]

Generally speaking, models of aether, models of matter and models of electromagnetic actions were deeply interwoven in British electromagnetic theories, in the last decades of the nineteenth century. Those links are effectively analysed in some papers of G.F. FitzGerald, the talented Irishman who deeply influenced British physics, even though he published few systematic researches.[24] In 1885, in a paper published in *Proceedings of the Royal Dublin Society*, FitzGerald envisaged a universal plenum giving rise to matter and aether: aether consisted of a *sea* of open vortex rings and matter consisted of closed vortex rings. He displayed a detailed mechanical model of electromagnetic actions through aether, a model consisting of "a series of wheels, rotating on axes fixed in a plane board, and connected together by indiarubber bands". In that mechanical model, "the rate of rotation of the wheels is proportional to the rate of increase of polarisation"; this led FitzGerald to associate the magnetic force to that rate of rotation. The angular momentum of the wheels was associated to "the kinetic energy of the currents producing the magnetic force". Moreover, by means of his model, he was able to account for electric dissipation.[25] FitzGerald's model did not succeed in explaining the force of attraction between two electrified bodies, for "this force depends entirely on the connexions between the ether and matter"; as he explicitly acknowledged, "this connexion is not represented on my model". Even "phenomena of magnetised media" could receive no explanation from his mechanical model. However he did not think that "the ether is actually made up of wheels and India-rubber bands": the model was looked upon as a fruitful analogy, from which "we may learn several things". The main issue pointed out by

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broken up and their energy dissipated as heat. At the same time some of the energy becomes magnetic, this occurring as the difference of potential between the plates lowers, so that the tubes contain fewer unit cells."

[23] Poynting J.H. 1885, p. 294.

[24] On the role of Fitzgerald in British physics, see Hunt B.J. 1991, chapters 1 and 2, in particular p. 8.

[25] FitzGerald G.F. 1885, pp. 407 and 409-10. It is worth mentioning that, in those years, even Lodge, professor of Physics at the new University College in Liverpool since 1881, devised aethereal machinery (quite close to Maxwell's *first* mechanical models of cells and idle-wheels in the aether), in order to represent electromagnetic phenomena. See chapter 9 of the present book.

FitzGerald concerned the structure of aether or the necessity that it had a structure.[26]

Beyond the specific and unsatisfactory features of FitzGerald's mechanical model we find an attempt to devise a microscopic structure both for matter and aether. He thought that such a structure could be dynamical, as suggested by W. Thomson's "very beautiful theory of matter". Nevertheless, he acknowledged that Thomson's vortex-atoms were not completely reliable: it seemed to him "unlikely … that the simple hypothesis that an atom is a mere vortex ring in a liquid otherwise at rest is a sufficient hypothesis". However, FitzGerald thought he could rely on a quite general model of fluid aether whose rigidity was warranted by some kind of vorticity, and on the "most general supposition" of a medium as a "vortex-sponge", namely "everywhere endowed with … an equal number of vortex motion in all directions". Electric polarisation of the medium could correspond to a polarisation of those motions: vortex rings, for instance, "might also have their motions polarised so as to move parallel to lines or planes". The conservation of the angular momentum had an important consequence: the appearance of one kind of polarisation in a given place of the medium were balanced by an opposite polarisation elsewhere. That model of polarisation could account for a sort of conservation of electrification, for "we could not produce one kind of electrification without producing somewhere an equal and opposite electrification".[27]

However general and hypothetical the theoretical model might be, it offered a unified *dynamic* foundation both for matter and electric charge. FitzGerald's model had many flaws but, beyond its specific features, it was an attempt to go beyond the electromagnetic model of matter displayed by Maxwell in his *Treatise*. Calling for a structure of aether implied the dissolution of Maxwell's simplified continuous model both for aether and matter. In the same year, Poynting, following a different conceptual path, was trying to represent the aether as a sea of Faraday's tubes of force.

In a "Presidential Address", read before the *British Association for the Advancement of Science* at Bath in September 1888[28] and published the next year, FitzGerald once again drew the attention of his colleagues to aether and electromagnetic actions. He avowed hopefully that "[w]e seem to be approaching a theory as to the structure of the ether": structure and

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[26] He stated that "the ether must have some structure" and "it must be capable of being a vehicle of heat energy of exactly the same form as that in material bodies". See FitzGerald G.F. 1885, pp. 408 and 414-6.

[27] FitzGerald G.F. 1885, pp. 417-9.

[28] For a report on that eventful meeting, see Hunt B.J. 1991, chapter 7.

properties of ether could explain all kinds of physical actions. Not only was aether "the means of propagation of light" and "the means by which electric and magnetic forces exist"; in addition, hopefully as well, "it should explain chemical actions and, if possible, gravity".[29]

W. Thomson's theory of vortex rings was still put forward as a suitable solution, able to unify physics and chemistry, in particular a physical theory of electromagnetic actions and a physical theory of matter. Although FitzGerald acknowledged that the hypothesis of material atoms as "simple vortex rings in a perfect liquid otherwise unmoving is insufficient", that hypothesis was fascinating in its attempt at "reducing matter to motion and potential to kinetic energy". He acknowledged that the fascination was purely theoretical or, as he wrote, it was put forward on "metaphysical grounds".[30]

Some years before, in 1883, J.J. Thomson, then a fellow of Trinity College, Cambridge, had published a book which was a slightly refined version of the essay with which he had won the 1882 Adam Prize. The title of the book was *A Treatise on the Motion of Vortex Rings*, corresponding to the subject of the Prize, "A general investigation of the action upon each other of two closed vortices in a perfect incompressible fluid". He had mathematically developed the model of vortex rings and outlined some application to the structure of matter, following the path traced by W. Thomson. He remarked that "the theoretical model of atom of matter as vortex ring in a universal fluid" assured that atoms were "indestructible and indivisible". Moreover, he continued, "it can possess, in virtue of its motion of translation, kinetic energy"; at the same time, "it can also vibrate about its circular form, and in this way possess internal energy". In brief, the model offered "promising materials for explaining the phenomena of heat and radiation".[31] Afterwards, in the 1890s, as we will see later, J.J. Thomson tried to go beyond Maxwell's electromagnetic theory and beyond W. Thomson's theory of matter.

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[29] The necessity of an aether was explained by FitzGerald in a way "not for specialists", starting from a rhetorical question: "What becomes of light for the eight minutes after it has left the sun and before it reaches the earth?". In other words, the existence of aether was tightly linked to the existence of contiguous actions propagating in a finite time; in that same year, FitzGerald remarked, Hertz's experiment had *confirmed* "the ethereal theory of electro-magnetism". FitzGerald G.F. 1889, pp. 558-9 and 561.

[30] FitzGerald G.F. 1889, pp. 561-2. FitzGerald claimed that, on "the sure ground of experimental research", we have already "enslaved the all-pervading ether", for "in electro-magnetic engines we are using as mechanism the ether".

[31] Thomson J.J. 1883, p. 1.

9. Enthusiasm and criticism about the flux of energy

Poynting's papers immediately raised a debate. In 1885, Oliver Lodge published a paper in the *Philosophical Magazine* with the title "On the Identity of Energy: in connection with Mr. Poynting's Paper on the Transfer of Energy in an Electromagnetic Field; and on the two Fundamental Forms of Energy". He was an enthusiastic upholder of Poynting's 1884 thesis and immediately pointed out the concept of "continuity in the existence of energy"; continuity of energy appeared to him as "a natural though not necessary consequence of its conservation." In other words, the principle of conservation of energy did not require a principle of continuity of energy: the latter was a specific interpretation of the former. In the first lines of his paper, Lodge made use of the similarity between matter and energy: it was the first time that such a similarity was so boldly claimed in the context of post-Maxwell British electromagnetism.[1]

According to Lodge, we should imagine energy as transferred part after part, and we could also trace paths for energy throughout space. Lodge outlined a theoretical model for energy which would have led physics even beyond Poynting's model, towards an actual substantialisation of energy.

> "On the new plan we may label a bit of energy and trace its motion and change of form, just as we ticket a piece of matter so as to identify it in other places under other conditions; and the route of the energy may be discussed with the same certainty that its existence was continuous as would be felt in discussing the route of some lost luggage which has turned up at a distant station in however battered and transformed a condition."[2]

This new conception appeared to Lodge "much simpler and more satisfactory than in its old form", for it could be based simply on two assumptions: first, what he called "Newton's law" of motion and, second, the general model of contiguous actions. The law of Newton which Lodge referred to was a sort of reinterpretation of the whole set of Newton's three laws of motion. Lodge thought that they could be summarized in the

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[1] See Lodge O. 1885, p. 482: "… whenever energy is transferred from one place to another at a distance, it is not to be regarded as destroyed at one place and recreated in another, but it is to be regarded as transferred, just as so much matter would have to be transferred; and accordingly we may seek for it in the intervening space, and may study the paths by which it travels."

[2] Lodge O. 1885, p. 482.

following statement: "*Force is always one component of a stress*". *Force* is here the action taking place in the space between two bodies. Newton's third law was interpreted as a sort of symmetry in a couple of bodies with regard to their interaction. Simple mathematical passages linked force to energy: energy was what a body loses when it does work; conversely a body does work "when it exerts force through a distance". This was all we need, Lodge wrote, in order to establish a new law of conservation of energy which took into account the "*identification*" of energy.[3]

He indeed offered a re-interpretation of Newton's laws of mechanics and Newton's concept of force. The stress taking place between *A* and *B* was the part of the model bridging the gap between Newton's laws and contiguous action. According to Lodge, spatial continuity of energy required continuity of matter, for continuous paths for energy could take place only throughout a continuous medium. This general requirement of continuity forbade discontinuity even on a microscopic scale; the existence of an actual contact between A and B was assured by the existence of aether, "the perfectly continuous space-filling medium".[4]

Lodge thought that even the concept of potential energy could be better understood by the assumption that all physical interactions are contiguous actions taking place through aether. Although the intimate "nature of gravitation, elasticity, cohesion, etc." was not yet understood, the theoretical model of contiguous action offered a "consistent mental image" of potential energy. In the case of a stone undergoing the Earth's gravitational action, for instance, potential energy was stored neither in the stone nor in the Earth; even the conception of a certain amount of energy possessed by the system stone-Earth appeared to Lodge too confusing. The general theoretical model of contiguous action required that energy was stored in the medium surrounding that system.[5]

Lodge's re-interpretation of the foundations of mechanics entailed a re-interpretation of energy and its transformations. It was well known that work required two components, force and displacement; for displacement

[3] See Lodge O. 1885, p. 483: "If A does work on B it exerts force on it through a certain distance; but (Newton's law) B exerts an equal opposite force, and (being in contact) through exactly the same distance; hence B does an equal opposite amount of work, or gains the energy which A loses. The stress between A and B is the means of transferring energy from A to B, directly motion takes place in the sense AB."

[4] See Lodge O. 1885, p. 483: "And the energy cannot *jump* from A to B, it is transferred across their point of contact, and by hypothesis their 'contact' is absolute: there is no intervening gap, microscopic, molecular or otherwise. The energy may be watched at every instant. Its existence is continuous; it possesses identity."

[5] See Lodge O. 1885, p. 484, in particular the footnote.

means motion, Lodge stated, work requires both force and motion. On the contrary, he claimed, energy also exists when only one component acts: potential energy would correspond to force and kinetic energy would correspond to motion. In other words, energy "has two fundamental forms because work has two factors, force and motion". Moreover, force was associated to elasticity, whereas motion was associated to inertia, so that kinetic energy corresponded to "motion combined with inertia" and potential energy corresponded to "force combined with elasticity". The reference to inertia and elasticity appears consistent with the attempt to re-interpret energy from the point of view of contiguous action, for inertia and elasticity were just the two properties attributed to the universal medium, at least in the context of British electromagnetic theories. In a certain sense the two kinds of energy could be looked upon as "potential work", or two different components of work, each of them "as real and actual as the other". In some parts of the paper, kinetic energy and potential energy seem in opposition rather than complementary components. That opposition mirrored the opposition between force and motion: motion indeed "shall continue even against some force", and force "shall continue even though motion be permitted".[6]

To sum up, potential energy and kinetic energy were represented as two different aspects of physical work, each of them able to transform into the other. This transformation appeared to Lodge tightly linked to the process of transfer of energy. This is the keystone of his specific model: the transfer of energy required the transformation of energy from potential to kinetic or, conversely, from kinetic to potential.

> "An important thing is now evident moreover, a thing which I have never seen accepted, though it has been previously pointed out [in previous Lodge's papers]. This statement is in two parts: (1) *Energy cannot be transferred without being transformed*, and (2) *it always transforms itself from Kinetic to Potential, or vice versa*.
>
> When A does work on B energy is transferred from A to B; and I say that if the energy which A lost is kinetic, then what B gains is potential; if, on the other hand, A loses potential, then B gains kinetic.
>
> I may make a converse statement, viz. that *energy cannot be transformed without being transferred*; cannot take on a different form without being at the same time shifted to a different body."[7]

[6] Lodge O. 1885, pp. 484-5.
[7] Lodge O. 1885, pp. 485-6.

The transformation of energy from potential to kinetic should not take place in the body but in the surrounding medium, even in the case of falling bodies. In this case, potential energy would spread throughout the medium and would be transferred from the medium to the body: during its transfer, energy undergoes the transformation from potential to kinetic.[8] In the case of a body thrown upwards, the body would transfer its kinetic energy to the "gravitation medium" and the medium would receive it in the form of potential energy, until the body has reached the highest point. Immediately the medium begins to give back its potential energy to the body, which increases its kinetic energy. This theoretical model could be applied, for instance, to the pendulum: energy is transferred from the swinging matter to the medium and conversely from the latter to the former. Even in the case of a body moving through a medium and giving rise to frictional phenomena, there would be a transfer of energy accompanied by a process of transformation: the heat produced by friction would consist of internal vibrations both in the body and in the medium. In their turn, those vibrations were nothing else but a sequence of continuous transformations of energy from kinetic to potential and so on. Lodge concluded that "change of form is necessary and universal whenever energy is transferred, i.e. whenever any kind of activity is exhibited by any known kind of material existence".[9]

Starting from the electromagnetic theory, he tried to apply his theoretical sketch to all fields of physics. The universality of the theory relied upon the universality of the medium: space was everywhere filled with that medium, and matter was embedded in it. That general model had already been outlined by Lodge in a paper published in two parts in *Nature* two years before. According to Lodge, the hypothesis of a universal medium was intrinsically linked to the hypothesis of contiguous action. He imagined a medium "continuous, not molecular" in its structure, in particular "a continuous frictionless medium possessing inertia" and able "to act as the transmitter of motion and of energy". In order to support the propagation of light, this medium, or aether, "must have properties which, if it were ordinary matter, we should style *inertia* and *rigidity*".[10] The second part of the paper is the most interesting, for in it Lodge put forward a conceptual shift from a medium considered as a simple carrier of energy to a medium endowed with the powerful property of being the matrix of matter and

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[8] Lodge O. 1885, p. 486.

[9] Lodge O. 1885, pp. 486-7. It is worth mentioning that, in 1879, Lodge had published a paper, "An Attempt at a Systematic Classification of the Various Forms of Energy" in the *Philosophical Magazine*, wherein he allotted three kinds of energy to five kinds of matter. For an analysis of the paper, see Smith C. 1998, pp. 291-93.

[10] Lodge O. 1883, pp. 305-6.

electricity. He suggested that "positive and negative electricity together may make up the ether", or that a single aether "may be sheared by electromotive forces into positive and negative electricity"[11]. In other words, electricity was imagined as a condition of polarisation of aether. Even ordinary matter could be imagined as a structure of aether: according to "Sir William Thomson's theory of matter", atoms of matter could be looked upon as vortex rings in a universal fluid. At the same time, this fluid aether had to possess that rigidity which would have enabled it to support transverse vibrations of light. He acknowledged that "rigidity was precisely what no fluid possessed": in other words, fluids cannot support transverse vibration. Nevertheless, if the fluid is "at rest this is true; in motion it is not true": the required "elasticity of a solid may be accounted for by the motion of the fluid; that a fluid in motion may possess rigidity". In this way, he thought he had solved the difficulty involved in satisfying two opposite requirements: a fluid aether, in order to account for the *birth* of matter, and a solid aether, in order to account for the transfer of radiant energy. Without making any resort to a "transition of substance", particles of matter became a dynamical structure of aether; they did not have to be considered as "foreign particles imbedded in the all pervading ether". Unfortunately, that general conceptual framework could not account for gravitation: Lodge himself avowed that "before the theory can be accepted, I think it must account for gravitation".[12] He was aware of such an important flaw, and acknowledged that, at that moment, the "Thomsonian theory of matter is not a verified one". Nevertheless, he hoped that a general theory of that kind could account for matter, electricity and propagation of light, and even for the transmission of every kind of contiguous action. In spite of some cautious remarks spread throughout the paper, the last passages sound quite optimistic and rhetoric.

> "I have now endeavoured to introduce you to the simplest conception of the material universe which has yet occurred to man. [...]
> One continuous substance filling all space: which can vibrate as light; which can be sheared into positive and negative electricity; which in whirls constitutes matter; and which transmits by continuity, and not by impact, every action and reaction of which matter is capable. This is the modern view of the ether and its functions."[13]

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[11] Lodge O. 1883, p. 328.
[12] Lodge O. 1883, p. 329.
[13] Lodge O. 1883, p. 330.

This attempt at a great unification was neither detailed nor fully satisfactory from the physical point of view. If some dynamical properties of aether, giving rise to vortex atoms, could account for the elasticity and rigidity of matter, how could the surrounding aether become rigid and elastic, in order to transfer transverse waves? How could the mechanism of "shearing", imagined by Lodge, actually operate, in order to split aether in its opposite electric components? The model was less convincing than the previous quotation leads us to imagine; nevertheless, it allowed Lodge to outline a unified qualitative explanation for both the properties of matter and the transfer of energy. As I have already shown, general sketches of the same kind where not uncommon in the community of British *mathematicians* and *natural philosophers*.

In the same year (1883), J.J. Thomson published the already quoted *A Treatise on the Motion of Vortex Rings*. In the first passages of the "Preface", he specified that, "in addition to the set subject, … I have endeavoured to apply some of the results to the vortex atom theory of matter". In particular, these additions were placed in the fourth part of the book, "which treats of the vortex atom theory of chemical action".[14] He was aware that his mathematical model could not explain "what matter is": the query about the nature and the existence of matter could simply be transformed into the query about "the existence of a fluid possessing inertia". The model could only "explain by means of the laws of Hydrodynamics all the properties of bodies as consequence of the motion of this fluid". When applied to the kinetic theory of gases, it explained the interaction among atoms on a pure kinematical basis, avoiding that "clash of atoms" based on "forces which themselves demand a theory to explain them".[15]

After two years, in the same year wherein Poynting published his second paper on the transfer of electromagnetic energy, J.J. Thomson became Professor of Experimental Physics at *Cavendish Laboratory*, holding the chair previously held by Maxwell and then by W. Strutt (Lord Rayleigh). In 1885, J.J. Thomson published a "Report on Electrical Theories" in the *British Association for the Advancement of Science* review. In the "Appendix Ist" to this paper, he wrote an appraisal of Poynting's already published (1884) paper and the 1885 paper, which he had kindly received from the author himself, before the publication in *Philosophical Transaction*. Thomson did not qualify Poynting's theory as a real new theory but as "a new way of looking at Faraday and Maxwell's theory": he claimed that the main feature of

[14] Thomson J.J. 1883, p. v.
[15] Thomson J.J. 1883, pp. 1-2.

Poynting's specific model consisted in bringing "the action of the dielectric into great prominence".[16] Thomson's criticism focused on the *indeterminateness* of the energy flux; he thought that it was a consequence of the scant knowledge about "the mechanism which produces the phenomena which occur in the electromagnetic field".[17]

Thomson pointed out that we can measure nothing more than a net amount of energy: in other words, we can measure only differences of energy. The mathematical aspect of this indeterminateness involved the surface-integral which expressed the increase of the energy inside any closed surface. Poynting had written the integral as

$$\frac{1}{4\pi}\iint\left[l\left(\beta R'-\gamma Q'\right)+m\left(\gamma P'-\alpha R'\right)+n\left(\alpha Q'-\beta P'\right)\right]dS\ ,$$

where (α, β, γ) are the components of a magnetic force and (*P*', *Q*'. *R*') are the components of an electric force. We have already seen that, in a more modern and compact notation (which Heaviside had recently introduced), this integral can be written as

$$\frac{1}{4\pi}\iint\left[H\times \mathbf{E'}\right]dS\ .$$

Thomson defined a vector ***K*** = (*u*, *v*, *w*), which consisted of the *curl* of a second vector ***X***, namely ***K*** = *curl* (***X***). The indeterminateness consisted in the fact that the addition of this term to Poynting's integral does not modify it. The transformation of the surface-integral into a volume-integral clarifies the whole mathematical process. If

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[16] Thomson J.J. 1885, p. 150. Four years younger than Poynting, J.J. Thomson had attended the same college in Manchester; subsequently they studied at Trinity College. Starting from the end of the nineteenth century, they published some physics textbooks as co-authors.

[17] Thomson J.J. 1885, p. 151: "The problem of finding the way in which the energy is transmitted in a system whose mechanism is unknown seems to be an indeterminate one; thus, for example, if the energy inside a closed surface remains constant we cannot unless we know the mechanism of the system tell whether this is because there is no flow of energy either into or out of the surface, or because as much flows in as flows out. The reason for this difference between what we should expect and the result obtained in this paper is not far to seek."

$$\iint [H \times E'] dS = \iiint div [H \times E'] dS \ ,$$

and we add the new term K = *curl* (X) into the integral, we have *div [curl (X)] = 0*. The integral representing the energy flux is insensitive to the addition of a vector of the kind K: this is the indeterminateness which Thomson pointed out.

In addition to this mathematical-physical appraisal, Thomson expressed another kind of criticism, which involved Poynting's interpretation of magnetism. In Poynting's theoretical model, the magnetic force stemming from electric currents was an effect of the transfer of energy. The transfer of electromagnetic energy from the medium to a conducting wire entailed a transformation of energy: part of this energy transformed into the magnetic field linked to the electric current. It was, Thomson remarked, as if "there must be transference of energy from one part of the field to another to give rise to magnetic force".[18] This link between magnetic force and energy transfer appeared to Thomson unsatisfactory and not consistent. He analysed two phenomena, in order to support his criticism: one of them was the well-known case of the condenser.

> "Thus, according to his view, no magnetic force would be exerted by the discharge of a leaky condenser, because in this case he considers the energy to be confined to the space between the plates of the condenser and to be converted into heat where it stands. If the plates were connected by a metallic wire, the energy could flow out and be converted into heat in the wire and this motion of energy would give rise to magnetic forces, so that magnetic forces would be produced by the discharge of a condenser in this way, but not by leakage. In this case the theory differs from Maxwell's, as according to that theory the alteration in the electromotive force would produce magnetic forces in either case."[19]

In other words, Thomson criticised the fact that only the transfer of energy to the conducting wire could give rise to a magnetic field linked to the current of conduction, whereas the transfer of energy through the dielectric placed between the plates could not give rise to a similar field. Thomson's remark involved the core of Maxwell's theory, for Maxwell had assumed a sort of symmetry between electric currents in conductors and

[18] Thomson J.J. 1885, p. 152.
[19] Thomson J.J. 1885, pp. 152-3.

electric currents in dielectrics. The asymmetry above pointed out, involving the different behaviour of energy in dielectrics and conductors, was actually alien to Maxwell's conception.

Thomson's criticism, in particular the mathematical indeterminateness of the energy flux, was widely developed in a thick paper published after some years in *Philosophical Transactions.* The paper, "On the Mathematical Theory of Electromagnetism", was written and revised between 1891 and 1892 by A. McAulay, a scholar of Ormond College, Melbourne.[20] The author devoted many pages to the transfer of electromagnetic energy. The first pages deal with aether, matter and the electric *displacement*: starting from these fundamental entities, the author developed his theoretical model. He claimed that space is filled with a "medium of some sort, which is intimately related to matter, and certainly affected in some way by the motion of matter". Indeed, even aether could be considered as a peculiar kind of matter, endowed with its specific properties. In this representation, the medium appeared "merely as matter with zero density, but other physical quantities not zero". Electric polarisation, which he identified with electric displacement, was "a property that is carried about by the medium experiencing it".[21]

McAulay stated that he shared Maxwell's theoretical conception of electrification and electric current. In his model, expressed by a heavy mathematical notation, space was filled with an incompressible fluid. Inside dielectrics the fluid had the structure of cells, reacting elastically to electric forces, which try to displace them. Inside conductors the fluid had not an ordered structure: it did not react elastically but yielded to the force and offered only a frictional resistance to motion. In both cases we are dealing with a sort of "original" fluid, spread throughout matter and aether in standard conditions. Now, McAulay suggested, let us imagine pouring some more fluid in a certain volume: this "foreign" fluid would be what we usually call "electric charge". Provided that the fluid is incompressible, electric charge is nothing more than "the surface integral over the boundary of the space considered of the original liquid outwards".[22] This sounds only in part consistent with Maxwell's theory. According to Maxwell, electric displacement was a sort of flux of an incompressible fluid put in a state of strain from electric forces, but electric charge was not the excess of fluid:

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[20] I would like to briefly discuss this less-known paper, in order to cast light on the different aspects of the debate on the electromagnetic energy, in English speaking countries. However, McAulay's paper was also read on the Continent: it was quoted, for instance, by G. Mie. See Mie G. 1898, p. 8.

[21] McAulay A. 1892, p. 685.

[22] McAulay A. 1892, p. 694.

it seems rather the excess of tension, or potential energy, in the original fluid. In McAulay's paper, the distinction between "original" fluid and "foreign" fluid was associated to the difference between conduction currents and displacement currents.[23]

A "simple conductor" was, for McAulay, a real conductor not experiencing a perfect conduction and sharing the behaviour of both pure conductors and pure dielectrics. From this point of view, the distinction made by McAulay between two "displacements", the "dielectric displacement" ***d*** and the "conduction displacement" ***k***, does not appear strange. The "whole displacement" ***D*** would be the sum of the two, and the "whole current" would be

$$\boldsymbol{C} = \dot{\boldsymbol{D}} = \dot{\boldsymbol{d}} + \dot{\boldsymbol{k}}$$ [24]

This stress on *displacement* rather than on energy put McAulay in contrast with Poynting, who had undertaken the opposite step, disregarding displacement and focusing on energy stored in the medium and travelling throughout the medium. Thus we are not astonished by McAulay's statement, "I disagree entirely with Professor POYNTING'S interpretation of his own results". He thought he had found a different and "simpler flux of energy" accounting for "the changes of intrinsic energy in different parts of the fluid". Indeed, McAulay's criticism concerned the core of Poynting's theory: a flux of energy travelling perpendicularly to electric currents.

> "In particular, this interpretation would restore credence in what Professor POYNTING considers he has shown to be a false view, viz., that among other aspects of a current of electricity it may be looked upon as something conveying energy along the conductor. This part of the subject, although deduced from the present theory, is shown to be true on Professor POYNTING'S own premises."[25]

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[23] McAulay A. 1892, p. 695: "The 'conduction' current is measured by the current of *foreign* liquid, and the 'displacement' current (indicated in the present paper by the term 'dielectric' current) by that of the original liquid. In a simple conductor there is nothing to distinguish foreign from original liquid, and the conduction current in this case is represented by the whole liquid current."
[24] McAulay A. 1892, p. 699.
[25] McAulay A. 1892, p. 698.

The thesis was widely developed in the corresponding section "The Transference of Energy through the Field", where McAulay was "led to the necessity of finding the time flux of intrinsic energy in general". After having translated Poynting's formula (***P***) in his heavy notation, he compared it with his own formula (***L***). The difference between the two vectors was *curl* $\left(\varphi \boldsymbol{H}\right)$, where φ was the electric potential and ***H*** the magnetic field.[26] This does not sound strange because J.J. Thomson had already cast light on this query. He had spoken of a sort of "indeterminateness" in the definition of the energy flux: adding the *curl* of a certain vector, the divergence of the flux would result unchanged. McAulay was aware of this contribution and quoted a long passage of Thomson's 1885 *Report*.[27]

This mathematical equivalence between ***L*** and ***P*** was quite meaningful from the point of view of theoretical physics. In Poynting's theory, the energy flux was perpendicular to the electric currents: this means that the electromagnetic energy was not conveyed by electric currents crossing conductors. On the contrary, in McAulay's theory, there was a contribution to the energy flux in the direction of electric currents. The disagreement was not without importance in the interpretation of electromagnetic actions.

> "Now, if we take **L** as the true time flux of energy, we see that one way in which we *must* regard a current is precisely the way professor

[26] We can write both expressions as $\boldsymbol{L}=\boldsymbol{X}+v\boldsymbol{C}$ *and* $\boldsymbol{P}=\boldsymbol{X}-V\nabla v\boldsymbol{H}/4\pi$, where ***L*** represents McAulay flux, ***P*** represents Poynting flux, and ***X*** represents the set of terms common to both equations. In the right side of McAulay's flux, ***v*** represents the scalar potential and ***C*** the total current; in Poynting's flux, the term $V\nabla v\boldsymbol{H}$ is nothing else but the vector product $\left(\nabla v\right)\times \boldsymbol{H}$. McAulay took into account the difference between L and P, namely $4\pi\left(\boldsymbol{L}-\boldsymbol{P}\right)=4\pi v\boldsymbol{C}+V\nabla v\boldsymbol{H}=v(V\nabla\boldsymbol{H})+V\nabla v\boldsymbol{H}=V\nabla(v\boldsymbol{H})$, where Ampère's law $4\pi\boldsymbol{C}=\nabla\times\boldsymbol{H}$ was used. The last passages can be translated in the more compact modern notation (the scalar potential is now φ) as $\varphi\nabla\times\boldsymbol{H}+\boldsymbol{H}\nabla\times\varphi=\nabla\times\left(\varphi\boldsymbol{H}\right)$, showing that the difference between the two fluxes is the curl of the vector φ ***H***. See McAulay A. 1892, p. 770.

[27] See McAulay A. 1892, p. 772: "He then goes on to point out* how, so far from **P** being necessarily the time flux of energy, $P+V\nabla\varepsilon$, where ε is any vector, such that at surfaces of discontinuity $\left[VU\nu\varepsilon\right]_{a+b}=0$, might equally well be taken as the time flux of energy. It so happens that (assuming *v* continuous), **L** - **P** is such a vector, so that the difference between the results arrived at in this paper and Professor POYNTING'S is just such a case as Professor THOMSON warned us to expect. We cannot then say that either **L** or **P** is the time flux of energy, but only that if we assume either the one or the other (…) to be the flux, the real changes of intrinsic energy will be accounted for."

> POYNTING denies us, namely 'as something conveying energy along the conductor'. In fact, from the term $v\mathbf{C}$ in **L**, we see that in this respect, as in so many others, a current and the potential are the exact analogue of a liquid current and its pressure. Without doubt, the view that **L** is the true flux is simpler for steady fields than the view that **P** is. This statement is not so obvious – perhaps on the whole not true – for varying fields."[28]

Beyond the mathematical machinery made of vectors, *curls* and *divergences*, there was a sharp theoretical difference between Poynting and McAulay. According to Poynting, the pivotal entity in electromagnetic phenomena was energy and basic processes were its storage in the medium, its transfer through the medium and its transformations. Electric currents were nothing more than an outward effect, a consequence of the transformation of energy. According to McAulay, the two "displacements" and the two corresponding currents were as fundamental as energy. From the point of view of mathematical physics the two theories were, at least to a certain extent, equivalent; from the point of view of theoretical physics, the two theories were quite different. This shows how the debate on energy, in British scientific journals, was rich and branched, as well as how complex the relationship between mathematical physics and theoretical physics really was.

[28] McAulay A. 1892, p. 772.

10. Looking for Maxwell's true *theory*

In the last two decades of the nineteenth century, the theoretical debate on matter and energy, in the context of British electromagnetic theories, had in O. Heaviside one of its chief characters. He wrote many papers for *The Electrician*, a journal devoted more to engineering than to science. He never graduated and never held an academic position inside the British scientific community: he achieved a deep and detailed competence in electromagnetism by personal study and experience.[1] He kept a close scientific relationship with many scientists: W. Thomson, Lodge, FitzGerald, Larmor and even Hertz. In physics textbooks we meet *Maxwell's four equations* but these equations are not Maxwell's. They are Heaviside's: he was the first to write the equations for the electromagnetic fields in that vector form nowadays well-known to physicists, but quite unusual for physicists in the late nineteenth century.[2] He collected his papers in two volumes in 1892, under the title *Electrical Papers*, and subsequently other papers in three volumes, under the title *Electromagnetic theory*. The first of the three was published in 1893: it contains papers written in the years between 1891 and 1893.

In the Preface of this first volume, some short passages summarise the subject matter and the theoretical point of view of the author. He stated that he shared the "Faraday-Maxwell point of view, with some small modifications", following the "idea of lines and tubes of force".[3] Furthermore, he pointed out that his theoretical approach grew around two pivotal issues: a formal symmetry between electricity and magnetism and the primacy of "field" quantities with regard to potentials.

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[1] Buchwald introduced Heaviside with the following adjectives: "self-educated, eccentric, wilful, isolated, suspicious and brilliant". (Buchwald J.Z. 1885b, p. 288) See also p. 324: "Heaviside was perhaps the last autodidact to have a significant impact on the development of physics. He was eccentric to an almost absurd degree, but British Victorians, despite their present reputation for stodginess, were often more willing than censorious moderns to tolerate an original but unconventional mind."

[2] He claimed that he was putting forward "some small modifications and extensions upon Maxwell's equations"; he stressed the "unsuitability of quaternions and his "preference for a vector algebra". (Heaviside O. 1893, pp. iii-iv) In 1889, he had deduced a mathematical law for the compression (nowadays deduced by Einstein's Theory of Relativity) of the radial electric field stemming from an electric charge in rectilinear uniform motion. See Heaviside O. 1889a, p. 332: "As the speed increases, the electromagnetic field concentrates itself more and more about the equatorial plane, …". This result had already been published in 1888, in *The Electrician*.

[3] Heaviside O. 1893, p. iii.

> "[The theory] is also done in the duplex form I introduced in 1885, whereby the electric and magnetic sides of electromagnetism are symmetrically exhibited and connected, whilst the 'forces and 'fluxes' are the objects of immediate attention, instead of the potential functions which are such powerful aids to obscuring and complicating the subject, and hiding from view useful and important relations."[4]

Heaviside crowned Maxwell as the first scientist to have put forward a general consistent electromagnetic theory. After a short history of theories of electricity and magnetism in the nineteenth century, he expressed the greatest appreciation for Maxwell's work, making use of words which sound quite surprising when coming from a man described by scholars and biographers as not having talent either for diplomacy or for compliancy. According to Heaviside's historical reconstruction, Maxwell was the champion of contiguous action applied to electromagnetic phenomena. He had collected several scattered theoretical fragments and had given them an inner consistency, dismissing the unreliable attractions and repulsions. Starting from different kinds of *bricks* he had offered a theoretical *house* to electromagnetic phenomena, keeping the spectre of action at a distance apart.

> "There was then a collection of detached theories, but loosely connected, and embedded in a heap of unnecessary hypotheses, scientifically valueless, and entirely opposed to the spirit of Faraday's way of thinking, and, in fact, to the spirit of the time. […] the physics of the subject required to be rationalised, the supposed mutual attractions or repulsions of electricity, or of magnetism, or of elements of electric currents upon one another, abolished, and the electromagnetic effects accounted for by continuous actions through a medium, propagated in time. All this, and much more, was done. The crowning achievement was reserved

[4] Heaviside O. 1893, p. iv. In the context of British electromagnetic theories, in particular Maxwell and Heaviside's theories, *forces* and *fluxes* had different meanings. In Maxwell's *Treatise*, ***E*** was a force and ***D*** was a flux. This is consistent with Maxwell's representation of electric-elastic actions taking place in an elastic medium, involving an inducing action ***E*** and the corresponding elastic answer ***D***. On the couple *force/flux*, or *intensity/quantity*, see, for instance, Darrigol O. 2000, pp. 144-7 and 257-8. In the course of the twentieth century, after subsequent re-interpretations of Maxwell's theory, the difference between ***E*** and ***D*** has become less and less meaningful.

> for the heavensent Maxwell, a man whose fame, great as it is now, has, comparatively speaking, yet to come."[5]

Heaviside considered himself as a *Maxwellian* in a deep and very sophisticated sense: he thought he would have been able to develop some ideas outlined but not accomplished by Maxwell. In this sense he pursued the aim of making Maxwell's theory become truly *Maxwellian*. He had to perform a very demanding task: being more *Maxwellian* than Maxwell had been.[6] The theory of his master contained seeds which he had not been able to completely develop: Heaviside thought of himself as being the gardener who would have managed to grow the tree. He thought that Maxwell's theory could become clear and fully consistent only when freed from the different possible interpretations it had suffered from: only one interpretation was consistent with Maxwell's theoretical view. In order to pursue this improvement, he was ready to cross the fuzzy borderline between loyalty to his master's texts and the inner consistency of the theory.[7]

At this point, some specifications may be useful, when referring to the community of British physicists who took part in the enlargement and transformation of Maxwell's heritage, the so-called community of *Maxwellians*. Some years ago, B. Hunt put FitzGerald, Heaviside, Lodge and Larmor in that community, namely the scientists who were more committed to devising mechanical models of aether. Why were Poynting and J.J. Thomson less *Maxwellians* than the others? In reality, they were less interested in aether machinery than the others. Nevertheless, if that was the reason, it would mean that Maxwell has been definitely associated only to those mechanical models which he had widely explored but subsequently abandoned. It seems to me that the community of *Maxwellians* should be

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[5] Heaviside O. 1893, p. 14.

[6] Both Heaviside and Fitzgerald distinguished "between Maxwell's *Treatise* and Maxwell's theory": see Hunt B.J. 1991, pp. 201-2.

[7] See Heaviside O. 1893, p. vii: "[Maxwell's theory] may be, and has been, differently interpreted by different men, which is a sign that it is not set forth in a perfectly clear and unmistakeable form. There are many obscurities and some inconsistencies. Speaking for myself, it was only by changing its form of presentation that I was able to see it clearly, and so as to avoid the inconsistencies. Now there is no finality in a growing science. It is therefore impossible to adhere strictly to Maxwell's theory as he gave it to the world, if only on account of its inconvenient form. But it is clearly not admissible to make arbitrary changes in it and still call it his. He might have repudiated them utterly. But if we have good reason to believe that the theory as stated in his treatise does require modification to make it self-consistent, and to believe that he would have admitted the necessity of the change when pointed out to him, then I think the resulting modified theory may well be called Maxwell's."

further enlarged, in order to include those who, like Poynting and J.J. Thomson, though having started from Maxwell' theory, subsequently preferred to rely upon Faraday's tubes of force. Heaviside's sharp dismissal of potentials, for instance, was no less an injury to Maxwell's tradition than J.J. Thomson's discrete models for matter and fields, or Larmor's model of a subatomic particle as a knob of kinetic energy in the aether. In any case, all of them, Poynting, Lodge, Heaviside, Larmor and J.J. Thomson, claimed to be a *Maxwellian*, even though those claims did not prevent them from pushing, straining and twisting Maxwell's theory, in order to achieve what they imagined to be a better representation of electromagnetic phenomena. Although in contrast with Buchwald's previous (1985) classification, Hunt shared with Buchwald two criteria of identification for such a community: to belong or not to the "Cambridge school", and to make use or not of "Lagrangian methods".[8] I think that neither Lagrangian methods nor the Cambridge school can in any way establish the borderline between *Maxwellians* and *non-Maxwellians*. I prefer to take into account all physicists who in some way started from Maxwell's theory rather than defining a community of *pure Maxwellians*. Finally, I think that we should dare to *cross the boundaries* we have contributed to set up. As recently pointed out by Buchwald and Hong, in whatever way the *Maxwellians* may be identified, it is noteworthy that meaningful theoretical developments of Maxwell's theory were carried out "by people with such utterly different backgrounds and training as John Henry Poynting and Oliver Heaviside".[9]

In the first volume of his *Electromagnetic Theory*, more specifically in the first section of the fourth chapter, written in December 1892, Heaviside focused on general issues. The first lines show Heaviside wearing the suit of the natural philosopher and claiming the universal role of motion and

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[8] See Buchwald J.Z. 1985c, p. 227. See Hunt B.J. 1991, p. 202, note 92: "This characterization reflects an important difference in how Buchwald and I use the term *Maxwellian*: where Buchwald identifies it with the Cambridge school and J.J. Thomson's Lagrangian approach, I contend that the main line of Maxwellian development lay outside Cambridge and centered on Heaviside, Fitzgerald, Lodge and Hertz, all of whom moved away from the Lagrangian methods Maxwell himself used." However Hunt, in a subsequent page (p. 207), acknowledged the existence of problems emerging from his interpretation.

[9] Buchwald J.Z. and Hong S. 2003, p. 180. As Warwick recently suggested, we should distinguish between "the first generation of Cambridge Maxwellians", namely W.D. Niven, C. Niven and H. Lamb, and the second generation, whom Hunt referred to. Warwick remarked that the first generation of *Maxwellians* was not interested in electromagnetic waves or in the queries arising from the concept of *electric displacement*. See Warwick A. 2003, pp. 325 and 329-32. I interpret the approach of the first generation as an instance of mathematical physics, and the approach of the second generation as an instance of theoretical physics.

transfer of motion. In the universe, he claimed, "Nothing is still". Motion, he added, "once produced, … is diffused or otherwise transferred to other matter". Moreover, motion appeared as the main feature of every kind of process, "observed in the moral and intellectual worlds as in the material". Confining himself to the physical world, he concluded that "an important subject of study by physicists" was placed in those laws "by which motions, or phenomena which ultimately depend upon motion, are transferred".[10]

According to Heaviside, the "two extreme main views" on the transfer of physical actions were, on the one hand, "the theory of instantaneous action at a distance between different bodies without any intervening medium" and, on the other hand, "the theory of propagation in time through and by means of an intervening medium". Although the two theoretical model were alternative, Heaviside thought that they "may be somewhat harmonised" imagining a sort of limiting case, when the velocity of actions become infinite. Besides this physical approach to the subject, there was a mathematical "way of regarding the matter", consisting in the mathematical equivalence between the laws explaining phenomena in both representations. According to Heaviside, the mathematical equivalence and the fact that the velocity of electromagnetic interactions is exceedingly higher than ordinary velocities, could explain why the attention of scholars had first been drawn towards actions at a distance.[11]

Although Heaviside considered himself a champion of contiguous action and a defender of Maxwell's *true* natural philosophy, he expressed balanced meta-theoretical considerations on the complex relationship between theories and experiments. If the "old view persisted (…) in spite of the large amount of evidence in support of the view that some medium (…) was essentially concerned in the electrical phenomena", it should not necessarily be interpreted as a theoretical mistake. It was a different theory, a different interpretation; he acknowledged that "value and validity of evidence varies according to the state of mind of the judge". At the same time, his first commitment was still the development of a theory of contiguous action: his theoretical preference had recently been reinforced by Hertz's experiments showing "that electromagnetic waves are propagated outside conductors". The existence and necessity of a medium carrying electromagnetic waves seemed to him as evident as the existence of air for the transmission of sound. The battle to make contiguous actions prevail in

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[10] Heaviside O. 1893, p. 306.

[11] Heaviside O. 1893, p. 308.

electromagnetic theories corresponded to the battle "to propagate a knowledge of the theory of electromagnetic waves".[12]

Heaviside's path to Maxwell's theory entailed the dismissal of electric and magnetic potentials, which he looked upon as old-fashioned tools. In Heaviside's view, they belonged to the archaeology of physics, which was identified with action-at-a-distance models. In reality, Maxwell thought that potentials, as one of "the most fertile methods of research discovered by mathematicians", could be better expressed "in terms of ideas derived from Faraday", rather than "in their original form". He found that potentials suited contiguous action as well as at-a-distance action. Because of that theoretical pliability, in the Preface to his *Treatise*, he announced that "the mathematical discoveries of Laplace, Poisson, Green and Gauss" would have found "their proper place in this treatise".[13]

Heaviside also saw other flaws in Maxwell's theory: one of them could be found in the electrodynamics of moving transparent bodies. Fresnel's formula for light travelling through water in motion was satisfactory with regard to experimental results but widely unsatisfactory with regard to the foundation of a consistent electromagnetic theory. The partial "aether drag" was a concept artificially associated to the electromagnetic theory and led to a theoretical clash between the electromagnetic theory and optics, otherwise unified correctly by Maxwell. The theoretical clash could be interpreted as a clash between mechanics and optics, because electromagnetic theory, in Heaviside's view, was nothing else but a physics of aether, a specific mechanics of a specific continuous medium. This medium, just like ordinary, material dielectrics, was taken into account only from the macroscopic point of view, without any reference to its hypothetical, microscopic structure. The old mechanics could rely on the law of composition of motions and velocities: it appeared to Heaviside that the law could be transferred,

[12] Heaviside O. 1893, pp. 308-9.

[13] See Maxwell J.C. 1881, p. xi: "The whole theory, for instance, of the potential, considered as a quantity which satisfies a certain partial differential equations, belongs essentially to the method which I have called that of Faraday. According to the other method, the potential, if it is to be considered at all, must be regarded as the result of a summation of the electrified particles divided each by its distance from a given point." This passage shows Maxwell's awareness of the difference between mathematical physics and theoretical physics. On the connection between potentials and contiguous action in Maxwell's early scientific papers, see Harman P.M. 1998, pp. 72-3. Buchwald pointed out that Heaviside's rejection of Lagrange's equations and Hamilton's principle prevented him from taking into account the electromagnetic phenomena emerged in the last decade of the century, namely the phenomena "in which the pure field equations had to be altered and new constants introduced". (Buchwald 1985b, p. 294)

without any change, from the old mechanics to that new kind of mechanics corresponding to the electromagnetic theory.[14]

He thought that, in order to explain the result of Fizeau's experiment and the more startling result of Michelson's experiment, which had shown "the absence of relative motion between the earth and surrounding ether", a theory of matter was required. The solution he shortly outlined, that motion could increase the permittivity of matter, was a macroscopic solution, which associated some properties of matter to a macroscopic constant, relinquishing a detailed microscopic explanation of the interactions between electromagnetic waves and matter.

> "… Fresnel's speculation is roughly equivalent to supposing that the molecules of transparent matter act like little condensers in increasing the permittivity, and that the matter, when in motion, only carries forward the increased permittivity."[15]

According to Heaviside, the key to the question could be found in the aether: Maxwell's theory could not help us, for it was only "the first step towards the full theory of the ether". Maxwell's theory was not complete, Heaviside noted: a satisfactory aether theory should have accounted for gravitation. In other words, he was looking for a more complete theory, which should have been nothing more than a detailed aether theory, which would have yielded an explanation for all known physical phenomena. He found that two scientists had realized some improvements: FitzGerald and Poynting. The former had explored "the nature of diverging electromagnetic waves, and how to produce them, and to calculate the loss of energy by radiation". The latter had made "an important step", namely he had displayed "the formula for the flow of energy".[16]

The two issues pointed out in the last pages of the first chapter were the role of dielectrics and the role of energy. Choosing as examples a "very long

[14] See Heaviside O. 1893, p. ix: "Maxwell's theory is a theory of propagation through a simple medium. Fundamentally it is the ether, but when we pass to a solid or liquid dielectric it is still to be regarded as a simple medium in the same sense, because the only change occurring in the equations is in the value of one or both ethereal constants, the permittivity and inductivity – practically only the first. Consequently, if we find, as above, that when the medium is itself moved, its velocity is *not* superimposed upon that of the velocity of waves through the medium at rest, the true inference is that there is something wrong with the theory."

[15] Heaviside O. 1893, pp. ix-x.

[16] Heaviside O. 1893, pp. x and 5.

solenoid of fine wire" and a "very long straight round wire" supporting an electric current, he claimed that "the transfer of energy takes place transversally, not longitudinally". This fundamental statement was linked to another fundamental statement: energy flows towards the wires from the surrounding medium. The medium was the seat of energy and the keystone in the comprehension of all electromagnetic phenomena.

> "The source of energy must, therefore, first supply the dielectric surrounding the wire before the substance of the wire itself can be influenced; that is the dielectric must be the real primary agent in the electromagnetic phenomena connected with the electric current in the wire."[17]

To sum up, electromagnetic actions travel through dielectrics surrounding conducting wires, and are perpendicular to the wires. Dielectrics were the seat of primary electromagnetic actions and even the name *conductors* appeared unsuitable to Heaviside, for conductors are not able to sustain electric displacement, although they are able to steer electromagnetic waves. In simple words, we could say that, from Heaviside's point of view, there are two kinds of matter: a first-rate matter, namely dielectrics, and a second-rate matter, namely conductors.[18]

On the first page of the second chapter, "Outline of the electromagnetic connections", devoted to the foundations of the theory, the outstanding role of matter in electromagnetic actions was emphasized. First come the fields of force, then the fields act on matter and finally matter reacts to fields; different reactions correspond to different electromagnetic properties of matter.[19] In that theoretical framework, matter had properties only in

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[17] Heaviside O. 1893, p. 17.

[18] See Heaviside O. 1893, p. 18: "We learn from it that the battery or other source of energy acts upon the dielectric primarily, producing electric displacement and magnetic induction; that disturbances are propagated through the dielectric at the speed of light; that the manner of propagation is similar to that of displacements and motions in an incompressible elastic solid; that electrical conductors act, as regards the internal propagation, not as conductors but rather as obstructors, though they act as conductors in another sense, by guiding the electromagnetic waves along definite path in space, instead of allowing them to be immediately spread away to nothing by spherical enlargement at the speed of light; …."

[19] Heaviside O. 1893, p. 20: "The conception of fields of force naturally follows, with the mapping out of space by means of lines or tubes of force definitely distributed. A further and very important step is the recognition that the two vectors, electric force and magnetic force, represent, or are capable of measuring, the actual physical state of the

connection with electromagnetic fields; conversely, electromagnetic phenomena would have been nothing without a medium, either aether or matter. According to Heaviside, only the existence of a medium gave physical meaning to the relationship between forces and fluxes. Forces were the causes: they acted on matter and fluxes were the effects on matter itself. Concepts like electromagnetic actions taking place in vacuum were alien to Heaviside's view: the existence of any kind of physical effects required the presence and the reaction of matter.

> "Electric force is then to be conceived as producing or being invariably associated with a flux, the electric displacement; and similarly magnetic force as producing a second flux, the magnetic induction.
>
> If $\boldsymbol{E}$ be the electric force at any point and $\boldsymbol{D}$ the displacement, we have
>
> $\boldsymbol{D} = c\boldsymbol{E}$;
>
> And similarly, if $\boldsymbol{H}$ be the magnetic force and $\boldsymbol{B}$ the induction, then
>
> $\boldsymbol{B} = \mu \boldsymbol{H}$
>
> Here the ratios c and μ represent physical properties of the medium. The one (μ), which indicates capacity for supporting magnetic induction, is its inductivity; whilst the other, indicating the capacity for permitting electric displacement, is its permittivity (or permittancy)."[20]

According to Heaviside, matter possessed two main properties and those two properties corresponded to the relationships between forces and fluxes. Every medium (aether or matter) possessed elasticity and a sort of inertia (the reluctancy) with regard to electromagnetic actions. In particular, the first affects electric actions and the second affects magnetic actions.[21] In aether, *c* and μ were well defined and constant numbers. The presence of matter simply modified their numerical value. Permittivity was always greater in ordinary matter than in pure aether whilst inductivity could be

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medium concerned, from the electromagnetic point of view, when taken in conjunction with other quantities experimentally recognisable as properties of matter, showing that different substances are affected to different extents by the same intensity of electric or magnetic force."

[20] Heaviside O. 1893, pp. 20-21.

[21] See Heaviside O. 1893, p. 21: "Otherwise we may write $\boldsymbol{E} = c^{-1}\boldsymbol{D}$ $\boldsymbol{H} = \mu^{-1}\boldsymbol{B}$; and now the ratio c^{-1} is the elasticity and μ^{-1} is the reluctivity (or reluctancy)."

smaller than in pure aether (diamagnetic matter), greater (paramagnetic matter) or much bigger (permanent magnets). Forces and fluxes, together with permittivity and inductivity defined energy, in accordance with the already known Maxwell formulas $U = \frac{1}{2}\,\boldsymbol{ED} = \frac{1}{2}\,c\boldsymbol{E}^2$ and $T = \frac{1}{2}\,\boldsymbol{HB} = \frac{1}{2}\,\mu\,\boldsymbol{H}^2$. U was considered as potential energy and T as kinetic energy: both energies had to be stored in the medium.

In the case of isotropic matter, if we call c_0 and μ_0 permittivity and inductivity of aether, the ratios c/c_0 and μ/μ_0 are called the *specific* permittivity and inductivity of matter: they are pure numbers. We could even choose a unit of measure such as $c_0 = 1$ and $\mu_0 = 1$; this was Hertz's choice.[22] From the mathematical point of view there are no problems but from the point of view of theoretical physics this choice could put Heaviside's theory seriously in danger. In his theory, c_0, μ_0, c and μ represented actual physical properties of aether and matter: the choice $c_0 = 1$ and $\mu_0 = 1$ hid the differences between forces and fluxes, for instance the crucial difference between electric (inducing) force and electric (induced) displacement. The choice $c_0 = 1$ led to $\boldsymbol{D} = \boldsymbol{E}$ in pure aether, namely an identity between the action of the field and the reaction of the medium. In that case, a sharp severance between mathematical physics and theoretical physics emerged.

> "I do not see how it is possible for any medium to have less than two physical properties effective in the propagation of waves. If this be admitted, I think it may also be admitted to be desirable to explicitly admit their existence and symbolise them (not as mere numerics, but as physical magnitudes in a wider sense), although their precise interpretation may long remain unknown."[23]

Heaviside was aware that a detailed physical explanation for both permittivity and inductivity was still missing. Nevertheless, a provisional interpretation could be given and a more detailed correspondence be established. Heaviside suggested that $\boldsymbol{H}$ corresponded to medium velocity, μ to its density, $\boldsymbol{E}$ to a torque and c^{-1} to a coefficient of elasticity, rigidity

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[22] This is a very important issue. See Hertz H. 1890, in Hertz H. 1962, p. 200: "… the specific inductive capacity and the magnetic permeability are not intrinsic constant of a substance. There is nothing wrong in saying that these constants are equal to unity for the ether: but this not state any fact derived from experience; it is only an arbitrary stipulation on our part." In December 1893, in a letter, Heaviside criticized Hertz's choice and interpretation. See Hunt B.J. 1991, p. 200.

[23] Heaviside O. 1893, pp. 23-24.

or quasi-rigidity. In that theoretical framework, $\frac{1}{2}\mu\boldsymbol{H}^2$ corresponded to kinetic energy and $\frac{1}{2}c\boldsymbol{E}^2$ corresponded to the energy of the strain[24].

As energy can be stored but also dissipated, the model was expected to offer something corresponding to dissipation. Electric conductors were the suitable candidates: through them electric energy was continuously dissipated into heat, the well-known Joule effect. In the case of metallic conduction, we have an electric current $\boldsymbol{C} = k\boldsymbol{E}$ and a (Joule) waste of energy $Q = k\boldsymbol{E}^2 = \boldsymbol{EC}$. There was a new constant k, the electric conductivity, whose reciprocal was the resistivity appearing in Ohm's law. In general, it depended on temperature and it was not a simple number but a linear operator. The physical dimension of Q was that of a power, the time derivative of energy, named "activity" by Heaviside.[25]

Heaviside thought that fluxes had to be considered as the main physical entities; electric charge and electric current had to be considered as derived entities. In the same way, the mathematical link between electric currents and magnetic induction could be taken into account starting from the sources (electric currents) to arrive at the fluxes (the magnetic induction $\boldsymbol{B}$), or starting from the fluxes to arrive at the sources. From the point of view of mathematical physics, the two conceptions were equivalent. It was not so from the point of view of theoretical physics.[26]

Heaviside shared Poynting's conception of electric currents as a consequence of the flow of energy from the surrounding medium towards the conducting wire. He knew that the whole scientific community did not share that view, and statements like "velocity of electricity in wires" were still widely used. He considered himself as a *true* Maxwellian, for he thought that Maxwell had to be emended or purified from the sin of having taken seriously into account the possibility of expressing electromagnetic actions in terms of charges, currents and potentials. The two pillars of a *true* Maxwellian theory had to be the medium, matter or aether, and the fields of

[24] See Heaviside O. 1893, p. 24.

[25] See Heaviside O. 1893, pp. 24-25. He was interested in generalising his expression for energy or activity, and that generalisation dealt with the symmetry he had introduced between electric and magnetic forces. He imagined a magnetic conduction current K and a total "magnetic current" $\boldsymbol{G} = \boldsymbol{K} + d\boldsymbol{B}/dt$, corresponding to the total electric current $\boldsymbol{J} = \boldsymbol{C} + d\boldsymbol{D}/dt$. See Heaviside O. 1893, pp. 25 and 36.

[26] See Heaviside O. 1893, p. 66: "It is the two fluxes, induction and displacement (or equivalently the two forces to correspond), that are important and significant; and if we wish to know the electric current (which may be quite a useless piece of information) we may derive it readily from the magnetic force by differentiation; the simplicity of the process being in striking contrast to that of the integrations by which we may mount from current to magnetic force."

force or, equivalently, the energy spread and flowing through aether.[27] According to Heaviside, in physics we have in front of us two main entities, closely linked to each other: matter and energy. Matter without energy would give us no electromagnetic phenomena, and energy without matter would make no sense, for energy can be produced, stored and spread only inside matter and through matter.

He obviously included the principle of conservation of energy in his theory, and added to it a principle of continuity of energy: he considered the latter as "a special form" of the former.

> "If it possessed continuity in time only, it might go out of existence at one place and come into existence simultaneously at another. This is sufficient for its conservation. This view, however, does not recommend itself. The alternative is to assert continuity of existence in space also, and to enunciate the principle thus: -
>
> When energy goes from place to place, it traverses the intermediate space."[28]

Heaviside noted that the equation for the flux of energy was similar to the "equation of continuity of matter used in hydrodynamics and elsewhere"; this suggested the attribution of identity to energy, just like matter. Although this was exactly the conception his friend Lodge had already claimed in 1885, Heaviside was more cautious and pointed out two problems. The first objection could be summarised in the following way: the principle of relativity entails the relativity of velocities and therefore the relativity of kinetic energy. That principle prevented Heaviside from endowing energy "with objectivity, or thinginess, or personal identity, like matter". The second objection appeared to him even more serious: it dealt with the mysterious nature of gravitation, in particular the nature of gravitational potential energy. Differently from kinetic energy, which we can try to localise, he found it hard to localise potential energy. He accepted that even gravitational energy could be imagined as stored inside aether and that energy travelled through aether. Nevertheless, he remarked that, at present, scientists had been able to write neither a law for the storage nor a law for the propagation of gravitational energy yet.[29]

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[27] Heaviside O. 1893, p. 73.
[28] Heaviside O. 1893, pp. 73-74.
[29] Heaviside O. 1893, p. 75.

In Heaviside's theoretical framework, energy was closely linked to the unspecified "internal structure of the ether". The more reliable representation of the state of the medium, and of dynamical processes taking place in it, seemed to Heaviside W. Thomson's rotational aether. In that theoretical model, ***H*** represented the velocity of the medium, and ***E*** was a torque. The potential energy of the rotation was associated to *U*, the translational kinetic energy was associated to *T*, and the flux of energy was associated to ***E*** *x* ***H***. He acknowledged that "it is very difficult to extend this analogy to include electromagnetic phenomena more comprehensively": it was doubtful whether that dynamical model could account for all known electromagnetic phenomena.[30]

However, he did not give up looking for both a mathematical model of aether and a theory of gravitation. He undertook the first task in the "Appendix" to the second chapter of the first volume of *Electromagnetic Theory*, and the second task in an "Appendix" to the fourth (and last) chapter.

In the first Appendix, the problem tackled by Heaviside was: how can a mechanical representation of electromagnetic equations be put forward in a convincing way? Every electromagnetic equation should have been associated to its corresponding mechanical action and all the mechanical actions associated to all the electromagnetic equations should have given rise to a consistent model. The mechanical model had to act as a sort of mirror of the consistent set of electromagnetic equations. In Heaviside's theoretical framework, mechanics and electromagnetic theory were looked upon as different languages to *speak* of the physical world, rather than two different fields of physics.

> "I have shown that when impressed electric force acts it is the curl or rotation of the electric force which is to be considered as the source of the resulting disturbances. Now, on the assumption that the magnetic force is the velocity in the elastic solid, we find that the curl of the impressed electric force is represented simply by impressed mechanical force of the ordinary Newtonian type. This is very convenient."[31]

The passage seems quite foggy, particularly because of the linguistic superimposition of "electric" forces, "impressed electric" forces,

[30] Heaviside O. 1893, p. 80.

[31] Heaviside O. 1893, p. 127.

"mechanical" forces and "Newtonian" forces. We should imagine a certain vector $\boldsymbol{S}$, which was "the source of the resulting disturbances", or a sort of primitive displacement in the medium. Its time derivative was $\boldsymbol{H}$, the magnetic force, and, at the same time, its *curl* was $\boldsymbol{D}$, Maxwell's electric displacement. Mathematically, $\boldsymbol{H} = d\boldsymbol{S}/dt$ and $\boldsymbol{D} = c\boldsymbol{E} = curl\ (\boldsymbol{S})$. Putting them together, we have

$$curl\ (\boldsymbol{H}) = curl\ (d\boldsymbol{S}/dt) = d/dt\ [curl\ (\boldsymbol{S})] = d\boldsymbol{D}/dt.$$

This was one of the fundamental circuital equations of the electromagnetic theory. The so-called *Ampère's law* (only for dielectrics!) was a pure mathematical consequence of the identification of $\boldsymbol{H}$ with $d\boldsymbol{S}/dt$ and $\boldsymbol{D}$ with $curl(\boldsymbol{S})$. In addition, if $\boldsymbol{H} = d\boldsymbol{S}/dt$, then $\boldsymbol{B} = \mu \boldsymbol{H} = \mu\, d\boldsymbol{S}/dt$. When associating to μ the inertia or density of the medium, $\boldsymbol{B}$ would correspond to a mechanical quantity of motion $\boldsymbol{P} = m\boldsymbol{v}$. Following this mechanical interpretation, $d\boldsymbol{B}/dt = \mu d^2\boldsymbol{S}/dt^2$ would correspond to the Newtonian term $d\boldsymbol{P}/dt$. Therefore, the so-called Faraday's law, $-\ curl(\boldsymbol{E}) = d\boldsymbol{B}/dt$, would become nothing else but the equation of motion for the medium: $d\boldsymbol{P}/dt = \boldsymbol{F}$, where $\boldsymbol{F} = -\ curl\ (\boldsymbol{E})$.[32]

A generalisation of the mechanical model would have required a frictional resistance, both translational and rotational. From the mechanical point of view, the equation $d\boldsymbol{P}/dt = \boldsymbol{F}$ should have been generalised into $d\boldsymbol{P}/dt = \boldsymbol{F} - k\boldsymbol{v}$, or $\boldsymbol{F} = d\boldsymbol{P}/dt + k\boldsymbol{v}$. The equation $-curl\ \boldsymbol{E} = \mu \dot{\boldsymbol{H}}$ could actually be extended in order to include a dissipative term,

$$-curl\ \boldsymbol{E} = g\boldsymbol{H} + \mu\dot{\boldsymbol{H}}\ .$$

The constant g would correspond to a coefficient of "translational frictionality" multiplying the vector $\boldsymbol{H}$, which corresponded to a velocity. This appears consistent from the mechanical point of view (frictional *forces* depending on velocity) but quite mysterious from the electromagnetic point of view. When subjected to the same generalisation, the equation $curl\ (\boldsymbol{H}) = c\dot{\boldsymbol{E}}$ would become

$$curl\ (\boldsymbol{H}) = k\boldsymbol{E} + c\dot{\boldsymbol{E}}\ .$$

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[32] Heaviside O. 1893, p. 128.

In this case, the electromagnetic representation is utterly consistent, for the last equation is nothing else but *Ampère's law* for both dielectrics and conductors; the constant k corresponds to electric conductivity, the term $k\mathbf{E}$ to the conduction current and $cd\mathbf{E}/dt$ to the *displacement* current. Nevertheless Heaviside acknowledged that the meaning of k was not immediate from the mechanical point of view, and he wrote "k will be considered later". For the moment he pointed out both the "parallelism in every detail" and the symmetry between the two equations, in spite of the oddness of the term representing "the magnetic conductivity" in the first circuital equation. Further mathematical investigations undertaken in the long third chapter of the first volume of *Electromagnetic Theory* led to a similar impasse: the stumbling block of all models was represented by electric and magnetic *dissipative* terms, corresponding to conduction.[33]

That failure did not shake his trust in the model of contiguous action: he tried to apply it even to the explanation of gravitation. Indeed, in the second half of the nineteenth century, some scientists had inquired into the possibility of devising a new theory of gravitation; they followed both the conceptual model of contiguous action and the conceptual model of action at a distance. Maxwell, for instance, had not managed to cope with the puzzle of the gravitational field, which dramatically increases its energy when work is released, namely when two bodies approach. He wondered where such energy came from and how a physical theory could deal with such an amount of negative energy.[34] As M. Hesse remarked some decades ago, "[a]t the end of the nineteenth century gravitation was understood no better than in the seventeenth century". Heaviside's attempt to build up such a theory shows us, on the one hand, that there were theoretical reasons to pursue that project, and, on the other hand, that *macroscopic* Maxwellian models still had

[33] Heaviside O. 1893, pp. 128-131. Historians have already analysed Heaviside's attempts to cope with electric conduction. See, for instance, Buchwald J.Z. 1985c, p. 236.

[34] Maxwell J.C. 1865, p. 493: "An energy is essentially positive, it is impossible for any part of space to have negative intrinsic energy. […] The assumption, therefore, that gravitation arises from the action of the surrounding medium in the way pointed out, leads to the conclusion that every part of this medium possesses, when undisturbed, an enormous intrinsic energy, and that the presence of dense bodies influences the medium so as to diminish this energy wherever there is a resultant attraction. As I am unable to understand in what way a medium can possess such properties, I cannot go any further in this direction in searching for the cause of gravitation." Renn and Schemmel quoted Mossotti, Weber and Zöllner's, attempts to interpret gravity "as a residual effect of electric forces". In 1882, for instance, Zöllner tried to modify the law of gravitational force, starting from Weber's electrodynamic law and Mossotti's previous suggestions. See Renn J. and Schemmel M. 2006, in Renn J. (ed.) 2006, Vol. 3, pp. 8-9.

a remarkable heuristic power. At the same time, from the empirical point of view, nothing suggested that Newton's theory of gravitation had to be overturned or simply improved. The only query was theoretical, namely the query concerning the way of propagation of gravitational actions.[35]

In July and August 1893, Heaviside tried to include gravitation into the general theoretical model of contiguous action: that theory of gravitation appeared on the last pages of the first volume of his *Electromagnetic Theory*. The key concept was still energy, more specifically the localisation of energy and the transfer of energy. The starting point was the localisation of gravitational energy in the aether, consistently with Maxwell's electromagnetic theory. He undertook two subsequent steps: in July 1893, an outline of a *field* theory for gravitation, and, in August, a mathematical law for gravito-dynamical effects.

He was able to deduce a sort of *Ampère's law* for gravitation: the Sun in motion and the Earth in motion would interact similarly to electric currents. In the last lines of this "Appendix", which are at the same time the end of chapter four and of the first volume of his *Electromagnetic Theory*, Heaviside was quite wary: if the foreseen effect had existed it would have been small. The question of the velocity of propagation for gravitational actions was still unsolved; also "the question of the ether in its gravitational aspect" was waiting for a solution. Heaviside acknowledged that his gravitational theory had been only outlined, but he hoped that his "suggestion may not be wholly useless".[36] Although his macroscopic model of contiguous action had shown some flaws, just its application to the puzzle of gravitation showed how fruitful it could be when stretched to its limits.

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[35] Hesse M. 1961, pp. 224-5. I do not find that "there was no theoretical reason to pursue these speculations until the advent of the theory of relativity". However, Hesse reported in some detail how Faraday, and subsequently Maxwell and Hertz, were interested in gravitation. In a recent study, Renn and Schemmel singled out three alternatives to "Newtonian" model of gravitation: the "gas model", dealing with gradients of densities and pressures through aether, the "umbrella model", dealing with fluxes of hypothetical cosmic particles, and "Lorentz model", dealing with the "dichotomy" between sources and fields. What Renn called the "umbrella effect" is, for instance, LeSage's model of gravitation. The fact is that both the *Newtonian* model and alternative models had shown internal flaws. LeSage's model, for instance, did not account for the dependence of gravitation on bodily mass; the *Newtonian* model had to face Olber's paradox. See Renn J. and Schemmel M. 2006, in Renn J. (ed.) 2006, Vol. 1, p. 28, and Vol. 3, pp. 2-6. As it is well known, Newton himself outlined a "gas model", both in letters and in some "queries", at the end of his *Opticks* (for instance, *query* 21).

[36] See Heaviside O. 1893, p. 465-6.

Appendix: *Heaviside's field theory for gravitation*

Following the analogy with the electromagnetic theory, Heaviside noted that "the flux of energy depends upon the magnetic force as well". What was the "analogous to magnetic force in the gravitational case"? And what would be its relationship with the gravitational quantity corresponding to the electric current? Heaviside guessed that even gravitation had its "magnetic side" and that we could start from an ordinary flux of matter: if *u* is the velocity of ρ, then $\rho\,\boldsymbol{u}$ would be the density of the *current* of ordinary matter. That inertial current, which Heaviside identified with a gravitational current, was akin to "a convective current of electrification". This correspondence led him to define a circuital *gravitational* current, corresponding to a *magneto-gravitational* field.

> "Also, when the matter ρ enters any region through its boundary, there is a simultaneous convergence of gravitational force into that region proportional to ρ. This is expressed by saying that if
>
> $\boldsymbol{C} = \rho\boldsymbol{u} - c\dot{\boldsymbol{e}}$,
>
> then *C* is a circuital flux. It is the analogue of Maxwell's true current; for although Maxwell did not include the convective term $\rho\boldsymbol{u}$, yet it would be against his principles to ignore it. Being a circuital flux, it is the curl of a vector, say
>
> $curl\,\boldsymbol{h} = \rho\boldsymbol{u} - c\dot{\boldsymbol{e}}$
>
> This defines h except as regards its divergence, which is arbitrary, and may be made zero."[37]

In this correspondence, the analogue of the electric force is $-\boldsymbol{e}$ and the analogue of the *displacement* current is $-c\dot{\mathbf{e}}$. Multiplying the last circuital equation by $\boldsymbol{e}$ Heaviside obtained

$$\boldsymbol{e}\,curl\,\boldsymbol{h} = \boldsymbol{e}\rho\boldsymbol{u} - \boldsymbol{e}c\dot{\boldsymbol{e}}\,.$$

[37] Heaviside O. 1893, p. 457.

Taking into account the rules of vector analysis $\boldsymbol{e}\,curl\,\boldsymbol{h} - \boldsymbol{h}\,curl\,\boldsymbol{e} = -\,div\,V\boldsymbol{eh}$, $curl\,\boldsymbol{e} = curl(\nabla P) = 0$,[38] and the expression for potential energy $U = \frac{1}{2}c\boldsymbol{e}^2$, the last equation becomes

$$-div\,V\boldsymbol{eh} = \boldsymbol{e}\rho\boldsymbol{u} - \boldsymbol{e}c\dot{\boldsymbol{e}} \quad or \quad conv\,V\boldsymbol{eh} = \boldsymbol{F}\,\boldsymbol{u} - \dot{U}.$$

In a more compact notation, we could write $\nabla \bullet (\boldsymbol{e} \times \boldsymbol{h}) = \dot{U} - \boldsymbol{F} \bullet \boldsymbol{u}$.

The term ***Veh*** (namely ***exh***) would represent the flux of gravitational energy; in the right side, the term ***Fu*** would correspond to "the activity of the force on ρ, increasing its kinetic energy"; the term *dU/dt* would correspond to "the rate of increase" of potential energy. The comparison with the electromagnetic case showed an opposite direction, arising from "all matter being alike and attractive, whereas like electrifications repel one another".[39]

At this point Heaviside undertook another step: he assumed that gravitational actions propagate over time, "although immensely fast". The gravitational force ***e*** could be propagated at a finite speed ***v***, in accordance with the typical equation of propagation without dissipation

$$\boldsymbol{v}^2\nabla^2\boldsymbol{e} = \ddot{\boldsymbol{e}}.$$

Remembering a rule of vector analysis, $\nabla^2 = \nabla div - curl^2$, and noting that *div* ***e*** *= 0* in pure aether (free from matter), the equation of propagation becomes

$$-\boldsymbol{v}^2 curl^2\boldsymbol{e} = \ddot{\boldsymbol{e}}.$$

Under the same conditions, in free aether, the first circuital equation would be

$$-curl\,\boldsymbol{h} = c\dot{\boldsymbol{e}}$$

38 Heaviside O. 1893, p. 457 (footnote) and p. 200. At that stage, ***e*** was considered a static field.

39 Heaviside O. 1893, pp. 456-9.

and its time-derivative offers another expression for $\ddot{\boldsymbol{e}}$:

$$-\frac{1}{c}\mathit{curl}\,\dot{\boldsymbol{h}} = \ddot{\boldsymbol{e}}\,.$$

Equating the two expressions for $\ddot{\boldsymbol{e}}$ Heaviside obtained

$$-\boldsymbol{v}^2\,\mathit{curl}^2\boldsymbol{e} = -\frac{1}{c}\mathit{curl}\,\dot{\boldsymbol{h}} \quad or \quad c\boldsymbol{v}^2\,\mathit{curl}\,\boldsymbol{e} = \dot{\boldsymbol{h}}\,.$$

Introducing a new constant μ, such that $\mu cv^2 = 1$, the last equation becomes

$$\mathit{curl}\,\boldsymbol{e} = \mu\dot{\boldsymbol{h}}$$

and it would be the second circuital equation, to be put besides $\mathit{curl}\,\boldsymbol{h} = \rho\boldsymbol{u} - c\dot{\boldsymbol{e}}$.[40]

When multiplying the former by **h**, Heaviside attained a quite meaningful expression from the point of view of energy:

$$\boldsymbol{h}\,\mathit{curl}\,\boldsymbol{e} = \mu\dot{\boldsymbol{h}}\boldsymbol{h}\,.$$

He noted that the right side corresponds to the time-derivative of the expression $T = \frac{1}{2}\mu h^2$, which is the gravitational analogue of magnetic energy. Now *curl* $\boldsymbol{e} \neq 0$ and the equation for the energy balance, previously deduced from the first circuital equation, becomes

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[40] Heaviside O. 1893, pp. 459-60.

$$\boldsymbol{e}\,curl\,\boldsymbol{h} = \boldsymbol{e}\rho\boldsymbol{u} - \boldsymbol{e}c\dot{\boldsymbol{e}}$$

$$-div\,V\boldsymbol{eh} = \boldsymbol{e}\rho\boldsymbol{u} - \boldsymbol{e}c\dot{\boldsymbol{e}} - \boldsymbol{h}\,curl\,\boldsymbol{e}\,.$$

$$conv\,V\boldsymbol{eh} = \boldsymbol{F}\,\boldsymbol{u} - \dot{U} - \dot{T}$$

The introduction of a *magneto-gravitational* field, obeying to the corresponding circuital law, led to the introduction of the appropriate *magneto-kinetic* term in the balance of energy. Heaviside guessed, as other physicists did, that the velocity of gravitational perturbations $\boldsymbol{v}$ might be much higher than the velocity of electromagnetic perturbations. It would imply that the constant μ is "of the necessary smallness" and kinetic energy associated to the medium is "an almost vanishing quantity".[41] The effects foreseen by this theory of contiguous action had not yet been observed: according to Heaviside, this was the reason for the success of the corresponding gravitational theory stemming from the alternative model of action at a distance.

Then he pointed out two conceptual queries arising from the new interpretation of gravitation. The first corresponded to the well-known properties of gravitational force and gravitational potential: when bodies are "infinitely widely separated, and the forces are least, the potential energy is at its greatest, and when the potential energy is most exhausted, the forces are most energetic". A medium exhausting its potential energy when a system of bodies has been collapsing onto each other seemed to Heaviside quite a "mysterious matter". The second query dealt with the absence of known gravitational effects corresponding to the electromagnetic effects of attraction between two electric currents.[42]

One month later Heaviside took into account those effects and wrote the second part of the "Appendix". He tried to develop other consequences of his main hypothesis that "the ether is the working agent in gravitational effects" and that "it propagates disturbances at speed v". He took into account the usual gravitational interaction between the Sun and the Earth, whose masses were labelled S and E. At first he wrote the "unmodified force" f, which obeys to the law

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[41] Heaviside O. 1893, p. 460.

[42] Heaviside O. 1893, p. 461.

$$f = \frac{SE}{4\pi c r^2},$$

where r was the distance between Earth and Sun and c was the elastic-gravitational constant of the medium. Then he introduced the "modified force" $\boldsymbol{F}$, which was the force in the case that the Sun was in motion with speed $\boldsymbol{u}$ through aether. The modification consisted of the concentration of lines of force in a direction perpendicular to the line of motion, which he had demonstrated in the electromagnetic case. Heaviside had already foreseen and discussed this effect in a previous paper published in 1889 in *Philosophical Magazine*.[43] The force increased its intensity in all directions perpendicular to $\boldsymbol{u}$, in accordance with the expression

$$\boldsymbol{F} = f \frac{1-s}{\left(1 - s \sin^2\theta\right)^2},$$

where $s = u^2/v^2$ and ϑ was the angle between $\boldsymbol{r}$ and the line of motion. The "slight strengthening" in perpendicular direction was accompanied by a "slight weakening" of the force in the line of motion. Heaviside computed the difference between the two extreme values of force corresponding to $\vartheta = 0$ and $\vartheta = \pi/2$, namely $F = f\,(1 - s)$ and $F = f\,(1 + \frac{1}{2}s)$. The difference was of the order of s and Heaviside thought that it amounted to 10^{-6}; he used for s the "speed attributed to fast stars", and for $\boldsymbol{v}$ the same velocity of light. The consequence was an expected "slight change in the shape of the orbit".[44] However, the "change in the Newtonian law" was not the more interesting theoretical development; the latter should be found in "the force brought in by the finiteness of v which is analogous to the 'electromagnetic force'". That sort of *gravitational magnetism* or "auxiliary force", labelled $\boldsymbol{G}$, had to follow the law

$$\boldsymbol{G} = F \frac{xqu}{v^2} V\boldsymbol{q}_1 V\boldsymbol{r}_1\boldsymbol{u}_1,$$

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[43] See Heaviside O. 1889a, in particular p. 332. See also Heaviside O. 1893, p. 460: "It may be worth while to point out that the lines of gravitational force connected with a particle of matter will no longer converge to it uniformly from all directions when the velocity v is finite, but will show a tendency to lateral concentration, though only to a sensible extent when the velocity of the matter is not an insensible fraction of v."

[44] Heaviside O. 1893, pp. 463-5.

where q was "the actual speed of the Earth", X was a numerical factor which "cannot exceed 1" and the vector factor in the right side contains the three unit vectors corresponding to $\boldsymbol{q}$, $\boldsymbol{r}$ and $\boldsymbol{u}$.[45] We can translate this law in more modern symbols:

$$\boldsymbol{G} = X\frac{1}{v^2}qFu\,\hat{\boldsymbol{q}}\times\left(\hat{\boldsymbol{r}}\times\hat{\boldsymbol{u}}\right) = X\frac{1}{v^2}\hat{\boldsymbol{q}}q\times\left(\hat{\boldsymbol{r}}F\times\hat{\boldsymbol{u}}u\right) = X\frac{1}{v^2}\boldsymbol{q}\times\left(\boldsymbol{F}\times\boldsymbol{u}\right)$$

In a footnote, in order to explain that equation, Heaviside referred to a section of the second chapter of the book, dealing with "motional electric forces" and "motional magnetic forces". They were forces arising when electric or magnetic forces are in motion with regard to the medium or, conversely, the medium is in motion with regard to the forces. Those induced or "motional" forces had to obey to the laws

$$\boldsymbol{e} = \boldsymbol{q}\times\boldsymbol{B} \quad and \quad \boldsymbol{h} = \boldsymbol{D}\times\boldsymbol{q}\,,$$

where $\boldsymbol{q}$ was the velocity of forces in motion.[46]

In Heaviside's theoretical framework, $\boldsymbol{G}$ was associated to the electromagnetic force, the *gravito-dynamical* field of the Sun was associated to the magnetic induction of a magnet or a coil, and the Earth in motion was associated to an electric current.

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[45] Heaviside O. 1893, p. 465.

[46] Applying them to the usual electromagnetic case, we see that an electric field $\boldsymbol{e}$ in motion (corresponding to Sun's gravitational field in motion) produces a magnetic field $\boldsymbol{h} = \boldsymbol{D}\times\boldsymbol{u} = \varepsilon\,\boldsymbol{e}\times\boldsymbol{u}$.

This magnetic field, when in motion (corresponding to Sun's *magneto-gravitational* field in motion with regard to the Earth), would produce an additional electric field $\boldsymbol{e}' = \boldsymbol{q}\times\boldsymbol{B} = \boldsymbol{q}\times(\mu\boldsymbol{h}) = \boldsymbol{q}\times(\mu\varepsilon\,\boldsymbol{e}\times\boldsymbol{u}) = \frac{1}{v^2}\boldsymbol{q}\times(\boldsymbol{e}\times\boldsymbol{u})$.

Applying these equations to the gravitational case, we actually have (apart from the constant X) $\boldsymbol{G} = (1/v^2)\,\boldsymbol{q}\times(\boldsymbol{F}\times\boldsymbol{u})$.

Part II: *MICROSCOPIC MODELS*

11. J.J. Thomson: new features for matter and energy

During the 1880s, new conceptions of matter, energy and electric charge were put forward by scientists who claimed that they were following Maxwell's conceptual path, even though they were exploring new paths, leading towards new lands. J.H. Poynting turned his attention to the transferring of electromagnetic energy throughout space and time. He thought he had found in Faraday's tubes of force, rather than in Maxwell's "electric displacement", the *hardware* corresponding to the processes involving energy. Moreover, he thought he had attained a unified explanation, for both induction and conduction, in terms of tubes of force. Lodge gave an even more radical interpretation of electromagnetic energy. He interpreted Poynting's flux of energy in a substantial way, and assumed a deep similarity between matter and energy. This extreme conception was not shared by other members of the British scientific community and was explicitly criticized by another *Maxwellian*, O. Heaviside. The latter developed Maxwell's model of a continuous medium, composed in part of ordinary matter and in part of aether. He found that aether and matter were different only because of the different values in the physical constants of an elastic medium: density and elasticity. Energy was spread with continuity throughout the medium and the electric charge was an effect of the distribution of the "electric displacement" in the passage from conductive matter to dielectric matter or aether. Nevertheless, Heaviside found it hard to cope with electric conduction and to associate conduction to its mechanical analogue, namely dissipation.

In the same years, two Cambridge *mathematicians*, J.J. Thomson and Larmor, tried to insert conduction consistently into their electromagnetic theories, rather than superimpose it. The former came from Trinity College and the latter from St. John's College: they had qualified respectively second and first wrangler in the 1880 Mathematical Tripos. In the 1890s, both of them upheld discrete, quantized, models for matter, energy and electric charge: discrete rotational strains, namely electrons, in Larmor's theories, and discrete tubes of force, in J.J. Thomson's theories. In both models, units of energy were tightly linked to units of electric charge and these, in their turn, were tightly linked to units of matter. According to Larmor, localised concentrations of rotational energy gave rise to the *electron*, a unit of matter associated to a unit of electric charge. According to J.J. Thomson, bundles of aethereal structures, namely units of tubes of force, could propagate throughout aether in the form of electromagnetic radiation, or could link together units of matter and electric charge. What both theories had in common was the resort to discrete, dynamical structures, either translational or rotational, emerging from a continuous, universal aether. I believe that the story of the emergence of their theories deserves to be analysed in some detail.

In 1884, J.J. Thomson was, at the age of 28, appointed as Cavendish Professor of Experimental Physics (the chair held by Maxwell and then by Rayleigh), though he had only recently committed himself to experimental physics. As we have already seen, in the same year, he published an important paper in the *Report of British Association for the Advancement of Science*, under the title "Report on Electrical Theories". It was a survey on electromagnetic theories put forward in Europe in the last sixty years. It is worth mentioning the first pages of the paper, containing an appraisal of the different theories, in order to better appreciate his conceptions of matter and energy in the context of his subsequent electromagnetic theories. Thomson stated that he was taking into account "theories of electrical action which only profess to give mathematical expressions for the forces exerted by a system of currents". The criteria excluded purely electrostatic theories like Coulomb's, and non-mathematical theories like Faraday's. At that stage, Thomson preferred not to take into account too speculative inquires into the intimate nature of electric currents.[1]

That classification was historical, ranging from the first "geometrical" theories, taking into account only spatial relations between electric currents, to later and more sophisticated theories, taking into account the medium surrounding conductors. The second, third and fourth classes had much in common: they all had explicitly faced the principle of the Conservation of Energy, even when it was not yet a general, widespread requirement for every physical theory.[2] It seems that Thomson, at least at that stage, overshadowed the familiar distinction between theories of actions at a distance and theories of contiguous actions. Gauss, Weber and Riemann's

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[1] He divided the theories in five classes. See Thomson J.J. 1885, pp. 97-8: "1. Theories in which the action between elements of currents deduced by geometrical considerations combined with assumptions which are not explicitly, at any rate, founded on the principle of Conservation of Energy. This class includes the theories of Ampère, Grassmann, Stefan, and Korteweg. / 2. Theories which explain the action of currents by assuming that the forces between electrified bodies depend upon the velocities and acceleration of the bodies. This class includes the theories of Gauss, Weber, Riemann, and Clausius. / 3. Theories which are based upon dynamical considerations, but which neglect the action of the dielectric. This class contains F.E. Neumann's potential theory and v. Helmholtz extension of it. / 4. C. Neumann theory. / 5. Theories which are based upon dynamical considerations, and which take into account the action of the dielectric. This class includes the theories of Maxwell and Helmholtz."

[2] On the Principle of Conservation of Energy as a "regulative principle" see Bevilacqua F. 1983, pp. 122-36.

theories assumed an electric current consisting of positive electricity moving in one direction and an equal amount of negative electricity moving in the opposite way (Fechner's hypothesis). The theory of Clausius assumed the motion of only one kind of electrification.[3] Differently from Weber and Riemann's, Clausius' law depended on the absolute velocity and acceleration of electrified bodies and therefore it required a privileged reference frame. This means that we should assume the existence of an absolute space or the existence of a medium. In addition, Thomson noted, the actions between two bodies were not perfectly balanced. The difference of momentum had to be yielded by the surrounding medium or given back to it.[4]

In this "Report", Thomson discussed the theoretical links among energy, matter and electrification in the quoted theories. Energy was the pivotal entity: to start from the energy stored in a physical system appeared to Thomson better than to start from forces acting between electrified bodies or electric currents. At first energies were given, then forces followed.

It was expected that, in electric phenomena, potential energy depended on electrification: this accounted for the existence of electrostatic forces. If kinetic energy also depended on electrification, as a consequence "the forces between two electrified bodies in motion would be different from the forces between the same bodies at rest".[5] In other words, if electrification affected even kinetic energy, the equation of motion would have shown typical electrodynamical effects. He imagined a symmetrical, electrified body, charged with a quantity of electricity e, moving through an isotropic dielectric with velocity $\boldsymbol{q}$, and assumed a very general expression for its kinetic energy T:

$$\frac{1}{2}mq^2 + f(e)q^2.$$

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[3] Thomson J.J. 1885, p. 107.

[4] See Thomson J.J. 1885, pp. 109-10. Clausius' law raised also an objection: if the force depends on absolute velocity, on the Earth's surface, which is in motion, an electric current should exert electromagnetic induction on a charged body at rest. Some scholars had even suggested that an electric circuit should have exerted electromagnetic induction on itself. Two different solutions had been put forward, and Thomson reported on them. First, that force was derived from a potential, "so that the integral of the force taken round a closed curve would vanish, and thus (…) two circuits would not induce currents in each other if they were relatively at rest". Secondly, the velocities in Clausius' law were supposed to be relative to aether; if aether had been dragged by the Earth's surface, no effects would have been detected. See Thomson J.J. 1885, p. 111.

[5] Thomson J.J. 1885, p. 111.

The term *f(e)* was assumed to be some function of *e*, and was expected to be positive, in order to assure that kinetic energy was positive. Among all possible expressions he chose $f(e) = \alpha e^2$, where α was a positive constant; therefore kinetic energy becomes

$$\left(\frac{1}{2}m+\alpha e^2\right)q^2 .$$[6]

Then Thomson took into account two electrified bodies, together with their masses *m* and *m'*, their electric charges *e* and *e'* and their velocities ***v*** and ***v'***. For this physical system, he assumed a kinetic energy

$$\frac{1}{2}mq^2+\frac{1}{2}mq'^2+\alpha e^2q^2+\beta e^2q'^2+ee'kf(q,q'),$$

where *f(**q**, **q'**)* was a quadratic function in ***q*** and ***q'***. Taking into account only the terms depending on q^2, we have

$$T=\left(\frac{1}{2}m+\alpha e^2+k\frac{ee'}{r}\right)q^2 \quad .$$

The coefficient of the kinetic energy inside the brackets could be negative in case of *e'* were negative, and either *e'* conveniently great or *r* conveniently small: this was the sense of Thomson's statement "the body behaves as if its mass were negative". It was not the *mass* in the ordinary sense which could become negative: in the above expression for kinetic energy, we deal rather with a sort of mass, or inertia, which is both mechanical and electromagnetic.[7] The meaning of this new kind of mass seemed to Thomson more consistent with a theoretical framework quite different, for instance, from Weber's: Maxwell's theory. The latter, Thomson remarked, had emphasised the role of the surrounding medium rather than the embedded electrified bodies: in Maxwell's theoretical model, a change in the electric

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[6] See Thomson J.J. 1885, p. 112.

[7] Weber and Riemann's laws of force corresponded to a function which, in the case of electrified bodies moving along the line joining them, had the simple form $f = (q-q')^2/r$. See Thomson J.J. 1885, pp. 112-3.

polarisation of the medium "produces the same effects as an electric current". The following remarks echoed the contents of the paper J.J. Thomson had written in 1881, on the electromagnetic effects of an electrified body in motion.[8] When we put in motion a non-electrified body, we need a certain amount of work, which is transformed into the kinetic energy of the body itself. When we put in motion an electrified body, the work gives rise to two different phenomena: the motion of the body and the establishment of a *displacement* current in the medium. In some way, the energy transferred to the system must be spent to overcome the inertia of both the body and the medium. More energy must be delivered to an electrified body, in order to allow it to reach the same velocity, with regard to a body with the same mechanical mass but not electrified.

> "For let us suppose that we have an electrified body at rest, and consider the amount of work necessary to start it with a velocity q. It is evident that it will be greater that when it is not electrified, for when it is electrified and in motion the electric polarisation in the surrounding dielectric will be in changing, and so in addition to starting the body with a velocity q we have, if Maxwell's hypothesis be true, to establish what is equivalent to a field full of electric currents. The production of these currents of course requires work, so that more work is required to start the body with a velocity q when it is electrified than when it is not; in other words, the kinetic energy of a moving electrified body is greater than that of one not electrified, but under similar conditions as to mass and velocity. In fact in this case electricity behaves as if it possessed inertia."[9]

Thomson's reference to Maxwell's theory and electromagnetic inertia led him far away from the query of the negative mass in Weber's theory. The two theories were quite different and statements like "polarisation of the surrounding medium" made no sense in Weber's theory: there was nothing like this in that theory. In the following passages Thomson forsook Weber's theory and its problems regarding mass; he focused rather on some results he had already attained some years before. He reminded the reader of his previous (1881) paper, wherein he had shown the existence of an electrokinetic term in the kinetic energy of "a charged sphere of radius a and mass m moving at a velocity $\boldsymbol{q}$".

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[8] See Thomson J.J. 1881, pp. 229-31; see also chapter 1 of the present book.

[9] Thomson J.J. 1885, p. 114.

He noted that the expression he had found entailed the existence of forces between charged spheres which were "exactly the same as those given by Clausius' formulae". At the same time he acknowledged the existence of a theoretical gap between his and Clausius' theory, for "Clausius conception of an electric current does not accord with that of the displacement theory". Nevertheless, Thomson claimed that, in his *Maxwellian* theoretical model, the electrokinetic energy was always positive: this was another mathematical feature in common with Clausius.[10] In short, we could say that the two models of force and energy were equivalent from the point of view of mathematical physics but quite different from the point of view of theoretical physics. On the one hand, he pointed out the difference between theories taking into account the medium and theories taking into account only electrified bodies; on the other, he took note of their equivalence on the empirical ground. The comparison between the two classes of theories took place at two different levels: the experimental level and the theoretical level.

> "In the theories we have hitherto considered, the influence of the medium which exists between the currents has been left altogether out of account. In the theories which we shall now proceed to discuss, the influence of this medium is taken into consideration. This is, perhaps, the most important step that has ever been made in the theory of electricity, though from a practical point of view it is comparatively of little importance; in fact, for practical purposes almost any one of the preceding theories will satisfy every requirement."[11]

With regard to Maxwell's theory, Thomson acknowledged its contribution to a more complete comprehension of electromagnetic phenomena. Nevertheless he found in it some conceptual and linguistic difficulties which led him to prefer Faraday's theoretical model. Faraday had been the first to stress the importance of the medium, either aether or ordinary matter, in the propagation of electromagnetic actions. In addition, according to Thomson, Faraday's concept of "dielectric polarisation" was clearer than Maxwell's concept of "electric displacement". Although "mathematically the two things are identical", the word "displacement" suggested something

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[10] See Thomson J.J. 1885, p. 114. In 1881, in the case of a moving sphere, he had found a kinetic energy $\frac{1}{2} m q^2 + \frac{2}{15} \frac{\mu e^2}{a} q^2$.

[11] Thomson J.J. 1885, p. 123.

moving, while "polarisation only implies that there is a vector change of some kind in the dielectric". In the following pages of the paper, we can see the development of a conceptual path going back from Maxwell's theoretical model to Faraday's theoretical model, a conceptual path quite similar to that undertaken by Poynting the same year. The stress on the word "polarisation" had a further meaning: that word allowed Thomson to focus on matter and on the structure of matter. Polarisation entailed some change in the structure of dielectrics: in order to realise that change, some work was required. According to Thomson, polarisation would correspond to a condition of greater density of energy throughout matter; otherwise, "the dielectric would go into the polarised conditions of itself, without the application of any external forces".[12]

He thought that even the meaning of the expression "quantity of electricity" in Maxwell's theory was ambiguous and deserved some specifications. He found that the expression had a definite meaning more in the context of "the old two-fluid theory" than in Maxwell's theory. Nevertheless, he wanted to save the core of Maxwell's theory, although, once again, he borrowed something from Faraday: lines of force or tubes of force. In that re-interpretation, electrification could be associated to a net amount of tubes of force approaching and leaving a body. To sum up, we can see a sort of *materialisation* of Maxwell's words and concepts. If the term *displacement* made reference to geometry and kinematics, the term *polarisation* made reference to a more concrete structure, the structure of matter or, in general, the structure of a medium. If the word *electrification* was associated by Maxwell to the surface integral of the electric displacement, it was associated by Thomson to the computation of the "tubes" of force.

> "A line of force is a line whose direction at any point coincides with the direction of the electromotive force at that point, so that we may conceive the electric field to be filled with lines of force. If

[12] J.J. Thomson's conceptual shift from "displacement" to "polarisation" seems an overlapping rather than a replacement. See Thomson J.J. 1885, p. 125: "The polarisation or displacement is in isotropic media in the direction of the electromotive force and proportional to it, just as the magnetic induction in isotropic media is in the direction of the magnetic force and proportional to it. It was this proportionality combined with the fact that as soon as the electromotive force is removed the dielectric springs back, as it were, to its original state, that led Maxwell to use the word displacement. He looked on the case as analogous to that of an elastic solid, which springs back to its original position when the external force is removed, and in which the displacement is proportional to the impressed force."

> we consider the lines of force passing through some small closed curve, they will form a tube, and such a tube is called tube of force; and if the dimensions of the tube are such that the product of the cross section at any point and the electromotive force at that point is constant and equal to 4π, the tube is called a unit tube. We thus may conceive space to be filled with unit tubes of force. Since the electromotive force inside a conductor vanishes these tubes will end at the surface of a conductor. And the quantity of electricity on the conductor will be equal to the excess of the number of lines of force which leave the conductor over those which enter it."[13]

When a conductor is in motion, Thomson suggested, "it may be supposed to carry the tubes of force along with it, so that the number of tubes which end on the conductor remains constant". Even the relative inductive capacity of two dielectrics could be expressed in terms of tubes of force. When tubes passed from the first dielectric to the second, he computed the product between the cross section and the electric force on both sides of the limiting surface. The ratio between the two products had a well-defined value, depending "only on the nature of the dielectrics": it corresponded to the ratio between the inductive capacities of the two dielectrics.[14]

In his survey on electromagnetic theories that took into account the role of the medium, Thomson compared Helmholtz's theory to Maxwell's. Helmholtz's electric currents in conductors and electric polarisations in dielectrics sounded quite similar to conduction currents and *displacement* currents in Maxwell's theory. Furthermore, some results seemed to fit to each other, after having adjusted the value of a constant in Helmholtz's potentials. Nevertheless, as Thomson pointed out, the role of non-conducting media, both aether and ordinary dielectrics, was different in the two theories. According to Helmholtz, dielectric polarisation was added to the conduction currents and behaved like an incompressible fluid. According to Maxwell, electric displacement was placed in continuity with conduction currents, giving rise to a total current, which behaved like an incompressible fluid.[15] In Helmholtz's theoretical model, polarisation was imagined as a side-

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[13] Thomson J.J. 1885, p. 126.

[14] Thomson J.J. 1885, p. 126.

[15] See Thomson J.J. 1885, pp. 133-5. See, in particular, p. 135: "Thus on Helmholtz's theory the dielectric currents behave like the flow of an incompressible fluid, while on Maxwell's theory it is the total current, which is the sum of the conductions currents and the dielectric currents which behave in this way." He devoted some pages to Helmholtz's theory, which he considered a sort of *super-theory*, "as it includes all theories of this class", namely theories which involved elements of electric current and potentials. See Thomson J.J. 1885, p. 115.

effect of conduction currents. In Maxwell's theoretical model, conduction currents and displacement currents had a symmetric role, and, on some pages of his *Treatise*, conduction currents are even represented as side-effects of the electric *displacement*. Thomson's theoretical reconstruction cast light even on the mathematical aspect of this conceptual difference: in Maxwell's theory, the vector with zero divergence was the total current whereas, in Helmholtz's theory, it was the time derivative of dielectric polarisation. I would like to follow Thomson's reconstruction.

If *f*, *g*, *h* are the components of the electric *displacement*, *p*, *q*, *r* are the components of the conduction current, *u*, *v*, *w* are the component of the "effective" total current, and ρ is the "volume density of free electricity", Maxwell 's theory yielded:

$$
\begin{gathered}
u=p+\dot{f}, \quad v=q+\dot{g}, \quad w=r+\dot{h}, \\
\frac{dp}{dx}+\frac{dq}{dy}+\frac{dr}{dz}=-\frac{d\rho}{dt} \\
\frac{df}{dx}+\frac{dg}{dy}+\frac{dh}{dz}=\rho \\
\text{then} \quad \frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0
\end{gathered}
$$ [16]

If $\boldsymbol{X}$, $\boldsymbol{Y}$, $\boldsymbol{Z}$ are the components of electric polarisation, and *p*, *q*, *r*, *u*, *v*, *w* and ρ have the same meaning as above, Helmholtz's theory yielded:

$$
\begin{gathered}
u=p+\dot{\boldsymbol{X}}, \quad v=q+\dot{\boldsymbol{Y}}, \quad w=r+\dot{\boldsymbol{Z}}, \qquad or \qquad \dot{\boldsymbol{X}}=u-p, \quad \ldots \quad \ldots \\
\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=-\frac{d\rho}{dt} \\
\frac{dp}{dx}+\frac{dq}{dy}+\frac{dr}{dz}=-\frac{d\rho}{dt} \\
\text{then} \quad \frac{d\dot{\boldsymbol{X}}}{dx}+\frac{d\dot{\boldsymbol{Y}}}{dy}+\frac{d\dot{\boldsymbol{Z}}}{dz}=0.
\end{gathered}
$$ [17]

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[16] Thomson J.J. 1885, pp. 127-8.

[17] See Thomson J.J. 1885, pp. 134-5. See, in particular, p. 135: "Thus on Helmholtz's theory the dielectric currents behave like the flow of an incompressible fluid, while on Maxwell's

From the empirical point of view, Thomson claimed the necessity for two subsequent steps. The first step required an experimental check, in order to decide whether the medium had to be really taken into account in the explanation of electromagnetic phenomena. The second step had a different target: to decide which, among theories taking into account the medium, was the more suitable to explain the known phenomena.[18]

With regard to the first step, Thomson thought that recent experiments performed by Rowland, Schiller, Helmholtz and Röntgen corroborated those theories which took into account the medium. Rowland's experiments had shown that a moving electrified body set in motion a magnetic needle. This led Thomson to conclude that a change in the polarisation of a dielectric produced a magnetic force: it had the same property of conduction currents. In this way, the first step seemed to him fully accomplished.[19] The core of the question was "the continuity of these dielectric currents" or, in other words, "whether Maxwell's assumption that they always form closed circuits with the other currents is true or not": in 1885 this was still an open question.[20]

In the "Appendix II", placed at the end of his "Report", J.J. Thomson offered further explanations on stresses taking place in dielectrics: the conception was "due to Faraday", and the "magnitude and distribution" of stresses had subsequently been "investigated by Maxwell". Under the effect of an electric force, the medium should become the seat of tensions and

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theory it is the total current, which is the sum of the conduction currents and the dielectric currents which behave in this way."

[18] See Thomson J.J. 1885, pp. 142 and 149-50.

[19] See Thomson J.J. 1885, p. 143: "Thus we see that a change in the polarisation of the dielectric must produce all the effects of an ordinary conduction current, so that it is only absolutely necessary to consider how the experimental evidence affects those theories which take the action of the dielectric into account." The available experiments led Thomson to conclude that "the potential theory is wrong if we neglect altogether the action of the dielectric, and assume the current to stop at the end of the wire". He also made reference to Helmholtz's experiment which had shown that "the potential theory leads to wrong results unless the action of the dielectric is taken into account". In the end, Thomson quoted Röntgen's "preliminary account" on some experiments he had recently performed; they showed that "the variations in the electric polarisation produce effects analogous to those due to a current". See Thomson J.J. 1885, pp. 144, 147 and 149.

[20] See Thomson J.J. 1885, pp. 149: "This completes the account of the experiments which have been made to test the various theories. As the result of them we may say that they show that it is necessary to take into account the action of the dielectric, but they tell us nothing as to whether any special form of the dielectric theory, such as Maxwell's or Helmholtz's, is true or not." For a detailed analysis of some of those experiments, see Buchwald J.Z. 1994, pp. 31-41.

pressures, namely "a tension equal to $KR^2/8\pi$ per unit area along the lines of force combined with a pressure of the same amount at right angles to these". This theoretical framework allowed him to outline a unified interpretation of the relationship among matter, electricity and energy stored in the medium.

> "These stresses are in equilibrium at a point in a dielectric where there is no free electricity. At the junction of two media, whose specific inductive capacities are K_1 and K_2, and in which the electromotive forces are R_1 and R_2, and whose interface is perpendicular to the lines of forces, the stresses are not in equilibrium, but there is an unbalanced stress $(K_1R_1^2- K_2R_2^2)/8\pi$ which will tend to make the boundary move towards the medium whose specific inductive capacity is K_1; if these dielectrics are liquid, their interface may become curved so that the forces due to surface tension balance this stress."[21]

In 1885, the attempt to integrate Maxwell's theory with some hypotheses on matter and with Faraday's conceptual model of lines or tubes of force was not realised in detail; in particular, the interaction between the tubes of force and the structure of matter and electricity was still waiting for further theoretical investigations.

[21] Thomson J.J. 1885, pp. 154.

12. A discrete model for the electromagnetic field

In a paper published in 1891 in *Philosophical Magazine*, "On the Illustration of the Properties of the Electric Field by Means of Tubes of Electrostatic Induction", J.J. Thomson continued to pursue his long-term aim already mentioned in 1885: to explain the core of Maxwell's electromagnetic theory in a more concrete way. He claimed he would have offered to students some "physical interpretation of results which are perhaps too frequently regarded as entirely expressed by equations". For this purpose he resorted to Faraday's tubes of force, "which are assumed to be distributed throughout the field". In particular, he aimed at explaining the processes taking place in the electric field "in terms of changes in the form or position of tubes of electrostatic induction". He explicitly faced the problematic link between mathematical physics and theoretical physics. Thomson acknowledged that "in the case of Electricity, the analytical theory is well established"; in other words, there were some equations and those equations were mathematically consistent. Nevertheless, this was not enough for a physicist interested in a deep comprehension of the physical world. He needed "methods … of materialising … mathematical conceptions": a theory could be correct but, at the same time, too abstract to be a suitable representation of phenomena. Tubes of force could be that "mental picture" endowed with "freshness and a power of rapidly giving the main features of a phenomenon". He still focused his criticism on Maxwell's concept of "electric displacement" which was "too general" and unsuitable "to the formation of a conception of a mechanism which would illustrate by its working the processes going on in the electric field".[1] The conceptual shift from Maxwell's equations to Faraday's conceptual model corresponded to a methodological shift in favour of theoretical physics.

> "For this purpose the conception of tubes of electrostatic induction introduced by Faraday seems to possess many advantages. If we regard these tubes as having real physical existence, we may, as I shall endeavour to show, explain the various electrical processes, - such as the passage of electricity through metals, liquids, or gases, the production of a current, magnetic force, the induction of currents, and so on, - as arising from the contraction of elongation of such tubes and their motion through the electric field."[2]

[1] Thomson J.J. 1891, p. 149.
[2] Thomson J.J. 1891, pp. 149-50.

In addition, tubes of force, in particular electric tubes of force rather than magnetic tubes of force, allowed Thomson to inquire into the structure of matter and, at the same time, into the structure of electricity. Thomson's theoretical inquiry involved both physics and chemistry: it was placed on the borderline between physics and chemistry.[3] Tubes of force represented the theoretical *tool* by which he could undertake the project of unification between the electromagnetic theory and a new theory of matter. He found that electric tubes of force, rather than magnetic ones, allowed him to bridge the gap between electric phenomena and the structure of matter.[4]

Tubes of force allowed him to undertake another conceptual shift: from the conception of electric field as a continuous entity to a new "molecular" theory, where electric fields were imagined as a collection or discrete, individual entities, endowed with their own identity. In my opinion, this point deserves further attention. Thomson introduced two levels of investigations, macroscopic and microscopic. With regard to matter, the macroscopic level of the theory of gases corresponded to the microscopic level of the kinetic molecular theory: in some ways, the latter was an *explanation* of the former. The microscopic level corresponded to a higher level of comprehension or to a finer interpretation. Regarding energy, to a macroscopic level, described in terms of continuous fields, corresponded a microscopic level, described in terms of an invisible, discrete structure: the tubes of electric induction. To sum up, J.J. Thomson put forward a conceptual shift towards a *kinetic molecular* theory of energy, the same conceptual shift already realized in the case of matter.

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[3] I have already quoted S. Abiko's "two research tradition, in physics in Western Europe around the turn of the [twentieth] century", namely "a chemico-thermal tradition" and a "particle-dynamical tradition": J.J. Thomson was classified in the second traditions. This classification seems to me unsuitable for Thomson: he was committed to both traditions. See Seiya Abiko 2003, p. 211, and chapter 3 of the present book. It is worth remembering that what we call *physics* and *chemistry* were separate academic worlds in Cambridge, in the years of J.J. Thomson's training. Physics was mainly involved in the Mathematical Tripos, whereas chemistry was part of the Natural Sciences Tripos. On the separate fate of physics and chemistry in Cambridge university, in the late nineteenth century, see, for instance, Navarro J. 2006, pp. 481.

[4] See Thomson J.J. 1891, p. 150: "We might, as we shall see, have taken the tubes of magnetic force as the quantity by which to express all the changes in the electric field; the reason I have chosen the tubes of electrostatic induction is that the intimate relation between electrical charges and atomic structure seems to point to the conclusion that it is the tubes of electrostatic induction which are most directly involved in the many cases in which electrical charges are accompanied by chemical ones."

> "We may regard the method from one point of view as being a kind of molecular theory of electricity, the properties of the electric field being explained as the effects produced by the motion of multitudes of tubes of electrostatic induction; just as in the molecular theory of gases the properties of the gas are explained as the result of the motion of its molecules."[5]

Thomson's theoretical model of tubes of force was akin to Poynting's conception, where the motion of tubes of force corresponded to the flux of energy. Thomson's *molecular* theory of the electric field corresponded to a *molecular* theory of energy. That theoretical model, although only roughly sketched, introduced a strong similarity between matter and energy. Thomson was drawing a conceptual path leading from a continuous model to a discrete model for both matter and energy. Another issue emerged naturally from that general sketch: the intimate link between matter and electricity. The connection between the atoms of matter and the sea of tubes of force floating throughout space enlightened the "close connexion between electrical and chemical properties". Thomson's 1891 paper offers an interesting landscape, made of different physical entities involved in a great project of unification: the structure of matter, the structure of electricity and the structure of fields, or, in other words, the structure of electromagnetic energy.[6]

Thomson also resorted to an analogy between tubes of force in electricity and "lines of vorticity in hydrodynamics". Tubes of force, he surmised, "must either form closed circuits or they must end on atoms"; in the same way, lines of vorticity "must either be closed, or have their extremities on a boundary of the fluid". Unclosed tubes of force realised an electric link between two atoms.[7] This was another interesting analogy between the structure of matter and the structure of energy. The model of vortex-atoms could be considered as an instance of a *dynamical* theory of matter, where matter was nothing else but a dynamical state in a medium, a steady structure consisting of a concentration of rotational energy. Thus we see that the structure of matter offered a model for the structure of energy and, at the same time, the structure of matter appeared as a peculiar

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[5] Thomson J.J. 1891, p. 150.

[6] See Thomson J.J. 1891, p. 150: "We assume, then, that the electric field is full of tubes of electrostatic induction, that these are all of the same strength, and that this strength is such that when a tube falls on a conductor it correspond to a negative charge on the conductor equal in amount to the charge which in electrolysis we find associated with an atom of a univalent element."

[7] Thomson J.J. 1891, p. 150.

structure of energy. There was a conceptual continuity between J.J. Thomson's re-interpretation of Faraday's tubes of force and the vortex tubes he had analysed in his 1883 essay on vortex rings. In both cases we have a discrete structure stemming from a continuous medium.[8]

The energy of the electric field, spread throughout the medium, was associated to the motion of the tubes of force, apart from the amount of energy stored inside the tubes or inside the medium itself. At the microscopic level, the interaction between tubes of force and matter affected the inner structure of atoms. Thomson guessed that some transfer of energy took place in that interaction, modifying "the internal motion of the atom". He imagined that energy was exchanged between atoms and unit tubes of force: a unit of energy w could be handed over by a unit tube falling on the atom, and a corresponding amount $-w$ by a unit tube leaving the atom. The amount w would depend on the kind of matter; it would be different, for instance, for conductors made of copper and conductors made of zinc. This mechanism appeared to Thomson suitable to explain contact electricity: it happened "as if the atoms of different substances attracted electricity with different degrees of intensity". Electricity emerged on the borderline between matter and tubes of force, on the surface where tubes of force got in touch with atoms. In spite of the specific differences between Maxwell's theory and Thomson's new theory, we can find in the latter the footprint of Maxwell's conception of electric charge as something happening at the common surface between dielectrics (in particular aether) and conductors. According to Thomson, even the links among atoms in a molecule involved tubes of force: chemical transformations consisted of electric transformation. We have in front of us a theory of electricity which transformed itself, step by step, into a theory of matter.

> "According to our view, the ends of a tube of finite length are on free atoms as distinct from molecules, the atoms in the molecule being connected by a short tube whose length is of the order of the molecular distance. On this view, therefore, the existence of free electricity, whether on a metal, an electrolyte, or a gas, always denotes the existence of free atoms. The production of electrification must be accompanied by chemical dissociation, the disappearance of it by chemical combination: changes in

[8] This does not mean that he trusted the same specific theoretical model of matter throughout the 1890s. See Falconer I. 1987, p. 264.

> electrification are in this view always accompanied by chemical changes."[9]

Thomson was aware of another transformation brought about by his assumptions: his representation of matter experienced a conceptual drift, from the model of solid dielectrics to the model of electrolytes. Electrolytes were exactly the kind of matter which was not easy to explain in the context of Maxwell's theoretical framework. Gases seemed to exhibit the same behaviour of electrolytes when electricity passed through them. Liquid electrolytes and ionised gases became the new model of matter "undergoing chemical changes when the electricity passes through them". In brief, Thomson's theoretical enterprise turned the attention from solids to liquids and gases. The theory Maxwell had put forward was essentially a theory based on solid dielectrics and conductors; now liquids and gases were on the stage and Thomson attempted to explain the properties of metals by means of the properties of liquids and gases. In other words, he tried to devise a model of matter based on the properties of fluids: in that new framework, solid matter had to be explained in terms of those properties.[10]

The theoretical programme had to cope with some difficulties: for instance, metals and electrolytes behaved differently when submitted to an increase of temperature. It was known that, in general, conductivity of metals decreased with temperature whilst conductivity of electrolytes increased. Nevertheless there were some exceptions and Thomson thought that those exceptions were "sufficient to show that increase of conductivity with the temperature is not a sufficient test to separate electrolytic from metallic conduction". In addition, that apparent mismatch could be profitably interpreted in chemical terms. An increase of temperature produced two effects on electric conduction: a dissociation of molecules into their atomic components and, at the same time, a delay in their recombination after that separation. The dissociation contributed to the process of conduction, whereas the delay in the recombination opposed that process. Moreover, an increase of temperature, in accordance with the kinetic theory of matter, would have increased the distances among the molecules and this

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[9] Thomson J.J. 1891, p. 151.

[10] See Thomson J.J. 1891, p. 151: "All these results seem to point to the conclusion that the passage of electricity through gases is accompanied by changes in the pairing of the atoms of the gas. Although we have no such direct evidence of the same effect when electricity passes through metals, it must be borne in mind that direct evidence in this case is very much more difficult to obtain, and there are many reasons for taking the view that the passage of electricity through metals is performed in much the same way as it is through electrolytes and gases."

represented a hindrance to maintaining chemical interactions.[11] This theoretical inquiry into the electric roots of the structure of matter overturned the macroscopic models of matter devised by Maxwell and by some of his followers like Heaviside. In the context of a *Maxwellian* electromagnetic theory, a continuous elastic medium appeared as the leading model; electrolytes or ionised gases appeared as the exception. The latter were hard to explain in terms of the former. On the contrary, Thomson endeavoured to transform solid bodies, in particular metal conductors, into a specific kind of electrolytes.[12]

Thomson was making every effort to force reluctant facts into the theoretical framework which he was developing. He was looking for some solid body which behaved in the same way as liquid electrolytes behaved. Electrolysis was gaining a new position inside the electromagnetic theory: it passed from the condition of disappointing phenomenon to the condition of keystone in a new electric theory of matter. Although "the electrical conductivity of metals are enormously greater than those of electrolytes", Thomson assumed that there was not a sharp theoretical difference between conduction in electrolytes and in non-electrolytes. He imagined a difference in degree rather than a difference in nature. Differences in electric conductivity could be looked upon just like differences in thermal conductivity: although, in different elements, differences in thermal conductivities were quite great, the nature of thermal conductivity did not change in its passage from one element to the other.

> "There is a greater disproportion between the thermal conductivities of silver and cement than there is between the electrical conductivities of mercury and fused lead chloride; but no one argues that, on this account, the method by which heat is propagated in silver is essentially different from that by which it is propagated in cement.
>
> It is also suggestive that the substances which are intermediate in their chemical properties between the metals and the non-metals, such as phosphorus, selenium, and tellurium possess properties with regard to metallic conduction intermediate between those of

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[11] Thomson J.J. 1891, p. 152.

[12] See Thomson J.J. 1891, p. 152: "The fact that the metals are solids is no reason why the conductivity through them should not be electrolytic in its nature, for there are many instances of solid electrolytes; thus Lehmann has shown that electrolysis takes place through a crystal of silver iodide placed between silver electrodes without any change being perceptible in the shape or size of the crystal, though it was watched through a microscope whilst the current was passing."

> metals and electrolytes, […] The changes in the chemical properties of the substance seem to proceed step by step with the changes in their behaviour with regard to electrical conduction."[13]

Thomson claimed that his theory could free Maxwell's electromagnetic theory from some theoretical faults. For instance, physicists knew that the "opacity of thin metal films is enormously less than that theory would indicate": in other words, the optical transparency of metal slices was greater than expected on the basis of Maxwell's theory. The expected behaviour would stem from the hypothesis that "conductivity of the film for very rapid electrical vibrations which constitute light were the same as for steady currents".[14] The actual reaction of metals to slow variations of electric fields (ordinary conduction currents) was quite different from the reaction to high frequency fields of electromagnetic waves: therefore, the previous hypothesis could not be correct. On the contrary, Thomson's *electrolytic* theory of matter could account for that behaviour which depended on the frequency of the fields. Electrolytes indeed behaved like dielectrics when submitted to high frequency oscillations, for instance light, and behaved like conductors when submitted to steady or slowly oscillating currents. In Thomson's view, the key of the puzzle was to be found in the similar behaviour of metals and electrolytes when they were submitted to fast oscillating fields and slowly oscillating fields.[15]

The electrolytic model appeared to be the suitable solution; Thomson concluded that "the processes concerned in metallic conduction are the same as those in electrolytic". The following pages of the paper exhibited a theoretical model well known to historians, widely discussed and considered as one of the most interesting attempts to go beyond Maxwell's theory in the comprehension of interactions between electricity and matter.[16] The description of the model started from a tube of force connecting two points *O* and *P* placed on the two opposite plates of a charged condenser, just before the discharge. When the condenser underwent the discharge through

[13] Thomson J.J. 1891, p. 153.
[14] Thomson J.J. 1891, p. 154.
[15] See Thomson J.J. 1891, p. 154: "On the view we have taken of metallic conduction, since the process of dissociation and recombination takes a finite time, if the polarisation is reversed in less than this time, the old polarisation will not have had time to disappear before the new is superposed, and the metal will, under these circumstances, behave more like an insulator than a conductor."
[16] See, for instance, Buchwald J.Z. 1985a, pp. 49-53, Falconer I. 1987, pp. 255-6, Darrigol O. 2000, pp. 299-300, and Smith G.E. 2001, pp. 32-4.

the gas filling the gap between the plates, Thomson imagined multiple breaks in the tubes of force.[17]

After those remarks on matter and electricity, wherein tubes of force represented a sort of physical and conceptual bridge between them, Thomson turned his attention to a different target. Tubes of force became the starting point of a mathematical theory; in the end, the already known electromagnetic equations would have emerged from that model. The reconstruction of Maxwell's electromagnetic theory in terms of tubes of force had already been put forward by Poynting in 1885[18]; differently from Poynting, Thomson started from the vector "electric displacement" ***D*** and associated "the number of unit tubes parallel to the axes of *x*, *y*, *z* respectively" to the components *f*, *g*, *h* of ***D***. The tubes could be in motion with a velocity of components *u*, *v* and *w*. Then he considered "the increase in the number of tubes parallel to *x* which occur in a time δt in an element of volume *dx*, *dy*, *dz*". This increase was split in two parts: "the increase due to the passage of the tubes across the faces of the element" and "the increase due to the deformation of the tubes inside the element". The total increase was nothing else but $d\mathbf{D}/dt$: in particular, for the *x*-component,

$$\frac{df}{dt} = \frac{d}{dy}(gu - fv) - \frac{d}{dz}(wf - uh) - \rho u \ ,$$

under the condition

$$\frac{df}{dx} + \frac{df}{dy} + \frac{df}{dz} = \rho .$$

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[17] See Thomson J.J. 1891, p. 155: "The molecules AB, CD, … of the intervening gas will be polarised by the induction, the tubes of force connecting the atoms in these molecules pointing in the negative direction; as the strength of the field increases the tube in the molecule AB will lengthen and bend towards the tube OP, until when the field is sufficiently strong the molecular tube runs up into the tube OP. The tubes then break up into two tubes OA and PB, and the tube OA shortens to molecular dimensions. The result of this operation is that the tube PO has shortened to PB, and the atoms O and A have formed a molecule. This process is then continued from molecule to molecule until the tube PO has contracted to molecular dimensions."

[18] See chapter 8 of the present book.

The left-side term corresponded to the displacement current and the last term in the right side corresponded to a conduction current. The equation was formally equivalent to the electromagnetic equation

$$\nabla \times \boldsymbol{H} = 4\pi\left(\dot{\boldsymbol{D}} + \rho\boldsymbol{v}\right),$$

provided that the components of the magnetic force depended on the electric displacement and on the velocity of tubes of force in accordance with the following equation:

$$\left.\begin{array}{l}\alpha = 4\pi(hv - gw)\\ \beta = 4\pi(fw - hu)\\ \gamma = 4\pi(gu - fv)\end{array}\right\}.^{19}$$

The vector language allows us to better appreciate the relationship between the magnetic force and the motion of tubes of electric induction: $\boldsymbol{H} = \boldsymbol{v} \times \boldsymbol{D}$.

The comparison between the already known electromagnetic equations and his model of moving tubes of force led Thomson to an easy identification of the momentum density $\boldsymbol{D} \times \boldsymbol{B}$ carried by the moving tubes. It was "at right angles to the tube and to the magnetic force produced by it" and corresponded to Poynting's energy flux. In addition there was an electromotive force $-\boldsymbol{v} \times \boldsymbol{B}$, produced by moving tubes, which was "at right angles to both the direction of motion of the tube and the magnetic force produced by it". The curl of the electromotive force, provided that $\nabla \bullet \boldsymbol{B} = 0$, led to the second circuital equation $\nabla \times \boldsymbol{E} = -\dot{\boldsymbol{B}}$.

> "Collecting these results, we see that a tube of electrostatic induction when in motion produce (1) a magnetic force at right angles to the tube and the direction of motion, (2) a momentum at right angles to the tube and the magnetic force produced by it, (3)

[19] See Thomson J.J. 1891, pp. 156-7: "In other words, a moving tube of electrostatic induction may be regarded as producing a magnetic force at right angles both to itself and the direction in which it is moving, and whose magnitude is 4π times the strength of the tube multiplied by its velocity at right angles to its direction. The direction of the force is such that the magnetic force and rotation from the direction of motion to that of the tube are related like translation and rotation in a right-angled screw."

> an electromotive intensity at right angles to the direction of motion of the tube and the magnetic force produced by it."[20]

That model took into account only the simple case of tubes moving with the same velocity, but Thomson tried to generalise his procedure: summations and mean values appeared in the equations. In particular, imagining positive tubes moving in one direction and negative tubes moving in the opposite direction, if "there are as many moving in one direction as the opposite", we would not have any resultant electromotive force.[21]

Thomson's next theoretical step is quite interesting: he tried to account for the attraction between two conducting wires carrying electric currents in the same direction. Starting from the momentum of the tubes of force approaching a conductor, and then transferred to the conductor itself, he got "the ordinary expressions for the force acting on a conductor carrying a current in a magnetic field". He interpreted the interaction between two currents as the interaction between a current and the magnetic field produced by the other current. Going beyond the equations and focusing on the motion of tubes of force, the attraction between the two currents was interpreted as the effect of an unbalanced collapse of tubes of force on the currents. He imagined two parallel currents *A* and *B* and tubes of force collapsing on them. Let us imagine *A* placed on the left and *B* on the right in a given plane surface. Tubes coming from the left towards *A* hand over their momentum to *A*, but some tubes coming from the right are prevented from collapsing on *A* by the presence of *B*. In the same way, some tubes coming from the left are prevented from collapsing on *B* by the presence of *A*. The current *A* receives more momentum from the left than from the right and *B* receives more momentum from the right than from the left. The result is that the electric currents *A* and *B* approach to each other.[22]

This explanation echoed old mechanical theories, in particular, as Thomson himself acknowledged, "Le Sage theory of gravitation".[23] In that theory

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[20] Thomson J.J. 1891, p. 159.

[21] See Thomson J.J. 1891, p. 161: "We see then that when the electromagnetic field is in a steady state, the motion of the tubes of electrostatic induction in the field will be a kind of shearing of the positive past the negative tubes, the positive tubes moving at one direction, and the negative at an equal rate in the opposite. When, however, the field is not in a steady state, this ceases to be the case, and then the electromotive forces due to induction are developed."

[22] See Thomson J.J. 1891, pp. 162-3.

[23] I have already mentioned Le Sage's theory in chapter 1. It was one of the scientific outcomes of the eighteenth century; it was a mechanical explanation for gravitation, quoted sometimes by Maxwell, mainly in the context of his kinetic theory of gases, and in

cosmic bodies behaved like shields with regard to a flux of hypothetical fast particles coming from the outer space. A fraction of particles flowing towards the Earth could not avoid hitting the Moon; conversely, the Earth would shield the Moon from a fraction of the particles flux. The net effect should be an attraction between Earth and Moon. The quotation of Le Sage's theory, which was quite a questionable theory, far from the scientific standards of the late nineteenth century, represents however a meaningful clue. Thomson was looking for a new theoretical framework, in order to explain the electromagnetic phenomena and the structure of matter. He shared Maxwell's general conceptual model, essentially based on matter and motion, but rejected the specific conceptual model for matter and energy which Maxwell had outlined. In that context, even an old theory based on particles and motion, however naive and simplified it might be, could be a reference for the specific theoretical model Thomson was looking for.[24]

It is worth mentioning that, in 1888, in the book *Applications of dynamics to physics and chemistry*, J.J. Thomson had outlined a very general *dynamical* representation of the physical world. In particular, he had pursued the project of reduction of all kinds of energy to kinetic energy. He imagined "potential energy of any system as kinetic energy arising from the motion of systems connected with the original system", in order "to explain natural phenomena by means of the properties of matter in motion". The concept of potential energy seemed to Thomson quite unsatisfactory and unable "in the strict sense of the term, to explain anything". The question was: what is the matter "whose motion constitutes the kinetic energy" of the auxiliary system, corresponding to the potential energy of the original system? That matter, Thomson answered, could be "either that of parts of the system, or the surrounding ether, or both; in many cases we should expect it to be mainly the ether".[25]

Some decades ago, J.J. Thomson's theoretical model was described as a *mechanical* world-view, because of the widespread use of "images", for

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correspondence with the word "Atom" in the *Encyclopaedia Britannica*. See Maxwell J.C. 1875, pp. 473-5. That theory had raised some interest at the turn of the nineteenth century and even in the course of the same century, but, at the same time, it had been sharply criticised. See Jammer M. 1957, pp. 192-4.

[24] As Buchwald pointed out some decades ago, Thomson's re-interpretation of Maxwell's theory retained some typical faults of the latter. In particular, Buchwald found that Thomson's theoretical model could not account for the interaction between aether and matter, as well as for the laws of the electric conduction in metals. See Buchwald J.Z. 1895a, p. 53. I find that J.J. Thomson did not really managed to account for conduction better than Maxwell; however, he undertook a step towards a better comprehension of the link between electric fields and matter.

[25] Thomson J.J. 1888, pp. 14-5.

instance molecules and tubes of force. Indeed, Thomson looked upon visual models and analogies not only as useful illustrations of a given class of phenomena, but also as heuristic tools "suggesting further expansions of the theory". Nevertheless, I do not find that making use of visual models suited only a specific world-view.[26] As I will show in some detail in the next chapter, I find in J.J. Thomson a strong commitment to integration and unification, and a deep trust in the theoretical and pedagogical power of conceptual models.

[26] The title of Topper's 1980 paper "To Reason by means of Images: J.J. Thomson and the Mechanical Picture of Nature", suggests the link between *mechanicism* and *imagery*. I agree with Topper on the statement that J. J. Thomson was committed to "the creation of a unified picture of nature integrating matter, ether, energy, electricity and magnetism". (Topper D.R. 1980, pp. 32, 38 and 40). I cannot agree with the attempt to insert all *Victorian-age* scientists in the class of a mechanical world-view. On this attempt, see Siegel D.M. 1981, p. 263.

After two years, J.J. Thomson published a book, Recent Researches in Electricity and Magnetism, whose title-page contained the addendum *INTENDED AS A SEQUEL TO PROFESSOR CLERK-MAXWELL'S TREATISE ON ELECTRICITY AND MAGNETISM*. He introduced himself to readers as an upholder of Maxwell, who pursued the accomplishment of Maxwell's scientific enterprise. As I will show, he developed Maxwell's theory along new directions which perhaps we cannot qualify as really *Maxwellian*.[1] In the first lines of his "Preface", he noted that twenty years had elapsed since the first edition of Maxwell's *Treatise*, and "great progress has been made in these sciences". However, he acknowledged that the progress was due to "the influence of the views set forth in that Treatise". Thomson's book was explicitly devoted to students and two aims were explicitly pointed out: the description of recent advancement in physics and the reference to Maxwell's Treatise "as the source from which they learn the great principles of science". In other words, he claimed that he would have thrown light on recent developments in electromagnetism, and would have made use of Maxwell's theory in order to enlighten them.[2]

He made reference to the chapter "General Equations of the Electromagnetic Field", wherein Maxwell had collected the most important equations of his theory. In spite of this reference to equations, he immediately pointed out that his approach to the electric field was "geometrical and physics rather than analytical". In opposition to a formal, or purely mathematical, approach to Maxwell's theory, he put forward a conceptual or theoretical approach. Theoretical physics rather than mathematical physics was considered by Thomson the best way to really understand physics. He placed his trust in models, pictures and mental representations: they were elements neither mathematical nor experimental. They would have helped students in their "mental training" in physics. A purely mathematical approach seemed to Thomson particularly misleading, in particular when the content of Maxwell's theory was involved.[3]

[1] We do not know whether Maxwell would have appreciated J.J. Thomson's accomplishment; we cannot answer this question but we know that the question concerns all late nineteenth century scientists who claimed to be *Maxwellian*.

[2] See Thomson J.J. 1893, p. v: "I have adopted exclusively Maxwell's theory, and have not attempted to discuss the consequences which would follow from any other view of electrical action. I have assumed throughout the equations of the Electromagnetic Field given by Maxwell in the ninth chapter of the second volume of his Treatise."

[3] Thomson J.J. 1893, pp. v-vi. See p. v: "I have been induced to dwell on this because I have found that students, especially those who commence the subject after a long course of

He claimed that mathematics should be looked upon as an intellectual tool, able to develop "the suggestions afforded by other and more physical methods". Once again, as an instance of a physical method opposed to an analytical method, he quoted Faraday's tubes of force. According to J.J. Thomson, the model of tubes of force was "distinctly physical" and was more effective than "symbols and differential equations". It was "more suitable for obtaining rapidly the main features of any problem"; only after this first physical approach, a problem could undergo a subsequent mathematical approach; only at that stage, the latter became useful and necessary.

> "In a research in any of the various fields of electricity we shall be acting in accordance with Bacon's dictum that the best results are obtained when a research begins with Physics and ends with Mathematics, if we use the physical theory to, so to speak, make a general survey of the country, and when this has been done use the analytical method to lay down firm roads along the line indicated by the survey."[4]

He questioned the heuristic power of mathematical physics and, in general, of researches devoted to "the manipulation of a large number of symbols in the hope that every now and then some valuable result may happen to drop out." Thomson did not underestimate the positive role of mathematics as a "thought-saving machine" but was worried about the attempts to replace physical comprehension with mathematical operations. He preferred a "rough solution", aiming at the essential features of a problem, rather than at "a complete solution arrived at by the most recent improvements in the higher analysis".[5]

Three of the four pages of the "Preface" were devoted to a celebration of the physical insight, and to a warning about the danger of a purely mathematical approach to physics. According to Thomson, theoretical physics, together with its conceptual devices, had to be the core of physical research and had to be the first step both in creating physics and teaching physics. Mathematics and experiments represented the second step: they, together with their specific devices, were devoted to the final appraisal and

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mathematical studies, have a great tendency to regard the whole of Maxwell's theory as a matter of the solution of certain differential equations, and to dispense with any attempt to form for themselves a mental picture of the physical processes which accompany the phenomena they are investigating."

[4] Thomson J.J. 1893, p. vi.

[5] Thomson J.J. 1893, p. vii.

acceptance of a theory. I believe that I will not betray the meaning of Thomson's passages if I synthesise them saying that scientific enterprise splits into two subsequent steps: first theoretical creation and then mathematical and experimental justification.

> "It is no doubt true that these physical theories are liable to imply more than is justified by the analytical theory they are used to illustrate. This however is not important if we remember that the object of such theories is suggestion and not demonstration. Either Experiment or rigorous Analysis must always be the final Court of Appeal; it is the province of these physical theories to supply cases to be tried in such a court."[6]

He stated that he had devised the first chapter in order to focus immediately on "the distinctive feature" of Maxwell's theory, namely the equivalence between electric currents in conductors and electric currents in dielectrics. In particular, the time variation of electric polarisation in dielectrics should produce the same effects as conduction currents. Nevertheless, if we detached the first passages of the first chapter from the above reported methodological issues, the former would appear quite surprising. Thomson expressed his appreciation for a theoretical model of electricity in competition with Maxwell's model: electricity represented as a fluid. Thomson thought that, from the historical point of view, that model had been useful, for it had tried to give a clear and definite representation of electricity. From the conceptual point of view, it deserved attention because of its intuitiveness: it satisfied Thomson's requirement of being a concrete and intelligible representation.[7]

Indeed, the quotation of a theory to be found far from Maxwell's theoretical horizon also aimed at criticizing Maxwell: the criticism was not addressed towards Maxwell's general framework but towards some concept which Thomson found too abstract. The first of these concepts was the "displacement current", already criticized by Poynting and Thomson himself.[8]

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[6] Thomson J.J. 1893, p. vii. It is worth noting that Thomson's trust in the *language* of creation, rather than in the *language* of justification, involved both the context of research and the context of teaching.

[7] See Thomson J.J. 1893, p. 1: "The influence which the notation and ideas of the fluid theory of electricity have ever since their introduction exerted over the science of Electricity and Magnetism, is a striking illustration of the benefits conferred upon this science by a concrete representation or '*construibar vorstellung*' of the symbols, which in Mathematical Theory of Electricity define the state of the electric field."

[8] See chapters 8, 11 and 12 of the present book.

He regretted that "the descriptive hypothesis" of electric displacement, used by Maxwell in order "to illustrate his mathematical theory", had puzzled many scholars, who had found in it a concept "neither so simple nor so easy of comprehension as the old fluid theory". This concept had been a hindrance on the path to a full appreciation of Maxwell's theory. For those reasons he offered "an alternative method of regarding the processes occurring in the electric field", mathematically equivalent to Maxwell's but more "useful". We have already encountered this alternative method: Faraday's lines or tubes of force. A short account of Faraday's theoretical model followed in Thomson's book. He focused on two main features of tubes of force: "their tendency to contract" and "the lateral repulsion which similar tubes exert on each other". Nevertheless, the interpretation of tubes of force immediately raised some queries: had they to be represented as a sort of rearrangement of matter when undergoing electric force, or as a sort of materialisation of electric force itself? In other words, do we have to look upon tubes of force as "chains of polarized particles in the dielectric" or as "something having an existence apart from the molecules of the dielectric"? Thomson chose the second alternative: tubes of force had to be considered as structures endowed with their specific existence, independently from the presence of ordinary matter.

> "It is this latter view of the tubes of electrostatic induction which we shall adopt, we shall regard them as having their seat in the ether, the polarisation of the particles which accompanies their passage through a dielectric being a secondary phenomenon. We shall for the sake of brevity call such tubes Faraday Tubes."[9]

Thomson's choice can be summarised in the following way: first we assume tubes of force, then polarisation. Following an explicit analogy with the dynamics of fluids, he distinguished between open tubes and closed tubes: he imagined that the former connected matter to matter, while the latter, embedded in aether, could be found even far from matter. The former dealt with electricity, the latter dealt with the structure of aether, in a way to be specified later. In some way, closed tubes were in connection with the distribution of energy throughout aether: that distribution transformed aether into a discrete or "fibrous" structure.[10]

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[9] Thomson J.J. 1893, p. 2.

[10] See Thomson J.J. 1893, p. 2: "In addition to the tubes which stretch from positive to negative electricity, we suppose that there are, in the ether, multitudes of tubes of similar constitution but which form discrete closed curves instead of having free ends; we shall

An interesting difference between Faraday's model and J.J. Thomson's model had already been pointed out by Thomson himself in 1891: Faraday had taken into account both electric and magnetic tubes of force while Thomson took into account only electric tubes of force, being magnetic phenomena explained by the motion of electric tubes. Open tubes connected positive electricity to the corresponding amount of negative electricity and they all had the same strength. We can imagine a sea of open tubes of force of the same kind always connecting quantities of electricity of the same kind. In other words, unit tubes of force connect couples of unit electric charge. This is an important issue in Thomson's theory, for it points out, once again, the difference between mathematical physics and theoretical physics. From the mathematical point of view, nothing would prevent us from imagining "tubes of continually diminishing strength"; in the theory he was conceiving, those tubes were "no longer merely a form of mathematical expression, but as real physical quantities having definite sizes and shapes".[11]

Thomson was moving away from Maxwell's specific theoretical models of matter and energy, even though he shared the general outline of Maxwell's electromagnetic theory. In his 1893 book, he accomplished the conceptual drift towards a discrete model for matter, electricity and fields. Each tube of force had its specific identity and was associated to each unit of matter and electricity; matter, electricity and energy were endowed with their specific discreteness and identity. One unit of matter corresponded to one unit of electricity, and one unit of tube of force connected units of matter-charge to each other.[12] A continuous distribution of matter was excluded, at least in the case of ordinary matter; aether also probably had a structure and that structure dealt with the distribution of energy throughout it.

> "If we take this view, we naturally regard the tubes as being all of the same strength, and we shall see reasons for believing that this strength is such that when they terminate on a conductor there is

call such tubes 'closed' tubes. The difference between the two kinds of tubes is similar to that between a vortex filaments with its ends on the free surface of a liquid and one forming a closed vortex ring inside it. These closed tubes which are supposed to be present in the ether whether electric forces exist or not, impart a fibrous structure to the ether."

[11] Thomson J.J. 1893, pp. 2-3.

[12] Navarro's recent thesis, Thomson "always thought that discrete conceptions of matter were contrary to common sense", seems to me quite puzzling. (Navarro J. 2005, p. 261). I think that Thomson's papers and books show a peculiar interplay between continuous and discrete theoretical models: what appears as continuous at a macroscopic level, shows a discrete structure at the microscopic level. J.J. Thomson imagined Faraday tubes as the structure of aether, even in the absence of electric fields: in some way, tubes were the discrete structure of aether, namely its *granular*, or "molecular", structure.

> at the end of the tube a charge of negative electricity equal to that which in the theory of electrolysis we associate with an atom of monovalent element such as chlorine.
>
> This strength of the unit tubes is adopted because the phenomena of electrolysis show that it is a natural unit, and that fractional parts of this unit do not exist, at any rate in electricity that has passed through an electrolyte. We shall assume in this chapter that in all electrical processes, and not merely in electrolysis, fractional parts of this unit do not exist."[13]

Thomson put forward a discrete structure for matter, for electricity and for energy, provided that the tubes of force represented a sort of substantialisation of the electromagnetic energy stored in the field. The deep connection among matter, electricity and tubes of force gave rise to a draft of electric theory of matter. Even ordinary matter was embedded in a net of tubes of force connecting atoms to each other, in order to produce those structures which we call molecules. Inside a molecule, Thomson saw short tubes of force keeping atoms close to each other, in order to assure that the complex structure of a molecule was stable: in this case, the length of the tubes were of the same order of molecular dimensions. On the contrary, if the length of the tubes was far greater than molecular dimensions, we would have in front of us atoms "chemically free".[14] Not only was matter embedded in a net of tubes of force but even aether was. Indeed, tubes of force were not a mere materialisation of electric forces: Thomson imagined a sea of tubes of force spread throughout the aether even without any electric force. There was a distribution of tubes corresponding to an unperturbed state. The effect of electric forces was an overbalance in the sea of tubes: electric forces made tubes move towards a specific direction. The drift of the tubes, driven by the electric forces, gave rise to electrodynamic effects, for instance the establishment of a magnetic field.[15]

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[13] Thomson J.J. 1893, p. 3.

[14] Thomson J.J. 1893, p. 3.

[15] See Thomson J.J. 1893, p. 4: "The Faraday tubes may be supposed to be scattered throughout space, and not merely confined to places where there is a finite electromotive intensity, the absence of this intensity being due not to the absence of the Faraday tubes, but to the want of arrangement among such as are present: the electromotive intensity at any place being thus a measure, not of the whole number of tubes at that place, but of the excess of the number pointing in the direction of the electromotive intensity over the number of those pointing in the opposite direction."

All electromagnetic phenomena could be reduced to the motion of tubes or "to changes in their position or shape". The "molecular" aspect of Thomson's theory was explicitly stated: this is the *theoretical* core of the theory, giving sense to equations and mathematical deductions. We have in front of us an *atomic* theory of matter joined to an *atomic* theory of electricity and to an *atomic* theory of energy. Just as in Poynting's theory, tubes of force were the *hardware* associated to energetic processes. This interpretation is also supported by a very meaningful statement: tubes must undergo a sort of Law of Conservation. They can be neither created nor destroyed. A symmetry between matter and energy was explicitly assumed: in Thomson's theoretical model, the *sea* of tubes of force behaved as a *cloud* of molecules in a gas.

> "Thus, from our point of view, this method of looking at electrical phenomena may be regarded as forming a kind of molecular theory of Electricity, the Faraday tubes taking the place of the molecules in the Kinetic Theory of Gases: the object of the method being to explain the phenomena of the electric field as due to the motion of these tubes, just as it is the object of the Kinetic Theory of Gases to explain the properties of a gas as due to the motion of its molecules. The tubes also resemble the molecules of a gas in another respect, as we regard them as incapable of destruction or creation."[16]

In this passage, a statistical aspect of Thomson's theory emerged, an aspect which connected electromagnetism to thermodynamics. The macroscopic picture was the statistic effect of a great number of microscopic events: microscopic, discrete structures gave rise to a macroscopic, continuous picture. Thomson was strongly committed to a very general issue, which flowed through the specific features of his theory like an enduring conceptual stream. This issue was the pursuit of the unity of physics. The theoretical model of "molecular" electric tubes of force allowed him to realize at least a certain degree of unification.[17]

In Thomson's model, energy was really linked to tubes of force, in particular to aether contained in them and surrounding them: energy was kinetic energy of aether, of both rotational and translational kind. Rotational

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[16] Thomson J.J. 1893, p. 4.

[17] I agree with Navarro when he stresses J.J. Thomson effort to attain a unified representation of physical and chemical phenomena, but I do not find that the "metaphysics of the continuum" was the unifying element. See Navarro J. 2005, pp. 272-3.

kinetic energy was associated to the potential energy of the electrostatic field and translational energy was associated to the energy of the magnetic field, without any other specification. Probably Thomson, in that moment, was not interested in the debate on the models of aether, which took place in those years. His model seems quite similar to Heaviside's model of rotational elastic aether, already discussed in the present book, but there is an important difference.[18] Heaviside took into account both the elastic answer and the translation of the medium; Thomson spoke only of rotations and translations without any elastic resistance: energy was only kinetic in its nature.

> "We suppose that associated with the Faraday tubes there is a distribution of velocity of the aether both in the tubes themselves and in the space surrounding them. Thus we may have rotation in the ether inside and around the tubes even when the tubes themselves have no translatory velocity, the kinetic energy due to this motion constituting the potential energy of the electrostatic field: while when the tubes themselves are in motion we have super-added to this another distribution of velocity whose energy constitutes that of the magnetic field."[19]

The association of energy to tubes of force was indeed quite vague: in the last lines of the chapter, Thomson acknowledged that his theory was "geometrical rather than dynamical". He had not tried to offer a definite physical explanation for the nature of Faraday's tubes. He relied on "the analogies which exist between their properties and those of tubes of vortex motion": those analogies "irresistibly suggest that we should look to a rotatory motion in the ether for their explanation". Thomson had simply taken tubes "for granted": that choice was justified by the fact that it allowed him to give "a vivid picture of the processes occurring in the electromagnetic field", and to attain a better comprehension of "the relations which exist between chemical change and electric action".[20]

Energy could be transferred from aether to matter when tubes of force collapsed onto matter. The interaction took place between a single tube of force and a single atom, and that interaction affected the internal energy of the atom. Even atomic internal energy was imagined by Thomson as being kinetic in its nature. The variation of energy of atoms *shaken* by tubes of

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[18] See chapter 10 of the present book.
[19] Thomson J.J. 1893, pp. 4-5.
[20] Thomson J.J. 1893, p. 52.

force was different for atoms of different elements. At the same time, the internal energy of molecules depended on the arrangement of tubes of force connecting the atoms belonging to that molecule. The link between tubes of force and electrification inside molecules suggested that the atom placed at one end of the tube possessed positive electric charge and the atom placed at the other end possessed negative electric charge. The collapse of a tube of force on a molecule affected the distribution of electricity among the atoms there contained, "as if the atoms of different substances attracted electricity with different degrees of intensity". Thomson assumed that, after having reached a conductor, tubes of force "shrink to molecular dimensions", and then interact with the short tubes already existing.[21] The detailed explanation of the way tubes of force interacted with conductors can be found in a subsequent section of the chapter. There, Thomson put forward the model already introduced in his 1891 paper, enriched with some eloquent pictures.[22] We find once again the equivalence between electric conduction in solid bodies, in liquids and in gases, accompanied by descriptions of experiments supporting this view. He thought that in a galvanic cell there was a sort of symmetry between electrolytic decomposition and production of electric currents: in particular, "the production of a current by a cell is the reverse process to the decomposition of an electrolyte by a current". In terms of tubes of force, "the chemical processes make a long Faraday tube shrink to molecular dimensions, in the former they produce a long tube from short molecular tubes".[23]

With regard to dielectrics and electric actions in dielectrics, electric "polarisation" was the new key word, a word introduced both in analogy and in opposition to Maxwell's electric "displacement".[24] He considered the former

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[21] Thomson J.J. 1893, p. 5.

[22] See Thomson J.J. 1893, p. 45-7. See, in particular, p. 44: "The atoms in the molecule of a compound which is chemically satured are already connected by the appropriate number of tubes, so that no more tubes can fall on such atoms. Thus on this view the ends of the tube of finite length are on free atoms as distinct from molecules, the atoms in the molecule being connected by short tubes whose lengths are of the order of molecular distances. Thus, on this view, the existence of free electricity, whether on a metal, an electrolyte, or a gas, always requires the existence of free atoms. The production of electrification must be accompanied by chemical dissociation, the disappearance of electrification by chemical combination; in short, on this view, changes in electrification are always accompanied by chemical changes. This was long thought to be a peculiarity attaching to the passage of electricity through electrolytes, but there is strong evidence to show that it is also true when electricity passes through gases." The model has widely been discussed in the secondary literature. See, chapter 12 of the present book.

[23] Thomson J.J. 1893, pp. 44-5 and 48-9.

[24] See Thomson J.J. 1893, p. 6: "The 'polarisation' is defined as follows: Let *A* and *B* be two neighbouring points in the dielectric, let a plane whose area is unity be drawn between

as mathematically equivalent to Maxwell's "displacement" but endowed with "a different physical interpretation". Polarisation was also expressed in terms of tubes of force, more specifically in terms of an unbalance of tubes flowing in opposite directions. The difference between mathematical physics and theoretical physics was stressed with particular care.

> "The polarisation is evidently a vector quantity and may be resolved into components in the same way as a force or a velocity; we shall denote the components parallel to the axes of *x, y, z* by the letters *f, g, h*; these are mathematically identical with the quantities Maxwell denotes by the same letters, their physical interpretation however is different."[25]

For tubes could neither be created nor destroyed, a change in the net number of tubes crossing a unit area was due to their motion or deformation. On the track of his 1891 paper, Thomson assumed that the time variation of the vector "polarisation" was due to three causes: translation of tubes, variation of tubes density and variation of the tubes direction. The total change in the components *f, g, h* of the vector "polarisation", namely $\delta f = \delta f_1 + \delta f_2 + \delta f_3$ together with the other two components, led to the equation

$$\frac{dD}{dt} = \nabla \times (\boldsymbol{v} \times \boldsymbol{D}) - \rho \boldsymbol{v} ,$$

provided that $\nabla \bullet \boldsymbol{D} = \rho$. Comparing the above equation with the known electromagnetic equation $\nabla \times \boldsymbol{H} = 4\pi \boldsymbol{J}$, the relationship $\boldsymbol{H} = 4\pi \boldsymbol{v} \times \boldsymbol{D}$ followed.[26]

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these points and at right angles to the lines joining them, then the polarisation in the direction *AB* is the excess of the number of the Faraday tubes which pass through the unit area from the side *A* to the side *B* over those which pass through the same area from the side *B* to the side *A*."

[25] Thomson J.J. 1893, p. 6.

[26] See Thomson J.J. 1893, pp. 7-8. The change in *f*, due to the first cause, was $\delta f_1 = -\left(u\frac{df}{dx} + v\frac{df}{dy} + w\frac{df}{dz} \right)\delta t$, where *u*, *v* and *w* were "the components of the velocities of those tubes at any point". For the *x*- component, the change in *f* due to the

This result was consistent with Thomson's hypothesis on the nature of magnetic force as an effect of the motion of tubes of force. In addition, the computation of the momentum density led to an expression "proportional to the amounts of energy transferred in unit time across unit planes at the right angles to the axes of *x*, *y*, *z*", in accordance with "Poynting's theory of the transfer of energy in the electromagnetic field". Finally he showed that, when electromagnetic intensity was wholly due to the motion of the tubes, "the tubes move at right angles to themselves with the velocity $1/\sqrt{\mu K}$, which is the velocity with which light travels through the dielectric".[27]

In a subsequent section, "Electromagnetic Theory of Light", Thomson tried to give a more detailed account of propagation of light in terms of tubes of force. Even in this case, theoretical physics was allied with physics teaching: he thought that Faraday's tubes of force could help to "form a mental picture of the processes which on the Electromagnetic theory accompany the propagation of light".[28] The propagation of a plane wave could be interpreted as "a bundle of Faraday tubes" moving at right angles to themselves and producing a magnetic force oriented at right angles with regard to both the direction of the tubes and the direction of motion.

> "If there is no reflection the electromotive intensity and the magnetic force travel with uniform velocity v outwards from the plane of disturbances and always bear a constant ratio to each other. By supposing the number of tubes issuing from the plane source per unit time to vary harmonically we arrive at the conception of a divergent wave as a series of Faraday tubes travelling outwards with the velocity of light. In this case the places of maximum, zero and minimum electromotive intensity will correspond respectively to places of maximum, zero and minimum magnetic force."[29]

Going beyond the specific features of J.J. Thomson's model of propagation, I would like to focus on the fundamental theoretical issue

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second cause was $\delta f_2 = -f\left(\frac{dv}{dy} + \frac{dw}{dz}\right)\delta t$. Similar expressions can be written for *y* and *z* components. The third contribution (for the x-component) was $\delta f_3 = \left(g\frac{du}{dy} + h\frac{du}{dz}\right)\delta t$.

[27] Thomson J.J. 1893, p. 9.
[28] Thomson J.J. 1893, p. 11.
[29] Thomson J.J. 1893, p. 42.

concerning the nature of light: starting from Maxwell's electromagnetic fields represented as stresses propagating through a continuous, solid medium, Thomson arrived at a representation of fields as a sea of discrete units carrying energy and momentum. The wave theory of light, then a well-established theory, seemed violently shaken by a conception which echoed ancient, outmoded theories.

> "This view of the Electromagnetic Theory of Light has some of the characteristics of Newtonian Emission theory; it is not, however, open to the objections to which that theory was liable, as the things emitted are Faraday tubes, having definite positions at right angles to the direction of propagation of the light. With such a structure the light can be polarised, while this could not happen if the things emitted were small symmetrical particles as on the Newtonian Theory."[30]

This passage sounds particularly interesting because of its reference to long-term debates and long-term processes deeply rooted in the history of science. The debate on the nature of light and the clash, continuously renewed, between continuous models and discrete models was the vivid background of J.J. Thomson's *Recent Researches*. The conceptual tension between the *discrete* and the *continuous* affected aether, matter, energy and electric charge. This tension is one of the elements of a unified view, where a new symmetry emerged between matter and energy: both were represented as discrete structure emerging from the background of a continuous medium.[31] Invisible, discrete, microscopic structures explained the properties of apparently continuous, macroscopic phenomena. J.J. Thomson transformed Maxwell's theory into a unified picture where atomic models of matter stood beside *atomic* models of fields.

As far as I know, forty years ago, R. McCormmach was the first to devote a historical study to J.J. Thomson's discrete model of electromagnetic radiation. He remarked that "it was only six years after Hertz's experiments" on electromagnetic waves that J.J. Thomson "proposed a modified version of the discredited emission theory".[32] Differently from

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[30] Thomson J.J. 1893, p. 43.

[31] It seems to me that a similar synthesis was put forward by Falconer. See Falconer I. 1987, p. 252.

[32] McCormmach R. 1967, p. 362. It is worth mentioning McCormmach footnote, wherein he specified that J.J. Thomson "did not argue against Maxwell's theory but rather against the way in which it was usually presented". These words curiously echoed Einstein's 1905 criticism about "Maxwell's electrodynamics – as usually understood at the present time"

McCormmach, I find that "certain similarities between material radiations and energetic electromagnetic waves" subsequently remarked by J.J. Thomson had a theoretical rather than an experimental root. I find that such a similarity was a development of his model of discrete tubes of force. I do not find that J.J. Thomson put forward discrete models for fields and electric charge because of his "pioneering work on cathode rays": in the context of cathode rays experiments, that discreteness was not claimed until 1897, whereas the theoretical model of tubes of force had already been repeatedly stated.[33]

McCormmach saw a conceptual "tension" between Thomson as the "laboratory researcher" and Thomson the "mathematical physicist". He put experimental results, namely "apparently universal manifestations of discreteness", on the one hand, and the "tradition of an assumed universality of continuous processes in nature", on the other hand. The fact is that both of them were theoretical traditions. I find that the tension "between the empirical discreteness of things and the theoretical ideal of continuous action" was a tension between two ideals. That all "things" experimentally showed a discrete nature was not true in general: McCormmach himself had pointed out that recent experiments had shown the continuous nature of electromagnetic radiation. I think that the empirical root of Thomson's model of tubes of force should not be overestimated. It is true that, from the theoretical point of view, "a structure of light based upon a universal, discrete unit of the field" matched with "the existence of a universal, discrete unit of electric charge", but the discrete model for radiation was put forward before the emergence of experimental evidences in favour of the discreteness of the electric charge. However, McCormmach captured two important features of J.J. Thomson's model: first, the tubes of force "are described as real, physical things, possessing all the dynamical properties of matter", and, second, they give "a fibrous structure" to aether.[34] Aether was endowed with an intrinsic double nature, consisting of a continuous background, shaped into a discrete structure by the kinetic processes which we call electromagnetic energy.

("Elektrodynamik Maxwells – wie dieselbe gegenwärtig aufgefasst zu werden pflegt"), in the first lines of his paper on electrodynamics of moving bodies. See Einstein A. 1905b, p. 891.

[33] McCormmach R. 1967, p. 366.

[34] See McCormmach R. 1967, pp. 362-3. It seems to me that, in some way, McCormmach acknowledged the existence of a theoretical "tension in the science of his day" when he mentioned the "universality" of both discrete and continuous models.

14. Joseph Larmor: swinging between different theoretical models

Formerly fellow of St. John's College, Cambridge, then professor of natural philosophy at Galway's (Ireland) Queen College, Joseph Larmor returned in 1885 to St. John's as a lecturer. In the same year he published a paper in the *Philosophical Magazine*, "On the Molecular Theory of Galvanic Polarization", where he outlined a discrete theoretical model for matter and electricity. Larmor started from the analogy between "the polarisation action of a galvanic cell" and "an electrical condenser of very large capacity". His theoretical reference was "Clausius well-known molecular theory" and, in particular, the interpretation of electrolytic phenomena in terms of "transfer through the fluid of the temporarily dissociated hydrogen and oxygen under the action of the electric force"[1]

The calculations of W. Thomson and others from experiments on electrolytic solutions gave an estimate of the dielectric layer thickness, which could be considered an estimate of molecular distances, namely the distance "at which the two electrified layers are held by molecular chemical forces". The result, approximately 10^{-8} meters, was in accordance with previous measurement undertaken by Helmholtz. In his interpretation of those results, in order to "carry the analysis of the phenomenon still further", Larmor placed his trust in a model of attractions and repulsion between charged particles.[2]

> "The polarization consists in the transfer of charged particles towards the electrode under the action of the electromotive force, and they are finally brought to equilibrium at a distance from the electrode, whose order of magnitude has just been determined. As these equally charged particles repel one another, they will tend to settle down in equidistant positions along the electrode surface. Instead therefore of two electrified sheets analogous to an ordinary condenser, we have really two sheets, one consisting of

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[1] See Larmor J. 1885, p. 422: "… in the course of time a layer of hydrogen particles with their positive charges accumulates in the immediate neighbourhood of the kathode plate, and the complementary layer of oxygen particles with their negative charges at the anode. Each of these layers will form a sheet, with positive or negative charge, lying close to the metal plate. On the plate will therefore appear an equal and opposite charge by induction. There is thus a double electric layer formed at each electrode; … A double layer of this kind forms an actual condenser, whose capacity is inversely proportional to the distances between its faces."

[2] Larmor J. 1885, p. 427; in the paper there is a misprint, 10^{-10} meters instead of 10^{-8} meters.

> equidistant electrified particles, and the other of the charges brought opposite to them on the electrode by induction. Each charged particle and its corresponding induced charge will be brought by their mutual attraction so close together that this attraction will just be balanced by the chemical forces which hold them apart."[3]

The distinction between "equidistant electrified particles" and "an ordinary condenser" involved two different models of matter and electricity for electrolytes and metals. The latter were associated to a continuous model, the former were associated to a discrete model. According to Larmor, in the electrolytes, when polarization increased, there was a corresponding increase in the number of electrified particles on unit area, and the surface density of the electric charge increased. When observed from distances greater than 10^{-8} meters, only that surface density was detectable and the finer structure of the sheets disappeared. In some way, the discrete model was placed at a higher level of comprehension with regard to the continuous model: the former corresponded to a deeper, more detailed knowledge of the structure of matter and electricity.

> "The pair of opposed surfaces which is thus arrived at, not uniformly charged, but each with a system of equal isolated point-charges arranged uniformly all over it, does not, of course, act as an ordinary condenser in the sense of producing a constant fall of potential in crossing it at all points, in position whose distances from it are of the same order as the distance between neighbouring particles. But when we compare two points on opposite sides at distances from it great compared with this latter distance, it is immaterial whether the distribution is supposed to be in isolated points or uniformly spread over the surfaces. Therefore, as regards points not in the immediate molecular neighbourhoods of the electrode, the effect of this polarization is still to produce simply a difference of potential on the two sides, which is just the same as if the charges were uniformly spread over the surfaces at the actual distance apart."[4]

Larmor made use of another method for the evaluation of molecular distances in electrolytes. He started from the previously estimated

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[3] Larmor J. 1885, p. 428.
[4] Larmor J. 1885, pp. 428-9.

thickness of the dielectric layer, and the distance "between neighbouring atoms when their effective mutual action becomes comparable to that between opposed atoms". The third entity was the unit element of charge or, in Larmor's terms, "the constant aggregate charge of a single atom or radical". The result was in accordance with W. Thomson and Helmholtz's estimates.[5] Larmor pointed out the fact that different procedures led to the same approximate value for the same entity: the intermolecular distance. But he did not consider the experimental side as the most important issue; he was mainly interested in the theoretical model, whose "strong evidence" followed from the mutual consistency of the different calculations. He stressed the importance of "that representation of the phenomenon which has formed the basis of the discussion": a representation in terms of particles of matter and particles of electricity. Molecules consisted of electrically charged components, held together by electric forces and chemical forces, whose nature Larmor did not specify.[6]

The hypothesis of electricity embedded in matter and, at the same time, matter as a system of two opposite electricity, allowed a unified account of both the nature of electricity and the constitution of matter. That theoretical model, although it was nothing more than a sketch or an "illustration", represented a bridge between physics and chemistry and, according to Larmor, it spared the scientists the trouble "to speculate on the deeper question of the relation of the material atom to its electrical charge."[7]

It is worth noting that the theoretical framework of Larmor's paper could appear unusual to a British *mathematician* trained at Cambridge and interested in electromagnetism: apart from W. Thomson, he quoted Clausius and Helmholtz. A discrete model of matter was in prominence and the approach to electric actions sounds more *Continental* than *Maxwellian*.[8]

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[5] Larmor J. 1885, p. 430.

[6] See Larmor J. 1885, p. 432: "We are not required to explain the manner in which this double layer at the surface of contact of two dissimilar substances is brought about. We may illustrate it by the rather crude hypothesis that each molecule of an electrolyte consists of a positively charged cation radical and a negatively charged anion radical held together by electrical forces, but partly also by their forces of chemical affinity, so as to be analogous to a magnetic molecule with north and south poles; that along the surface of the electrode these molecules are all turned into the same direction (polarized) by reason of the greater chemical affinity of one of their constituents for the matter of the electrode; and that they thus form a double sheet analogous to a magnetic shell."

[7] Larmor J. 1885, p. 432.

[8] The fact is that different interpretations of Maxwell's *Treatise* were offered by different teachers and coaches in Cambridge. Around 1880, students could be introduced to electromagnetism by W.D. Niven lectures or by E. Routh training for the Mathematical

Nevertheless, Maxwell was not beyond Larmor's conceptual horizon. After six years and some other short papers, Larmor published the paper "On the Theory of Electrodynamics" in *Proceedings of the Royal Society*. The new theoretical framework involved contiguous action and continuous models for matter and electricity: the first words of the paper made reference to the "electrical ideas of Clerk Maxwell". Larmor appeared particularly interested in those "mechanical models of electrodynamic action", which had led Maxwell to the conception of electric currents as closed paths. The peculiar entity of Maxwell's theory was the "displacement current" in dielectrics, which prevented electric charge, whatsoever it was, from heaping up. The electric condenser, which some years before had been associated to a microscopic double layer of electric particles, was interpreted in a different way. It became a *new* device, suitable to show that electric charge could not have its seat on the plates or, better, that the plates were not electrified at all.

> "The principle also requires that the electric displacement shall not lead to any accumulation of charge in the interior of the dielectric, therefore that it shall be solenoidal or circuital, its characteristic equation being of the type
>
> $$\frac{d}{dx}\left(K\frac{dV}{dx}\right)+\frac{d}{dx}\left(K\frac{dV}{dx}\right)+\frac{d}{dx}\left(K\frac{dV}{dx}\right)=0,$$
>
> where V is the electric potential, and K a dielectric constant. The surface density of the electricity conducted to a face of a condenser must neutralise the electric displacement, and not leave any residual effective electrification on the surface."[9]

However, he tried to insert those "remarkable conclusions", in a more general theoretical framework or, in his words, "a more general view of the nature of dielectric polarisation". He thought he had found that framework in Helmholtz's theory: Helmholtz had put forward a theoretical model different from Maxwell's, a model which, as Larmor himself acknowledged, dated back to Poisson's theory of magnetisation. Not only did Larmor try to

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Tripos. As Warwick pointed out, if Niven probably made reference to Maxwell's contiguous action, Routh made reference to "an action-at-a-distance theory of electrostatics and, quite probably, a fluid-flow theory of electrical conduction". See Warwick A. 2003, p. 333. See also chapter 2 of the present book. Both J.J. Thomson and Larmor were trained by the coach Routh. Niven was the scholar who took care of the second edition (1881) of Maxwell's *Treatise*, after Maxwell's death.

[9] Larmor J. 1891, p. 521.

interpret Maxwell's *displacement* current in terms of matter polarisation, but he chose the conceptual reference frame of a Continental scientist who had re-interpreted Maxwell's theory in terms of polarisations *superimposed* to the action at a distance. Differently from Helmholtz, Maxwell had imagined displacement currents in all dielectrics (aether included) in continuity with conduction currents, as a part of the same path.[10]

Larmor started from a plane condenser, an electric force F between the plates and a "specific inductive capacity" $\mu = 1 + 4\pi\kappa$. If σ was "the surface density of the charge conducted to a plate", the "effective electrification" was not σ but a certain σ', less than σ, because of the partial compensation caused by the polarisation of the intervening dielectric. In short, $\sigma' = \sigma - \kappa F$, or $\sigma = \sigma' + \kappa F$, where the unshielded charge σ corresponded to the capacity μ.[11]

As already pointed out by J.J. Thomson in 1885, Helmholtz's theoretical model required a polarisation *superimposed* to the original electric force, differently from Maxwell's conception of a chain of actions travelling through aether, dielectric and conductors, without any superposition. In Maxwell's picture, electric actions taking place in a conducting plate of the condenser transformed into electric actions taking place in the intervening dielectric, which then transformed into electric actions taking place in the opposite plate. It was the different way of action of the electric force, when crossing the threshold between different media, which gave rise to the effect known as *electric charge*. Larmor was *Maxwellian* with regard to the result but *Helmholtzian* with regard to the theoretical model: currents became circuital when "μ, and therefore κ, were infinite". Helmholtz's theory appeared to Larmor more general than Maxwell's; it seems that he had underestimated the deep conceptual difference between them. From the mathematical point of view, when κ is much greater than 1, $\mu \approx 4\pi\kappa$ and we have no partial compensation or superposed actions between the plates of

[10] The question was whether the "displacement current" could "make all electric currents circuital". Once again, the standard device to be taken into account was "a condenser which is charged through a wire connecting its two plates". See Larmor J. 1891, p. 522. The problem had been explicitly analysed by J.J. Thomson in his 1885 paper; see chapter 11 of the present book.

[11] The equation connecting all these quantities was written by Larmor in the form of $\sigma = \frac{\mu}{4\pi}F = \kappa F + \frac{1}{4\pi}F$. See Larmor J. 1891, pp. 522-3.

the condenser. This was the "limiting case" of Helmholtz's theory, which, in Larmor's view, approached Maxwell's theory.[12]

Larmor had started from *Helmholtzian* general conceptions and therefore had arrived at a *Helmholtzian* result: Maxwell's theory was a peculiar case of Helmholtz's general theory, corresponding to an endlessly high value of the dielectric constant. The theoretical difference between Helmholtz and Maxwell, specifically concerning the relationship between electric actions and matter (or media in general), was not pointed out by Larmor in this paper, apart from some hints in the last pages.[13] In Helmholtz's theory, the choice of the value 1 for the specific inductive capacity in the vacuum-aether would have led to $\kappa = 0$. As a consequence, any polarisation would have disappeared and the only thing left would have been "nothing but action at a distance". There was a sharp distinction between vacuum/aether and ordinary matter in Helmholtz's theory, whereas, in Maxwell's theory, there was only a difference in the degree (of inductive capacity) between aether (not vacuum) and matter. Nevertheless, Larmor associated the "essential part" of Maxwell's theory to the mathematical contrivance of endowing "vacuum" with "an absolute inductive capacity greater than unity". That procedure assured "the transition to Maxwell's scheme", without involving "any undue stretch of the original hypothesis". In reality, that *scheme* was Helmholtz's representation of Maxwell's theory rather then Maxwell's original theory.[14]

The last sentence of the paper stated that electrodynamics was well expressed by "Maxwell's scheme", and that that scheme "has also so much to recommend it on the score of intrinsic simplicity". Indeed, the appreciation of Maxwell's theory had filtered out from Helmholtz's theory. During that process, the comparison between the theories was restricted to the aspect of mathematical physics: the more interesting comparison involving theoretical physics, at that stage, was completely overlooked by Larmor.

The following year, in a short paper published in the same *Proceedings*, "On the Theory of Electrodynamics, as affected by the Nature of the

[12] See Larmor J. 1891, p. 523: "In this way the Maxwell's scheme of circuital currents reveals itself as a limiting case of the more general polarisation theory. The infinite dielectric constant makes the excited polarisation of very great amount in comparison with the exciting cause; so that in the limit we may, in a sense, imagine the system as one of self-excited circuital polarisation, a point of view which approaches somewhat to that of Maxwell himself."

[13] See Larmor J. 1891, p. 534.

[14] Larmor J. 1891, p. 535. For a detailed analysis of Helmholtz's *constants* and their relationship with Maxwell's *constants*, see Darrigol O. 2000, p.p. 227-9. See also Darrigol O. 1993, pp. 232-8.

Mechanical Stresses in Excited Dielectrics", Larmor paid much more attention to theoretical issues. He took into account two historical-conceptual paths: the conceptual path which connected Faraday to Maxwell and the conceptual path which went through the theories of Poisson, Mossotti and Helmholtz. He qualified the former as based on "Faraday's view of the play of elasticity in the medium", even though the reference to the elasticity of a medium seems more suitable for Maxwell's conception than Faraday's. The latter dealt with "the picture of a polarised dielectric supplied by Mossotti's adaptation of the Poisson theory of induced magnetisation".[15] According to Larmor, the second view suffered a "defect of circuital character" for it did not consider all currents as closed currents: it required "the existence of absolute electric charges on the faces of an excited condenser". Every electric current made electric charges accumulate on the plates of a condenser, thus destroying the property known as "circuital or solenoidal". Nevertheless he found that the problem "practically disappears in the limiting case when the constant ratio of the polarisation to the electric force is extremely great". Here we see the same interpretation put forward in the previous paper: Maxwell's theory as a limiting case of Helmholtz's theory. The latter was considered by Larmor different from the former and more general, so general as to include Maxwell's theory as a subset of the set of possibilities Helmholtz's theory could take into account. It seems that once again Larmor overlooked the deep theoretical differences between the two theories, confining himself to the mathematical aspect of the comparison. Nevertheless, at the same time, he claimed to be interested in the foundations of Maxwell's theory: this was an issue definitely more theoretical than mathematical. In particular, he regretted that Maxwell's equations "involve nothing directly of the elastic structure of this medium, which remains wholly in the background". In this part of the paper, Larmor swung between different theoretical models and different methodological attitudes. In another passage he stated that Hertz's experiments had corroborated Maxwell's "special form" of Helmholtz's theory, rather than supported Maxwell's theory *against* Helmholtz's theory.[16]

He thought that Maxwell's theory required further investigation, involving mainly the relationship between the electromagnetic actions and the structure of matter, or media in general. He criticised Maxwell for having not developed his theoretical foundations in a complete way: that criticism could explain, at least in part, his choice to confine the comparison to the mathematical side. Probably he judged that Maxwell himself had not

[15] Larmor J. 1892, p. 55.
[16] Larmor J. 1892, pp. 55-6.

managed to make people appreciate the theoretical differences between the two theories, because he had not accomplished his theory, particularly with regard to the interactions between fields and matter, or aether.[17]

However, in the course of the paper, while taking into account some arrangements of condensers, wholly or in part filled with a fluid dielectric, Larmor pointed to the core of theoretical difference between the two theories. The difference between the two theories involved some important issues, connected to each other in a sort of conceptual net. In part explicitly and in part implicitly, Larmor singled out some elements of the comparison:

1. contiguous actions *versus* actions at-a-distance,

2. open currents *versus* closed currents,

3. electric charge as source of electric action *versus* electric charge as side-effect of the different reactions offered by media to electric force,

4. electromagnetic energy placed on charged bodies *versus* electromagnetic energy stored in the media.[18]

After having displayed the different features of the two theories, he expressed his trust in Maxwell's theory.

> "We shall find reason to conclude that there is no superficial part in the distribution of energy; this would carry the result that the excitation of a condenser consists in producing a displacement across the dielectric which just neutralise the charge conducted to the plates; it would also carry the result that all currents, whether in conductors or in dielectrics, must flow in complete circuits, and would therefore confirm the Maxwell theory of electrodynamics."[19]

In that picture, Maxwell's theory could be compared to Helmholtz's theory on the grounds of theoretical physics: on that basis, Maxwell's theory and

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[17] On the reasons for Larmor's dissatisfaction with Maxwell's theory, see also Darrigol O. 2000, p. 334.

[18] See Larmor 1892, p. 58: "The polarisation theory, in the form of Mossotti and Helmholtz, which locates part of the electrification in a displacement existing in the elements of the dielectric, and part of it in an absolute electric charge situated on the plates of the condenser the cause of that displacement, is the representation of a wider theory which suppose the electrostatic energy to be in part distributed through the dielectric as a volume density of energy, and in part over the plates as a surface density. If experiments show that the latter part is null, we are precluded from imagining any superficial change on the plates which has a separate existence, and is not merely the aspect at one end of the displacement across the volume of the dielectric."

[19] Larmor 1892, p. 58.

the *Maxwell-flavoured* mathematical limit of Helmholtz's theory appeared to Larmor quite different. The distinction between the mathematical side and the theoretical side of the comparison appeared in a subsequent passage, wherein Larmor specified that the limit of Helmholtz's theory "which coincides with Maxwell's as to form must be abandoned".[20] Now, matter and energy were explicitly involved in the comparison. The experiments performed with electric waves had shown that the storage of the electric energy took place even in air or vacuum. Energy could not be split in two parts, one linked to stresses taking place in material media and another linked to forces which acted independently from the presence of a medium.[21]

At that stage, the theoretical framework of Maxwell's theory was explicitly appreciated by Larmor, and he claimed that experiments had shown "that at any rate the basis of electrical theory is to be laid on Maxwell's lines". In the last passages of the paper, devoted to summarizing the "principal conclusions", Larmor claimed that phenomena taking place between the plates of a condenser had to be explained in terms of stresses in the dielectric, consisting of "a tension along the lines of force and an equal pressure in all directions at right angles to them". We can easily notice that he used the same words Maxwell had used in the second chapter of his *Treatise*.[22] Pressure and tension "would exist in a vacuum" too, and they were "the result of a uniform distribution of energy in the dielectric". Now the key point was the distribution of energy: it was the link between energy and matter which qualified Maxwell's theoretical model. Even the *Maxwellian* limiting case of Helmholtz's theory led to a vanishing small electric charge on the plates of a condenser: "in that case a slight surface charge produces a great polarisation effect". Nevertheless, from the theoretical point of view, this limiting case preserved the causal relationship between electric charge on conductors and polarisation in dielectrics: it was indeed a non-Maxwellian conception. Larmor acknowledged the difference and claimed that "even this

[20] Larmor 1892, p. 63.

[21] See Larmor 1892, p. 62: "Now the propagation of electrical waves across air or vacuum shows that even then, when there is no ponderable dielectric present, there must be a store of statical energy in the dielectric; and this fact appears to remove the only explanation which seems assignable for the division of the energy into two parts, one located in the dielectric, and the other located on the plates and absolutely independent of the dielectric, viz., that the latter might be the energy of a direct action across space which is not affected by the dielectric."

[22] See Larmor 1892, p. 62, and Maxwell J.C. 1881, p. 153; see also chapter 7 of the present book.

limiting polarisation theory must be replaced [...] by some dynamical theory of displacement of a more continuous character".[23]

In the end, Larmor faced the query which Maxwell had not managed to solve: the microscopic structure of aether and matter. To give "a more vivid picture of it", he hinted at "a very refined aethereal substratum, in which the molecular web of matter is embedded". We should imagine an aether probably continuous or endowed with a structure not specified, although finer than matter, and the scaffolding of matter superimposed to it. Matter seemed endowed with a discrete although interwoven structure. Larmor assumed that high frequency electromagnetic radiation could not affect the matter-web, probably because of the too great inertia of its structure. On the contrary, it could affect the subtler structure of aether. At the interfaces between aether and dielectrics, or between two different dielectrics, or between aether and conductors, "the aethereal part of the distribution of energy in the medium will be discontinuous." In free aether, the electric action induced a strain which propagated "with the velocity of light". He assumed that the presence of matter modifies this simple mechanism of "discharge of the system", giving rise to the ordinary phenomena of induction and conduction.[24] At the beginning of the paper, Larmor had criticised Maxwell for the lack of a detailed account on the structure of aether, on the structure of matter and on the interactions between those structures and electromagnetic actions. At that stage of his theoretical research, we can say that, apart from some vague hints, he had not been able to go far beyond Maxwell.

From 1893 to 1897, Larmor, then fellow of the *Royal Society*, published in *Philosophical Transactions* three thick papers devoted to putting forward a complete electromagnetic theory under the title "A Dynamical Theory of the

[23] Larmor 1892, pp. 64-5. See, in particular, p. 65: "The stress which would exist in a vacuum dielectric is certainly due in part to a volume distribution of energy, as is shown by the propagation of electric waves across a vacuum. There is thus no reason left for assuming any part of it to be due to a distribution of energy on its two surfaces, acting directly at a distance on each other. There is therefore ground for assuming a purely volume distribution of energy in the vacuous space, leading to a tension $F^2/8\pi$ along the lines of force, and a pressure $F^2/8\pi$ at right angles to them."

[24] Larmor 1892, pp. 65-6. See, in particular, p. 66: "At an interface where one dielectric joins another, the aethereal conditions will somehow, owing to the nature of the connection with the matter, only admit of a portion of the stress being transmitted across the interface; and there will thus be a residual traction on the interface which must, if equilibrium subsist, be supported by the matter-web, and be the origin of the stress which has been verified experimentally. Inside a conductor, the aether cannot sustain stress at all, so that the whole aethereal stress in the dielectric is supported by the surface of the matter-web of the conductor."

Electric and Luminiferous Medium". The title drew readers' attention to aether, which represented the keystone of the whole project: it was the seat of electrical and optical phenomena and it was involved in the constitution of matter. The first paper of the trilogy was first received in November 1893, read in December and revised in June 1894; some other sections were added in August.[25] Larmor immediately made known the mathematical and physical bases of his theory: he would have tried "to develop a method of evolving the dynamical properties of the aether from a single analytical basis", and the analytical basis of the theory dealt with energy. The starting point was "the mathematical function which represents the distribution of energy in the medium when it is disturbed"; then the mathematical *engine* would have developed "the dynamical analysis from the expression of this function". This was the mathematical-physical aspect of the theory. Another aspect concerned theoretical physics, for an active interpretation was required or, in Larmor's words, "the province of physical interpretation" was involved. The process, consisting of combining mathematical procedures and physical interpretations, was not looked upon by Larmor as a new method: he avowed that a "method of this kind has been employed by CLERK MAXWELL".[26]

The list of subjects he put forward began "with the optical problem, and was found to lead on naturally to the electric one". In other words, he would have arrived at an electromagnetic theory only after having displayed an optical theory.[27] His previous reference to Maxwell appears a bit puzzling when compared with the order he would have followed: first optics and then electromagnetism. A *Maxwellian* approach would have been more consistent with the opposite choice: first electromagnetism and then optics. Larmor's justification was placed immediately in the next lines, where he introduced the reference to MacCullagh, whom he credited with having applied "with success the pure analytical method of energy to the elucidation of optical phenomena". Some decades before, that Irish scientist had developed an

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[25] As reported by Buchwald and subsequently by Hunt, J.J. Thomson, Royal Society referee, read Larmor's paper. Thomson wrote to Rayleigh that the paper was "exceedingly long" and dealt with "a very large subject being a kind of Physical Theory of the Universe". See "Referee Reports", Library of the Royal Society, London, 12.160 (5 February 1894). J.J. Thomson's appraisal is quoted in Buchwald J.Z. 1985a, p. 162, and Hunt B.J. 1991, pp. 215-16.

[26] Larmor J. 1893-4, p. 719.

[27] See Larmor J. 1893-4, p. 719: "We shall show that an energy-function can be assigned for the aether which will give a complete account of what the aether has to do in order to satisfy the ordinary demands of Physical Optics; and it will then be our aim to examine how far the phenomena of electricity can be explained as non-vibrational manifestations of the activity of the same medium."

optical theory based on a model of aether endowed with rotational elasticity. The model had raised some debate but had not gained much success. Nevertheless, after some decades, in 1878, FitzGerald had tried to transform MacCullagh's optical aether theory into an electromagnetic aether theory.[28] Larmor re-evaluated that model and thought that it could account for optical as well as for electromagnetic phenomena. He acknowledged that MacCullagh had faced "supposed incompatibilities with the ordinary manifestations of energy as exemplified in material structures". However, he thought that those difficulties had been overcome "by aid of the mechanical example of a gyratory aether, which has been imagined by LORD KELVIN".[29]

The link between mathematical physics and theoretical physics was the "Law of Least Action, expressible in the form $\delta \int \left(T - V\right) dt$, where T denoted the kinetic energy and W the potential energy". Larmor was confident that "the remainder of the investigation involves only the exact processes of mathematical analysis", provided that the energy was expressed in a physically suitable way.[30] In other words, once physics had warranted that energy was rightly specified, the mathematical procedures warranted that the corresponding phenomena were explained. The physical content was confined to the energy expression: additional phenomena could be described by simply adding other terms to the energy function.

> "In each problem in which the mathematical analysis proceeds without contradiction or ambiguity to a definite result, that result is to be taken as representing the course of the dynamical phenomena in so far as they are determined by the energy as specified; a further more minute specification of the energy may however lead to the inclusion of small residual phenomena which had previously not revealed themselves."[31]

The actual physical world could be explained following a strategy of subsequent refinements, realized by means of subsequent additions of more

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[28] See MacCullagh J. 1848, in Schaffner K.F. 1972, pp. 187-93. FitzGerald tried a more general dynamical approach to electromagnetism than Maxwell's. On the role of FitzGerald in the development of a dynamical theory of the electromagnetic field, see Stein H. 1985, pp. 312-13, and Hunt B.J. 1991, pp. 15-9.

[29] Larmor J. 1893-4, pp. 719-20. MacCullagh's model, FitzGerald's subsequent reinterpretation, and Larmor's reference to it have already been analysed by historians. See, for instance, Darrigol O. 2000, pp. 334-5.

[30] Larmor J. 1893-4, p. 720.

[31] Larmor J. 1893-4, p. 721.

specific terms to the energy function. This procedure appeared not so easy to Larmor, for we are dealing with "a partly concealed dynamical system" and we should imagine "some mechanical system which will serve as a model or illustration of a medium possessing such an energy function". There was a problematic link, in general, between mathematics and physics, and, in particular, between the standard procedures of mathematical physics and the wider choice of the corresponding conceptual representations, concerning theoretical physics. More than one representation could be associated to a given mathematical model and therefore the theoretical physicist had to decide which was the best among them. Larmor suggested that we should prefer the solution "which lends itself most easily to interpretation", the solution which offers the closest relationship between that representation and real phenomena. Nevertheless there was another requirement, which could have been in contrast with the former, namely the theoretical power, or the heuristic power of the representation. Larmor thought that we should prefer a representation less close to phenomena, at least as they appear to us, if it shows to be "distinctly more fertile in the prediction of new results, or in the inclusion of other known type of phenomena within the system".[32]

[32] Larmor J. 1893-4, p. 721.

In the first part of his 1893 paper, "Physical Optics", Larmor credited FitzGerald with having been the first to profitably combine MacCullagh's optical aether with Maxwell's electromagnetic aether. At the same time, MacCullagh was credited with having successfully applied dynamical methods to optics, although he had not managed to give a detailed physical representation of actions taking place in the aether.[1] He had assumed an aether, endowed with constant density but variable elasticity, which could resist rotations but not translations. To that aether MacCullagh had associated a potential energy depending on "a quadratic function of the components of this elementary rotation". From "a purely rotational quadratic expression for the energy" MacCullagh had deduced "all the known laws of propagation and reflexion for transparent isotropic and crystalline media".[2]

The optical equations, as reinterpreted by FitzGerald and Larmor, emerged from a mathematical and physical entity $\boldsymbol{K} = (\xi, \eta, \zeta)$, representing "the linear displacement of the primordial medium", and from the vector $\boldsymbol{D} = (f, g, h) = \nabla\times\boldsymbol{K}$, representing "the curl or vorticity of this displacement".[3] The mathematical-physical strategy had already been outlined at the beginning of the paper: first to look for the mathematical expressions for potential and kinetic energy, and then to insert them in the Principle of Least Action.[4]

In a short section added in June 1894, Larmor outlined the correspondence between Maxwell's electromagnetic theory and MacCullagh's theory: when assuming that magnetic induction corresponds to "the mechanical displacement of the medium, the electric theory coincides formally" with

[1] Larmor J. 1893-4, p. 723.

[2] Larmor J. 1893-4, pp. 727-9.

[3] Larmor J. 1893-4, p. 729.

[4] After a series of mathematical steps, Larmor obtained an "equations for elastic vibrations in the medium" which, in modern vector notation, can be written as $\rho\frac{d^2\boldsymbol{K}}{dt^2} + \nabla\times\left(2\alpha\boldsymbol{D}\right) = 0$ *or* $\rho\frac{d^2\boldsymbol{K}}{dt^2} + \nabla\times\left(2\alpha\nabla\times\boldsymbol{K}\right) = 0$.

Remembering that, for every vector **K**, $\nabla\times\left(\nabla\times\boldsymbol{K}\right) = \nabla\left(\nabla\cdot\boldsymbol{K}\right) - \nabla^2\boldsymbol{K}$, the last equation becomes $\rho\frac{d^2\boldsymbol{K}}{dt^2} + \nabla\left(2\alpha\nabla\cdot\boldsymbol{K}\right) = 2\alpha\nabla^2\boldsymbol{K}$, which corresponds to the standard wave equation $\rho\frac{d^2\boldsymbol{K}}{dt^2} = 2\alpha\nabla^2\boldsymbol{K}$, provided that $\nabla\cdot\boldsymbol{K} = 0$. See Larmor J. 1893-4, pp. 729-30.

MacCullagh's theory. Indeed, if we consider the time derivative of $\boldsymbol{D} = \nabla\times\boldsymbol{K}$, we obtain

$$\frac{d\mathbf{D}}{dt} = \nabla\times\frac{d\mathbf{K}}{dt},$$

which becomes the well known circuital equation for the free aether $\frac{d\mathbf{D}}{dt} = \nabla\times\boldsymbol{H}$, provided that $\boldsymbol{H} = d\boldsymbol{K}/dt$. In other words, the magnetic force should correspond to the velocity of the medium. The medium would be endowed only with rotational elasticity and would offer no resistance to translational motions.[5]

The last section of "Physical Optics" dealt with both the properties of the medium envisaged by MacCullagh and the questions it had raised. The main objection had been made by Stokes (1862), who had claimed that "an element of volume of such a medium when strained could not be in equilibrium under the elastic tractions on its boundaries". In order to restore the equilibrium, an external couple would have been required and that couple would have been "of amount proportional to its surface, and therefore very great in proportion to its mass."[6] To overcome that hindrance, Larmor hinted at the possibility that "the medium had acquired its rotational elasticity by means of a distribution of rotating simple gyrostats". Another possibility was given by "an ordinary elastic medium full of elementary magnets", whose conditions of internal equilibrium "will be correctly deduced … by the application of the Lagrangian analysis". The conclusion of Larmor, "we are not warranted in denying the possibility of such a medium", was neither convincing nor conclusive, and called into play further theoretical researches. This kind of aether raised even other queries concerning gravitation, the possibility of actions at a distance, and the supposed elementary magnets embedded in it. Larmor put forward a net of queries rather than a clear, well-ordered theory: perhaps the most interesting feature of his theoretical sketch is the explicit acknowledgement of all the problems raised by it.[7]

[5] Larmor J. 1893-4, p. 735. A medium endowed with those peculiar qualities, which offered "no resistance whatever to irrotational distortion" but resisted elastically "nondistorting rotation", had already been criticized short after MacCullagh paper was published. See Stein H. 1981, pp. 314-5.

[6] Larmor J. 1893-4, p. 745. For a cubic element of dimension Δa, the ratio between surface and volume is $6(\Delta a)^2/(\Delta a)^3 = 6/(\Delta a)$; it becomes greater when Δa becomes smaller.

[7] See Larmor J. 1893-4, p. 746: "It becomes indeed clear when attention is drawn to the matter, that there is something not self-contained and therefore not fundamental, in the

The second part of the paper, "Electrical Theory", was devoted to electromagnetic phenomena, corresponding to "the application of the properties of non-vibrational types of motion of the primordial medium". Electric *displacement* had already been associated to the "absolute rotation" of the medium and the magnetic force to "the velocity of its movement $d/dt(\xi,\eta,\zeta)$".

Larmor chose a potential-energy function

$$W = \frac{1}{2}\int \left\{ a^2\left(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\right)^2 + b^2\left(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\right)^2 + c^2\left(\frac{d\eta}{dx}-\frac{d\xi}{dy}\right)^2 \right\} d\tau .$$

and then applied the Principle of Least Action, assuming that "the electrostatic energy is null inside a conductor"; this entailed that "in statical questions the conductors may be considered to be regions in the medium devoid of elasticity". Although the theory did not explicitly assume that the electric displacement was circuital, namely $\nabla \bullet \boldsymbol{D} = 0$, the fact that $\boldsymbol{D} = \nabla \times \boldsymbol{K}$ assured it automatically, on purely mathematical grounds. Moreover, the last two equations, together with the requirement that $\boldsymbol{D}$ was proportional to ∇V, led to $\nabla^2 V = 0$, "so that the characteristic equation for V is involved in the data, without the necessity of any appeal to observation".[8]

Larmor tried to give a picture of the transfer of energy at the microscopic level, when conductors "encroach by forward movement into the excited dielectric". Taking a look *under the surface* of the conductor, he surmised a discrete structure of matter and a simple mechanism of interaction between

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notion of even a gyrostatic medium and the resistance to absolute motion of rotation which it involves. For we want some fixed frame of reference outside the medium itself, with respect to which the absolute rotation may be specified: and we also encounter the question why it is that rotatory motion reveals absolute directions in this manner. Another aspect of the question appears when we consider the statical model with its rotational property produced by small magnets interspersed throughout it, the medium being in internal equilibrium in a magnetic field when unstrained; the unbalanced tractions on the element of volume are here supplemented by a couple due, as to sense, to magnetic actions at a distance, and it is the energy of this action at a distance which constitutes the rotational part of the energy of the model. We may if we please suppose some analogous action at a distance to exist in the case of the actual aether, the ultimate explanation of which will be involved in the explanation of gravitation."

[8] Larmor J. 1893-4, pp. 747-9. It is worth noting that, in Larmor's theory, aether *displacement* was different from Maxwell's electric *displacement*, although the latter was also interpreted as a mechanical action in/through aether.

the molecules of matter and the medium *charged* of energy. Each molecule behaved like a spring, undergoing a sort of compression immediately followed by a release. The process of charge and subsequent discharge should go on as long as the conductor travels through the excited aether. There was a sort of asymmetry between aether and matter, for the former was supposed to be continuous and the latter discrete: aether should answer as a whole to perturbations taking place at the borderline with matter.[9]

When Larmor took into account the typical electro-dynamical effects, he had to cope with the problem of energy transfer from aether to matter. From the mathematical point of view, he transformed the kinetic energy of the medium

$$T = \frac{1}{2}\int\left[\left(\frac{d\xi}{dt}\right)^2 + \left(\frac{d\eta}{dt}\right)^2 + \left(\frac{d\zeta}{dt}\right)^2\right]d\tau \quad (1)$$

into an energy function containing the electric currents

$$\begin{aligned} T &= \frac{1}{8\pi}\int\left[\left(F\frac{df}{dt}\right)+\left(G\frac{dg}{dt}\right)+\left(H\frac{dh}{dt}\right)\right]d\tau = \\ &= \frac{1}{8\pi}\int\frac{1}{r}\left[\left(\frac{df}{dt}\frac{df'}{dt}\right)+\left(\frac{dg}{dt}\frac{dg'}{dt}\right)+\left(\frac{dh}{dt}\frac{dh'}{dt}\right)\right]d\tau \end{aligned} \quad (2),$$

after having introduced an electrodynamic potential

$$(F,G,H) = \left(\int\frac{1}{r'}\frac{df'}{dt}d\tau'\,;\int\frac{1}{r'}\frac{dg'}{dt}d\tau'\,;\int\frac{1}{r'}\frac{dh'}{dt}d\tau'\right).$$

From the theoretical point of view, the transfer of energy from aether to matter involved a conceptual transition from a theoretical model of energy as spread throughout the medium to a theoretical model of energy as placed

[9] Larmor J. 1893-4, p. 752.

on conductors or spread out from them. This was exactly the same conceptual shift discussed by Maxwell in his *Treatise*.[10]

Larmor pointed out that the currents of the kind $d\mathbf{D}/dt$, appearing in (2), had a different meaning than in Neumann's theory; they were "simply mathematical terms for such flow of electric displacement along each wire as would be required to make the displacement throughout the field perfectly circuital", in accordance with Maxwell's prescription. In other words, the expression (2) for kinetic energy appeared as a sort of bridge, both theoretical and mathematical, between the different conceptions of energy. The two different ways of representing energy corresponded to the asymmetry between continuous aether and discontinuous matter. At that stage, the problematic link between aether and matter was no more satisfactorily explained by Larmor's theory than by Maxwell's theory. However, a detailed knowledge of the structure of matter and of the interaction between aether and matter was not at stake when only Lagrangian methods were involved.[11]

Indeed, Larmor was following the path traced by Maxwell, even though he hoped to be able to find a more meaningful bridge between the two representations. In a following section, dealing with electrodynamic effects of the motion of charged bodies, he tried to give the same double representation, both in terms of displacement currents and in terms of electrified matter in motion. He thought that both representations could be reduced to a unified explanation, in terms of a chain of strains across the aether.

> "When a charged body moves relatively to the surrounding aether, with a velocity small compared with the velocity of electric propagation, it practically carries its electric displacement-system

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[10] See Larmor J. 1893-4, pp. 75-8. He remarked that the expression (2) for kinetic energy was very similar, from the mathematical point of view, to Neumann's "well-known form of the mechanical energy of a system of linear currents", namely $T = \frac{1}{8\pi} i_1^{\,2} \int \frac{1}{r_1} cos\varepsilon_{11}\, ds_1\, ds_1 + ... + \frac{1}{4\pi} i_1 i_2 \int \frac{1}{r_{12}} cos\varepsilon_{12}\, ds_1\, ds_2 + ...$. See Maxwell J.C. 1881, vol. II, part IV, chapter XI; see also chapter 7 of the present book.

[11] Larmor J. 1893-4, p. 758. See also p. 759: "The electrodynamic forces between linear current-systems are thus fully involved in the kinetic-energy function of the aethereal medium. The only point into which we cannot at present penetrate is the precise nature of the surface-action by which the energy is transferred (…) from the electric medium to the matter of the perfect conductor; all the forces of the field are in fact derived from their appropriate energy-functions, so that it is not necessary, though it is desirable, to know the details of the interaction between aether and matter, at the surface of a conductor."

> (*f*, *g*, *h*) along with it in an equilibrium configuration. Thus the displacement at any point fixed in the aether will change, and we shall virtually have the field filled with electric currents which are completed in the lines of motion of the charged element of the body, so long as that motion continues. On this view, Maxwell's convection-current is not differentiated from conduction-current in any manner whatever, if we except the fact that viscous decay usually accompanies the latter."[12]

The stumbling block on the path towards a satisfactory electromagnetic theory was the link between electric and magnetic phenomena, on the one hand, and the structure of matter on the other. In the section "On Vortex Atoms and their Magnetism", he tried to link free aether motions to magnetism and magnetism to molecular structure of matter. Larmor assumed that vortex-rings of aether with an empty core were the basic structure of matter, following a tradition going from W. Thomson to J.J. Thomson: as a consequence, "a permanent electric current of this kind is involved in the constitution of the atom". Whilst in magnetic matter all elementary vortices should have the same orientation, in ordinary matter they should have different orientations.[13] A theory wherein vortex-rings of aether represented atoms and the velocity of the same medium represented a magnetic field, was a step towards the integration among different aspects of mechanics, electromagnetism and chemistry or, in Larmor's words, "a step towards a consistent representation of physical phenomena". Molecules were considered as sets of atoms which could be linked to each other by the magnetic forces they produced. Nevertheless, those magnetic bonds raised a query concerning the property of matter, for in that case all kind of atoms and molecules would have created a structure endowed with strong magnetic properties. In other words, all substances would have exhibited magnetisation: it meant, Larmor acknowledged, that his specific model failed and he was forced to "find some other bond for the atoms of a molecule".[14]

Although aware of the flaw in his theory, Larmor went on developing it, and trying to include in it all kinds of radiation. Atoms and molecules were

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[12] Larmor J. 1893-4, p. 763.

[13] See Larmor J. 1893-4, p. 764: "A permanent magnetic element will thus be represented by a circuital cavity or channel in the elastic aether, along the surface of which there is a distribution of vorticity; it will in short be a vortex-ring with a vacuum (or else a portion of the fluid devoid of rotational elasticity) for its core. An arrangement like this must be supposed, in accordance with Ampère's theory, to be a part of the constitution of a molecule in iron and other magnetic metals."

[14] Larmor J. 1893-4, p. 765.

imagined as the seat of pulsations and vibrations, under the influence of surrounding atoms inside the same molecule, or of surrounding molecules, the former influence being stronger than the latter. While the former dealt with actions commonly named chemical, the latter dealt with phenomena qualified as cohesion and elasticity. Both of them could be interpreted as "purely hydrodynamic vibrations due to the inertia simply of the aether" and were different from phenomena involving "rotational distortion of the medium". The distortion, due "to the permanently strained state of the aether" surrounding atoms, namely the electric charge, led to electric vibrations propagating through aether in the form of light, or other electromagnetic waves. He imagined a sort of transfer from matter to aether: "all the vibrational energy due to any very rapid type of molecular disturbance must finally be transformed into energy of electric strain and in this form radiated away". The detectable effect of those radiations was the "persistent and sharply-marked periods which are characteristic of the lines of the spectrum". According to Larmor, that kind of "hydrodynamic" energy was transferred from the discrete structure of matter to the continuous structure of aether, which had to convey the energy without suffering any break in its structure. On the contrary, when a transfer of electricity took place through aether, the latter would have experienced a break in its elastic structure. For instance, the transfer of electricity through an electrolyte should "only occur along lines of effective rupture (such as may be produced by convection of an ion) of its aethereal elastic structure".[15]

The relationship between electricity and matter or between electric and chemical phenomena was the most important issue underlying Larmor's 1893-4 paper. Both the electromagnetic theory and the theory of matter were based on the assumption of a rotationally elastic aether. His model of atom was nothing more than "a singular point in the fluid medium of rotational elastic quality": it was a seat of fluid circulation, consisting of an "elastic twist converging on it". Larmor pointed out that the hypothesis on the nature of matter was not an independent hypothesis but was consistent with and depending on his electromagnetic theory.[16]

In another page added in June 1894, Larmor tried to further explain the relationship between electricity and structure of matter and tried to put forward further details on the process of emergence of a molecule from a pure collection of atoms. The lines of twist starting from an atom and ending on another atom of the same molecule resemble the short tubes of force connecting the atoms in a molecule as suggested by J.J. Thomson some years

[15] Larmor J. 1893-4, p. 768.
[16] Larmor J. 1893-4, p. 770.

before. In both representations, the bonds between atoms were electric bonds and a molecule became a charged fragment of matter, or ion, when some bond was free and the molecule looked for a partner. In that theoretical model, the transfer of electricity as pure propagation of breakdowns of elasticity across the aether appeared not completely satisfactory, for the seat of electricity could also be inside matter. To fill the gap, Larmor took a step forward: the transfer of electricity consisted of the "convection of atomic charges". The electric charge became closer to matter, and endowed with a discrete rather than continuous structure. The unifying element was however the aether: the discrete structure of matter and electricity could be imagined as "evolved from some homogeneous structural property of the aether". How deep was the change proposed in that 1894 page inserted approximately in the middle of the 1893 paper? The most meaningful change involved the electric charge, which underwent a conceptual shift from a phenomenon connected to the distribution and transfer of energy to a phenomenon connected to the distribution and transfer of matter. Conversely, matter became a peculiar entity, stemming from dynamical actions taking place in the aether. However, a sort of conceptual continuity was assured, for the transfer of particles, represented as dynamical structures of the aether, was not so different from the transfer of *pure* energy. In other words, in Larmor's general framework, matter and energy, in their intimate nature, were not radically different from each other.[17]

According to Larmor, there was a fundamental unit of matter, or "monad", stemming from the continuous structure of aether, and a hierarchy of discrete entities: at the more elementary level we have the monad, then collections of monads, corresponding to the different elements, and, eventually, the molecules, corresponding to the ordinary substances. To be more precise, the model required two kinds of monads perfectly symmetric: positively charged monads and negatively charged ones, the latter being "simply perversions or optical images" of the former. The symmetric monads were welcome from the theoretical point of view, for "electric transfer from ion to ion would arise from interchange of monads by convection" without any

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[17] See Larmor J. 1893-4, p. 771: "The charged atoms will tend to aggregate into molecules, and when this combination is thoroughly complete, the rotational strain of each molecule will be self-contained, in the sense that the lines of twist proceeding from one atom will end on some other atom of the same molecule. If it is not the case, the chemical combination will be incomplete, and there will still be unsatisfied bonds of electrical attraction between the different molecules. A molecule of the complete and stable type will thus be electrically neutral; and if any cause pull it asunder in two ions, these ions will possess equal and opposite electric charges."

reference to a "breaking down of the continuity of the aether".[18] Nevertheless, that symmetry did not match up with the known chemical properties of substances. In nature, Larmor remarked, H^+Cl^- really exists, but its electrically symmetric H^-Cl^+ does not exist. Chemistry broke the electromagnetic symmetry between positive and negative electric charge. Larmor acknowledged that, at that stage, neither the present theory nor "any dynamical theory" could account for that asymmetry.[19]

Another flaw in the foundations of the theory came from the motion of matter through aether, as emerged from the theoretical debate regarding the experiment of Michelson and Morley. In Larmor's theory, an irrotational flow of aether corresponded to a magnetic field. As a consequence, if material bodies in motion had dragged away the inner and the surrounding aether, a magnetic field would have come out. Some effects would have followed, including perhaps an "influence of magnetization on the velocity of light". Those effects could not be accepted: therefore the hypothesis that aether was not dragged by matter in motion was assumed. Larmor also quoted some experiments which Lodge had recently performed, devoted to checking "the effects produced by a magnetic field on the velocity of light". The results had been negative and the section devoted by Larmor to them contains very general cogitations on kinetic energy of aether, on aether inertia and on the relationship between its density and elasticity.[20] Specific remarks on a principle of Relativity for both electromagnetism and mechanics seem beyond the horizon of Larmor's 1893 theory. If aether were assumed to be at rest and not in motion together with matter, no magnetic field would arise in the reference frame of aether, but if we chose a reference frame joining the matter in motion, then we would experience a reverse flow of aether and then a magnetic field. Larmor did not face the query. He simply assumed that the molecules should have been placed "at a distances from each other considerable compared with their linear dimensions" in order to allow aether to "stream past between them".[21]

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[18] See Larmor J. 1893-4, p. 771: "It is, again, difficult to imagine how the chemical elements should be invariably connected, through all their combinations, with the same constant of gravitation, unless they have somehow a common underlying origin, and are not merely independent self-subsisting systems. We may assume that it is these ultimate atoms, or let us say monads, that form the simple singular points in the aether; and the chemical atoms will be points of higher singularity formed by combinations of them."

[19] Larmor J. 1893-4, pp. 771-2.

[20] Larmor J. 1893-4, pp. 772, 774 and 778-9. On the problems arising from the identification of magnetic *force* with a flow of aether, see also Hunt B.J. 1991, p. 215, and Stein H. 1981, p. 332.

[21] Larmor J. 1893-4, p. 775.

In his attempt to build up a physical theory on *everything*, Larmor came back to the problem of radiation, namely energy "sent out into the aether from the vibrations somehow set up in the atomic charges". According to Larmor, electromagnetic radiation did not start from aether but from matter. In the case of heated and then incandescent bodies, the production of radiation would have required the transformation of the "motion of agitation into electrical energy in the molecules, and thence into radiation". In the case of dissociation or a violent split of molecules, the displacement of atoms entailed "the performance of work against electric attractions, at the expense of the heat energy and chemical energy of the system". In both cases, the emergence of radiation would have involved the transformation of various kinds of energy into electric energy. However, he specified that the pure molecular motions of gases, in themselves, could not give rise to electromagnetic radiation.[22]

The new relationship assumed between chemical actions and electric actions shows us, page after page, what importance Larmor attached to the link between electric phenomena and structure of matter. Electricity had a crucial role in the building-up of molecules and, conversely, electricity had its seat inside molecules. However, another query emerged: the hydrodynamic basis of the model of vortex-atoms put in danger the physical consistency of the whole theory. The model required that "a rise of temperature is represented by increase of the energy, and that involves an expansion of each ring and a diminution of its velocity of translation". The first consequence was the wrong dependence of velocity from temperature, from the point of view of the kinetic theory of gases. The second consequence concerned the change in the dimensions of atoms: how could the model assure that the frequency of radiation did not change?[23] At that stage, the

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[22] See Larmor J. 1893-4, pp. 781-2: "There appear to be experimental grounds for the view that a gas cannot be made to radiate [at any rate with the definite periods peculiar to it] by merely heating it to a high temperature, so that radiation in a gas must involve chemical action or, what is the same thing, electric discharge. This would be in agreement with the conclusion that motion of a molecule through the aether, however the latter is disturbed, will not appreciably set up electric vibrations, unless it comes well within range of the chemical forces of another molecule; ….."

[23] He applied once again the Principle of Least Action, also taking the term $\varpi = \nabla \cdot \boldsymbol{K}$ into account. In a synthetic vector form, we would have $\rho \dfrac{d^2\boldsymbol{K}}{dt^2} + \nabla \times \left(\alpha^2 \boldsymbol{D}\right) - A\, \nabla\varpi = 0$. Applying the divergence to both sides, the second term on the left side disappears, and we find that ϖ satisfies the equation $\rho \dfrac{d^2\varpi}{dt^2} - A\, \nabla^2\varpi = 0$. This is the well-known wave equation, "so that

attempt to unify, or at least put together without any mismatch, kinetic theory of gases, properties of electromagnetic radiation and hydrodynamic models was probably a too demanding theoretical task. The theory lacked new general principles and innovative mathematical approaches, in order to account for the microscopic structure of matter, and for the connections between electromagnetic radiation and that structure. The task was really too demanding, for it entailed a great unification involving mechanics, electromagnetism and thermodynamics, namely all fields of physics then known.[24]

All these difficulties did not discourage Larmor and did not prevent him from outlining a physical theory of *everything*. Could he leave gravitation out of the door? Could he give up looking for an explanation of the intimate nature of mass? Some years before, W.M. Hicks had attempted to account for gravitation in terms of volume pulsations of the empty cores of vortex-atoms, but Larmor rejected that purely hydrodynamical explanation because of the objections subsequently raised by the same Hicks. He claimed he had followed another path, widening his original hypothesis of an incompressible aether and taking into account "the effect of a compressional term in the potential energy of the medium", namely a term ϖ corresponding to the divergence of the basic vector $\boldsymbol{K} = (\xi, \eta, \zeta)$. However, Larmor's choice did not completely dismiss Hicks' model, for he followed the path leading to perturbations of compression through the medium.[25] Moreover Larmor thought that a theoretical bug undermined his deduction, a bug dealing with the interpretation of energy, and already pointed out by Maxwell and Heaviside. He answered to the failure of the above model with a hint to a different model: gravitational effects could be associated to a slight difference between negative and positive electric charge of ions in the molecule. The excess of charge could give rise to a "force of gravitational type, transmitted by a stress in a rotational aether". Even this hypothesis was not so original: it had emerged in the context of action at-a-distance German theories.[26]

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the compressional wave is propagated independently of the rotational one". See Larmor J. 1893-4, p. 782.

[24] Some historians have described Larmor's theories as too hard to understand, only roughly sketched, and pretentious. See Buchwald J.Z. 1985a, p. 141-2, and Darrigol O. 2000, p. 332. I agree with the two scholars on some specific point, although my appraisal is more positive in general. In 1893-4, Larmor's theories were a net of unusual ideas, interesting remarks and new physical concepts.

[25] Larmor J. 1893-4, p. 793.

[26] See Larmor J. 1893-4, p. 794, and Maxwell J.C. 1865, pp. 492-3; see also chapter 10 of the present book.

He dared new, bold hypotheses and, at the same time, he relied on already existing theories of gravitation and their corresponding hypothesis. For instance, he wrote that it was *proved* by Laplace that "the velocity of gravitation must be enormously great compared with that of light". He went on writing that "gravitational energy, whatever its origin, must preserve a purely statical aspect with respect to all the other phenomena that have been here under discussion"; it was a theoretical approach not consistent with whatsoever theory of contiguous actions. He insisted that "mass is a dynamical conception", but he associated to that bold statement the very formalistic remark that "the ultimate definition of mass is to make it a coefficient in the kinetic part of the energy function of the matter".[27] Nevertheless, I think that it would be a mistake to underestimate Larmor's ambitious project. He tried to link the old concepts of mechanics to new concepts emerging from the more recent tradition of electromagnetic theories; he tried to connect continuous models to discrete models; he tried to connect the intimate nature of matter to the intimate nature of energy. In particular, he aimed at unifying physics, starting from a primitive medium, whose motions could produce regular structures and regular perturbations. On these grounds, I do not see Larmor at ease inside the boundaries of the so-called electromagnetic world-view misleading. His world-view was at the same time mechanical and electromagnetic or, better, he pursued the foundation of a sort of proto-physics, from which mechanics and electromagnetism should have been deduced. I would like to quote the next passage concerning the nature of mass, in order to show that net of concepts, hints and hypotheses which was the hallmark of his 1893 theoretical project.

> "To make a working scheme we must suppose a layer of the medium, possessing actual spin, to cover the surface of each coreless vortex-atom; we might imagine a rotationless internal core which allowed no slipping at the surface, and this spin would be like that of a layer of idle-wheels which maintained continuity between this core and the irrotational circulatory motion of the fluid outside. A gyrostatic term in the kinetic energy thus appears to introduce and be represented by the kinetic idea of mass of the matter; it enters as an aelotropic coefficient of inertia for each vortex, but when averaged over an isotropic aggregate of vortices, it leads to a scalar coefficient for a finite element of volume."[28]

[27] Larmor J. 1893-4, p. 794.
[28] Larmor J. 1893-4, p. 796.

Immediately after the last lines of Larmor's 1893 paper, *Philosophical Transactions* reported some pages which Larmor had added in June and August 1894. The part added in June consisted of two sections and a conclusion. The first section dealt with natural magnets and faced some difficulties in the hydrodynamic theory of matter when coping with magnetism inside matter. Furthermore, Larmor drew attention to the conception of electric current as convection of atomic charges. He remarked that an electric current should involve two kinds of convection: a "circulation of the medium … around the conducting part of the circuit" and "the convection of charged ions".[1]

This interplay between aether flows and ions flows could account, Larmor noted, for ordinary currents but could be unsuitable to account for microscopic currents or "molecular circuits". On that scale of length, "in a molecular circuit", electric convections could not take place, "but only permanent fluid circulation through it". This difference led to an asymmetry between the magnetism stemming from macroscopic electric currents, and the magnetism stemming from permanent magnets, due to microscopic or molecular circuits. At a deeper theoretical level, the asymmetry involved the conceptual tension between continuous and discrete models. In ordinary currents, a continuous flow of aether was associated to a flow of discrete entities; in magnetic matter, only the continuous flow was involved. But Larmor had in store some more guesswork, which allowed him to restore the *molecular* features of magnetism. According to the new interpretation, magnetism of a permanent magnet could be regarded "not as a steady circulation of aether, …, but as the statistically steady resultant of the changing fields of the incessantly moving molecules which make up the magnet". He established a sort of correspondence between the ordered net motion of ions, superimposed to their kinetic disorder, typical of ordinary currents, on the one hand, and the net *magnetic momentum* of matter, superimposed to the irregular *magnetic momentum* of the molecules, on the other hand.[2]

The introduction of discrete units of matter and electric charge in a *Maxwellian* context led Larmor to the theoretical re-valuation of the *old* potentials and Helmholtz's *old* approach. He found suitable "the lines of Helmholtz's theory of 1870", and claimed that "the vector (*F*, *G*, *H*) is a physical entity as distinct from a mathematical expression". The physical

[1] Larmor J. 1893-4, p. 798.
[2] Larmor J. 1893-4, p. 800.

meaningfulness of potentials "would not be inconsistent with general principles", he claimed, even though "there are very various distributions of electric current and magnetism in the more distant parts of space which lead to the same distribution of magnetic induction in the neighbourhood of the system".[3] The conceptual tension between fields and potentials concerned the core of British physics, namely the theoretical model of contiguous action. Larmor challenged that tradition and pointed out both the theoretical tension between discrete models and continuous models, and the methodological tension between mathematical physics and theoretical physics.

Nevertheless, in the "Conclusion" of the section added in June 1894, Larmor came back to the foundation of what he called his "present view": a medium which is "a perfect incompressible fluid as regards irrotational motion" but is endowed with rotational elasticity. The medium was "the seat of energy of strain", and throughout it "undulations of transverse type" were propagated. To the usual objection that such a medium was "a mathematical abstraction which does not exist in nature", Larmor replied that it was endowed with the right properties to account for known phenomena. Differently from other parts of the paper, Larmor seems here to lean towards a *mathematical phenomenology*. However, the design for a great unification was still at stake: both matter and electricity were permanent dynamical effects taking place in that kind of aether. The discreteness of matter stemmed from the continuity of the medium and the tension between continuous and discrete representations seemed thus overcome.

> "A cardinal feature in the electrical development of the present theory is on the other hand the conception of intrinsic rotational strain constituting electric charge, which can be associated with an atom or with an electric conductor, and which cannot be discharged without rupture of the continuity of the medium. The conception of an unchanging configuration which can exist in the present

[3] Larmor J. 1893-4, pp. 803-4. See, in particular, p. 804: "The electric influence arising from a disturbance of one system is propagated elastically to other systems across the intervening medium, the propagation being nearly instantaneous without showing any sensible trace of the disturbance during its transit through the medium, and this on account of the high elasticity and consequent great velocity of propagation. The magnetic field is a residual effect of this propagation; that field is sufficient to represent the aggregate features of the result in cases in which the current is mostly conducted, but it need not represent the features of the propagation in detail."

> rotational aether is limited to a vortex-ring with such associated intrinsic strain: this is accordingly our specification of an atom."[4]

An interesting feature of that model, which connected so tightly aether, matter, electricity and energy to each other, was a unified account of convection currents and *displacement* currents. The motion of a charged particle through aether produced an "elastic effect of convection through the medium", consisting of "a twist round its line of movement". The effect was not so different from the propagation of elastic actions in *displacement* currents: such a twist was just the common feature of every kind of electric current. At the same time Larmor acknowledged that he had not managed to enlighten what he considered the core of every electromagnetic theory: "the detailed relations of aether to matter". Moreover, the theory tried unsuccessfully to cope with some difficulties concerning magnetism. He realised that "the law of the attraction between permanent magnets is left unexplained" and the magnetic field associated to the flow of aether was made undetectable only associating "a high value to the coefficient of inertia of the free aether".[5] Nevertheless Larmor had in store other hypotheses and remarks.

The pages added in August 1894 consisted of two sections; the second was devoted to optical phenomena, already discussed in the first part of the 1893 paper, whereas the first dealt with "atomic charges", or "primordial atoms", or "monads", the concepts he had introduced in the middle of the paper, in June 1894. In the first section, the elementary units of electric charge were named "electrons", a name recently used by J. Stoney, and the section was entitled "Introduction of Free Electrons".[6] In some way, the new electrons were different from the previous atomic charges, for they were

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[4] Larmor J. 1893-4, p. 805.

[5] Larmor J. 1893-4, pp. 805-6. At that stage, the model of electric charge associated to atoms was only roughly outlined in Larmor's theory. This led Buchwald to qualify the model as "mysterious" as Poynting and J.J. Thomson's *dissolution* of tubes of force. See Buchwald J.Z. 1985a, p. 152. I think that the conceptual path going from rotational strains to electric charge and then to the atom, when set in its historical context, appears at least as fertile as "mysterious", because of its power of unification. Furthermore, it seems to me that Larmor's *atomic electricity* was not in competition with Poynting and J.J.Thomson's theoretical models but tried to explain what would happen *after* tubes dissolution.

[6] On the use of the word "electron" from Stoney to Larmor through FitzGerald, and the role of FitzGerald in the emergence of Larmor's new theory (August 1894), see Hunt B.J. 1991, p. 220. G.J. Stoney, secretary to Queen's University in Dublin, had introduced a basic unit of electric charge in the paper "On the Physical Units of Nature", presented at the 1874 meeting of the British Association. In 1891 he introduced the word "electron" for that fundamental unit. He was FitzGerald's uncle.

placed at a different level in the structure of matter: they were not atoms but entities more elementary than atoms. Atoms were no longer the elementary building blocks of matter: they became complex structures and, in those structures, electrons were involved. The starting point of the new theory was one of the difficulties faced by the previous theory: the explanation of forces acting between two permanent magnets. Larmor found the solution in the hypothesis that even molecular currents were convective currents; he assumed that the core of vortex-rings consisted of "discrete electric nuclei or centres of radial twist in the medium". A discrete model of matter and electricity became necessary even at the sub-atomic level, even though the discreteness was of a particular kind: these nuclei consisted of dynamic structures emerging from the continuous medium itself. The new solution, the "electron", confirmed the integration between the continuous medium and the discrete unit, in some way a *particle*, of electric charge.[7] The specific unifying element of the new theory was the convective nature of all kind of electric currents, both macroscopic and microscopic.

> "A magnetic atom, constructed after this type, would behave like an ordinary electric current in a non-dissipative circuit. It would for instance be subject to alteration of strength by induction when under the influence of other changing currents, and to recovery when that influence is removed; in other words, the Weberian explanation of diamagnetism would now hold good."[8]

Larmor tried to come up with some numerical results. He assumed a geometrical-kinematical model for the motion of the electron in an atom: its electric charge corresponded to the "ionic charge" q, v was its velocity along the atomic orbit, A was the area of that orbit, and L was its length. He also assumed that n was the number of atoms in a cubic centimetre of matter, that "from electrochemical data" the product nq was known, and that "from molecular dimensions" the ratio A/L was known as well. Starting from those data, he computed the value of v corresponding to "an intensity of magnetization of 1700 c.g.s., which is about the limit attainable for iron" and

[7] See Larmor J. 1893-4, p. 807: "The circulation of these nuclei along the circuit of the core would constitute a vortex which can move about in the medium, without suffering any hydrodynamic pressural reaction on the circulating nuclei such as might tend to break it up; the hydrodynamic stability of the vortex, in fact, suffices to hold it together."

[8] Larmor J. 1893-4, p. 807.

found for *v* a value "not many hundred times smaller than the velocity of radiation".[9]

A planetary structure and a statistical approach were the main features of the molecular model which Larmor attempted to outline. The sketch consisted of a magnetic molecule "composed of a single positive or right-handed electron and a single negative or left-handed one revolving round each other". He made use of the analogy between planetary motions in the Solar System and electronic motions in the atom of matter. At the same time he thought that we should have given up localising the position of the electron over time. He looked upon the mass of a planet as "distributed round its orbit": at any point of the orbit, we should imagine a mass density "inversely proportional to the velocity the planet would have when at that point". He interpreted the measurable effects as a statistical result, reckoned over a large number of microscopic events.[10]

The magnetic effect of the whole molecule had to be zero, for "their secular effects just cancel each other". The "exact cancelling" of the magnetic effect could be avoided by imagining molecules with more than two electrons or more sophisticated structures. At that stage, however, the model was roughly outlined and Larmor did not inquire into the intimate structure of the atom. The statistical nature of electronic motions made them different from the previous flow of aether, for those motions underwent a sort of fluctuation.[11]

Independently from their peculiar nature of dynamical singularities in the aether, electrons were electric charges in motion along closed paths and then undergoing an accelerate motion. Consistently with Maxwell's electromagnetic theory of radiation, accelerated electric charges would have sent forth electromagnetic waves. That effect was in contrast with Larmor's atomic model, for a swift damping of electronic motion would have followed. To save the model, Larmor introduced (*ad hoc*, indeed) the concept of "steady motion", and the concept of perturbation of a steady motion. Electric waves could stem only from those perturbations.

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[9] Larmor J. 1893-4, p. 807, in particular the first footnote.

[10] See Larmor J. 1893-4, p. 807: "Just in same way here, the steady flow of the medium, as distinguished from vibrational effects, is the same as each electron were distributed round its circular orbit, thus forming effectively a vortex-ring, of which however the intensity is subject to variation owing to the action of other system."

[11] Larmor J. 1893-4, p. 807, second footnote. See also p. 808: "This mode of representation would leave us with these electrons as the sole ultimate and unchanging singularities in the uniform all-pervading medium, and would build up the fluid circulations or vortices – now subject to temporary alterations of strength owing to induction – by means of them."

> "It may be objected that a rapidly revolving system of electrons is effectively a vibrator, and would be subject to intense radiation of it energy. That however does not seem to be the case. We may on the contrary propound the general principle that whenever the motion of any dynamical system is determined by imposed conditions at its boundaries or elsewhere, which are of a steady character, a steady motion of the system will usually correspond, after the preliminary oscillations, if any, have disappeared by radiation or viscosity. A system of electrons moving steadily across the medium, or rotating steadily round a centre, would thus carry a steady configuration of strain along with it; and no radiation will be propagated away except when this steady state of motion is disturbed."[12]

This new condition of "steady motion" newly broke the symmetry between macroscopic and microscopic level, for the condition of *steadiness* appeared suitable only for the latter. Unfortunately, the tension between macroscopic and microscopic, which seemed to have been overcome by the attribution of a convective nature even to microscopic currents, re-appeared once again. In Larmor's theoretical researches, the boundary between microscopic and macroscopic level was continuously crossed but, in the end, he did not manage to remove that gap. There was a difference between the intimate nature of matter, concerning microphysics, and its visible features, concerning ordinary physics.[13]

Larmor took into account the steady motion of a microscopic electric charge and the field spread from the electric charge itself. As we have already seen, J.J. Thomson and subsequently O. Heaviside had faced the same question, giving solutions qualitatively akin to each other. Larmor seemed specifically interested in the relationship between the velocity of the electric charge and the velocity of radiation, and in the interpretation of the limiting case, when fast electrons approached the velocity of radiation.

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[12] Larmor J. 1893-4, p. 808.

[13] The conceptual tension between the visible, macroscopic, physical world, and the invisible, microscopic structures underlying it affected all the models put forward by Larmor in 1893-4. It is worth noticing that, since the dawn of natural philosophy, two general conceptions on the link between *macroscopic* and *microscopic* world had been on the stage. On the one hand, the conception of an invisible small-scale structure as a tiny copy of the large-scale world; on the other hand, the conception of an invisible small-scale structure endowed with specific features, following different laws. The main hallmark of ancient atomism was the conceptual gap between the ordinary, visible world and the invisible world of atoms: the latter was an *explanation* of the former.

> "As the velocity of the electric system is taken greater and greater the permeability, in the direction of its motion, of the uniaxial medium of the analogy becomes less and less, and the field therefore becomes more and more concentrated in the equatorial plane. When the velocity is nearly equal to that of radiation, the electric displacement forms a mere sheet on this plane, and the charge of the nucleus is concentrated on the inner edge of this sheet. The electro-kinetic energy of a current-system of this limiting type is infinite (..) and so is the electrostatic energy; thus electric inertia increases indefinitely as this state is approached, so that the velocity of radiation is a superior limit which cannot be attained by the motion through the aether of any material system."[14]

Therefore the velocity of an electron affected the geometry of its electric field, as well as its inertia and its energy. Larmor wondered whether the inertia of matter could be split into an *electric* inertia and a *material* inertia; if the latter could be associated to thermal kinetic energy of the molecules, the former was associated to phenomena taking place inside the atom. For instance, the electric inertia could be the kind of inertia involved in the motions of electrons in the atom and, in particular, in those periodic motions which gave rise to atomic radiation. In the context of atomic radiation, Larmor made reference to the Solar System and to some kind of gravitational radiation. The reference seems quite puzzling even though in some way consistent with the concept of atomic steady state he had put forward. Alongside the planetary "mean circular orbits", representing the steady motion, Larmor assumed no specified disturbances, which would have entailed "planetary inequalities which would give rise to radiation of corresponding periods". [15]

Indeed, the August 1894 addition to the 1893 paper is full of queries and suggestions, which are as interesting as generically sketched. One of them concerns the ultimate constitution of aether: was its intimate structure discrete or continuous, was its elasticity intrinsic or consequence of some molecular structure? A page of cogitations led to the conclusion, logical,

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[14] Larmor J. 1893-4, p. 809. On *electric charge* in motion, see Heaviside O. 1889, in particular p. 332; see also chapter 10 of the present book.

[15] Larmor J. 1893-4, p. 809. The supposed gravitational radiation was the analogue of electromagnetic radiation from atoms. Although the reference to gravitational radiation appears quite surprising, also because it was not further specified, Larmor was really interested in bridging the gap between electromagnetism and gravitation. I remind the reader that the last section of his 1893 paper had been devoted to "Gravitation and Mass", and Larmor had attempted to outline a field theory of gravitation.

rather than physical, that "there must be a final type of medium which we accept as fundamental without further analysis of its properties of elasticity or inertia". Electrons themselves were the discrete structure of aether, a structure of dynamical origin, as they were the centre of rotational strains. Nevertheless, once electrons had been shaped, they became individual and self-contained entities and Larmor remarked that the "fluidity of the medium allows us to apply the methods of the dynamics of particles" to describe their motions and interactions. But the energy of "a system of moving electrons" was in some way the energy of aether, for potential energy consisted of "the energy of the strain in the medium" and kinetic energy "was that of the fluid circulation of the medium", although associated to "a quadratic function of the velocity-components" of the individual electrons.[16]

The double nature of electrons, as individual building blocks of matter, on the one hand, and as dynamical structures of aether, on the other, affected their behaviour with regard to velocity. As long as their velocity remained far less than the velocity of radiation, their dynamical properties could be expressed "in terms of the position of the electrons at the instant". When their velocities approached that of radiation, Larmor suggested that they were "treated by the methods appropriate to a *continuum*"[17]. In other words, low velocity electrons behaved like particles, whilst high velocity electrons behaved like radiation. Electrons could be described either like particles or like radiation, and the choice depended on their energy: the transition from the first description to the second took place in some unspecified way. The old clash between continuous and discrete models faded into a new representation, where *continuous* and *discrete* became complementary aspects of an entity endowed with an intimate double nature.

Phenomena taking place in conductors could be explained either in a simplified way, assuming the conductor as a continuum and taking into account the streams of energy coming from the surrounding dielectric, or in a more detailed way, taking into account the motion of charged ions. According to Larmor, ions, rather than electrons or monads, were involved in conductors: the average effect of their motions corresponded to the discharge of the electric stress in the conductor itself. The gap between macroscopic and microscopic models was thus bridged: the macroscopic,

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[16] Larmor J. 1893-4, p. 811. I disagree with Buchwald on the supposed sharp dichotomy Larmor would have introduced between aether and matter, or between matter and fields. I find that the "divorce" between matter and fields was not as sharp in Larmor's as in Lorentz's theory, because Larmor's "electron" sprang out from the aether. It seems to me that, in some way, Buchwald himself, in a subsequent passage, acknowledged the difference. See Buchwald J.Z. 1985a, p. 128 and 134.

[17] Larmor J. 1893-4, p. 811.

Maxwellian model of the loss of elasticity in the transition from dielectrics to conductors had its counterpart in the microscopic route of ions through the structure of the conductor.

> "In the general theory of electric phenomena it has not yet been necessary to pay prominent attention to the molecular actions which occur in the interiors of conductors carrying currents: it suffices to trace the energy in the surrounding medium, and deduce the forces acting on the conductors, considered as continuous bodies, from the manner in which this energy is transformed. The calculations just given suggest a more complete view, and ought to be consistent with it; instead of treating a conductor as a region effectively devoid of elasticity, we may conceive the ions of which it is composed as free to move independently, and thus able to ease off electric stress; the current will thus be produced by the convection of ionic charges."[18]

Larmor thought that the total current could arise from a double stream of positive and negative ions flowing in opposite directions with different velocities, thus suggesting a model akin to "ordinary electrolysis". He claimed that the *electric* motion in itself did not involve any dissipation but dissipation was due to the mechanical interactions between electrons and the molecular structure, shaken by thermal motion. The free motion of electrons, carrying kinetic *electric* energy, was "disturbed and mixed up by the thermal agitations of the molecules of the conductors". The molecules carried a kinetic energy of not well-known origin but the amount of energy exchanged, he pointed out, "was independent of any question as to the origin of the inertia of the atoms".[19]

Larmor acknowledged that the query concerning the nature of inertia was not completely solved by his theory, and the relationship between electrons and ordinary matter was held over. How could he match up electric inertia of electrons with inertia of ordinary matter? For ordinary matter was made of molecules, molecules were made up of atoms and atoms contained electrons,

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[18] Larmor J. 1893-4, p. 814. In August 1894, Larmor tried to overcome the conceptual tension between two different representations of conduction: either a side-effect of the waste of electric displacement, in the passage from dielectrics to conductors, or a flow of microscopic electric charges. In the passage from 1893 to 1894, where Buchwald saw an overturn, I see a remarkable process of integration. See Buchwald J.Z. 1985a, p. 127. I think that, both in June (electric atoms) and in August 1894 (electrons), Larmor undertook an important theoretical step.

[19] Larmor J. 1893-4, p. 815, in particular the footnote.

could inertia of matter be brought back to electric inertia of electrons? He was unable to answer in a definite way: his *electromagnetic* theory of matter was undertaking its first steps. At that stage he was not able to successfully compete with the "original vortex-atom theory of matter" triggered off by "VON HELMHOLTZ'S fundamental discovery of the permanence of vortices". He was forced to accept a sort of dichotomy between ordinary matter and *electric* matter, which corresponded to the distinction between material energy and electric energy.[20]

The query about inertia was under discussion even in the last section added in August 1894, which Larmor devoted specifically to optical dispersion and optical propagation throughout moving media. He suggested that "it is only the electric inertia of the molecules that affects the electric waves": the supposed other kind of inertia or "material inertia" did probably have "no direct influence on the radiation". He surmised that molecules, "in their relations to the aether, behave as systems of grouped electrons"; their presence would not have disturbed "the fluidity of that medium".[21]

When he faced the propagation through moving media, he took into account two ways of conceiving the relationship between matter and electric waves, corresponding to two different ways of conceiving the relationship between matter and aether. The first model corresponded to what he named "the theory of a loaded mechanical aether". In it, "the molecules must act simply as a load upon the vibrating aether" and every explanation was based on "the influence of the inertia of the load of molecules". Matter affected "the inertia but not at all the elasticity of the medium"; the load corresponded to an excess of density. The second model was indeed *his* theoretical model, "the theory of a rotational aether", where "the treatment of the same problem (…) follows a rather different course". In this case we should take into account two displacements ϑ_1 and ϑ_2; the first corresponded to "the inducing displacement … which belongs to the waves and provides the stress by which they are propagated" and the second was "due to the orientation of molecules" and furnished "no stress for the wave-propagation".[22]

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[20] See Larmor J. 1893-4, p. 818: "In the absence of any such clue, a guiding principle in this discussion has been to clearly separate off the material energy involving motions of matter and heat, from the electric energy involving radiation and chemical combination, which alone is in direct relation to the aether. The precise relation of tangible matter, with its inertia and its gravitation, to the aether is unknown, being a question of the structure of molecules; but that does not prevent us from precisely explaining or correlating the effects which the overflow of aethereal energy will produce on matter in bulk, where alone they are amenable to observation."

[21] Larmor J. 1893-4, p. 819.

[22] Larmor J. 1893-4, pp. 819 and 821.

The result of the comparison between the two different models led to the same formula, the well-known Fresnel formula, expressing the partial influence of the motion of transparent matter on the velocity of light.[23] Larmor did not consider the models equivalent from the theoretical point of view: before starting the two mathematical deductions, he pointed out explicitly the different hypotheses which underlay them. Nevertheless he undertook both deductions in some detail. Why? Why the interest in the fact that his theoretical model and a different competing model offered the same mathematical result? Larmor seems less interested in claiming his theory than in enlightening the methodological attitude whose implementation was one of the hallmarks of late nineteenth century theoretical physics. He showed that two different theoretical models were equivalent from the mathematical point of view and explained with success a given set of phenomena. What better way to point out that, to a certain extent, theoretical physics was independent from mathematical physics?

I find in Larmor, to a high degree, the main hallmark of late nineteenth century theoretical physics: the most speculative side of natural philosophy associated to the advanced tools of mathematical physics. Larmor's best asset was his attempt to integrate complementary models, in order to attain a great unification in physics. Because of that definite commitment, it is hard to associate him to a specific *mechanical* or *electromagnetic* world-view.

Some decades ago, Giusti Doran set Larmor against the background of a British scientific tradition, which she identified with "the search for a nonmechanical view of nature": she found that both W. Thomson and Maxwell belonged to it. In Larmor I find the convergence of two different conceptual roots, corresponding to W. Thomson and Maxwell, rather than a single "nonmechanical" tradition common to both of them. I see in W. Thomson the pursuit of an ultimate mechanical explanation, and an attempt to outline a kinetic origin of matter. I see in Maxwell a different pursuit, involving a complex interplay between electromagnetic phenomena and mechanical explanations. What Giusti Doran called "Larmor's synthesis" was, in my view, the attempt to integrate the two conceptual roots. In this sense, I find that Larmor's theoretical contribution cannot be qualified as an electromagnetic world-view, just because he tried to go beyond a purely mechanical or a purely electromagnetic foundation of physics. It seems to me that, in some way, Giusti Doran herself acknowledged the existence of a double tradition and Larmor's commitment to a subsequent integration between them, when

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[23] We do not know whether Larmor had really read the long paper Lorentz had published in French two years before, wherein he had put forward an explanation of Fresnel formula, at the end of a demanding deduction. See Lorentz H.A. 1892a, pp. 525-6.

she stated that Larmor managed to offer "what both the vortex-atom and Maxwell's electromagnetic theory lacked", namely "an understanding of the relation between charge and matter". Nevertheless, I cannot accept that Larmor's unified view be qualified as "providing the field-theoretic view with an electromagnetic basis". The identification of Larmor's view with an electromagnetic world-view hides its most interesting commitment, namely the attempt to bridge the gap between mechanical explanations and electromagnetic entities.[24]

I find that even Kragh's portrait of Larmor as "the great ether theoretician and advocate of the electromagnetic world view" could be misleading, for the general project of a universal aether theory was something different from the *electromagnetic* world-view. However, I agree with him on the claim that "Doran probably over-emphasises the dematerialisation of the British ether": the fact is that dematerialisation was considered by Giusti Doran as a hallmark of the electromagnetic world-view. Warwick also inserted Larmor in the set of physicists committed to the so-called electromagnetic world-view, which consisted of imagining "an universe made only of ether and electrons", following the "ideal of reducing mechanics to electrodynamics". I disagree with everybody who credits Larmor with having overturned the relationship between mechanics and electrodynamics. In particular, I find that an aethereal conception of matter cannot be identified with the attempt to pursue that overturn.[25]

With regard to the so-called electromagnetic world-view, McCormmach correctly noted that, although Larmor's aether "was not an ordinary body", we cannot underestimate that "its only defining properties – inertia and elasticity – were mechanical." McCormmach pointed out that, in Larmor and J.J. Thomson's theories, all entities involved, namely lines of force, electric *particles* and molecules, "were thought to be reducible in principle to vortices and strains in the ether."[26] In particular, *mechanical* was also the

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[24] See Giusti Doran B. 1975, pp. 134-6. She found that, in general, "British physicists conceived of the aether's *inertia* in a nonmechnaical sense" (Giusti Doran B. 1975, p. 206). That the "sense" was definitely "nonmechanical" seems to me quite debatable.

[25] See Giusti Doran B. 1975, p. 206, and Kragh H. 2002, p. 69 and p. 112, footnote 76. See Warwick A. 1991, pp. 33 and 369. In a very detailed paper, Neri and Tazzioli also identified the commitment to that overturn with an aethereal conception of matter. See Neri D. and Tazzioli R. 1994, p. 17. I obviously disagree.

[26] McCormmach R. 1970a, pp. 460-61: "The British usually did not hold an electromagnetic view of nature in the European sense. They endowed the ether with the mechanical concept of mass conceived of as an elementary property rather than deriving it as a secondary phenomenon from a totally nonmechanical, electromagnetic ether. Their intention in this regard differed fundamentally from that of their European colleagues, who wished to eliminate all mechanical concepts and laws in favour of electromagnetic ones."

attempt to derive discrete matter from kinetic structures emerging from a continuous aether. I share McCormmach's interpretation of British theories as a combination of mechanical and electromagnetic features: I see an alliance, rather than a competition, between mechanics and electromagnetic conceptions.

The fact is that every historian has described the electromagnetic world-view in a slightly different way from the others.[27] Larmor envisaged a world which, at its fundamental level, consisted of aether and its dynamical structures. Differently from Lorentz, who imagined a world consisting of two distinct entities, aether and ions (later *electrons*), Larmor imagined his *electron* as nothing else but a rotational strain in the aether. His representation of the physical world can be looked upon as *electromagnetic* only in a very broad sense, for those structures were both mechanical and electromagnetic. In the end, with regard to the comparison between J.J. Thomson and Larmor, I find unconvincing Topper's appraisal, namely "Larmor's conception of the ether was at variance with that of Thomson, who remained committed to a mechanical ether."[28] I find Larmor's aether no less mechanical than Thomson's. I find that the most interesting feature of Larmor and J.J. Thomson's theories is exactly their commitment to overcome the distinction between what we nowadays call *mechanical* and *electromagnetic* world-views.

[27] See chapter 3 of the present book.

[28] Topper D.R. 1980, p. 50.

Appendix: *Larmor's mathematical deductions of Fresnel's coefficient*

Larmor undertook the deductions of Fresnel's formula making use of the density of aether ρ, the density of the *load* ρ', the elasticity of aether κ and the displacement of the medium ϑ. In order to better understand the two deductions, I find useful to add some mathematical steps. According to the first model, the equation of propagation for the medium at rest was

$$(\rho+\rho')\frac{d^2\vartheta}{dt^2}=\kappa\frac{d^2\vartheta}{dx^2}.$$

The equation for propagation through a medium, "in which the load ρ' is moving on with velocity *v* in the direction of propagation", was

$$\rho\frac{d^2\vartheta}{dt^2}+\rho'\left(\frac{d}{dt}+v\frac{d}{dx}\right)^2\vartheta=\kappa\frac{d^2\vartheta}{dx^2}.$$

The mathematical term *d/dt* transformed into *(d/dt – v d/dx)*, for an aethereal component of density ρ' would move with velocity *v* during the propagation.

If *V* is the velocity of propagation of radiation through free aether and μ the refractive index of the moving medium, we have

$$\kappa/\rho=V^2 \text{ and } \frac{\kappa}{\rho+\rho'}=\frac{V^2}{\mu^2}.$$

According to the model, the latter is the velocity of electromagnetic waves across the medium at rest, so that

$$\frac{\rho}{\rho+\rho'}=\frac{1}{\mu^2} \text{ and } \frac{\rho'}{\rho+\rho'}=\frac{\rho'+\rho-\rho}{\rho+\rho'}=1-\frac{\rho}{\rho+\rho'}=1-\frac{1}{\mu^2}.$$

Larmor chose $\vartheta = Ae^{i\frac{2\pi}{\lambda}(x - V_1 t)} = Ae^{i\alpha(x - V_1 t)}$ as a solution of the differential equation, where V_1 is the velocity of electromagnetic waves across the transparent medium in motion through the aether. We need the following derivatives

$$\frac{d^2\vartheta}{dt^2} = -\alpha^2 V_1^2 \vartheta \qquad \frac{d^2\vartheta}{dx^2} = -\alpha^2 \vartheta \qquad \frac{d}{dx}\left(\frac{d\vartheta}{dt}\right) = \alpha^2 V_1 \vartheta .$$

Inserting them into the wave equation, we have

$$-\rho V_1^2 - \rho' V_1^2 + 2\rho' v V_1 - \rho' v^2 = -\kappa$$

$$(\rho + \rho')V_1^2 - 2\rho' v V_1 + \rho' v^2 - \kappa = 0$$

Dividing the latter by $(\rho + \rho')$, the equation becomes

$$V_1^2 - \frac{2\rho' v V_1}{\rho + \rho'} + \frac{\rho' v^2}{\rho + \rho'} - \frac{\kappa}{\rho + \rho'} = 0$$

$$V_1^2 - 2\left(1 - \frac{1}{\mu^2}\right) v V_1 + \left(1 - \frac{1}{\mu^2}\right) v^2 - \frac{V^2}{\mu^2} = 0$$

It is an algebraic equation of second degree, whose coefficients are

$$1, \quad -2\left(1 - \frac{1}{\mu^2}\right)v \quad \text{and} \quad +\left(1 - \frac{1}{\mu^2}\right)v^2 - \frac{V^2}{\mu^2} .$$

The solutions are given by the formula

$$V_1 = +\left(1-\frac{1}{\mu^2}\right)v \pm \sqrt{\left(1-\frac{1}{\mu^2}\right)^2 v^2 - \left(1-\frac{1}{\mu^2}\right)v^2 + \frac{V^2}{\mu^2}} =$$

$$+\left(1-\frac{1}{\mu^2}\right)v \pm \sqrt{\frac{V^2}{\mu^2} + \left(1-\frac{1}{\mu^2}\right)v^2\left(1-\frac{1}{\mu^2}-1\right)} =$$

$$+\left(1-\frac{1}{\mu^2}\right)v \pm \sqrt{\frac{V^2}{\mu^2} - \frac{1}{\mu^2}\left(1-\frac{1}{\mu^2}\right)v^2}$$

At the first order in v/V_1, the second term inside the squared root is negligible and we have Fresnel formula

$$V_1 = +\left(1-\frac{1}{\mu^2}\right)v \pm \frac{V}{\mu} \quad \text{or} \quad V_1 = \pm\frac{V}{\mu} + \left(1-\frac{1}{\mu^2}\right)v \ .$$

The first term corresponds to the velocity of waves in the transparent medium at rest in the aether and the term $\left(1-\frac{1}{\mu^2}\right)v$ corresponds to Fresnel's partial aether drag.

At this point Larmor introduced the second model, namely his own model. If K is the "effective specific inductive capacity of the medium" (the dielectric constant, in modern terms), the theory established between θ_1 and θ_2 the simple relationship "of electrostatic, $\theta_1 + \theta_2 = K\theta_1$", which echoed the well-known relationship $\boldsymbol{D} = \varepsilon \boldsymbol{E}$ between the two electric vectors $\boldsymbol{E}$ and $\boldsymbol{D}$.[29]

The equation of propagation for the medium at rest would be

$$\rho \frac{d^2(\vartheta_1 + \vartheta_2)}{dt^2} = \kappa \frac{d^2 \vartheta_1}{dx^2} \ .$$

[29] Larmor J. 1893-4, p. 821.

Making use of the above displayed relationship between θ_1 and θ_2, we have

$$\rho K \frac{d^2\vartheta_1}{dt^2} = \kappa \frac{d^2\vartheta_1}{dx^2}.$$

In this case the waves velocity would be $\frac{\kappa}{K\rho} = \frac{V^2}{\mu^2}$. In the model, only the strain or displacement θ_2 is affected by motion; as a consequence, the additional operator *(d/dt – v d/dx)* must be applied only to θ_2. The equation of propagation, "when the molecules are moving through the stationary aether with velocity *v* in the direction of the wave motion", should be

$$\rho \frac{d^2\vartheta_1}{dt^2} + \rho \left(\frac{d}{dt} + v\frac{d}{dx}\right)^2 \vartheta_2 = \kappa \frac{d^2\vartheta_1}{dx^2}.$$ [30]

Taking into account that $\theta_2 = K\theta_1 - \theta_1 = (K-1)\,\theta_1 = (\mu^2 - 1)\,\theta_1$, we can write

$$\rho \frac{d^2\vartheta_1}{dt^2} + \rho \left(\mu^2 - 1\right) \left(\frac{d}{dt} + v\frac{d}{dx}\right)^2 \vartheta_1 = \kappa \frac{d^2\vartheta_1}{dx^2}$$

Introducing the same solution $\vartheta = Ae^{i\frac{2\pi}{\lambda}(x - V_1 t)} = Ae^{i\alpha(x - V_1 t)}$, and the same derivatives

$$\frac{d^2\vartheta}{dt^2} = -\alpha^2 V_1^2 \vartheta \qquad \frac{d^2\vartheta}{dx^2} = -\alpha^2 \vartheta \qquad \frac{d}{dx}\left(\frac{d\vartheta}{dt}\right) = \alpha^2 V_1 \vartheta,$$

we arrive at the equation

[30] Larmor J. 1893-4, pp. 821-2.

$$-\rho V_1^2 - \rho\left(\mu^2 - 1\right)V_1^2 + 2\rho\left(\mu^2 - 1\right)v\, V_1 - \rho\, v^2\left(\mu^2 - 1\right) = -\kappa$$

or $$-\rho\,\mu^2\, V_1^2 + 2\rho\left(\mu^2 - 1\right)v\, V_1 - \rho\, v^2\left(\mu^2 - 1\right) - \kappa = 0.$$

Dividing the latter by $\left(-\rho\,\mu^2\right)$, it becomes

$$V_1^2 - 2\left(1 - \frac{1}{\mu^2}\right)v\, V_1 + \left(1 - \frac{1}{\mu^2}\right)v^2 - \frac{V^2}{\mu^2} = 0\,.$$

It is the same equation of second degree in V_1, deduced from the previous theoretical model: obviously, it yields the same solutions.

In 1885, Poynting linked the new conception on the transfer of the electromagnetic energy to the model of tubes of force. That model, which could seem outdated when compared to Maxwell's more abstract theory, re-emerged with an unexpected heuristic power, for it challenged the intrinsic continuous nature of the electromagnetic field. Generally speaking, the re-emergence of lines of force or tubes of force in British electromagnetic theories challenged the sharp distinction between continuous and discrete representations for both matter and energy. Following Poynting's theoretical model, J.J. Thomson put forward discrete models of matter and energy long before his 1897 experiments on cathode rays and, even more important, from a purely theoretical point of view.[1] Larmor had been dealing with both continuous and discrete models of matter and electricity since 1885. Both Larmor and J.J. Thomson tried to realise a deep integration between continuous and discrete models, both for matter and energy. In the 1880s, they had undertaken a theoretical dialogue with Helmholtz and Maxwell's theories. Furthermore, in J.J. Thomson and Larmor's theories we can find an original integration between two different British traditions: Maxwell's contiguous action applied to electro-dynamics and W. Thomson's kinetic model of matter.

I acknowledge the existence of differences between J.J. Thomson and Larmor, but I find that both of them were strongly committed to theoretical physics and that their "different approaches" cannot be identified with the practise of mathematical physics and experimental physics. They were theoretical physicist and the differences between them were authentically theoretical.[2]

J.J. Thomson shared Poynting's belief that the concept of "electric displacement" was misleading, and supported Poynting's attempt to revive Faraday's tubes of force. He put forward a reinterpretation of the equations for the electromagnetic fields $\boldsymbol{E}$, $\boldsymbol{D}$, $\boldsymbol{H}$, $\boldsymbol{B}$, starting from Faraday's tubes and,

[1] See chapters 13 and 14 of the present book. I agree with Falconer when he states that J.J. Thomson's "experiments in 1897 were not the origin of the corpuscle hypothesis; instead they acted as a focus around which Thomson synthesized ideas he had previously developed." (Falconer I. 1987, p. 254) I disagree with Navarro, when he states that J.J. Thomson was "the discoverer of the first discrete subatomic particle", in spite of his faith in "metaphysical continuity of nature" or, in other terms, "his deep belief in the ultimate continuity of matter".

[2] I disagree with Noakes when he states that "Larmor and J.J. Thomson came to represent the different approaches to electrodynamics adopted by the increasingly distinct corps of experimental and mathematical physicists." (Noakes R. 2005, p. 420)

early in the 1890s, arrived at a discrete theoretical model for matter, energy and electricity. Energy, placed both *in* the tubes of force and in the motion *of* tubes of force, spread and propagated by discrete units, in accordance with a theoretical model quite different from Maxwell and Heaviside's. In the same years, Larmor developed a different theoretical model, where discrete units of matter and electricity stemmed from the continuous structure of aether and fields. In particular, J.J. Thomson outlined discontinuous structures for the electromagnetic field and Larmor outlined a subatomic structure of matter, wherein that discrete structure consisted of nothing else but dynamical actions propagating through aether. They represented a vanguard: they offered new landscapes to subsequent researchers in theoretical physics. J.J. Thomson and Larmor's aether theories allowed, for the first time and some years before the turn of the twentieth century, new professionalized and specialised physics to cross both the boundaries between matter and energy, and the boundaries between discrete and continuous models.

I would like to focus on Darrigol's appraisal of J.J. Thomson and Larmor's theoretical physics in the early 1890s, for I consider his appraisal the most interesting and complete. I find correct his stress on the influence on both scientists of both Maxwell's electromagnetic theory and W. Thomson's theory of matter. Nevertheless, I do not agree with the remark that, differently from Larmor, who criticized the concept of *electric displacement*, "J.J. Thomson never tried to explicate the mechanism underlying Maxwell's electromagnetic field". I find that J.J. Thomson also criticized that concept: he adopted Poynting's model of tubes of force in order to overcome the supposed oddness of Maxwell's electric *displacement*, and in order to avoid misleading interpretations.[3] Both Larmor and J.J. Thomson put forward a more effective representation of the electromagnetic field, even though the specific representations they chose were different: translations and rotations in McCullagh's aether for the former, Poynting's tubes of force for the latter.

Another difference noticed by Darrigol seems more convincing to me: Poynting and J.J. Thomson's theoretical model of electric current as an effect of the convergence and dissolution of tubes of force "preserved a Maxwellian intuition of the electric current". Seemingly, the *electron* Larmor introduced in 1894, represented an alternative to Maxwell's leading theoretical model, as well as *particles* (1892) and *ions* (1895) which Lorentz

[3] Darrigol O. 2000, p. 333. See chapters 11 and 12 of the present book.

introduced in the same years.[4] Nevertheless, as I pointed out in the previous chapter, Larmor's electron as a rotational stress in the aether led to a model of electric current not so different from Thomson's, for an electronic flow could be looked upon as a motion of some kind of aethereal perturbation. I find that, beyond some specific, important features, which differentiated Larmor's electrons from Thomson's tubes of force, both entities consisted of dynamical and aethereal structures propagating through aether itself. Moreover, in both cases, we are dealing with the propagation of a series of discrete units, either tubes of force or electrons.[5]

In brief, I think that these British theoretical physicists cannot be easily classified: this is what makes them so interesting from the point of view of the history of science. The sharp distinction between mechanical and electromagnetic world-views seems not suitable for them. J.J. Thomson and Larmor's theoretical models were based at the same time on mechanical and electromagnetic foundations. Aether and elementary structures *in* aether, or *of* aether, were considered as the common root for both mechanical and electromagnetic entities, in particular matter and fields. Larmor cannot be put into the category of the so-called *electromagnetic* world-view, and J.J. Thomson cannot be put into the category of the so-called *mechanical* world-view.[6] They tried to bridge the gulf between mechanics and electromagnetism. For this reason, I find early 1890s physics more interesting and meaningful than assumed by the received view of the history of physics.

Although the history of electromagnetism from Maxwell to J.J. Thomson and Larmor, through Poynting and Heaviside, can be considered as a theoretical *evolution*, I think that it would be quite hard to depict it as an instance of scientific progress. The concept of progress itself seems to me

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[4] What Darrigol called "Maxwell's intuition" is Maxwell's leading representation of electric charge and electric current: besides this representation, other representations are deployed in his *Treatise*, as Darrigol himself acknowledged. In chapter 7 of the present book, I have already commented on Darrigol's clear distinction between "core" and "periphery" in Maxwell's theoretical models of electric charge and electric current.

[5] Some decades ago, Miller stressed a difference "between the use of mental imagery by British and German Physicists". He claimed that, if the former, like Maxwell, made use of imagery "in the initial developments of a theory", for the latter, "mental images became an intrinsic part of electromagnetic theories". (Miller A.I. 1984, pp. 120-21) The case of lines of force shows that British scientists, like Poynting and J.J. Thomson, made use of mental imagery even in the last stage of their theories. However, Miller's claim was slightly modified in his "Concluding Remarks". See p. 310: "This study has found that each well-developed theory has images".

[6] This was claimed by B. Giusti Doran and D.R. Topper respectively, some decades ago; see the last passages of chapters 13 and 16 of the present book.

quite questionable when applied to the history of theoretical physics. From the point of view of present-day standard conceptions on electromagnetism, selected passages from Hertz, echoing some kind of mathematical phenomenology, would appear as a progress when compared to J.J. Thomson's substantialised fields or Larmor's aethereal electrons. It is the result of the *formalistic* drift, which has taken place in the twentieth century, both in the field of research and in the field of teaching. At the same time, some conceptions emerging from theoretical physics of the last decades of the twentieth century appear in general terms similar to J.J. Thomson and Larmor's conceptions of particles and fields.[7] The concept of theoretical *evolution* could perhaps be associated to a higher level of unification. If we compare Maxwell and W. Thomson's theories, on the one hand, with J.J. Thomson and Larmor's, on the other, we find that the latter actually managed to better integrate a theory of matter with an electromagnetic theory, mechanical models with electromagnetic equations, and discrete with continuous models.

The actual and clearly perceived *scientific* progress, which took place in the late nineteenth century, was a *technological* progress: indeed, electromagnetic devices had their share of success in it. The progress consisted in the spread of electric energy, electric lighting and telegraphy: by the end of the nineteenth century, a hundred thousand miles of telegraph cables connected the most important towns in the world, crossing mountains and oceans. Some contemporaries emphasized the new "century of electricity" emerging from the old "century of heat": electricity appeared as a more versatile source of energy, and more easily transferable. Moreover, electric energy appeared as a new kind of *clean* energy when compared to smoke and offensive smells given out by steam engines and oil lamps.[8] This is another kind of history, as interesting as the history of theories, and in

[7] The so-called *empty* space of recent physics is represented as a sea of virtual particles and radiation. In late nineteenth century aether, some dynamical structures gave rise to particles and fields. In general, apart from their specific features, the two models have much in common. See, for instance, Barone M. 2004, p. 1976. See also Cantor G.N. and Hodges M.J.S. 1981, pp. 53-4.

[8] On the awareness of social advantages brought about by electric technologies, see, for instance, *Dictionnaire encyclopédique et biographique de l'Industrie et des Arts industriels, Supplément*, 1891 (Lami E.O. editor), p. 743: "En effet, l'électricité fournissant une lumière pure et fixe, ne chauffant pas et ne viciant pas l'air, constitue non pas un éclairage de luxe, mais un éclairage sain et salubre, et, par conséquent, véritablement de première nécessité. Détrônant le gaz pour cet usage, l'électricité ne le bannira pas de la maison : bien au contraire, elle lui ouvrira tout grand son débouché normal, qu'il n'a jusq'ici envisagé que timidement et comme pis-aller, le chauffage." On the effects of the widespread telegraphic net, see Galison P. 2003, pp. 174-80.

many ways linked to the latter, even though proceeding at its own pace. The most interesting fact is that in the last decades of the nineteenth century there was a dramatic increase in theoretical debates and, at the same time, a dramatic increase in technological applications. For the first time in the modern age, physics produced meaningful transformations in everyday life. During the so-called *Scientific Revolution* of the seventeenth century, the emerging science influenced and transformed intellectual life but did not manage to affect material conditions and the habits of ordinary people. On the contrary, a widespread material transformation was the specific effect of scientific practice in the late nineteenth century. In some way, there was a *revolution*, namely the occurrence of meaningful events, which deeply transformed both the material and intellectual life. Nevertheless, physicists of the late nineteenth century never claimed that they were doing something revolutionary; only contemporary historians and observers acknowledged that a deep transformation was taking place, involving both science and social life. Even nowadays, more historians than physicists look at that *fin de siècle* as a particularly meaningful stage.[9]

It seems to me that L, Boltzmann clearly pointed out the different historical effects of the two aspects of late nineteenth century physics, namely the theoretical debates and technological achievements. In a lecture held in 1904, in St.Louis (USA), at the *Congress of Arts and Science*, he qualified "the development of experimental physics" as "continuously progressive". He saw some permanent achievements: among them, "the various applications of Röntgen rays" or "the utilisation of the Hertz waves in wireless telegraphy". On the contrary, he acknowledged that the "battle which the theories have to fight is, however, an infinitely wearisome one". Theoretical physics dealt with "certain disputed questions which existed from the beginning" and which "will live as long as the science". In other words, theoretical physics deals with conceptions which continuously emerge, then are neglected and subsequently re-emerge. One of the "problems" which he found "as old as the science and still unsolved" concerned the choice between *discrete* and *continuous* in the representation of matter. He found that those queries had their natural seat on the boundary between the history of physics and the history of ideas; in Boltzmann's words, they "form the boundary of philosophy and physics". Moreover, taking for granted that such a boundary exists, Boltzmann wondered where it was placed exactly. The historical consciousness, which had already emerged in scientists of the

[9] According to the four criteria for the existence of a *Revolution* in science, established by I. B. Cohen in 1985, we would not be allowed to speak of a revolution. See Cohen I.B. 1982, chapter II.

last decades of the nineteenth century, found in Boltzmann an advanced interpretation. Physical theories cannot be looked upon as "incontrovertibly established truths", for they are based on hypotheses which "require and are capable of continuous development".[10]

I find that, if not a revolution, J.J. Thomson and Larmor realized a deep transformation in physics. Larmor's theoretical model of electron shared some features of matter and some features of radiation: it was a rotational strain in the aether and, at the same time, an elementary, microscopic, building block of matter. J.J. Thomson's theoretical model of electromagnetic radiation, interpreted as a bundle of propagating tubes of force, thus endowed with a discrete nature, outlined a common nature for matter and radiation, at the microscopic level. In both theoretical models, a deep integration between discrete and continuous representations was achieved. This link between matter and radiation, and the integration between continuity and discreteness, at a fundamental, microscopic level, should be acknowledged as a milestone in modern physics and, in general, in modern science.

Planck's 1900 theoretical model of radiation, and Einstein's 1905 theoretical models for matter and radiation were different, sharply different implementations, of the same attempt to integrate complementary conceptions. The connection between J.J. Thomson and Larmor, on the one hand, and Planck and Einstein, on the other hand, is a meaningful connection, underlying the different, specific features of their correspondent theories.[11]

We know that, in the last years of the nineteenth century, P. Lenard, W. Kaufmann, E. Wiechert, J. Perrin, J.J. Thomson and others undertook experimental and theoretical researches on the microscopic interactions between the structure of matter, the electric charge and the electromagnetic field. We know that Planck undertook theoretical researches at the borderline between electromagnetism and thermodynamics, in order to overcome the conceptual tension between electromagnetic and thermodynamic properties of radiation. We know that Lorentz and Poincaré undertook theoretical researches at the borderline between mechanics and electromagnetism, in order to overcome the conceptual tension between classic kinematics and electromagnetic

[10] Boltzmann L. 1905, pp. 592-5.

[11] I remind the reader that Planck's 1900 hypothesis cannot be wholly appreciated without taking into account Planck's 1887 conceptions on the transfer of energy. On Planck's "infinitesimal theory" and "elements of energy" see Planck M. 1887, pp. 244-47. See also chapters 1 and 4 of the present book.

properties of matter in motion. Finally, we know that the young Einstein, in 1905, published some papers, wherein he offered solutions to the above-mentioned queries.[12]

I find that a deep commitment to integration and unification flowed through the theoretical researches of J.J. Thomson and Larmor in the early 1890s and went on throughout Einstein's above-mentioned researches.[13] I think that we can correctly stress changes and innovation introduced by early twentieth century theoretical physics and, at the same time, acknowledge the importance of theoretical researches taking place at the end of the nineteenth century. My historiographical sketch does justice to both the historiographical views associated to Einstein's theories: the widespread view of a revolutionary break or the far less widespread view of a continuous development. I find that continuity can be found in the attempt to integrate complementary conceptions for matter and energy; a revolution can be found in the specific features of his theories. History of science is a collection of many histories mutually interwoven: among them, academic disciplinary histories, and the history of scientific thought. If the former include history of physics, the latter is quite close to the wider-scope history of ideas. My historiographical sketch tries to take into account both the history of physics and the history of ideas: I would like to integrate the two histories, as well as to integrate innovation and continuity. Some decades ago, talking on the concept of "Differential History", A. Funkenstein criticised who assumed "continuity and innovation to be disjunctive, mutually exclusive predicates", and I share his criticism.[14] On the nature of Einstein's

[12] I find interesting Renn's general interpretation of Einstein's 1905 papers. The hypothesis of light quanta was interpreted as an attempt to solve the problems at the borderline between electromagnetism and thermodynamics. The hypothesis of the equivalence between electromagnetic radiation and inertial mass was interpreted as an attempt to solve the problems at the borderline between mechanics and electromagnetism. See Renn J. and von Rauchhaupt U. 2005, p. 32. See also Renn J. 2006, in Renn J. (ed.) 2006, vol. 1, p. 43.

[13] Some decades ago, M. Hesse traced back that commitment to unification, concerning matter and energy, to "the notion that matter be endowed with intrinsic powers and conversely that active forces or influences be in some way substantial". (Hesse M.B. 1961, p. 38) With regard to late nineteenth century British electromagnetism, I must underline the difference between the model of matter as endowed with an "intrinsic power", and the model of matter as a kinetic structure in a universal medium. Moreover, the connection between J.J. Thomson and Larmor, on the one hand, and Einstein, on the other hand, must be better specified. (See the *Afterword*, at the end of the present book) In the years under consideration, Hesse's "notion" could be specified in the following way: energy shared some properties of matter, for instance inertia and discreteness, and, conversely, matter shared some properties of energy, or consisted itself in a concentration of energy.

[14] In this context, I reject any reductionism. I cannot endorse Miller's claim that history of science can be "defined broadly enough to be considered part of the history of ideas", but

revolution, I also agree with Miller's interpretation of the history of physics in the first half of the twentieth century: the concept of scientific revolution "describes only the gross structures of scientific change". When we take into account the fine structure, we find that "change is gradual" and we have the opportunity to appreciate elements of both continuity and discontinuity.[15]

As E. Giannetto recently noted, "nature and origins of quantum physics" had meaningful roots in Larmor's theoretical researches. He found that, in Larmor's theory, on the one hand, "electromagnetic field must present wave but also corpuscular aspects to explain the origin of matter"; on the other hand, "matter particles must present corpuscular but also wave aspects as long as they derive from the electromagnetic field".[16] The fact is that J.J. Thomson's and Larmor's theories, although different from each other and even more different from *Quantum theory* (in its various interpretations), involved an intrinsic integration between discreteness and continuity, both for fields and for particles. An intrinsic integration between different and complementary models emerged long before the manifold attempts to devise a *Quantum theory*.

There are two issues mutually interwoven, which deserve further analysis: first, the nature of the link between a specific physical theory and the more general conceptions, or *conceptual streams*, converging on it, and, second, the nature of the link between late nineteenth century and early twentieth century theoretical physics. I will discuss the former in the following passages, and the latter in the *Afterword*.

The specific features of the theories under consideration, namely electrons and elementary tubes of force, can be considered as their *first level*. Those specific features made reference to general models of continuity and discreteness, which can be considered the *second level*. Furthermore, we can find a methodological tension between the tradition of

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I acknowledge that history of ideas can help us to better understand history of science, and even specific disciplinary histories like history of physics. See Miller A.I. 1984, p. xii. "A Differential History" is the title of the third section of Funkenstein's *Introduction*. He claimed that what we look upon as "new", often "consists not in the invention of new categories or new figures of thought, but rather in a surprising employment of existing ones". (Funkenstein A. 1986, p. 14).

[15] See Miller A.I. 1984, pp. 312. I think that my sketch does justice to the old-fashioned concepts of *forerunner* and *anticipation*. At the level of specific theoretical features of a theory, these concepts make no sense, for specific features are untranslatable. At the level of general conceptual models, we find persistence or recurrent re-emergence of themes or models: therefore nobody can claim to have *anticipated* a long-term tradition.

[16] Giannetto E. 2007, pp. 178 and 181.

phenomenological natural philosophy and applied mathematics, on the one hand, and the more recent theoretical physics, on the other. If the latter aspired to an intimate representation and explanation of natural phenomena, the former confined itself to a mere description or to a quantitative generalisation. If the latter made use of mental pictures and displayed sophisticated concepts and models, the first pointed to facts and equations.[17] I call *third level* the level corresponding to such a methodological or meta-theoretical commitment. It is worth noticing that, in the writings of J.J. Thomson, that methodological tension transformed into a pedagogical tension between a technical and formal teaching, on the one hand, and a teaching taking care of student's mental representations, on the other.[18] Neither Larmor nor J.J. Thomson gave original contributions to Cambridge's tradition of mathematical physics; indeed, they went far beyond that tradition and brought a meaningful contribution to theoretical physics.

To sum up, Larmor's electrons or J.J. Thomson's bundles of tubes of force are specific theoretical models: they are an instance of *first level* option. According to Larmor, elementary masses emerge as dynamic structures in a universal continuous medium, and, according to J.J. Thomson, electromagnetic radiation was endowed with microscopic discrete structure: they are general models, or *second level* options, belonging to a long-term conceptual stream. That specific and general conceptual models were an essential component of physics, as essential for researchers as for teachers, was an important methodological or meta-theoretical issue: it was a *third level* option. In the context of late nineteenth century physics, that option was involved in the emergence of what we call theoretical physics.

Among the conceptual streams flowing underneath late nineteenth century theoretical physics I could single out many general statements: matter has a continuous structure, matter has a discrete structure, energy has a continuous structure, energy has a discrete structure, interactions between bodies are contiguous actions, interactions between bodies are actions at a distance, matter has only passive properties, matter has active properties,

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[17] See Boltzmann L. 1899, in Boltzmann L. 1974, p. 95: "… others felt that physics must henceforth pursue the sole aim of writing down for each series of phenomena, without any hypothesis, model or mechanical explanation, equations from which the course of the phenomena can be quantitatively determined; […] This is the most extreme form of phenomenology, which I should like to call mathematical, …"

[18] See the first pages of J.J. Thomson's 1893 book, which I have analysed and discussed in chapter 13 of the present book.

light consists of continuous waves, light consists of discrete bundles of tubes, ….[19]

Confining myself to the conceptual melting pot of the late nineteenth century, I have found an extraordinary concentration of both clashes and cross-fertilisations between conceptual streams. The scientific debate was explicitly undertaken with the awareness that alongside specific physical hypotheses and mathematical tools, the scientific practice required more general hypotheses, involving the nature of physical science, its methods and its aims. Neither before nor after the late nineteenth century, the boundaries of physics were so wide and transparent. Conceptual components were not sophisticated additions but essential components: physics could neither be practised nor understood when setting them aside. In the late nineteenth century, the conceptual tensions between specific models, between long-term conceptual streams and between meta-theoretical and methodological options, gave rise to a process of trespassing of boundaries between those models, conceptual streams, and methodologies. The boundaries between matter and energy, between mechanics and electromagnetism, between continuous models and discrete models, between macroscopic description and microscopic descriptions, between contiguous action and at-a-distance action, and between mathematical physics and theoretical physics were repeatedly crossed. Following those debates allows us to encounter the last generation of physicists who were proud of being natural philosophers; some years later, even the expression *natural philosophy* appeared unsuitable and puzzling to the next generation of physicists. In the course of the twentieth century, theoretical physicists have underestimated their primitive link with the more speculative side of that long-lasting tradition. The generation of J.J. Thomson and J. Larmor, who gave their original contribution to physics in the late nineteenth century, was the last generation of natural philosophers and, at the same time, was the first generation of professionalized physicists. They lived on a boundary and they dared to cross both theoretical and meta-theoretical boundaries. With regard to matter and energy, they opened a path: they realised the first systematic integration between discrete and continuous models.

Even though my book tries to bridge the gap between specific issues involved in the history of physics and the more general issues involved in the

[19] These conceptual streams have nothing to do with Kuhn's paradigms, or Lakatos' research programmes, or Laudan's research traditions. They are simple, single units of scientific thought: their content of knowledge is much wider but much weaker than the content of knowledge of every given theory. See the *Afterword*, at the end of the present book.

history of ideas, I have left aside the connections between British electromagnetic theories and contemporary political, religious or philosophical issues. The fact is that some of those connections appear to me questionable. If some historian imagined a well-defined Cambridge "approach" or "a systematically connected natural philosophy", wherein "the ethereal constitution of matter and its continuity with radiation" appeared intrinsically linked to an "ontological realism" and to a "transcendent continuity in nature", others pointed out how problematic was the relationship between physical models and religious, political and metaphysical beliefs in British *Victorian* physics. I think that every sharp reduction should be avoided: I do not find convincing the hypothesis of definite and predictable connections between a scientific conception and its social and intellectual context. I also find questionable Giusti Doran's claim that "the antimaterialist, neoidealist philosophic climate that spread throughout Europe undoubtedly contributed to the proliferation of field theories of matter in Britain". It seems to me that in the 1890s, when what Giusti Doran called *field theory of matter* emerged, materialistic and positivistic influences were at least as strong as those anti-materialistic or neo-idealistic. Both "materialism" and "idealism" are philosophical labels too naive for physicists who dared cross the boundaries.[20]

The Cambridge community of natural philosophers and mathematicians, the community of scholars committed to theories of aether, the community of scientists coming from the social upper class, the community of scientists politically conservative, the community of *spiritualist* scientists (further specifications would be required in the use of this adjective), and the community of scientists interested in psychical researches did not exactly overlap. Lodge and J.J. Thomson, for instance, did not belong to the upper social class. Lodge, in addition, had not studied at Cambridge, but this did not prevent him from being deeply interested both in devising mechanical models of aether, and in pursuing psychical researches. Even an uneducated scientist like Heaviside, who belonged to neither the upper social class, nor had studied at Cambridge, took part in the adventure of *Victorian* science. Moreover, I disagree with Wynne's thesis that the interest in psychical researches was "antirationalist". Those researches can be interpreted in a different way: they represented an attempt to widen the field of rational

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[20] On the thesis of an "intimate social connection between the upper-class Cambridge intellectuals, the leading members of the SPR [Society for Psychical Researches], and the physicists who constituted the orthodoxy of the late Victorian period", see Wynne B. 1982, p. 217. I think that Wynne's thesis is quite suggestive but too sharp and general. For a different approach see Noakes R. 2005, pp. 427, 431, 435 and 444. On "antimaterialism" and "idealism", see Giusti Doran B. 1975, p. 150, footnote 30.

researches, in order to transfer into the boundaries of natural knowledge a class of phenomena looked upon, until then, as *non-natural*. What Wynne "ironically" considered a peculiar and naive desire of unification, I consider an advanced intellectual commitment, consistent with a more general commitment to cross the boundaries.[21]

Two decades ago, Oppenheim pointed out the difference between the commitment to psychical researches and the profession of spiritualism. Among the specific features of the latter, she stressed the faith "in human survival after death" and "the possible activity of disembodied human spirits". Those who found interest only in psychical researches were more interested in exploring "the mysteries of the human mind" than in looking for evidence supporting immortality. I find the distinction quite convincing, even though there was a group of scientists, including Crookes and Lodge, who combined interest in psychic phenomena with faith in immortality. Others, like W. Strutt (Lord Rayyleigh) and J.J. Thomson, confined themselves to a specific, intellectual interest, which stemmed from a wider interest in natural phenomena rather than from a precise, however existing, religious faith.[22] Some spiritualists, as Oppenheim noted, emphasized "the purportedly scientific foundations of their beliefs"; this shows us how complex was the relationship between scientific practice and psychic or spiritual practices. The specific context of late nineteenth century physical sciences can account for both the interest and the opposition towards that kind of *scientific spiritualism*, as I will venture to qualify. On the one hand, interests in psychism and spiritualism was naturally linked to attempts to re-define and widen the boundaries of science; on the other hand, part of the scientific community feared that those interests and practices could delay or overturn the process of specialisation and professionalisation recently undertaken.[23]

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[21] See Wynne B. 1982, p. 221 and 222. See, in particular, p. 222: "Ironically, psychical research turned out to be a naturalization of the supernatural – a form of *scientific* supernaturalism which attempted to trump the naturalism of the professionalizers with the more comprehensive, 'trascendent' naturalism."

[22] See Oppenheim J. 1985, p. 3. It seems to me that Oppenheim's concept of "Pseudoscience", labelling psychic researches, appears unsuitable from both the methodological and historical points of view. From the historical point of view, in the late nineteenth century, when the process of differentiation and professionalisation took place in science, the boundaries between science and not science, and the boundaries among different sciences, were widely debated. See Oppenheim J. 1985, p. 199.

[23] Oppenheim acknowledged that "British spiritualists … persistently sought to stretch the boundaries of the natural world beyond physical causes and effects into the realm of spirit". Psychical researches in the late nineteenth century could appear not so different from electric researches in the previous century. Lectures devoted "to the uninitiated

Lodge actively participated in the life of the Psychical Society, and was for years its president and performed experiments on many phenomena, which were supposed to involve both living and dead people. He was enjoying, at the same time, a long-lasting career as a professional scientist, performing important experiments on electromagnetic phenomena, and was appointed head of the new University of Birmingham and elected president of the Physical Society. What in retrospect could appear as a double life, when placed in its historical context, can be interpreted as a deep, intensive and wide-scope scientific commitment to decode all the mysteries of nature. J.J. Thomson joined the Society for Psychical Researches in 1883, took part in some events and demonstrations, but never actively undertook psychical researches on his own. In his autobiography, J.J. Thomson devoted seventeen pages to "Psychical Research", with specific sections devoted to telepathy and water dowsing. He claimed that there was "no doubt of the reality of the dowsing effect", and reported in some detail "an example of this at Trinity College". Moreover, he put forward some conjectures in order to explain the phenomenon. He thought that experiments with rods, performed in order to test the existence of underground water, were "well worth making", for the twist of the rod "is a mechanical effect" which could be submitted to a quantitative analysis.[24] In other words, experiments of that kind and ordinary physical experiments had many features in common. That a physicist, or a scientist in general, should have been ashamed of having undertaken psychical or *spiritual* researches was neither J.J. Thomson's belief nor a commonly shared belief in the community of British scientists in the late nineteenth century.[25]

According to this interpretation, even some vague adjectives like materialist and anti-materialist show their inadequacy. As I have already remarked, the trust in aether as a substratum, which could play a

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public", "entertaining demonstrations", and exhibitions set up by "travelling performers" were also specific features of *electric* science, in the course of the eighteenth century. See Oppenheim J. 1985, pp. 200-202.

[24] Thomson J.J. 1936, pp. 159-63. What Oppenheim found so remarkable, namely that a scientist involved in the disclosure of "the secrets of the atom" were also interested in "the subject of water dowsing", would have probably appeared not so surprising to J.J. Thomson himself or to some of his contemporaries. The inner structure of an atom probably appeared not less secret or mysterious than water dowsing or telepathy. See Oppenheim J. 1985, pp 334-5.

[25] In the tenth chapter, Thomson complained that "such subjects were regarded as untouchable" by "scientific men". However, as Oppenheim pointed out, apart from personal feelings, to none of the physicists committed to scientific spiritualism, "the professional recognition that was his due" was denied. See Thomson J.J. 1936, pp. 298-9 and 383, and Oppenheim J. 1985, p. 393.

fundamental role in physical, psychical and spiritual phenomena, cannot be strictly interpreted as a profession of anti-materialism. If physical aether, however different from ordinary matter it was, offered the universal basis for all kinds of phenomena, can we speak of spiritualism or anti-materialism? Adjectives like *materialist* and *spiritualist*, when applied to the late nineteenth century, appear quite misleading. The attempt to bridge the gap between the material world and phenomena concerning mind and spirit was a specific commitment of that generation of physicists. J.J. Thomson and Lodge shared the faith in human progress and in the power of human knowledge, and the belief that the complex harmony of the universe was the result of an intimate rational texture. Neither clues of trivial anti-materialism nor clues of trivial anti-rationalism can be found in the representations of the world of these British physicists who shaped theoretical physics in the late nineteenth century.[26]

[26] J.J. Thomson's methodological attitude towards psychic phenomena sounds quite different from other attempts to face psychic and spiritual phenomena. Tait and Stewart, for instance, in the successful book published and re-published from 1875 to the end of the century, claimed that they were "absolutely driven by scientific principles to acknowledge the existence of an Unseen Universe". They also made reference to a "scientific analogy" which led them "to conclude that it is full of life and intelligence". Their naïve methodology aimed at making science and religion "meet together and recognise each other's claim". See Stewart B. and Tait P.G. 1894, pp. 5 and 272.

Afterword: a theoretical heritage

Although the specific theoretical models of Larmor's aethereal matter and J.J. Thomson's discrete structure of radiation were formally dismissed in the transition between late nineteenth century and early twentieth century theoretical physics, at a deeper level we find a persistence of themes or conceptions. The general conception of an intimate link between matter and energy, in particular between the structure of the electromagnetic field and elementary corpuscles, survived and found new implementations: for instance, Einstein's conceptions on matter and energy. The more general commitment to integrate continuous and discrete representations of the physical world survived as well. I think that the debates on electric charge, matter and energy, which took place in Great Britain in the late nineteenth century, can be considered one of the roots which fed twentieth century theoretical physics.[1] I think that the fruitfulness of those debates consisted in the queries they raised and in the process of integration they triggered off. In the course of the twentieth century, those queries were reinterpreted or, in some cases, neglected: in any case, the answers subsequently given became alien to the scientists who had formulated them.[2] Indeed, Einstein's theories are quite different from Larmor and J.J. Thomson's theories, both at the level of specific theoretical models (*first level*), and at the level of methodological attitudes towards models and representations (*third level*). The specific theoretical content of Einstein's 1905 papers on the inertia of energy and on quanta of energy can be deemed not comparable (even *incommensurable*) with the electromagnetic theories J.J. Thomson and Larmor outlined early in the 1890s. The last generation of natural philosophers made use of theoretical models which, in the course of the twentieth century, were looked upon by physicists as at least outmoded if not definitely wrong. The meaning and the role of conceptual models changed: the meaning and the actual practice of theoretical physics changed as well.[3] At the same time, at the level of conceptual streams, (*second level*),

[1] On the existence of a meaningful link between late nineteenth century electromagnetic theories and twentieth century *quantum* theories, see Giannetto E. 2007, p. 178. In the 1980s, Buchwald saw a conceptual overturn in the passage from the former to the latter, whereas I see both elements of continuity and discontinuity. See Buchwald J.Z. 1985a, p. 41.

[2] The so-called community of *Maxwellians* was not at ease with the most successful interpretations of *Relativity* theory and *Quantum* theory. See Warwick A. 2003, chapters 7 and 8.

[3] In the subsequent years, the role of conceptual models became less and less important in theoretical physics. I find that, in the last decades of the twentieth century, theoretical

I find in Einstein the same commitment to integrate discrete and continuous models in the description of matter and energy, as I find in J.J. Thomson and Larmor. If I see meaningful differences at the first and third level, I see meaningful analogies at the second level.

Some decades ago, a historian, Giusti Doran, pointed out a deep conceptual link between Larmor and Einstein; more than a half century before, the 1922 Nobel Prize winner Millikan had claimed the existence of a similar continuity between J.J. Thomson and Einstein. I find that they failed to satisfactorily explain that continuity, for they failed to identify the different levels involved in the comparison. Nevertheless, the connection between J.J. Thomson and Larmor's theories, on the one hand, and Einstein's, on the other hand, is deep and meaningful. They all tried to bridge the gap between matter and energy; moreover they tried to bridge the gap between continuous and discrete models for matter, and between continuous and discrete models for electromagnetic energy. The papers the young Einstein wrote in 1905, in particular those on the inertia of energy and on the new "heuristic point of view" on radiation, would appear less astonishing if only we took into account British electromagnetic theories which emerged in the late nineteenth century, alongside the better known Continental theories (Lorentz, Poincaré, …).[4]

In this *Afterword*, I would like to discuss in some detail the nature of the link between J.J. Thomson and Larmor's theories, and Einstein's theories. I would like to discuss the sources and, at the same time, undertake a dialogue with some interpretations put forward in secondary literature. I will take the two links into account separately. First of all, I will focus on the comparison between Larmor's theoretical model of aether and inertia, and the new model of aether which Einstein devised after 1905. Then I will focus on the comparison between J.J. Thomson hypotheses on the nature of electromagnetic radiation and Einstein's 1905 theoretical model of light *quanta*.

A suitable starting point is offered by the short paper Einstein wrote in September 1905 on the connection between the inertia of matter and its

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physics has suffered a sort of *formalistic drift*, corresponding to an analogous and more general formalistic drift in physics training. I am indebted to B. Bertotti for informal talks on the subject.

[4] The deep conceptual links among electrodynamics, inertia of energy and light *quanta* in Einstein's 1905 papers were pointed out by M. Klein some decades ago. See Klein M. 1964, p. 6. More recently, B.R. Wheaton claimed that an "integral part of Einstein's rejection of the medium for light waves was his suggestion of the lightquantum hypothesis". See Wheaton B.R. 1983, p. 106.

content of energy.[5] At the outset, Einstein placed his trust in the "Maxwell-Hertz equations for empty space" and in his own *Relativitätprinzip*. Then he took into account both the electromagnetic radiation sent forth by a body and the remaining energy of the body, when observed from two different inertial reference frames. In the end, he found that when "a body loses an amount L of energy, its mass decreases of L/V^2", where v is the velocity of light. Finally he assumed that, in general, "the mass of a body is a measure of its content of energy".[6]

With regard to the first level, Larmor and Einstein's specific models are quite different. On the one hand, we have an electron, namely a microscopic concentration of rotational energy, corresponding to a concentration of electric energy: when in motion, it should experience an electromagnetic inertia. On the other hand, we have a macroscopic body, which, after having sent out electromagnetic radiation, finds its energy shortened by a precise, given amount. As a consequence, its inertia should decrease in a proportional way. At the first level of specific theoretical models, there are not many similarities: microscopic, dynamical structures in the aether, on the one hand, and macroscopic bodies and a macroscopic energy balance, on the other. The analogies can be found at the second level, wherein both Larmor and Einstein realised a process of *substantialisation* of the electromagnetic energy and, conversely, a process of *desubstantialisation* of matter. The two complementary processes led, in both cases, to state equivalence between inertia of matter and electromagnetic energy.

After 1894, Larmor continued to inquire into the aethereal concentration of energy which was peculiar to his electron. In 1895, in the first lines of the second paper of the trilogy "A Dynamical Theory of the Electric and Luminiferous Medium", he re-introduced "electrons or permanent strain-centres in the aether, which form a part of, or possibly the whole of, the constitution of the atoms of matter". The same model was put forward in a

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[5] On 18 Mars 1905, *Annalen der Physik* received the paper "Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtpunkt" the young Einstein had sent the day before. The paper, then published in the *Annalen*, put forward a new interpretation of the generation and transformation of light. On 30 June the *Annalen* received the paper "Zur Elektrodynamik bewegter Körper", which correspond to what we now call Special Theory of Relativity. On 27 September, the short paper on the inertia of energy, "Ist die Trägheit eines Körpers von seinem Energieinhalt abhängig?", was received.

[6] See Einstein A. 1905c, p. 641: "Gibt ein Körper die Energie L in Form von Strahlung ab, so verkleinert sich seine Masse um L/V^2. Hierbei ist es offenbar unwesentlich, dass die dem Körper entzogene Energie gerade in Energie der Strahlung übergeht, so dass wir zu der allgemeineren Folgerung geführt werden: die Masse eines Körpers ist ein Maß für dessen Energieinhalt; …"

following page, where he tried to devise a specific model of rotational aether and rotational strains giving rise to electrons. He surmised that, "if the nuclei of the electrons are supposed small enough, the inertia of matter would be definitely represented by the electric inertia of electrons". In the same paper, he described aether "as containing a distribution of electrons, that is of intrinsic centres or nuclei from each of which a configuration of rotational strain spreads out into the surrounding space". Moreover, every electron, when in motion through aether, will "carry its atmosphere of strain along with it, practically without alteration unless the velocity of the electron is so great as to approximate to the velocity of radiation".[7]

These repeated references to aether, which was the keystone of Larmor's theory, appear definitely in contrast with the sharp rejection of aether announced in Einstein's 1905 electrodynamics. Nevertheless, after the accomplishment of his General Relativity, Einstein himself began to take into account a new kind of aether. It is known that, in 1920, he held a lecture at Leiden in honour of Lorentz, wherein he outlined a new, more sophisticated model of aether. In that outline Giusti Doran found an implementation of "the primordial medium of Thomson's vortex-atom and Larmor's strain-center electron". In order to appreciate to what extent Giusti Doran's claim is convincing, and in order to evaluate similarities and differences in Larmor's and Einstein's models of aether, we must analyse both Larmor's other sources and Einstein's Leiden lecture.[8]

In 1897, in "A Dynamical Theory of the Electric and Luminiferous Medium – part III: Relations with Material Media", Larmor qualified aether as a "continuous, homogeneous, and incompressible medium, endowed with inertia and with elasticity purely rotational". In that kind of medium, electrons

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[7] Larmor J. 1895, pp. 695, 697 and 706.

[8] See Giusti Doran B. 1975, p. 258. Miller quoted a letter sent from Einstein to Lorentz on June 1916, where Einstein, for the first time, took into account a new kind of aether consistent with his General Relativity. Other conceptual developments can be found in a subsequent paper published in 1918, "Dialog über Einwände gegen die Relativitätstheorie". See Miller A.I. 1984, p. 55. There were some changes in Einstein's theoretical attitude towards aether, in the course of his scientific career. We could start from 1894, when he was sixteen years old, and wrote a short paper on the state of aether in a magnetic field. Around 1899 he cast doubt on the usefulness and consistency of aether, as documented by some letters to his fiancée M. Mari č. His 1905 rejection of aether is well known. In 1909 the rejection of aether was based on two grounds: the validity of the Principle of Relativity and the double representation, both wave-like and particle-like, of electromagnetic radiation. The subsequent restoration of a new kind of aether is less known. For a wide discussion on the nature of Einstein's *aether*, see chapter 5 of Kostro L. 2000. In particular, on the words "aether", "physical space" and "field", or "total field", in Einstein's papers published from 1918 to 1955, see, pp. 184-5.

"exist as point-singularities, or centres of intrinsic strain", and "atoms of matter are in whole or in part aggregations of electrons in stable orbital motion". In that long paper of the trilogy, Larmor expressed even meta-theoretical remarks about aether and its functions. Aether was "entirely supersensual", he claimed: we could even "ignore the existence of an aether altogether" and confine ourselves to describing phenomena "in accordance with the system of mathematical equations". Although "in strictness, nothing could be urged against this procedure", he thought that aether offered "so overwhelmingly natural and powerful an analogy" that to assume its existence was useful "for purposes of practical reason". In that stress on the power of analogy I find one of the hallmarks of late nineteenth century theoretical physics. In the following passage, he specified the meaning of that *practical* function of aether: the "aim of a theory of aether" was "the practical one of simplifying and grouping relations and of reconciling apparent discrepancies in existing knowledge".[9] This detached phenomenology sounds quite astonishing, for aether, in Larmor's theory, was the common ground for both the electromagnetic actions and the structure of matter. Moreover, even in his 1897 paper, he was committed to devising detailed theoretical models for the dynamical features of aether and electrons. The fact is that, in Larmor's theoretical physics, aether served two different purposes, the first being theoretical and the second meta-theoretical or methodological. It was the universal, primitive substratum, giving rise to matter and fields, but it also was a *mental tool*, which allowed the scientist to go beyond the accumulation of "descriptive schemes of equations". In that sense, aether was "more or less a priori". Larmor thought that, "without the help of simple dynamical working hypotheses", we would be prevented from going "very far below the surface" of phenomena involving matter and fields. Without aether we could not understand "how this interaction between continuous aether and molecular matter takes place".[10] On the third-level *methodological-philosophical* context, the role played by aether was not so different from the role of space or time, namely the role of entities which allow us to represent a wide set of phenomena.

In the "Preface" to his 1900 *Aether and matter*, Larmor qualified the "suprasensual aethereal medium" as a conceptual tool, which "may of course be described as leaving reality behind us". According to Larmor, it was indeed a "result of thought", an attempt to interpret physical reality: it was "more than a record or comparison of sensations". Larmor's specific theoretical model involved "a system of discrete or isolated electric

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[9] Larmor J. 1897, p. 207.
[10] Larmor J. 1897, p. 215.

charges" embedded in "an elastic aether"; they were as "singular points involving intrinsic strain in the structure of the medium". Matter had the same structure of electricity, consisting of "a permanent nucleus or singularity in and belonging to the aether". An atom of matter could be represented by means of two slightly different theoretical models: either "a minute vortex ring in perfect fluid", echoing W. Thomson's models, or a "centre of permanent strain in rotationally elastic medium". [11]

In order to overcome "the somewhat misleading antithesis of contact *versus* distant actions", and in order to understand the nature of matter and interactions, Larmor saw a solution "which recommends itself on purely philosophical grounds". His general model entailed "the fundamental consequence that the structure of matter is discrete or atomic" but "the ultimate reality" required a conceptual shift "from sensible matter to a uniform medium which is a *plenum* filling all space". In general terms, the discrete structure of matter stemmed from a pre-existent continuous medium, so that "all events occur and are propagated in this *plenum*". That discrete structure was kinematical or dynamical in its nature, for "ultimate elements of matter" consisted of "permanently existing vortices or other singularities of motion and strain located in the primordial medium". He specified that those ultimate, elementary elements could "never arise or disappear".[12]

His model entailed a remarkable, unified view for both electromagnetic fields and matter. On the one hand, electromagnetic actions consisted of "elastic actions across the aether", so that "an electric field must be a field of strain". On the other hand, *protions*, endowed with intrinsic electric charge, "must be surrounded by a field of permanent or intrinsic aethereal strain" and therefore they must be "in whole or in part a nucleus of intrinsic strain in the aether". Propagations of pure fields and propagation of elementary matter yielded the same effects; in other words, Maxwell's *displacement* currents and convective electric currents shared the same intimate nature. He portrayed *protions* or *electrons* as something which "can

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[11] Larmor J. 1900, "Preface", pp. vi-vii. Larmor thought that all theoretical models were provisional and not complete, and he guarded against the pursuit of "the impossible task of reducing once for all the whole complex of physical activity to rule". See pp. x, xiv and xv.

[12] Larmor J. 1900, pp. 23-24. Larmor traced back his general model to recent and less recent traditions of Natural Philosophy. At the dawn of modern science, "the ideal towards which Descartes was striving", namely the identification of matter with space, appeared to Larmor an instance of a long-lasting conceptual stream. He found that W. Thomson had implemented "on a precise scientific basis" Descartes' ideal, having put forward a theory connecting matter to aether.

move or slip freely about through that medium much in the way that a knot slips along a rope". [13]

At this point, we can take into account the lecture Einstein held in Leiden in 1920, *Äther und Relativitätstheorie*, which is interesting not only because of the *meaning variance* experienced by the concept of aether. That text shows us an Einstein committed to integrate not only theoretical models of matter with theoretical models of field, but also an electromagnetic field with a gravitational field. He started from the conceptual tension between the electromagnetic equations and their mechanical explanation: "Maxwell's laws … were clear and simple, the mechanical interpretations clumsy and contradictory", he stated. Einstein found a dualism between mechanics and electromagnetism and thought that the dualism could be traced back to Hertz's conception of "electric and magnetic force as fundamental concepts side by side with those of mechanics". In other words, Hertz's theory had "the defect of ascribing to matter and ether, on the one hand mechanical states, and on the other hand electrical states, which do not stand in any conceivable relation to each other".[14] He claimed he had managed to realize an important unification in 1905, for "according to the special theory of relativity, both matter and radiation are but special forms of distributed energy". Nevertheless, he acknowledged, "the special theory of relativity does not compel us to deny ether". Although he thought that "we must give up ascribing a definite state of motion to it", he also thought that to deny the aether in itself "is ultimately to assume that empty space has no physical qualities whatever". A new point of view seemed to him "justified by the results of the general theory of relativity". Moreover, in order to make Mach's concept of inertia match with contiguous action, he thought that we should invent a sort of "Mach's ether", an aether which, "not only *conditions* the behaviour of inert masses, but *is also conditioned* in its state by them".[15] That hypothesis can be considered as the gravitational counterpart of J.J. Thomson and Larmor's hypothesis that the inertia of an electric particle could stem from its interaction with the electromagnetic field.

It is worth noticing that, in 1912, on the path towards a *relativistic* theory of gravitation, in a paper on "gravitational induction", Einstein considered "likely" Mach's hypothesis that "the entire inertia of a massive particle is an

[13] Larmor J. 1900, pp. 26 and 86.

[14] Einstein A. 1920, in Einstein A. 2002, pp. 165-7.

[15] Einstein A. 1920, in Einstein A. 2002, pp. 171-4. According to Einstein's view, there is not an *empty* space but a *physical* space whose nature is specified by electromagnetic or gravitational fields. In this sense, we can imagine an aether, which can be identified with that physical space: such an aether could not be conceived as a specific reference frame. See, for instance, Einstein A. 1953, p. XVII: "… there is no space without a field."

effect of the presence of all the other masses". In other words, he thought that inertia was a gravitational effect, "based on a sort of interaction" between the particle itself and the other masses. In 1949, in his "Autobiographical Notes", Einstein remarked that what he had named *Mach's Principle*, namely "inertia would have to depend upon the interaction of the masses", did not fit "into a consistent field theory", for it "presupposes implicitly … masses and their interactions as the original concepts". Indeed, the so-called Mach's Principle and the subsequent "Mach's ether" stemmed from different conceptual models of physical *space*.[16]

In 1920, Einstein looked upon his "ether of the general theory of relativity" as the heir of *Mach's aether*, namely "a medium which is itself devoid of *all* mechanical and kinematical qualities, but helps to determine mechanical (and electromagnetic) events".[17] The new aether had to be intrinsically gravitational, he claimed, for we cannot define space without taking into account gravitation. Nevertheless, that requirement restored the dualism between gravitation and electromagnetism: the two fields, gravitational and electromagnetic, when considered independent from each other, led to a fundamental asymmetry. On the one hand, we have a gravitational field "inseparably bound up with the existence of space"; on the other hand, "a part of space may very well be imagined without an electromagnetic field". In other words, "in contrast with the gravitational field, the electromagnetic field seems to be only secondarily linked to the ether". For he assumed that "the elementary particles of matter are also, in their existence, nothing else than condensations of the electromagnetic field", his theoretical model required "two realities which are completely separated from each other conceptually, although connected causally". Matter appeared like the two sides of a coin: on the first side, matter was conceived as a concentration of electromagnetic energy; on the other, it was intrinsically linked to the gravitational field or gravitational aether. The demanding task of "comprehending the gravitational field and the electromagnetic field together as one unified conformation" appeared to Einstein as the greatest achievement of twentieth century "theoretical physics".[18]

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[16] Einstein A. 1912, in *The Collected Papers of Albert Einstein*, vol. 4, p. 177. See the quotation and J. Stachel's remarks in Stachel J. 2006, in Renn J. (ed.) 2006, Vol. 1, pp. 90-1. See Einstein A. 1949, in Einstein A. 1951, p. 29.

[17] Einstein A. 1920, in Einstein A. 2002, p. 177. See Renn J. 2006, in Renn J. (ed.) 2006, Vol. 1, p. 40, footnote 36: "The observation that the assumption of the aether being immobile amounts to the assignment of a mechanical property is due to Einstein, …"

[18] Einstein A. 1920, in Einstein A. 2002, pp. 176-80.

If we take earnestly into account Einstein's 1920 lecture, then we must take earnestly into account Giusti Doran's interpretation, which called for the existence of a conceptual stream connecting Larmor's 1894 theory with Einstein's 1920 remarks. To begin with, I do not agree with her interpretation of Einstein's 1920 aether as "the physical medium of electromagnetic propagation", for that new aether had more gravitational than electromagnetic features. I find Einstein's attempt to devise a new kind of aether, endowed with both gravitational and electromagnetic properties, more interesting than "Einstein's attempts after 1915 … to construct a field theory of the material particle".[19] Nevertheless, I find in Larmor and Einstein a common commitment to look for a unified theory of matter and radiation. Larmor's aether and Einstein's new aether were different, but in some way complementary: whereas Larmor hoped to explain gravitation by means of his mechanical-electromagnetic proto-aether, Einstein was looking for an electromagnetic integration of his "gravitational aether". For both of them, the tension towards a great unification was a long-lasting commitment: they tried to include all properties of matter, energy and interactions in a unified view.

The comparison between Larmor's aether and Einstein's aether leads us to a more general comparison involving the nature of their mental representations, namely their meta-theoretical options. Miller stated that the philosophical, scientific and technological "matrix" wherein the young Einstein was embedded "placed a high premium on visual thinking". I find this statement true only in part, for, besides a theoretical physics relying on specific and general conceptual models, at the turn of the century, in German speaking countries, there was a tradition of mathematical phenomenology which firmly opposed imagery and models. It seems to me that Miller acknowledged the existence of a double tradition when he stressed the effects of "Mach's empiricist emphasis" and the intellectual commitment of Hertz and Poincaré to overcome that emphasis or conception. Differently from Miller, I find that Einstein's methodological attitude, and therefore his interpretation of theoretical physics, was closer to "Hertz's brilliant use of axioms as organizing principles" than to Boltzmann's "mental pictures".[20] However, as Miller himself pointed out, "Einstein knew by 1905

[19] Giusti Doran B. 1975, p. 256.

[20] In many essays Boltzmann stressed the subjective and historical features of physical models and physical world-views. He, for instance, qualified the choice between atomism and energetics as "a matter of taste". In another essay he repeated that "[a]ll our ideas are subjective". Then he focused on the intrinsic, historical nature of "theoretical physics" and "all branches of man's intellectual activity". See Boltzmann L. 1974, pp. 36, 41 and 79. Miller noted the difference between Hertz's trust in the laws of thought "in the Kantian

that the electromagnetic world-picture could not succeed", and that the "electromagnetic theory and mechanics could not serve as the basis for all of physics": he refused "constructive efforts" and looked for "universal formal principles".[21] His models were quite different, for instance, from J.J. Thomson and Larmor's specific models for the structure of matter and fields: aethereal electrons and electric tubes of force. I find that Einstein's "mental pictures, such as ideal measuring rods and clocks or point masses of electrons" are imagery and models which cannot be associated either to Boltzmann's *German* models or to J.J. Thomson and Larmor's *British* models. The microscopic structures devised by *British* and *German* physicists were quite different from Einstein's abstract, macroscopic rods and clocks, or from Einstein's microscopic electrons and *quanta*.[22]

Getting back to Einstein's 1905 first paper, we notice that the title shows its theoretical *flavour*: a "heuristic point of view" concerning "the production and transformation of light". Purely theoretical was the starting point of the paper, namely "the deep formal difference between the theoretical models" of matter and electromagnetic radiation. He remarked that matter was represented by means of "a very great number of atoms and electrons", endowed with specific positions and velocities, while electromagnetic radiation was represented by means of "a continuous function through space". Electromagnetic energy, in particular, was represented as "a spatially continuous function", while energy of matter was represented as a discrete "summation over a finite number of atoms and electrons". Einstein thought that the deep asymmetry between matter and radiation could be overcome by the assumption that "the energy of light propagated in a discontinuous way through space". That assumption was consistent with phenomena like "black body radiation" or "the creation of cathode rays by means of ultra-violet light". In brief, electromagnetic energy was supposed to "consist of an

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sense", and Boltzmann's more dynamic and plastic representation of conceptual models evolving "in the Darwinian sense". See Miller A.I. 1984, pp. 48, 49 and 51.

[21] See Einstein A. 1949, in Einstein A. 1951, p. 53, and Miller A.I. 1984, pp. 50-51. I find that Miller's vivid picture of Einstein's 1905 theoretical approach is too "hybrid". See Miller A.I. 1984, p. 51: "We can depict Einstein's approach to an axiomatic formulation of the special relativity theory as a hybrid version of the views of Boltzmann, who emphasised mental pictures …; of Hertz's brilliant use of axioms as organizing principles, …; of Poincaré's far-reaching neo-kantian organizing principles, …; of Mach, …; of Wien's suggestion of axiomatic as a goal; and of Abraham's 1904 paper …."

[22] Miller A.I. 1984, pp. 51, 82 and 87. It is worth mentioning also Einstein's subsequent self-criticism about *rod* and *clocks*: "… strictly speaking measuring rods and clocks would have to be represented as solutions of the basic equations (…), not, as it were, as theoretically self-sufficient entities." (Einstein A. 1949, in Einstein A. 1951, p. 59)

endless number of *Energiequanten* localised in points of space".[23] Another phenomenon consistent with the hypothesis of electromagnetic *quanta* was the photoelectric effect, wherein light of suitable frequency forced metal plates to send out negative electric charges.[24]

The year before (1904), J.J. Thomson had published a booklet, *Electricity and Matter*, wherein he collected together some lectures he had held in Yale in 1903; within a few months, Thomson's booklet was translated into German. In the third chapter, "Effects due to acceleration of the Faraday's tubes", he focussed on the interaction between Röntgen rays and matter. He remarked that "Röntgen rays are able to pass very long distances through gases, and as they pass through the gas they ionise it". What he found difficult to explain was that "the number of molecules so split up is, however, an exceedingly small fraction, less than one billionth, even for strong rays, of the number of molecules in the gas". The question was: why were not all the molecules crossed by that kind of radiation affected in the same way? In other words, "if the conditions in the front of the wave are uniform, all the molecules of the gas are exposed to the same conditions": how could the fact "that so small a proportion of them are split up" be explained? Perhaps the concentration of energy able to modify the microscopic structure of matter had its seat not in Röntgen rays but in matter itself. Perhaps only high-energy molecules could experience the ionisation when interacting with the

[23] See Einstein A. 1905a, p. 132: "Zwischen den theoretischen Vorstellungen, welche sich die Physiker über die Gase und andere ponderable Körper gebildet haben, und der Maschwellschen Theorie der elektromagnetischen Prozesse im sogenannten leeren Raume besteht ein tiefgreifender formaler Unterschied [...] Nach der Maxwellschen Theorie ist bei allein rein elektromagnetischen Erscheinung, also auch beim Licht, die Energie als kontinuierliche Raumfunktion aufzufassen, während die Energie eines ponderabeln Körpers nach der gegenwärtigen Auffassung der Physiker als eine über die Atome und Elektronen erstreckte Summe darzustellen ist. [...] Nach der hier ins Auge zu fassenden Annahme ist bei Ausbreitung eines von einem Punkte ausgehenden Lichtstrahles die Energie nicht kontinuierlich auf größer und größer werdende Räume verteilt, sondern es besteht dieselbe aus einer endlichen Zahl von in Raumpunkten lokalisierten Energiequanten, welche sich bewegen, ohne sich zu teilen und nur als Ganze absorbiert und erzeugt werden können."

[24] Some decades ago, M. Klein pointed out that Hertz's discovery of such an effect was one of the "most ironic turns" in the history of physics: just when Hertz was corroborating the existence of Maxwell's electromagnetic waves, he found an effect "impossible to understand on the basis of Maxwell's theory". (Klein M. 1963, pp. 76-7) At that time, the effect was really difficult to understand in the context of Maxwell's electromagnetic theory: Lenard put forward an explanation, which had more success than Einstein's hypothesis. It should be remarked that, in 1905, the scientific community did not see a meaningful link between the photoelectric effect and the structure of light. See Wheaton B.R. 1978, p. 300. See also pp. 317-19, where Wheaton analysed Lenard's 1902 theory, and pointed out that the "triggering" theory "formed the bridge which connected the photoelectric effect to the issues of atomic structure".

rays. Nevertheless, in this case, the probability of the ionisation would have shown some kind of dependence on gas temperature, namely on its internal energy: "the ionisation produced by the Röntgen rays ought to increase very rapidly as the temperature increases".[25] This was not the case and therefore J.J. Thomson resorted to his 1893 theoretical model of electromagnetic radiation as a bundle of discrete tubes of force. He thought that the selective ionisation could be explained only if, "instead of supposing the front of the Röntgen ray to be uniform, we suppose that it consists of specks of great intensity separated by considerable intervals where the intensity is very small". According to that hypothesis, the microscopic properties of electromagnetic radiation were similar to the properties of microscopic particles: in J.J. Thomson's words, "the case becomes analogous to a swarm of cathode rays passing through the gas". Indeed, that flux of elementary corpuscles showed the same behaviour of X-rays: "the number of molecules which get into collision with the rays may be a very small fraction of the whole number of molecules". In 1904, J.J. Thomson imagined tubes of force "as discrete threads embedded in a continuous ether, giving to the latter a fibrous structure". He assumed that both aether and electromagnetic waves were endowed with a discrete structure: it was a solution, he remarked, "which I have not seen noticed".[26]

J.J. Thomson theoretical researches contributed to the re-emergence of an old conceptual stream, and to the integration between two complementary conceptual streams: continuous and discrete structures of radiation. Subsequently, the same process of integration was undertaken by Einstein in 1905. Obviously we must underline the different features of J.J. Thomson and Einstein's theories. I have already pointed out that the deep similarity is not to be found in those specific features but in the common attempt to integrate complementary conceptual streams. Besides the attempt to integrate discrete with continuous models for electromagnetic radiation, I find in J.J. Thomson a wider project of integration and unification between macroscopic and microscopic models, between physics and chemistry, and between mechanics and electromagnetic phenomena. I find in Einstein a

[25] Thomson J.J. 1904, pp. 63-4.

[26] Thomson J.J. 1904, pp. 63 and 65. As described in chapters 12 and 13 of the present book, since 1885, J.J. Thomson had shared Poynting's belief that the concept of "electric displacement" was misleading, and Poynting's preference for Faraday's tubes of force. Starting from Faraday's tubes, Thomson tried a reinterpretation of the equations for the electromagnetic fields, and (before 1897) arrived at a discrete theoretical model for matter, energy and electricity. Energy, spread and propagated by discrete units, could be found both *in* the tubes of force and in the motion *of* tubes of force; radiation had a discrete structure.

commitment to integrate macroscopic with microscopic models, and electrodynamics with thermodynamics.[27]

Now the question is: why in more recent secondary literature, has not the conceptual link between J.J. Thomson and Einstein (however problematic it may be) been taken into account? On the contrary, what appears as a sort of *missing link* in recent literature, was acknowledged as an important link by some physicists in the first half of the twentieth century.[28]

The landscape of secondary literature becomes quite different when we take into account the years spanning from the 1960s to the early 1980s. A quarter of a century ago, T. Kuhn, in the *Foreword* to Wheaton's book, claimed that, among the innovations in early twentieth century physics, "none was more difficult to accept and assimilate than Einstein's suggestion that light displays particulate properties". Carrying on with his historical reconstruction, he added that, "for nearly twenty years after Einstein's proposal in 1905, the concept of light-particles was almost everywhere rejected". At the same time, Kuhn pointed out that the same "suggestion" or "proposal" had been put forward in a different context, "far less known": the researches "associated with observations on x-rays and γ-rays".[29] The debate on the nature of those rays involved the choice between particles and waves, between the hypothesis of microscopic, atomic or subatomic particles, and the hypothesis of processes taking place in and through aether. Wheaton remarked that an intermediate solution had been put

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[27] The latter integration can be considered as the keystone of the second part of Einstein's paper: he showed that the dependence of entropy from volume, in the case of low-density and low-temperature monochromatic radiation, followed the law good for perfect gases or dilute solutions. See Einstein A. 1905a, pp. 139-43.

[28] A recent historical survey of Einstein's 1905 paper on light *quanta* begins with the sharp sentence: "Einstein was the first to propose that light behaves in some circumstances as if it consists of localized units, or quanta". Einstein's approach is compared to Planck's approach but any reference to J.J. Thomson's 1904 booklet or previous texts is missing. See Cassidy D.C. 2005, pp. 15 and 17. In a detailed and authoritative paper, J. Norton made subtle remarks on the model of *quanta* put forward by Einstein, in particular on queries concerning volume fluctuations, isothermal transformations and variability of the number of *quanta*. Nevertheless, in his "Introduction", he claimed that, differently from "special relativity and the inertia of energy", which he looked upon as "a fulfilment of the 19th century tradition in electrodynamics", Einstein's hypothesis of "spatially localized quanta of energy – stands in direct contradiction with that most perfect product of 19th century science". See Norton J.D. 2006, p. 72. The reason for this narrowing of historical perspectives can perhaps be found in what Shapin recently called "Hyperprofessionalism", namely a phenomenon involving "a narrowing of intellectual focus." Historians are probably frightened of the phantom of the old-fashioned, "big picture" history of science. See Shapin S. 2005, p. 238 and 241.

[29] Kuhn T.S. 1983, p. ix.

forward: the hypothesis of rays as *pulses* or *impulses*, for instance, "came to occupy a position midway between these extremes". He also noted that "Einstein's paper contains no reference to x-rays or to γ-rays": Einstein's path and J.J. Thomson's path did not start from the same point.[30] Wheaton mentioned the conceptual link between J.J. Thomson' 1903 lectures and 1893 book, wherein the model of a discrete structure of electromagnetic radiation had already been put forward. He remarked that, in 1903, Thomson "briefly revived ideas he had had for a decade about the macroscopic structure of the electromagnetic field". Moreover, he stressed that, since 1893, "Thomson had speculated that lines or 'tubes' of electric force might be more than just mathematical abstractions".[31]

In 1978, Tarsitani remarked that the query about the nature of radiation "had already been raised by J.J. Thomson before 1905, without any reference to photoelectric effect". Comparing Thomson's and Einstein's theoretical approaches, he found both a similarity and a difference. The wave-like nature of electromagnetic radiation was looked upon as "an average effect" in both theories: however, Thomson's discrete units were "electrons and individual tubes of force, rather than energy in itself".[32] It seems to me that the distinction between energy and tubes of force missed the point: in Thomson's view, tubes of force *were* nothing else but the structure of the electromagnetic field.

More explicitly, in 1967, McCormmach stated that the conceptual link between J.J. Thomson and Einstein deserved some attention: he found that Einstein's "views have certain close similarities with Thomson's, and they should be examined". Both Thomson and Einstein, differently from Planck, surmised a discontinuous structure of electromagnetic energy "not restricted to the material oscillators" but intrinsic to "the radiation field". Nevertheless, McCormmach specified, "Thomson's statements on the structure of light were not accompanied by a quantitative prediction, and Einstein's were".[33] Generally speaking, this is not completely true: in 1904, in the third chapter of his booklet *Electricity and Matter,* Thomson outlined a quantitative model of electromagnetic pulses emerging from the sharp deceleration of an electric particle surrounded by its own tubes of force. He stated that those pulses "constitutes in my opinion the Röntgen rays" and that the rays were produced "when the negatively electrified particles which

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[30] Wheaton B.R. 1983, pp. 16 and 109. The title of Wheaton's book, *The Tiger and the Shark*, was singled out from an expressive passage J.J. Thomson wrote in his 1925 book *The Structure of Light*. See Wheaton B.R. 1983, p. 306.

[31] Wheaton B.R. 1983, p. 78. See also p. 138.

[32] Tarsitani C. 1978, pp. 255-6.

[33] McCormmach R. 1967, pp. 370-71.

form the cathode rays are suddenly stopped by striking against a solid obstacle", for instance the wall of a cathode tube. The model and the calculations were based on a charged particle, either "moving so slowly that the lines of force are uniformly distributed around it", or moving so close to radiation speed that "before the stopping the Faraday tubes were all congregated in the equatorial plane of the moving particle". Assuming that whatsoever "disturbance communicated to one end of the tube will therefore travel along it with a constant and finite velocity", he identified that perturbation with Röntgen rays. For the energy of the pulse he found $(2/3)e^2v^2/\delta$, where δ is the pulse width, and *e* and *v* are velocity and electric charge of the decelerating particle. The width could be reckoned by the change in the magnetic field due to the superposition of "tangential Faraday tubes", namely the *new* tangential component of Faraday tubes, to the ordinary radial component.[34]

However, J.J. Thomson and Einstein's ways of leading to the hypothesis of a discrete structure of electromagnetic radiation were quite different. As McCormmach remarked, J.J. Thomson "was not naturally drawn to the statistical arguments of Planck and Einstein", namely to the problematic link between electrodynamics and thermodynamics. McCormmach explicitly pointed out another aspect: in the British context, wherein models of aether relied on a long-lasting tradition, energy units were identified with the discrete structure of aether. That solution was also the suitable contrivance able to overcome the thermodynamic difficulties associated to a continuous aether endowed with an infinite number of degrees of freedom.[35]

When McCormmach drew his conclusion he claimed that "Thomson's theory of light was inconclusive" and "the predicted structure remained largely qualitative in theory and undetectable in the laboratory". With regard to this appraisal, I confine myself to three remarks. First, the bold, general hypothesis of discreteness of radiation is the hallmark of both Einstein's 1905 paper, and J.J. Thomson's 1893 and 1904 books. Second, there are more detailed experimental references in the last part of Einstein's 1905 paper than in J.J. Thomson's books. Third, J.J. Thomson's general, second level hypothesis on the structure of light was as empirically testable as Einstein's; his specific model of tubes of force was testable as well. In reality, that model was more difficult to check directly and, afterwards, it

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[34] Thomson J.J. 1904, pp. 53-9. The "pulse" model of X-rays had been put forward by J.J. Thomson since 1898: a detailed reconstruction can be found in Wheaton B.R. 1981, pp. 371-74. He stressed the "unavoidable aspect of discontinuity in the impulse hypothesis, in contrast to the continuity implied by periodic light waves" (p. 378). See also Wheaton B.R. 1983, pp. 24-9.

[35] McCormmach R. 1967, pp. 373-6.

was found unsuitable to explain the *twofold* behaviour of light. The fact is that Einstein, differently from Thomson, did not put forward any specific model. His general model was easier to check: only the foreseen effects had to be looked for. Any further inquiry into the specific structure of quanta was not required: Einstein had not expressed any claim on it. I do not find J.J. Thomson's theoretical attempt meaningful and fruitful because of its first level, specific features, but because of its second level, general model. The most interesting contribution to the history of physics was not the specific discrete model of radiation, but the commitment to integrate discrete and continuous aspects of radiation, and the commitment to bridge the gap between the microscopic structure of matter and the microscopic structure of radiation. In this perspective we can appreciate J.J. Thomson's contribution to theoretical physics. Only in this perspective do I find intelligible the last lines of McCormmach paper, where he stated that "Thomson contributed to the twentieth-century revolution in the theory of light".[36] Outside my perspective, without a definite distinction between the two levels, I would be unable to see why his theory should be both "inconclusive" and "revolutionary". In my opinion, if "inconclusive" suits the first level, "revolutionary" suits the second level.

In 1963, M. Klein remarked that the theoretical dualism between the microscopic discontinuity of matter and the continuity of electromagnetic fields "was probably noticed by others besides Einstein". Nevertheless, "there is no record", he claimed, "that anyone else suggested removing it in the drastic way that Einstein then proposed". He quoted J.J. Thomson's 1904 booklet, and Millikan's subsequent overlap between J.J. Thomson and Einstein's theories. In the end, he noted that Einstein's 1905 paper did not show any evidence "that he was aware of or influenced by Thomson's ideas". As far as I know, there is no evidence that Einstein read Thomson's booklet: at the utmost, I can only mention that, in 1904, a German edition was still available.[37]

[36] McCormmach R. 1967, p. 387. McCormmach reported both British general reaction and J.J. Thomson's personal reaction to subsequent developments of Quantum Physics, after the corroboration of Einstein's photoelectric law, in the 1910s, and after the interpretation of Compton's X-ray scattering, in the 1920s. According to McCormmach, J.J. Thomson was committed to show that "Faraday's lines of force and Newtonian mechanics were sufficient to account for all the results of the quantum theory of light". (p. 385) The last pages of the autobiography J.J. Thomson published in 1936 are consistent with McCormmach's remarks. See Thomson J.J. 1936, pp. 431-33.

[37] Klein M. 1963, pp. 62 and 80. In 1953, Whittaker had acknowledged that the "apparent contradiction between the wave-properties of radiation and some of its other properties had been considered by J.J. Thomson in his Silliman lectures of 1903". See Whittaker E.T. 1953, p. 93.

The fact is that Millikan, in *The Electron*, the book published in 1917, and in his 1924 *Nobel lecture*, took explicitly into account the link between J.J. Thomson and Einstein. In the tenth chapter of Millikan's 1917 book, "The nature of radiant energy", there is a disparaging appraisal of Einstein's theoretical model of electromagnetic radiation. Millikan thought that the existence of aether could neither be denied nor had actually been denied by the upholders of the theory of *Relativity*. Nevertheless, he acknowledged that some difficulties arose "after the discovery of the electron and in connection with the relations of the electron to the absorption or emission of such electromagnetic waves". According to Millikan, J.J. Thomson had been the first to point out explicitly the query in 1903, in a lecture at Yale. [38] Two phenomena had been taken into account: the photo-electric effect and the unexpected rare occurrence of X-rays scattering, when X-rays crossed matter. Those phenomena could be accounted for "in terms of a corpuscular theory", where "the energy of an escaping electron comes from the absorption of a light-corpuscle". Einstein's 1905 hypothesis seemed to Millikan a daring implementation of Thomson's theoretical model: he wrote that "the boldness and the difficulties of Thomson's 'ether-string' theory did not deter Einstein in 1905 from making it even more radical". Einstein's hypothesis appeared to Millikan definitely unreliable: "I shall not attempt to present the basis for such an assumption, for, as a matter of fact, it had almost none at the time".[39] In any case, and independently from the unsatisfactory theoretical foundations, he acknowledged that the process of "emission of energy by an atom is a discontinuous or explosive process". That "explosive" feature suggested to Millikan the hypothesis that the cause of the photoelectric effect or X-rays scattering was placed in matter rather than in radiation. That alternative model was called by Millikan the "loading theory", for the process of accumulation of energy inside the atom was its

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[38] See Millikan R.A. 1917, pp. 217-9. After having reported four reasons in favour of "the ether or wave theory" of light, he regretted that "a group of extreme advocates of the relativity theory" had recently expressed "some opposition of a rather ill-considered sort". Nevertheless, Millikan thought that *Relativity* theory, as it was "commonly regarded", had "no bearing whatever upon the question of the existence or non-existence of a luminiferous ether". He claimed that aether was the "carrier for electromagnetic waves, and it obviously stands or falls with the existence of such waves *in vacuo*". It seemed to him that "this has never been questioned by anyone so far as I am aware".

[39] Millikan R.A. 1917, pp. 221-3. Einstein's "lokalisierten Energiequanten" appeared to Millikan nothing more than a specific feature of J.J. Thomson's *fibrous aether*. In eight pages (from p. 231 to p. 238), there are eight occurrences of expressions like "Thomson-Einstein theory", "Thomson-Einstein hypothesis of localized energy", "Thomson-Einstein theory of localized energy", "Thomson-Einstein assumption of bundles of localized energy travelling through the ether", or eventually "Thomson-Einstein semi-corpuscular theory".

main feature. According to Millikan, an unknown mechanism concerning the structure of the atom, and, some unknown structure of aether were involved. In this way, he completely overturned the meaning of Einstein's *quantum* theory: not only, in his words, the "Thomson-Einstein theory throws the whole burden of accounting for the new facts upon the unknown nature of the ether", but Thomson and Einstein were associated in their supposed attempt to make "radical assumptions about its structure".[40] That J.J. Thomson had always been committed to investigating the supposed structure of aether, sounds quite reasonable; that Einstein was credited with having shared, in 1905, the same commitment, sounds quite strange. That Einstein's theoretical model did not require any aether was perhaps beyond Millikan's conceptual horizon.

After seven years, in his 1924 Nobel lecture, he recollected his efforts to find "some crucial test for the Thomson-Planck-Einstein conception of localized radiant energy." He stated that such a conception "was introduced by J.J. Thomson in 1903, in order to account for two newly discovered experimental facts", and that, subsequently, the "Thomson semicorpuscolar conception of localized radiant energy was taken up in 1905 by Einstein". According to Millikan, Einstein's theory combined Thomson's conception with "the facts of quanta discovered by Planck through his analysis of black-body radiation", in order to obtain "an equation which should govern, from his viewpoint, the interchange of energy between ether waves and electrons". Although "the reality of Einstein's light quanta may be considered as experimentally established", he thought that "the conception of the localised light quanta out of which Einstein got his equation must still be regarded as far from being established".[41]

Following Millikan from 1917 to 1924, we find that his evaluation on the experimental reliability of Einstein's theory changed, but he did not change his interpretation on the close relationship between J.J. Thomson's 1903 theoretical model of radiation and Einstein's "quanta". Two elements are worth noting: first, Millikan failed to acknowledge Thomson's 1893 theoretical contribution, and, second, he misunderstood the nature of the conceptual link between J.J. Thomson and Einstein.[42]

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[40] Millikan R.A. 1917, pp. 234-7.

[41] Millikan R.A. 1924, pp. 61-65. Once again he only saw two alternatives: either "the mechanism of interaction between ether waves and electrons has its seat in the unknown conditions and laws existing within the atom", or such a *mechanism* "is to be looked for primarily in the essentially corpuscular Thomson-Planck-Einstein conception as to the nature of the radiant energy ".

[42] R. Stuewer pointed out two elements. First, "Millikan, in common with almost all physicists at the time, rejected Einstein's light quantum hypothesis as an interpretation of

Planck himself, in a paper published in 1910 in *Annalen der Physik*, associated Einstein and Stark to J.J. Thomson and Larmor. He noted that the four physicists had put forward an extremely radical interpretation of electromagnetic radiation: even in the case of "electromagnetic processes in pure vacuum", they had imagined "diskreten Quanten" or "Lichtquanten". He made reference to a paper Einstein had published in 1909 in *Physikalische Zeitschrift*, where the young scientist had shown that energy fluctuations of radiation, and momentum fluctuations, involved two terms: an expected wave-like term and an unexpected particle-like term.[43] Although Planck did not explicitly quote from it, in 1909 Larmor had published a paper (in *Proceedings of the Royal Society*) devoted to the statistical interpretation of electromagnetic radiation. According to Larmor, a "ray", or "filament of light", was represented as "a statistical aggregate": the statistical "constitution of the ray" mirrored the statistical distribution of energy "in the radiant element of mass". The "general thesis" he developed was a "molecular statistics of distribution of energy", which gave birth to a re-derivation of "Planck's formula for natural radiation".[44]

In his 1910 review, Planck faced the general query concerning continuity and discontinuity, both for matter and energy, but, in the end, he found that every "Korpuskulartheorie" appeared weak and unreliable to people "relying on the electromagnetic nature of light". He thought that a radical assumption of discontinuity in the structure of light would have led physics

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his photoelectric-effect experiments of 1915". Second, Millikan himself, in his Autobiography, published in 1950, revised his appraisal and stated that the phenomenon "scarcely permits of any other interpretation than that which Einstein had originally suggested". Stuewer qualified that sharp change as an instance of "revisionist history". On this issue, and on the attitudes of the scientific community towards Einstein's hypothesis in the 1910s, see Stuewer R.H. 2006, pp. 543-8.

[43] See Planck M. 1910, p. 761: "Am radikalsten verfährt hier von den englischen Physikern J.J. Thomson, auch Larmor, von den deutschen Physikern A. Einstein und mit ihm J. Stark. Dieselben neigen zu der Ansicht, daß sogar die elektrodynamische Vorgänge im reinen Vakuum, also auch Lichtwellen, nicht stetig verlaufen, sondern nach diskreten Quanten von der Größe $h\nu$, den ‚Lichtquanten', wobei ν die Schwingungszahl bedeutet." See Einstein A. 1909, pp. 188-190.

[44] See Larmor J. 1909, p. 91. He reminded the reader that in 1902 he had already published a very brief *Report* (eleven lines), "in which it was essayed to replace Planck's statistics of bipolar vibrators by statistics of elements of radiant disturbance." (Larmor 1909, pp. 86-8 and 91) See Larmor J. 1902 p. 546: "… various difficulties attending this [namely Planck's] procedure are evaded, and the same result attained, by discarding the vibrators and considering the random distribution of the permanent element of the radiation itself, among the differential elements of volume of the enclosure, somewhat on the analogy of the Newtonian corpuscular theory of optics." For some remarks on Larmor's papers and their diffusion, see Kuhn T.S. 1987, pp. 136-7 and 314.

back to the old debates taking place in the eighteenth century. Could a physicist put in danger the fruitful alliance between the wave theory of light and Maxwell's electromagnetic theory, for the sake of a questionable hypothesis? Although he acknowledged the existence of some connection between his view and J.J. Thomson, Larmor and Einstein's views, for the time being, Planck restated his trust in "Maxwell-Hertz's equations for empty space, which excluded the existence of energy quanta in vacuum".[45] Planck's review was really oversimplified: neither the differences between J.J. Thomson and Einstein, nor the differences between J.J. Thomson and Larmor were taken into account.

In order to appreciate the conceptual distance between J.J. Thomson and Einstein, it is worth reading what the former wrote in 1936 in his *Autobiography*, in the last chapter, "Physics in my Time". When he described the nature of Röntgen rays, he remarked that "if the wave front of a beam of these rays were continuous no molecule in the path of the beam could escape from being struck by the rays". As he had pointed out in 1903, in his Yale lectures, there was evidence "that the front of the beam could not be continuous". It appeared "like a series of bright spots on a dark background", as if the energy was "concentrated in separated bundles". He considered that picture as one of the two roots of "what was afterwards known as the Quantum Theory of Light", the other root being "Planck's Law that the energy in each bundle is equal to $h\nu$".[46]

J.J. Thomson stressed the electromagnetic origin of the *relativistic* effects: if "we take the view that the structure of matter is electric", he claimed, those effects "follow from Maxwell's equations without introducing relativity". He found it reasonable "to regard Maxwell's equations as the fundamental principle rather than that of relativity". Two consequences emerged: first, aether should be regarded as "the seat of the mass momentum and energy of matter", and, second, lines of force should be regarded as "the bonds which bind ether to matter". In the next page, he repeated the same concept and "special relativity" was described as a theory dealing with "electric and magnetic problems which can also be solved by Maxwell's equations". The nature of space, time and matter appeared to Thomson deeply linked to the existence of some kind of aether. To sum up, he claimed that "space must possess mass and structure"; in that case, he concluded, "it must possess the qualities postulated for the ether". It was a pity that on Einstein's *General Relativity* he avowed that "there is much of it

[45] See Planck M. 1910, pp. 763-4 and 767-8.
[46] Thomson J.J. 1936, p. 410.

I do not profess to understand": he could have found in that theory, and in Einstein's Leiden lecture, some connection with his own remarks.[47]

[47] Thomson J.J. 1936, pp. 431-33. Thomson claimed that, although Einstein had made "no mention of an ether but a great deal about space", if space has a physical meaning, then it "must have much the same properties as we ascribe to the ether". In other words, space cannot be a mere geometrical entity but "must therefore have a structure". With regard to the concept of time, he remarked that "there must be in space something which changes", in order "to distinguish one instant from another"; on the contrary, there would be "nothing to supply a *clock*". With regard to mass, he started from the fact that "the mass of a body increases as the velocity increases": as a consequence, "if the mass does not come from space it must be created".

Appendix: *Historiographical remarks*

In the history of science, from the point of view of general and long-term conceptions, we find a competition, or a wide-scope conceptual tension, between discrete and continuous models in the representation of the physical world. At the end of the nineteenth century, some scientists, in particular J.J. Thomson and J. Larmor, transformed that competition into an integration. The conceptual tension between continuous and discrete aspects of matter and energy transformed into the co-existence of complementary components. That integration was put forward by scientists who belonged to the last generation of natural philosophers and, at the same time, to the first generation of professionalized physicists.

In the debate which took place in late nineteenth century British electromagnetism, besides that conceptual tension, we encountered other tensions between other couples of issues: macroscopic representations *versus* microscopic representations, contiguous actions *versus* at-a-distance actions, mathematical approaches *versus* theoretical approaches. As I have already specified, those conceptual tensions concerned different levels of scientific practice: some of them, for instance the tension between discrete and continuous models, were *theoretical*; others, for instance the tension between mathematical physics and theoretical physics, were *meta-theoretical* or methodological. I have called *conceptual streams* the most general theoretical models: for instance the continuous or discrete conceptions of matter, and the continuous or discrete conceptions of energy.

My *conceptual streams* are simpler, less sophisticated and more easily identifiable than Kuhn's *paradigms*, Lakatos' *research programmes*, or Laudan's *research traditions*. My *streams* are units of scientific thought, which do not suffer mutual exclusion: in some theories which emerged in the second half of the nineteenth century, we find the convergence of two conceptual streams, for instance continuity of matter and contiguous action. Moreover, we even find a convergence and an attempt to integrate two complementary streams, for instance continuity and discreteness of matter. Although the conceptual streams correspond to simple statements, they cannot be looked upon as *logical* statements in accordance with *classic* logic.

I will not take into account historiographical theses like those of Kuhn or Lakatos: my specific, historical researches, confined within a narrow range of space and time, do not allow me to draw conclusions on the general structure of science. On the contrary, it seems to me that Laudan's theses deserve my attention: some features of his *research traditions* are akin to some features of my *conceptual streams*, even though the two entities are, on the whole, quite different. Laudan, for instance, put the "atomic theory" in the list of his *research traditions* and qualified it as founded "on the assumption that matter is discontinuous"; indeed, it is one of my *conceptual streams*. Nevertheless, he also quoted in the same list an entity like the "quantum theory", which is not a simple conceptual unit but a set of different theories: among them, as Laudan himself explicitly acknowledged, "there are huge conceptual divergences".[1] Other entities qualified as *research traditions*, like "Darwinism", or "the electromagnetic theory of light", or "Cartesian physics", have different natures: if Darwinism and Cartesian physics could be looked upon as theories or sets of theories, the electromagnetic theory of light could be looked upon as a specific issue of a theory. The fact is that Laudan's *research traditions* are entities more complex and more sophisticated than theories; on the contrary, my *conceptual streams* are less complex than theories. A more striking difference emerges when we note that many conceptual streams can converge on a theory; this multiple convergence appears more problematic for research traditions.

If a conceptual stream can carry, in a broad sense, "*metaphysical* … commitments", as Laudan claimed with regard to his *traditions*, I find that a conceptual stream cannot carry "*methodological* commitments". Laudan sharply stated that "*research traditions are neither explanatory, nor predictive, nor directly testable*" because of their abstract and complex nature.[2] On the contrary, a conceptual stream maintains more friendly relationships with explanations, predictions and experimental tests. The statement that cathode rays have a discrete structure was actually and repeatedly tested at the end of the nineteenth century, even though in a non-conclusive way. That Larmor's electron was not solid matter in a traditional sense but consisted of a dynamical structure of aether was as predictive as explanatory, even though, at that time, not easily testable. Changes or transformations affecting *research traditions* cannot act on conceptual streams in the same way. In professionalised and specialised physics of the late nineteenth century, a conceptual stream did not exist in

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[1] Laudan L. 1977, p. 72.

[2] Laudan L. 1977, pp. 78-9 and 81-2. Although both a *research tradition* and a *conceptual stream* have "a number of specific theories which exemplify … it", we can say that those theories "partially constitute it" only with reference to a research tradition. The fact is that conceptual streams are entities less complex than scientific theories, and therefore less complex than research traditions. Both every research tradition and every conceptual stream has undergone "a number of different, detailed … formulations and generally has a long history". Nevertheless, that those formulations are "often mutually contradictory" makes sense when referred to a complex research tradition, but makes not sense when referred to a simple conceptual stream.

itself, but required a specific implementation. The existence of different theories, as different implementations of the same conceptual stream, was the only kind of transformation actually taking place in a conceptual stream.

What Laudan in 1977 called "normative difficulties" and "worldview difficulties" are quite close to the meta-theoretical or methodological tensions I have introduced in my analysis of nineteenth century theoretical physics. He acknowledged that "normative conceptual problems" affected "the historical evolution of science", but he put world-view debates at a different level: a tension between science and "extra-scientific beliefs". In other words, he found that those debates concerned the relationships between science, on the one hand, and "metaphysics, logic, ethics and theology", on the other. This sounds quite strange when we note that, in the instances he singled out from the history of science, there are queries concerning "the ontology of forces" or "the priority of force over matter". Why should we qualify these queries as metaphysical or theological beliefs? If considered in their scientific context, these queries were *scientific* queries. In the specific context of the history of theoretical physics in the late nineteenth century, those debates were authentic *scientific* debates and were considered as such by the contemporaries.[3]

Generally speaking about the so-called *metaphysical* components in scientific practice, I think that there has been a long-lasting and unfortunate misunderstanding. Why should not some components of scientific practice be qualified as *scientific*? Why should some components of scientific practice be scientific, and others not be? Having focused my study on conceptual components, I have realised that those components were involved in specific theories, in more general and long-term mental representations or *conceptual streams*, and in meta-theoretical or methodological attitudes. If they were conceptual components of the actual scientific practice, I find it quite difficult to define some of them as *metaphysical* instead of *scientific* components.[4] In particular, the *conceptual streams* I am dealing with are scientific entities, as scientific as Hertz or

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[3] See Laudan L. 1977, pp. 60-62. In general, I do not find that "worldviews difficulties" emerged "traditionally" or "most often" from tensions "between science, on the one hand, and either theology, philosophy or social theory, on the other hand". Conceptual tensions leading to different world-views also emerged within the (fluctuating over time) boundaries of science.

[4] If the conceptual tensions on the first and second level, concerning either specific models of matter and energy, or more general discrete and continuous models, emerged from both British and Continental physics, the third-level methodological tensions mainly emerged from the theoretical researches of German physicists: Mach, Hertz, Boltzmann, Planck, Helm, ...

Michelson's experiments. Physics in the late nineteenth century shows us how complex and manifold it was, and how many conceptual and non-conceptual components took part in its development. We should not be misled by the fact that some theories, as for instance Maxwell's electromagnetic theory, even when deprived of their conceptual components, can survive and make sense as well. The remaining stump, left-over after the despoilment, namely the set of equations, can be given a meaning in the context of mathematical physics, but it has lost its meaning in the context of theoretical physics.[5]

Those conceptual tensions and the corresponding debates played an important role in late nineteenth century physics, and offered a fruitful background to subsequent theoretical researches. We should acknowledge the existence of a deep continuity, as well as a deep discontinuity, in the transition from late nineteenth century to early twentieth century theoretical physics. The fact is that, in order to correctly appreciate both historical continuity and discontinuity, we have to distinguish the *first level* of specific theoretical models from the *second level* of long-term *conceptual streams*. Laudan stated that the "chief element of continuity" in the history of science is "the base of empirical problems"; this common base would assure the "cumulative character" of science. In other words, "the important connections between successive research traditions" consists of "the shared empirical problems". I agree with Laudan on the existence of a cumulative component in science, involving the solution of empirical problems: nowadays we can rely on an accumulation of technologies stimulated by several kinds of problem solving. Nevertheless, in the course of history, new problems and new tasks emerge and their relative weight and meaningfulness change over time, giving rise to a certain degree of discontinuity. Conversely, "the level of explanation", namely theories and models, which Laudan associated to the discontinuous nature of science, is also a seat of continuity: the persistence of general and long-term conceptual models, which I have called *conceptual streams*, is an instance of such continuity.[6]

My historiographical sketch has something in common with Holton's approach: history of science has always been crossed by general conceptions

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[5] Starting from the 1920s, E.A. Burtt emphasised an inescapable *metaphysical* component in scientific enterprise. See Burtt E.A. 1932, in Burtt E.A. 2003, p. 227: "To begin with, there is no escape from metaphysics, that is, from the final implications of any proposition or set of propositions. The only way to avoid becoming a metaphysician is to say nothing". On the intrinsic "cosmological" component in scientific enterprise, in particular in the late nineteenth century, see Tarsitani C. 1983, pp. 11-12.

[6] See Laudan L. 1977, pp. 139-40. It seems to me that Laudan underestimated the double nature of conceptual components in science, especially when he criticised Lovejoy's "unit ideas" and Holton's "themes".

which Holton called *themes* and I call *conceptual streams*, in order to underline their historical nature. In 1973 Holton stated that we have to acknowledge "the existence, and even the necessity, at certain stages in the growth of science, of precisely such unverifiable, and yet not-quite arbitrary hypotheses". The "class of hypotheses", "thematic hypotheses", or "thematic propositions" were looked upon by Holton as "directly neither verifiable nor falsifiable". He imagined the scientific enterprise as endowed with three components: empirical ground, formal language and a "thematic content". The last component represented a specific *dimension* of scientific enterprise, "a dimension that can be conceived as orthogonal to the empirical and analytical content", where the adjective *orthogonal* suggests a sort of mutual independence among them. Furthermore, the thematic component would consist of a couple of "opposing or complementary theme and antitheme": for instance the thematic couple of "atomism and the continuum" or "discontinuity and the continuum".[7] Some years earlier, M. Hesse had acknowledged the existence of antithetic *themes* or principles in the history of science: among them, the continuous-discrete couple. Even she stated that those principles were "unconfirmable and unfalsifiable". As Lovejoy had pointed out some decades before, there have been "not many differences in mental habit more significant than that between the habit of thinking in discrete, … and that of thinking in terms of continuity".[8]

I think that the adjectives *unverifiable*, *unconfirmable* and *unfalsifiable*, used by Holton and Hesse, cannot be accepted in an absolute sense. History of science, in particular the period I am dealing with, shows a complex

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[7] See Holton G. 1973, pp. 11, 13, 29, 51, 57, 99 and 192. That his *themata* were "unverifiable" and "unfalsifiable", even though "not arbitrary", conceptions, was pointed out by Holton even in subsequent years. See Holton G. 1986, p. 53. In 1986 Holton still stressed the "orthogonal" relationship between "*phenomenic propositions*", and "*analytic propositions*", but it is questionable whether something like a pure empirical component or "dimension" can really exist. The three-dimensional Cartesian space envisaged by Holton, and endowed with *phenomenic*, *analytic* and *thematic* axes, is obviously a useful but simplified idealisation. See Holton G. 1986, p. 5 and 18.

[8] See Hesse M. 1961, p. 293. See Lovejoy A.O. 1936, in Lovejoy A.O. 1964, p. 57. In the history of Western thought, the couple discrete/continuous came early into play. Although Lovejoy claimed that this "essential opposition" could be traced back to Aristotle, meaningful traces can be found even before. When Lovejoy undertook his "Study of the History of an Idea", he avowed he would have broken up philosophical systems "into their component elements, into what may be called their unit-ideas". The history of those unit-ideas appeared to him "in great part a story of conflict, at first latent, eventually overt, between these ideas and a series of antagonistic conceptions". See Lovejoy A.O. 1936, in Lovejoy A.O. 1964, pp. 3, 4 and 22. I find that the history of conceptual streams in the history of physics can be represented as a history of basic ideas, which emerged, then faded into the background, and subsequently re-emerged over time.

interplay between hypotheses and experimental checks. Physicists in the late nineteenth century, for instance, tried to experimentally check the discrete structure of matter. At the same time, there were theoretical attempts to explain that discrete structure in terms of a hypothetical continuous structure of aether. The fact is that, when Holton introduced his themes, he did not take into account the difference between specific theoretical models, general conceptual models (namely my *conceptual streams*), and meta-theoretical or methodological commitments. On the centenary of Einstein's birth, Holton described some "themata and metaphors" endowed with "immense explanatory energy". Among them, Newton's *vis insita*, Faraday's lines of force, Einstein's freely falling elevator, Bohr's complementarity and Einstein's "Holy Grail of complete unification of all forces of nature". He listed specific theoretical models, like lines of force, together with meta-theoretical commitments, like Einstein's commitment to unification. Even in 1986 he collected a list of themes, wherein we find the meta-theoretical concepts of "simplicity" and "parsimony" next to "the continuum", which is a general theoretical model.[9] Adjectives like *verifiable* or *falsifiable* can be associated to some specific theoretical model or to some general conceptual model, but are unsuitable for meta-theoretical commitments. Moreover, I do not find that a "thematic hypothesis is often an impotency proposition" or "the thematic hypothesis is precisely built as a bridge over the gap of ignorance".[10] It is certainly true that our knowledge can never be complete, but a conceptual model allows us to go far beyond the amount of knowledge attained by means of empirical checks and formal language. Models are not patches by means of which we mend the canvas of knowledge: they are parts of the loom. The fact is that conceptual models are intrinsic components of scientific knowledge, even when they are not explicitly stated or are supposed to be unnecessary. I find that the emergence of conceptual tensions between conceptual streams, or *themes*, can act as an engine, which widens the boundaries of knowledge: in other words, *themes* can have an actual heuristic power. At the end of the nineteenth century, the emergence of theoretical debates, concerning long-lasting *themes* belonging to the history of science, had really a propelling effect.

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[9] See, for instance, Holton G. 1982, p. xxvii, and Holton G. 1986, p. 15.

[10] Holton G. 1973, pp. 52-3. Although in a more specific context (the emergence of Einstein's theories), Renn and Schemmel made similar remarks. They claimed that conceptual or "mental" models are "flexible structures of thinking that are suitable for grasping situations about which no complete information are available". See Renn J. and Schemmel M. 2006, in Renn J. (ed.) 2006, Vol. 3, p. 2.

I agree with Holton's claim that the presence of themes represents an element of continuity in the history of science, for they "indicate the obverse side of the iconoclastic role of science". Nevertheless, there are themes which have introduced meaningful discontinuities in the history of science. I find that the *conceptual streams* (mainly when they are members of a couple of opposite models) emphasise both the continuity and the discontinuity in scientific enterprise. On the one hand, I see a persistence of general models and general dichotomies; on the other, I see the extreme variability of their specific implementations. [11]

With regard to the persistence of themes or conceptual streams it is worth mentioning D'Agostino's criticism: he claimed that, whereas theoretical models have experienced frequent transformations, mathematical structures have survived for a longer time. In other words, mathematical structures are more persistent than conceptual models. Indeed, theoretical models underwent short-term transformations, differently from long-term persistence of mathematical structures, especially in nineteenth century physics. Nevertheless I find that, underneath those short-term transformations, there was the long-term persistence of conceptual streams or themes, which, periodically, emerged and then, after a long or short time, disappeared and then re-emerged once again. The series of subsequent disappearances and re-emergences of a given conceptual stream is a long-term phenomenon, even longer than the persistence of mathematical structures. The short-term phenomenon pointed out by D'Agostino corresponds to the specific implementations of a given *conceptual stream*.[12]

Holton claimed that themes, in general, are confined to a non-public aspect of scientific enterprise. He pointed out that "scientists speak only rarely in such terms" and the case of Maxwell, he went on, represented "an unusual concession". The emergence of theoretical physics in the last decades of the nineteenth century involved the emergence of conceptual, or thematic, debates, much more than in the previous decades of the same century. Those

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[11] See Holton G. 1973, p. 61. On the persistence of general themes, and the variability of their specific implementation, see, for instance, Tarsitani C. 1983, p. 15.

[12] See D'Agostino S. 2000b, p. 409: "In contrast to the mutation of physical concepts, there is a striking permanence in the mathematical structure of physics, that is, in the form in which physical laws are represented by mathematical equations. […] This asymmetric behaviour in the mathematical and physical structures of theories is prominent in the historical development of physics. Against the mutation of conceptual structures as a product of cultural evolution, mathematics thus can be taken as one of these 'artifacts' that, according to Jürgen Renn, are transmitted from one generation to the next and guarantee continuity in the development of science."

debates left definite footprints in *public* science: not only in scientific journals but even in the advanced textbooks written by scientists committed to those debates. The debates around scientific themes were part of *public* science and the case of Maxwell represented more an instance than an exception. In the historical context of the late nineteenth century, Holton's themes, or my *conceptual streams*, represented a sort of bridge between science and its intellectual context. From the historiographical point of view, taking into account these conceptual streams allows us to fill the gap between the history of physics and the history of ideas.[13]

In a brief, historical reconstruction, Holton pointed out the emergence of discrete entities in nineteenth century science. He stated that "between 1808 and 1905, physics, biology, and chemistry saw the introduction of remarkably similar conceptions", a deep change in "mental models … where the guiding idea is no longer a continuum, but a particle, a discrete quantum". Confining myself within the last decades of the nineteenth century, I must modify Holton's reconstruction in some points, even though I share the importance of the (re)emergence of the theme of discreteness. First, the most interesting theoretical or *thematic* conjectures on the electron emerged before 1897, and can be found in Larmor's writings; second, an interesting theoretical or *thematic* conjecture on the supposed discreteness of electromagnetic radiation emerged before 1900 and can be found in J.J. Thomson's writings; third, in both Larmor and J.J. Thomson, we find an original attempt to integrate discrete and continuous theoretical models, namely an attempt to integrate a thema with the corresponding antithema.[14]

I would like to specify that my historiographical sketch cannot be looked upon as an epistemological framework suitable for the whole history of physics or the whole history of science. It is an interpretative framework concerning theoretical physics in the late nineteenth century and its

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[13] See Holton G. 1973, p. 62. Making use of a different language, I think that the analysis of conceptual streams can help us to overcome "traditional and superseded distinctions such as that between internalist and externalist history of science". See Renn J. 1996, p. 2.

[14] Holton first singled out "Joule's kinetic theory (1847)", where "sensible heat was identified with the motions of discrete atoms and molecules", then the electron, namely "the smallest unit of negative charge … (1897)", and, eventually, the quantisation of "the energy of the source of radiation and then of the radiation itself … (1900 and 1905)". He skipped Maxwell's *continuous* electromagnetic theory. See Holton G. 1973, p. 100. In the second half of the nineteenth century, a meaningful instance of the conceptual tension between discrete and continuous models can be found in the whole of Maxwell's theories. The continuous structure of matter and electromagnetic fields in his electromagnetic theory was put forward alongside the discontinuous structure of gases in his kinetic theory. On that "disjunction in physical science", see Harman P.M.C. 1998, p. 2. J.J. Thomson and Larmor tried to overcome that disjunction.

connections with theoretical physics of the early twentieth century. Although I think that conceptual streams are long-term phenomena, I find that the explicit acknowledgement of their existence, the explicit role they played in scientific practice, and the existence of an explicit debate involving them were specific hallmarks of that historical period. I do not claim that long-term conceptual streams have always affected science in the same way in the course of the whole history of science. In this sense, my interpretative framework is merely *local*: it could be stretched across longer periods of time only after having undertaken further detailed historical investigations.[15]

[15] On the *historicity* of every historiographical framework and every *epistemology*, see Tarsitani C. 1983, p. 25.

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One of the queries emerging from the compilation of a bibliography concerns the distinction between primary and secondary sources. The two elements, which usually define the borderline between them, are chronology and content. We could say that primary sources are original researches belonging to the period of time under investigation. Conversely, secondary sources are texts written subsequently, containing remarks on those original researches. Nevertheless, the two dichotomies, *original researches* versus *remarks* or *appraisals* (concerning content), and *contemporary* versus *subsequent* (concerning time), give rise to four possible combinations. Two of them, *original* and *contemporary* researches, and *subsequent appraisal*, have just been taken into account. What about the other two, namely *contemporary appraisals* and *subsequent original* researches? Where should a J.J.Thomson's 1885 or Hertz's 1892 paper, dealing with appraisals of contemporary electromagnetic theories, be placed? What about the book J.J. Thomson wrote in 1936, wherein we found original ideas together with autobiographical notes, appraisals of other theories, and pedagogical remarks?

Moreover, we must distinguish a given field of knowledge from the field representing its *object*, in our case *history of science* and *science*. A book written by Kuhn around 1960 appears undoubtedly as a secondary source with regard to *science*, but it can be looked upon as a primary source with regard to the *history of science*. Going backwards through history, Cassirer's 1950 historical study, or J.T. Merz's early twentieth century history of science, can be considered as secondary sources with regard to *science*, but primary sources with regard to *history of science*.

As I cannot offer any ultimate solution to these historiographical queries well known to historians, I have confined myself to dividing my bibliography in two parts: in the former, there are sources (both primary and secondary) explicitly quoted in the book, and, in the latter, other sources taken into account, even though not explicitly quoted.

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