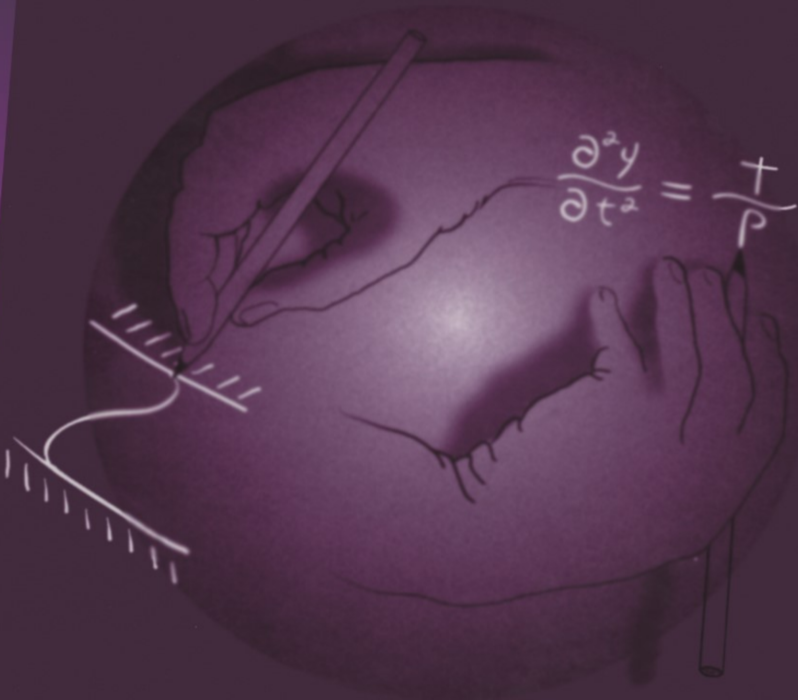
The Language of PHYSICS

The Calculus and the Development of Theoretical Physics in Europe, 1750–1914



Elizabeth Garber

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Preface

This study began as an attempt to understand mechanics in the nineteenth century. The terms mechanics and mechanical world view were being used as general descriptions of nineteenth-century physicists' assumptions and interpretations of nature. However, there were no studies of the particulars of these assumptions or the range and content of these interpretations. René Dugas' work on classical mechanics focused on France. The search for the particulars of these forms of "mechanics" led me to explore precisely what mechanics meant to physicists of a century and more ago.

However, none of Lagrange's, Hamilton's, or Jacobi's "mechanics," while elegant, fits easily within the history of physics. Lagrange reduced mechanics to an exercise in analysis; Hamilton and Jacobi used mechanics to explore solutions to partial differential equations. They were mathematicians doing mathematics. As I went deeper into the matter it became obvious that, in the nineteenth century, there were two kinds of mechanics, each containing a variety of forms, one physical, the other mathematical. There were a group of men using mechanics to understand nature and another group using the equations of mechanics to explore the calculus. However, when tracing these two traditions back into the eighteenth century, physics disappeared altogether.

The historical problem changed with eighteenth century physics. To understand physics required that both physics and the development of the calculus be studied simultaneously. Research in eighteenth century mechanics, light, sound, and some electrostatics, showed that physics in that era was experiment. John Heilbron's study of eighteenth century electrostatics confirmed this conception for me.

There were two quite distinct kinds of mechanics in the eighteenth century. The first was either experimental or demonstrative in some material fashion of the operations of nature through concrete examples. The second was mathematical. Mathematical solutions to problems of mechanics were generating some highly sophisticated work in the calculus. However, there were no mathematical investigations into the structure and physical function of nature, and none of the authors, save Daniel Bernoulli, used the calculus to investigate the actual workings of nature. Enmeshed in both types of works on mechanics were disputes over its metaphysical foundations. Some of this work needed a separate classification.

The traditional historiography of physics narrated an unbroken development for mechanics from the seventeenth through the nineteenth century. This traditional historiography ignored mathematics as an aspect of the development of physics. Historians assumed that mathematics was incorporated into physics as one of its essential elements since the seventeenth century. They also took as axiomatic that physicists argued theoretically in ways familiar from twentieth-century examples. Given Heilbron's work and the preliminary results of my own research, mathematics in physics became the problem. Where and how did it enter physics, then take over the expression and development of theoretical ideas?

Until recently most philosophers and historians of science did not consider the function, role, or place of mathematics in the development of scientific ideas. Mathematics was sidelined, downgraded to the status of a tool. Scientists took the tool off the shelf, used, and then returned it. It had no role in shaping the solutions to physical or any other scientific problems. However, theoretical physicists speak of mathematics as their "language." Max Dresden passionately argued about the interactions between mathematics and physical imagery and the depth to which they are interdependent. Mathematics shaped how physicists thought about solutions to physical problems, while at the same time physical imagery pushed their mathematical language in some directions rather than others.

The development of the calculus was a material part of the creation of modern theoretical physics. This meant discarding the image of mathematics as a tool and investigating it as language. Seeing mathematics as language, and having available the recent literature on the development of calculus in the eighteenth century, changed the history of physics. Mathematics and physics had to be defined with the terms available during the eighteenth then the nineteenth centuries. Many papers in mechanics in the eighteenth and early nineteenth century, traditionally taken as physical, had to be reconsidered. Using criteria derived from what mathematics meant in those eras, many papers historians have tried to integrate into the history of physics belong in the history of mathematics. Physical implications could be inferred from some of the results of these eighteenth century papers. However, these inferences seemed never to be commented upon, developed, or seen as significant by the authors themselves. Reactions to these papers, the issues discussed, points over which disputes flourished and the ensuing debates confirmed the sense that topics taken later as important in physics were discussed in terms that were mathematical. The research generated by this stream of work in mechanics in the eighteenth century was mathematical not mechanical, metaphysical not physical. The disputes that erupted were over mathematical not physical interpretations or implications. As historians of eighteenth-century mathematics know well, the solutions to problems taken from physics and elsewhere are an important key to understanding much about the development of the calculus. This needed to be incorporated into the history of physics.

The historical problem shifted once again. How, when and in what forms did the field that we know as theoretical physics emerge? In solving this problem it seemed crucial to keep the boundary between physics and mathematics, and the relationships between physicists and mathematcians firmly in view.

With these criteria in place it became clear that eighteenth century patterns continued, with some important modifications, well into the nineteenth century. French mathematicians expanded the range of mathematics to "mathematical physics" that incorporated electrostatics, magnetism, then heat, light and electrodynamics. Quantitative physical experiments provided new material as the starting points for these mathematical excursions. The new phenomena uncovered experimentally were subsumed by mathematics. Theoretical physics did not mediate the intellectual space between experiment and the calculus. Physical understanding of phenomena came from experimentalists or were unintended outgrowths of mathematical disputes. The significant elements of the mathematical papers were functions, coefficients, series, convergence criteria and mathematical operations. Indeed, acrimonious debates over the very foundations of the calculus itself erupted from some of these papers.

The physical content ascribed to many of these earlier mathematical papers was added retrospectively in the latter half of the nineteenth century by physicists. They were committed to a mechanical interpretation of nature expressed in the language of the calculus. They remolded the history of their discipline to conform to their needs and the discipline they were creating, theoretical physics. Their narrative still informs our accounts.

To construct theoretical physics from eighteenth and early nineteenth century mathematics meant remolding it and imposing new meanings upon its results. These meanings were not inherent in the mathematics itself. Physical meanings had to come from external sources. Theoretical physics was possible only after physical imagery and mathematical language converged and experimental results were integrated into the body of that theory. These convergences became possible only in the context of Britain and the German States, two cultures where the search for the actual structure of nature behind appearances was a legitimate goal of natural philosophers.

The process of the emergence of theoretical physics in its myriad forms is the subject matter of this book. The pathways from experimental philosophy and mathematics to theoretical and experimental physics were never linear. The end results were a rich mixture of conceptual structures, all based on the ideas of mechanics wedded, in a variety of ways to calculus.

This work is one of interpretation and necessarily dependent on the work of colleagues in the history of physics and mathematics. Historians of the calculus work in a technically demanding field that is often ignored by historians of the other sciences. It should not be. Mathematics has become the language of choice for many sciences. This problem alone needs historical attention. The historians of mathematics I have relied on heavily include Judith Grabiner, Joan Richards and particularly Ivor Grattan-Guinness. On various occasions Ivor discussed with me many of the mathematical issues, and the absence of physical content in much of eighteenth-century mechanical and later papers. His remarks on Liouville's work made sensible to me nineteenth century French mathematical physics. Historians of mathematics have cheerfully tolerated my presence and my questions at their conferences, even though, by and large, they are concerned with the development of physics only as it intersects their own scholarly interests in the development of mathematics itself. In comparison to those concerns the focus of this book is on the implications of the history of the calculus in the eighteenth and early nineteenth centuries for the history of physics.

Other colleagues have aided and abetted this research. Until his retirement Max Dresden argued with me regularly about issues in the history of physics in ways that were always stimulating, although often uncomfortable. I regret that he will be unable to give me any reactions to this book. Other colleagues include the members of the Eighteenth Century Consortium at Stony Brook. I am grateful to Arthur Donovan for discussions with him on the emergence of chemistry as a modern science at the end of the eighteenth century. Arthur expressed in detail what made chemistry with and after Lavoisier "modern" and scientific rather than an untidy and loosely federated set of practices and ideas. Fred Weinstein taught me how to think theoretically as a historian. Gary Marker and I have discussed eighteenth-century cultural history, especially that of Russia and the place of the St. Petersburg Academy in that culture and politics. He also taught me that historians of science and historians of culture and literacy share the same theoretical problems and the same sources for easing the solutions to those problems. I am particularly grateful to David Cassidy who read this manuscript, criticized it and forced me to improve it. On a less intellectual yet critical level I must thank the Interlibrary Loan Department at Stony Brook. Donna Sommers and the staff located sources, and in some cases pleaded my case with other librarians for loans of material that rarely left their care. Without their help I could not have completed this.

My husband Donald has lived with this research as long as I have. He has endured all its transformations, helped me clarify my ideas by listening, criticizing but never asking when I would be done with the book. He never asked why it took so long to mature and I hope that he finds the wait worthwhile.

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Serials

List of Abbreviations

Abh. Akad. Wissen. Berlin	*Abhandlungen der Königlich Akademie der Wissenschaften Mathematisch-Natutwissenschaftliche Klasse zu Berlin*
Abh. Boehm. Ges. Wiss.	*Abhandlungen der Königlich Boehmischen Gesellschaft der Wissenschaften, Prague*
Abh. Math-Phys. Cl. Bayerische Akad. Wiss.	*Abhandlungen der Mathematisch-Physiche Klasse der Bayerische Akademie der Wissenschaften*
Abh. Math-Phys. Cl. Gesell. Wissen., Leipzig	*Abhandlungen der Mathematische-Physiche Classe der Königlichen Sachsischen Gesellschaft der Wissenschaften, Leipzig*
Acta Acad. Sci. Petrop.	*Acta Academia Scientiarium Petropolitanae*
Acta Hist. Sci. Nat. Med.	*Acta Historium Scientiarum Naturalum et Medicinalium*
Acta Math.	*Acta Mathematica*
Amer. J. Phys.	*American Journal of Physics*
Amer. Hist. Rev.	*American Historical Review*
Amer. J. Phys.	*American Journal of Physics*
Amer. J. Sci.	*American Journal of Science*
Amer. Math. Monthly	*American Mathematical Monthly*
Amer. Sci.	*American Scientist*

Ann. Chim.	*Annales de Chimie*
Ann. Chim. Phys.	*Annales de Chimie et de Physique*
Ann. Phil.	*Annals of Philosophy*
Ann. Phys.	*Annalen der Physik*
Ann. Rev. Sociol.	*Annual Review of Sociology*
Ann. Sci.	*Annals of Science*
Arch. Hist. Exact Sci.	*Archives for History of Exact Sciences*
Arch. Int. Hist. Sci.	*Archives Internationales d'Histoire des Sciences*
Arch. Neerl.	*Archives Néerlandaises des Sciences Exactes et Naturelles*
Ber. Berlin	*Sitzungsberichte der Königlich Akademie der Wissenschaften Physikalisch-Mathematische Klasse, Berlin*
Ber. Leipzig	*Bericht über der Verhandlungen der Königlichen Sächsischen Gesellschaft der Wissenschaften zur Leipzig, Mathematische-Physiche Klasse*
Ber. Wien	*Sitzungsberichte Kaiserliche Akademie der Wissenschaften Wien, Mathematisch-Naturwissenschaftliche Klasse*
Ber. Wissen.	*Bericht zur Wissenschaft-geschicht*
Berlin Akad. Monats.	*Monatsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*
Boston Stud. Phil. Sci.	*Boston Studies in the Philosophy of Science*
British J. Hist. Sci.	*British Journal for the History of Science*
British J. Philos. Sci.	*British Journal for the Philosophy of Science*
Bull. Amer. Math. Soc.	*Bulletin of the American Mathematical Society*
Bull. Hist. Chem.	*Bulletin for the History of Chemistry*

Bull. Soc. Philo.	*Bulletin des Sciences par la Société Philomatique de Paris*
Cambridge (Dublin) Math. J.	*Cambridge (and Dublin) Mathematical Journal*
Chem. Rev.	*Chemical Reviews*
Comm. Acad. Sci. Imp.	*Commentarii Academia Scientiarium Imperialis Petropolitanae*
Comm. Pure Appl. Math.	*Communications in Pure and Applied Mathematics*
Comptes Rendus	*Comptes Rendus Hebdommadaires des Séances de l'Académie des Sciences, Paris*
Edinburgh J. Sci.	*Edinburgh Journal of Science*
Edinburgh Rev.	*Edinburgh Review*
Eighteenth-Cent. Stud.	*Eighteenth-Century Studies*
Fort. Phys.	*Fortschritte der Physik*
French Hist. Stud.	*French Historical Studies*
Göttingen Nach.	*Nachrichte von der Königlich Gesellschaft zu Göttingen*
Hist. Educ.	*History of Education*
Hist. Human Sci.	*History of the Human Sciences*
Hist. Math.	*Historia Mathematica*
Hist. Phil. Logic	*History and Philosophy of Logic*
Hist. Sci.	*History of Science*
Hist. Scientiarium	*Historia Scientiarium*
Hist. Studies Phys. Sci.	*Historical Studies in the Physical (and Biological) Sciences*
Hist. Tech.	*History of Technology*
Hist. Univer.	*History of Universities*
Jahresber. Dtsch. Math. Ver.	*Jahresberichte der Deutschen Mathematiker-Vereinigung*
J. Chem. Educ.	*Journal of Chemical Education*
J. Chem. Phys.	*Journal of Chemical Physics*
J. Chem. Soc. London	*Journal of the Chemical Society, London*
J. Mines	*Journal des Mines*
J. École Poly.	*Journal de l'École Polytechnique, Paris*
J. Sçavans	*Journal des Sçavans*
J. Hist. Astron.	*Journal of the History of Astronomy*

J. Hist. Ideas	*Journal for the History of Ideas*
J. Interdis. Hist.	*Journal of Interdisciplinary History*
J. Math. Pures Appl.	*Journal de Mathématiques Pures et Appliquées*
J. Nat. Phil. Chem. Arts	*Journal of Natural Philosophy, Chemistry and the Arts* (Nicholson's *Journal)*
J. Phys.	*Journal de Physique*
J. der Phys.	*Journal der Physik*
J. Rational Mech. Anal.	*Journal of Rational Mechanics and Analysis*
J. Reine Angew. Math.	*Journal für die Reine und Angewandte Mathematik*
J. Roy. Inst.	*Journal of the Royal Institution*
J. Scient. Instrum.	*Journal of Scientific Instruments*
J. Savants	*Journal des Savants*
Math. Ann.	*Mathematische Annalen*
Mem. Acad. Roy. Sci. Paris	*Mémoires et Histoires de l'Académie des Sciences, Paris*
Mem. Acad. Sci. Paris	*Mémoires de l'Académie des Sciences, Paris*
Mem. Acad. Sci. Berlin	*Mémoires de l'Académie des Sciences, Berlin*
Mem. Institut	*Mémoires de l'Institut*
Mem. Manchester Lit. Phil. Soc.	*Memoirs of the Manchester Literary and Philosophical Society*
Mem. Nat. Acad. Sci.	*Memoirs of the National Academy of Sciences*
Mem. Soc. Arcueil	*Mémoires de la Société d'Arcueil*
Misc. Taurin	*Miscellanae Taurinesis, Mélanges de Philosophie et Mathématique de la Société Royale de Turin*
Mon. Not. Astron. Soc.	*Monthly Notices of the Royal Astronomical Society*
Monats. Akad. Berlin	*Monatsberichte der Akademie der Wissenschaften zu Berlin*
Monats. Dtsch. Akad.,	*Monatsberichte der Deutschen Akademie der Wissenschaften, zu Berlin*

Monats. Preuss. Akad.	*Monatsberichte der Preussichen Akademie, Berlin*
NTM	*NTM: Zeitschrift für Geschichte der Naturwissenschaften, Technik und Medizin*
Notes Rec. R. Soc. London	*Notes and Records of the Royal Society of London*
Nouveaux Mem. Acad. Sci. Berlin	*Nouveaux Mémoires de l'Académie des Sciences, Berlin*
Novi Acta Acad. Sci. Petropolitanae	*Novi Acta Academia Scientiarium Petropolitanae*
Novi Comm. Acad. Sci. Petropolitanae	*Novi Commentarii Academia Scientiarium Petropolitanae*
Oxford Rev. Educ.	*Oxford Review of Education*
Past Present	*Past and Present*
Persp. Sci.	*Perspectives on Science*
Phil. J.	*Philosophical Journal*
Phil. Mag.	*Philosophical Magazine*
Phil. Trans. R. Soc. London	*Philosophical Transactions of the Royal Society of London*
Phys. Blätter	*Physikalische Blätter*
Phys. Rev.	*Physical Review*
Phys. Zt.	*Physikalische Zeitschrift*
Poly. J.	*Journal der Polytechnik, Zurich*
Proc. Amer. Acad. Arts Sci.	*Proceedings of the American Academy of Arts and Sciences*
Proc. Amsterdam K. Akad. Sci.	*Proceedings of Section of Sciences Koninklijke Akademie Wetenschappen te Amsterdam*
Proc. Cambridge Phil. Soc.	*Proceedings of the Cambridge Philosophical Society*
Proc. London Math. Soc.	*Proceedings of the London Mathematical Society*
Proc. Manchester Lit. Phil. Soc.	*Proceedings of the Manchester Literary and Philosophical Society*
Proc. R. Irish Acad.	*Proceedings of the Royal Irish Academy*
Proc. R. Inst.	*Proceedings of the Royal Institution*
Proc. R. Soc. Edinburgh	*Proceedings of the Royal Society of Edinburgh*

Proc. R. Soc. London	*Proceedings of the Royal Society of London*
Rep. British Assoc.	*Report of the British Association for the Advancement of Science*
Resultate	*Resultate aus den Beobachtungen den Magentischen Vereins*
Rev. sci. pures et appl.	*Revue Générale des Sciences Pures et Appliqués*
Rev. Hist. Sci.	*Revue d'Histoire des Science et leurs Applications*
Rev. Quest. Sci.	*Revue des Questions Scientifiques*
Sci. Context	*Science in Context*
Sci. Monthly	*Science Monthly*
Sci. Techn. Persp.	*Sciences et Techniques en Perspective*
Sitz., Berlin Chem. Gesell.	*Berlin Chemische Gesellschaft, Sitzungsberichte*
Sitz., Bonn	*Sitzungsberichte der Niederr-heinischen Gesellschaft für Natur- und Heilkunde zu Bonn*
Smithsoniam Misc. Coll.	*Smithsonian Miscellaneous Collection*
Soc. Stud. Sci.	*Social Studies of Science*
Stud. Eight. Cult.	*Studies in Eighteenth-Century Culture*
Studies in Hist. Phil. Sci.	*Studies in History and Philosophy of Science*
Stud. Romant.	*Studies in Romanticism*
Stud. Voltaire	*Studies in Voltaire and the Eighteenth Century*
Sudhoff's Archive	*Sudhoff's Archive für Geschichte der Medezin und der Natur-wissenschaften*
Techn. Cult.	*Technology and Culture*
Texas Quart.	*Texas Quarterly*
Trans. Cambridge Phil. Soc.	*Transactions of the Cambridge Philosophical Society*
Trans. Acad. Sci., Connecticut	*Transactions of the Connecticut Academy of Sciences*

Trans. Roy. Irish Acad.	*Transactions of the Royal Irish Academy*
Trans. R. Soc. Edinburgh	*Transactions of the Royal Society of Edinburgh*
Verh. Dtsch. Phys. Gesell.	*Verhandlungen der Deutschen physikalischen Gesellschaft*
Verh. K. Akad. Weten.	*Verhandlingen der Koninklijke Akademie van Wetenschappen, Amsterdam*
Vict. Stud.	*Victorian Studies*
Zt. Astron.	*Zeitschrift für Astronomie*
Zt. Math. Phys.	*Zeitschrift für Mathematik und Physik*
Zt. Phys.	*Zeitschrift für Physik*

Chapter I

Introduction

This book traces and explains the development of modern physics from the mid-eighteenth century to the first world war. The focus is on how physicists used mathematics and fused it with experimental results and physical imagery to create a new field–theoretical physics. The emphasis in this work is on mathematics, because consideration of this language is usually omitted from historical narratives of the development of physics. Mathematics is taken for granted as a natural aspect of physics and assumed to be so from the seventeenth century onwards. At the end of the twentieth century these two disciplines draw ever closer and mathematics is the expected language of physical theory.

Before the middle of the nineteenth century mathematics served only as a adjunct to experiment. Speculations about the physical behavior of nature were expressed in the vernacular.[1] Experiment and observation extended knowledge of nature by revealing new phenomena and this expansion was a mark of the discipline's success. Physics was both a body of knowledge and a discipline with widely varying practices. For most of these practitioners physics was an avocation not a profession.

By 1870 physics was redefined socially and intellectually. Socially, it became a profession whose practitioners were located in the university systems of Europe and the United States. Intellectually, the core of physics remained experiment. These experiments were quantitative and performed within laboratories stocked with technologically sophisticated equipment and instruments usually supplied by some form of state or external support. Physicists reorganized their interpretations of experiment around a series of principles–laws of nature–and images confined by the scope of the mechanical concepts encompassed in those principles. Theory,

1 The term vernacular occurs in several place in the text. It means language that was familiar to a broad, literate population of women and men, requiring little technical language or terminology.

now systematized and guided by mathematics, usually the calculus, dominated physicists' interpretive enterprises. The union of physical imagery and the logical efficiency of the languages of mathematics proved to be a powerful explanatory combination.

How did these changes come about? What were the driving forces behind them? In searching for answers to these questions, we look to the content, meanings and relationships between mathematics and physics since it is the actions of both mathematicians and physicists that defined and redefined their respective disciplines during the eighteenth and nineteenth centuries.

Mathematics and Modern Physics

Physicists use mathematics in many different ways that gives theoretical physics a range of characteristics. In some fields of physics, as in the past, physics **is** mathematics. But the context within which this occurs has changed from earlier centuries. Only since about 1850 have physicists consciously joined mathematics and physical imagery, with the understanding that physics consists of a more complex search than the transformation of physical phenomena into mathematical terms accompanied by an argument legitimated using purely mathematical criteria. And when physicists produced results of interest to both mathematicians and physicists, they tended to publish the mathematical portions in journals directed to mathematicians and the physical portions in the literature directed to physicists.

Modern theoretical physics and mathematics are closely intertwined, but although mathematics *is* the language of physics, theoretical physics is not mathematics. The mathematical representation of a physical situation can symbolize a relationship between physical entities, such as force, work and energy, for a particular physical instance. The mathematical expression can also delineate a physical process, such as a rotation, through the mathematical operations contained within it. Accomplishing the transformation from physical concepts applied to a particular case into a mathematical interpretation takes imagination, skill in abstraction so that a physical situation can be given mathematical form, and analytical ability to choose the mathematical relationship. However, what physicists then need to do to extract physical information from the particular mathematical expression is not clear. Just how general a mathematical solution is necessary and the ways of connecting the solution to principles and processes to extract physically meaningful results are open. The approach depends on the interplay between physical principles, the use of imagery and the interjections of possible experimental examples into the theoretician's argument. Our hypothetical theorist may choose to translate his mathematical solution directly into a physical interpretation, or may do so through the intermediary of a particular model.

Developing the mathematics of physics can also be research, and this is what some theoretical physicists do. In doing theoretical physics, when in the middle of mathematically expressing a physical process, a physicist may choose the next step in the argument according to a mathematical or a physical set of criteria. Sometimes limitations on the behavior of physical systems are imposed by the behavior of the mathematical relationships themselves. Thus, there are limitations imposed on the physical system by the properties of the mathematical relationships and operations. Physicists have drawn physical implications directly from such behavior as in the case of Rudolph Julius Emmanuel Clausius' early papers in the mechanical theory of heat. The change in value of the function representing the internal energy of the system was independent of the path taken from A to B. Thus internal energy shared that trait. He used no property of matter to argue this characteristic of the physical system.[2] This also means that, with the creation of new mathematical subfields, new languages open for possible exploitation by physicists.[3]

Inevitably, the development of the physical process is limited by the possibilities available in the mathematical relationships and operations being used. Also thinking about physical systems in terms of mathematical operations leads to physical processes that may be implausible while still being mathematically possible. Solving the problem mathematically yields a broader set of outcomes than solutions that mirror legitimate pathways for the physical system. Normally in developing mathematics for physics, the language is explored until mathematical relationships are reached that embody interesting physical relationships. (It may or may not embody anything of interest to mathematicians.) A self-consistent, mathematically developed theory is also conceivable that is consistent with the initial physical description. The resulting mathematical system does not necessarily represent the behavior of even hypothetical physical systems. There is a beautiful example of this in James Challis, *Mathematical Principles of Physics.* James Clerk Maxwell demolished Challis' system as a physical picture of the universe. The crux of his argument was that a system can be logically self-consistent and physically nonsensical.[4] Challis' system was a permissible interpretation of the operations of the universe, where the self-consistency of the mathematical language was the main criterion of legitimacy. However, by the middle of the nineteenth century when Challis composed his system such discourse was abandoned and its remnants now seem strange as physics or mathematics. By 1870, such purely hypothetical worlds, no matter how logically developed, that bore no connections with current interpretation of the operations of nature, were not part of the discipline of physics. They were also marginal to the discipline of mathematics.

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2 Clausius later tried to develop a mechanical model with limited success.

3 The most pertinent ones here are the development of vector algebra and Fourier analysis.

4 James Challis, *An Essay on the Mathematical Principles of Physics* (Cambridge: Deighton Bell, 1873). For his review see Maxwell, *Nature* 8 (1873): 338–342. John Herschel made this same point earlier, as we shall see.

The mathematician's solution of, say, a partial differential equation of the first order is not that of the physicist. The myriad of arbitrary functions and coefficients that satisfy the mathematician's definition of a solution do not contain any necessary physical inferences. The mathematician needs to show that a general solution for that type of partial differential equation exists and, if possible, the number of arbitrary functions and coefficients that make up that solution. Physicists focus on particular solutions defined through criteria from outside of mathematics and imposed on the behavior of mathematical functions. Understanding how to translate physical criteria into limitations on the behavior of mathematical functions, polynomials etc., and when, in the search for the mathematical solution, to inject them and how to apply them, are again the marks of the theoretical physicist.

The mathematical exploration of a relationship, a partial differential equation for example, is not the same as the construction of a physical theory in the mathematical form of partial differential equations and their physical solution. The mathematical solution may be visualized in terms of, for example, the proper behavior of a function. Physically the solution may lie in seeing how the physical conditions impact the kind of function actually sought as a solution. In this case mathematical rigor may give way to imaginative abstractions of possible and plausible experimental conditions. An understanding of the phenomena can cut into the intricacies of the mathematical argument and confine it to particular examples. These cases, usually mathematically uninteresting, can be the most fruitful for the immediate solution of the physical problem.

Normally the theoretical physicist compromises the generality and purity of the mathematical solution to fit the needs of physical explanation. Interpretation rather than mathematical nicety takes priority. Physicists may also run rough shod over the carefully crafted definitions and conditions of validity of mathematicians. Exploring the linguistic structure of a particular subfield of mathematics is not a major concern of physicists, although it may be necessary at times. The language is usually a means of understanding certain natural processes. Within the discipline of physics, it is appropriate to subordinate mathematics to the needs of solving a physical problem.

Physicists assume that mathematical operations mirror or can be connected to natural processes, and the developing mathematical logic related to physical operations. Therefore, there is a constant dialectic, a dialogue, between mathematical operations and physical processes and a developing symbiosis between mathematical form and physical imagery. A physical interpretation of what the mathematical operations represent is imposed on the development of the mathematics, while the characteristics of the mathematics may shape how the physical system might behave. Sometimes, this imposition is an intuition arising from an understanding of the behavior of the physical system under scrutiny. An example, without getting into particular physical models, is the ultra-violet catastrophe. Physically the energy available in a radiating system is finite and this must be directly reflected in the mathematical results representing the physical behavior of the radiators. The understanding of what a physical system would, could, or could not do, is represented by the different paths into which the mathematical operations lead us. Thus, some directions of development may well be mathematically interesting but will not represent the behavior of the physical situation under discussion.

On the other hand, the arithmetization of experimental results, even though precise, is not the same as the development of a mathematical, theoretical physics. The vernacular remained the language of physical theory well into the nineteenth century after the development of quantitative experiments. Precise and clear conceptualization was and is not necessarily expressed mathematically. Conversely, mathematical theories have not always clarified physical theories and can mask muddled thinking. Yet both mathematics and quantitative experiment were aspects of the development of modern theoretical physics.

Physical interpretation is generally crucial in dictating the direction of the development of the mathematical analysis. However, such theoretical explorations of specific cases often lead to reinterpretation of the physical concepts that were used to set up the example. One of the more dramatic conceptual transformations is that of the second law of thermodynamics, from unavailable energy in the nineteenth to information propagation in the late twentieth century. Physical conceptualization of the problem and its subsequent reconceptualization are central to the enterprise of modern theoretical physics. This condition means that its practitioners learn early how to control and develop logical theories based on presuppositions. However, general presuppositions, while necessary to theory, are not sufficient to define it. The same set of assumptions can lead to whole classes of theories. Theory involves the use of hypotheses whose implications emerge from extended strings of logic developed in detailed specific cases. Speculations in general terms used ad hoc to explain small groups of phenomena, or isolated instances, do not constitute theory. Nor are general ideas linked by analogy, metaphor, illustrative example or other rhetorical devices, to experimental cases, counted as theory.[5]

5 Analogy refers here to the literary device, not the method developed in mathematical physics in the nineteenth century. Only recently have the issues of rhetoric and language penetrated the history of science. Nancy Leys Stepan, "Race and Gender: The Role of Analogy in Science," *Isis* 77 (1986): 261–282 traces the literature on analogy and metaphor in science. See also, Roger S. Jones *Physics as Metaphor* (Minneapolis, MN.: University of Minnesota Press, 1982). Most studies of the languages of the sciences focus on rhetorical purpose and devices, see Wilda Anderson, *Between Library and Laboratory* (Baltimore: Johns Hopkins University Press, 1987), *The Literary Structure of Scientific Argument,* Peter Dear ed. (Philadelphia: University of Pennsylvania Press, 1991), and *Persuading Science: The Art of Scientific Rhetoric,* Marcello Pera and William R. Shea eds. (Canton MA.: Science History Publications, 1990). On the more technical use of analogy in physics, see Mary Hesse, *Models and Analogies in Science* (Notre Dame: Notre Dame University Press, 1966).

Historically, the calculus made the discipline of theoretical physics possible by providing a highly developed language already linked, although only mathematically when first encountered by physicists, to mechanics. Unlike their mathematical predecessors physicists of the mid-nineteenth century began to integrate experiment into the body of the theoretical enterprise. It was no longer enough to begin a mathematical exploration with the algebraic relations derived from the results of experiment. The results of experiment were used to develop theory. Sometimes experiments indicated what restrictions to impose on the mathematics to make the analysis mirror natural processes. Experiment also reflected directly upon physical interpretation, which in turn interacted with the direction of development of the mathematical language. Theory was also developed in the light of the continual scrutiny of experiment and experimental physicists.[6] Previously, mathematics and experimental physics existed as separate disciplines. Experimental physicists could dismiss mathematics. For their part, mathematicians could ignore mathematically inconvenient experimental results. Theoretical physicists could do neither. The results of their mathematical manipulations encoded physical meanings that were open to exploration by experimentalists.

Experiments have both a physical interpretation and a presence in mathematical theory, i.e., they are embedded into its structure, and are not merely a peripheral element. Experiments are also quantitative and evaluated in terms of expected ranges of error and instrumental performance. Mathematical theories must lead to numerical deductions whose values lie within the expected experimental error, or, theoretical physicists need to explain the discrepancies.[7] Conversely, the

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6 On this point see, Timothy Lenoir, "Practice, Context, and Dialogue between Theory and Experiment," *Sci. Context* 2 (1988): 3–22, and *Theory and Experiment: Recent Insights and New Perspectives on Their Relations,* Diderik Batens and Jean-Paul Bendegen eds. (Dordrecht: Reidel, 1988).

7 Historians are reassessing the "theory-laden" character of experiments. Experiments have broader uses within physics than Kuhn allowed and experimentalists more autonomy within the profession. For a view of the place of experiment in the historiography of science see Frederick L. Holmes, "Do We Understand Historically How Experimental Knowledge is Acquired?" *Hist. Sci.* 30 (1992): 119–136. Holmes' paper is a review of recent literature in which historians have tried to reestablish a place for experiment in post-Kuhnian historiography. Other attempts include, Andrew Pickering, *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992), 65–112, *Experimental Inquiries: Historical, Philosophical, and Social Studies of Experimentation in Science,* Homer E. LeGrand ed. (Dordrecht: Kluwer Academic, 1990), *The Uses of Experiment: Studies in the Natural Sciences*, David Gooding, Trevor Pinch and Simon Schaffer, eds. (Cambridge: Cambridge University Press, 1989), Peter Galison *How Experiments End,* (Chicago: University of Chicago Press, 1987), *The Development of the Laboratory,* Frank A. J. L. James ed. (New York: Macmillan, 1989), and *Observation, Experiment and Hypothesis in Modern Physical Science,* Peter Achinstein and O. Hannaway eds. (Cambridge MA.: MIT Press, 1985). See also Allen Franklin *The Neglect of Experiment* (New York: Cambridge University Press, 1986), an attempt to develop a

physically connected and constrained mathematical language can be interpolated to predict the results of hypothetical experimental situations. Predictions of new physical phenomena from physical imagery were and are powerful legitimations for theories.[8] Before the middle of the nineteenth century, experimental predictions from mathematical results came from a comparison of the mathematical forms encompassing an older, known phenomenon, with the mathematical form deduced from a newly explored one.[9] Later in the nineteenth century, experiments served important functions for theoreticians, as the initial step in the development of a theoretical train, an aspect of the development of that theory, or its end result.

The above description is not of the behavior of the archetypal theoretical physicist. Not every theoretician in physics is a paragon who trims his mathematical sails to fit the winds of interpreting nature, nor does he as an individual necessarily develop a mathematically appropriate and physically interpreted theory integrating the latest experimental data. Some experimentalists dealing with the time consuming and frustrating details of experiments are happy to present results, legitimated through known procedures of testing equipment, instruments and methodologies, that current theory cannot explain. It is not the experimentalist who fits his data to theory, experimentalists are happy to point out, but the theorist who must adjust to their results. Yet for the community of the discipline and profession of physics the above paragraphs reflect generally shared values and behaviors. Individuals may spend their research careers solving problems that seem to be purely mathematical, without appearing to glance at the vulgar realities of experiment. However, the majority function within the discipline between the extremes of the physicist fascinated by mathematics and the experimentalist ignoring the theorists.

From the eighteenth through to the twentieth centuries, mathematicians, philosophers, then theoretical physicists have discussed the relationship between mathematics and physics. Mathematicians in the late seventeenth and throughout the eighteenth centuries understood that physics generated the problems that became the foundations for new mathematics. While eighteenth-century thinkers considered experimental physics and mathematics as separate disciplines, they also considered mathematics as completing the investigative enterprise. Immanuel Kant echoed this separation in his ideas on mathematics. His contention that at least

new epistemology of experiment.

8 And, contrary to current philosophical and sociological theorists, prediction has served that function for physicists. This is why James Clerk Maxwell pursued the temperature behavior of gases as a unexpected consequence of his kinetic theory. If confirmed, his unusual approach to the study of gases using probability was legitimate and his deductions correct. His results were also repeated by a group of rather skeptical German physicists, and a mini-industry of experimentally tracing the transport coefficients of gases ensued. The career of Oscar Meyer was based on such a search.

9 Other cases of prediction before the mid-nineteenth century require more subtle historical examination. See the case of the shape of the earth in chapter III.

certain mathematical entities were independent of the senses guided mathematicians in their later struggles to legitimate their science. The character of geometry became problematic. While Kant believed the categories of Euclidean geometry were independent of observation, this argument was unsatisfactory with the development of non-Euclidean geometries. While mathematicians also happily developed non-arithmetic algebras, the philosophical status of geometry created a philosophical ruckus in the middle of the nineteenth century. At the center of the dispute Hermann von Helmholtz maintained that we describe the world in Euclidean terms only because we live in Euclidean space. Moreover, this space is only a special case of the geometry developed by Bernhard Riemann that was itself a special case of the spherical geometry of Eugenio Beltrami. Kant's a priori's were no more. The axioms of any geometry bore no relationship to real things. They were useful because joined to mechanics they could be verified by observation. "If such a system were to be taken as a transcendental form of intuition and thought, there must be assumed a pre-established harmony between form and reality."[10]

Helmholtz nicely reemphasized the primacy of experiment in physics and countered an argument used by mathematicians since the seventeenth century of such a "pre-established" harmony between mathematics and the structure of nature. This harmony guaranteed mathematics a transcendency never given to experiments or observations.[11] At the same time as they expressed the accuracy of analysis to investigate nature, mathematicians pointed out the fallibility of experiment. They never differentiated between qualitative or quantitative experiments; both were equally prone to error.[12] Quantification of experiment is not the mathematization of theory and the two processes are not simultaneous or coterminus. As physicists annexed the calculus for their own purposes they reinforced the experimental core of their discipline and did not claim transcendence for their theoretical explorations.

For their part, some late nineteenth-century mathematicians claimed such transcendence and a preestablished harmony that enabled them to subsume physical

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10 Hermann von Helmholtz, "On the Origin and Significance of Geometrical Axioms," in *Science and Culture: Popular and Philosophical Essays,* David Cahan ed. (Chicago: University of Chicago Press, 1995), 226–245, 245.

11 However, see Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995) that the transcendency claimed for mathematics was crucial in the seventeenth century to establishing the validity of experiment.

12 Historians have begun to explore the role of quantification and mathematics in claims of the sciences for the transcendence that surrounded mathematics. See Philip Mirowski, *More Heat than Light: Economics as Social Physics, Physics as Nature's Economics* (New York: Cambridge University Press, 1989), and Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton NJ: Princeton University Press, 1995).

theory within mathematics.[13] Physicists did not rise to this bait. Albert Einstein, who fretted about his lack of mathematical understanding, reminded David Hilbert that physics was not simply mathematics. However, some physicists, notably in the twentieth century, have worried about the mathematics appropriate for specific physical problems.[14] Ludwig Boltzmann took mathematics as the language of physics and was concerned, as was Max Planck and Albert Einstein, with the proper mathematics for theoretical physics. They all matched the characteristics of the mathematics to the characteristics of the physical system. They used summations for molecular and integrals for continuum systems.[15]

The ability to perform the difficult task of discerning, in the mathematics, possibilities that are physical plausibilities is rare and justly celebrated. If physics becomes mathematics (as has been true in important cases in the late nineteenth and the twentieth centuries) members of the discipline are conscious of it. Explanations of the physical meaning within mathematical symbols was required by other members of the discipline. Physical interpretation of what is fundamentally a physically uninterpretable theory seems to go on.[16]

Modern Physics

By 1870 a combination of characteristics that we recognize as theoretical physics existed in Europe, although not as a distinct subfield within physics.[17] In Europe,

13 Elie Zahar, "Einstein, Meyerson and the Role of Mathematics in Physical Discovery," *Brit. J. Phil. Sci.* 31 (1980): 1–43, 2–8, considers some of the issue raised here but from the point of view of the philosophy of mathematics rather than its history or the history of physics.

14 See Eugene Wigner, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," *Comm. Pure and Appl. Math.* 13 (1960): 1–14. For earlier attempts to examine the relationship between mathematics and physics see Maxwell, "Address to the Mathematical and Physical Section," *Rep. British Assoc.* 40 (1870): 1–9.

15 See Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein* (Chicago: University of Chicago Press, 1986) vol. 2, 13, 188, 336–337.

16 Particularly noteworthy are Ludwig Boltzmann's attempts to give mechanical meaning to his statistical derivation of entropy and Niels Bohr's attempts to interpret the quantum mechanics of Werner Heisenberg.

17 This study focuses on Europe because physics in the United States followed older patterns of research into the twentieth century. See John Servos, "Mathematics and the Physical Sciences in America, 1880–1930," *Isis* 77 (1986): 611–629. For a discussion of the reorientation of physics in the United States in the 1920s, see Spencer Weart, "The Physics Business in America 1919–1940: A Statistical Reconnaissance," in *The Sciences in the American Context: New Perspectives* Nathan Reingold ed. (Washington DC: Smithsonian Press, 1979), 295–358. However, S. S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States, 1920–1950," *Hist. Stud. Phys. Sci.* 17 (1986): 55–98, maintains the uniqueness of American physics well beyond the 1920s.

theoretical physicists remained on the margins of the profession, numerically, and in terms of status for some decades.[18] Within the research literature, recognizably modern theory papers appeared regularly amongst the experimental majority. These theoretical papers were published separately from any mathematical insights that arose from the solution to the problem under investigation. Mathematical results were printed in separate journals, and they were addressed to a different audience and written with the mathematical implications of the solution set firmly before the reader. The mathematical language of physical theory was usually the calculus coupled with physical imagery whose implications were developed in logical detail along with, and even through, the mathematics. Physical interpretation incorporated experiment into the body of theory that was sometimes extended beyond the limits of known, experimental results, using mathematics, to express plausible, observable outcomes.[19] Predicted phenomena were the subject of systematic experimental searches, initially conducted by the theorists themselves. If found, such phenomena were accepted as demonstrations of the physical interpretation proffered by theory. This was new. In earlier uses of the calculus experiments were treated as confirmations of the truth of the mathematics.[20]

Even as physicists use mathematics to express their ideas, the disciplines of physics and mathematics remain separate. The goals of practitioners remain distinct. Mathematicians have often begun their explorations within mathematics using problems suggested by other disciplines.[21] Recently new fields within mathematics have arisen from the problems of optimizing resources in warfare. However, mathematicians need not reflect on the impact of their mathematical results for the placement of artillery or machine guns, or the logistics of supply lines. These issues lie outside of mathematics proper. Yet when outsiders, such as physicists, use the mathematics thus developed, mathematicians sense that barbarians have invaded their territory. The standards of solution so painfully crafted by mathematicians are thrust aside for others. This sense of intrusion is particularly acute

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18 See Paul Forman, John Heilbron and Spencer Weart, "Physics *circa* 1900: Personnel, Funding, and Productivity of the Academic Establishments," *Hist. Stud. Phys. Sci.* 5 (1975): 30–32. Christa Jungnickel and Russell Mccormmach, *Intellectual Mastery of Nature* make the same important point although they do not always differentiate the title of a chair and the actual research of its occupant. Some physicists taught theoretical physics while their research was experimental.

19 The kinetic theory of gases is the first place in which these relationships occur consistently.

20 See James Clerk Maxwell's surprise at the predicted behavior of the viscosity of gases, and then his investigation of the transport coefficients, in, *Maxwell on Molecules and Gases* Elizabeth Garber, Stephen G. Brush and Francis Everitt, eds. (Cambridge MA.: MIT Press, 1986), 282–283, 359–386. This is in contrast to the earlier uses of experiment in the work of Pierre Simon Laplace, Denis Poisson and earlier mathematicians.

21 There are even claims that pure mathematics does not exist. See Nordon Didier *Les mathêmatiques pures n'existent pas!* (Paris: Actes Sud, 1981).

when the invaders annex languages whose power and structure were revealed only after considerations of definition and careful examination of the language. From the point of view of mathematicians the rigor, precision of expression and generality, the core of their discipline, is betrayed. Mathematicians see physicists using mathematics as sloppy, and making unjustifiable assumptions about how to use their language.

On the other side of the disciplinary and professional boundary, physicists see mathematicians as obsessed with the wrong issues. They spend too much time over whether a proof is adequate or whether another, more general one might be necessary. The point is to use the results of these theorems, with an intuitive sense that the theorems seem correct. Given the same problem, the mathematician's and the physicist's solutions are developed with different criteria of what those solutions might be. This makes for lively, if acrimonious, joint sessions of two departments with members that have overlapping research interests. If mathematicians begin with a physical problem, they do not have to reflect on the implications of their mathematical findings for the physical particulars of that problem. This closure is the point of the physicist's exploration of the same issue and separates the practitioners of the two disciplines. The numerically correct answer to particular problems is crucial for the physicist because of its significance for ideas about nature. In a certain sense it is irrelevant to the mathematician. Mathematics and physics remain as distinct disciplines, expressed institutionally as separate academic departments, curricula, professional societies, journals, and research methods. Yet, as disciplines and professions, they remain locked in an historical symbiosis that demands investigation.

Several elements join to characterize modern, theoretical physics. Any history of its development must consider all these factors and their fusion into this modern form. This is not merely a matter of intellectual history. Theoretical physics is an academic discipline, most of whose members are systematically training the next generation of theoretical physicists. The university also serves as the locus of research, much of it done by the same faculty and their students. The results of such research appears in the footnoted pages of specialist journals usually published by the professional societies, to which all claiming to be members of the discipline, belong. As aspiring members of the discipline, applicants to such societies must meet minimum requirements of certification. In turn, membership in such a society legitimates claims of practicing as a physicist and belonging to the profession. The practices of the societies' members are expressed and developed within the pages of its journals, and reinforced by a system of prizes, medals and other honors. The readership of these journals is small, usually restricted to those understanding the technical methods, language, and problems being addressed in their pages. Interpreting such publications for a broader public requires a battery of mediators. Access to training and practicing within physics is controlled by physicists as are judgments on the intellectual worth of their colleagues' research. So does the distributions of awards, status, and power within the discipline. The social, economic, and political support for the men and women who live immersed in research, teaching, training, and publishing comes largely from the state.

Before 1850 none of the social structure of the profession of physics existed, nor did its legitimating processes and its institutional structure. The social, political and intellectual characteristics of modern physics coalesced during the mid-nineteenth century. This study explains how and when theoretical physics came into existence by focussing on those aspects of the narrative and process omitted in earlier accounts.

To begin we need to examine where other historians of science have found the origins of theoretical physics and assess their assumptions about the development of physics. In most accounts one characteristic of physics is taken in isolation while, as we shall see, a whole collection of factors was involved and these later fused into the newly minted theoretical physics.

Earlier Historical Approaches to Modern Physics

Of the various ways of treating the history of physics, the most prominent is still intellectual. Conceptual changes are generally used to demarcate stages in the development of physics. Thomas Kuhn reenforced this approach.[22] This emphasis is unchanged whether the historian attributes the development of ideas to forces of change within the scientific disciplines or the larger cultural nexus. Concepts join science to society and culture, and for social constructivists and cultural historians only serve to demonstrate ideological activities.[23] Only recently have historians turned to other factors under the rubric of practices.[24]

Using conceptual change to mark the development of physics has severe limitations. The "origin" of modern physics depends on those concepts the historian

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22 Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962). See also, Kuhn, *The Copernican Revolution* (Chicago: University of Chicago Press, 1959).

23 The literature here is enormous and disputes between the protagonists within these approaches numerous. The common element is that the ideas and goals of scientific knowledge are grounded in societies and their ideologies and have no independence from those contexts. Steven Shapin, "Here and Everywhere: Sociology of Scientific Knowledge," *Ann. Rev. Socio.* 21 (1995): 289–321, traces its development. For a recent statement of the ideological content of science see Joyce Appleby, Lynn Hunt, and Margaret Jacob, *Telling the Truth about History* (New York: Norton, 1994).

24 For the range of concerns that the notion of practices brings to the history of physics see, *Scientific Practice: Theories and Stories of Doing Physics,* Jed Z. Buchwald, ed. (Chicago: University of Chicago Press, 1995). For some of the conceptual problems with the notion of practices see Stephen Turner, *The Social Theory of Practices: Tradition, Tacit Knowledge, and Presuppositions* (Chicago: University of Chicago Press, 1994).

chooses as "modern." The seventeenth-century ideas of Galileo Galilei, Isaac Newton, the conservation laws developed in the mid-nineteenth, and the idea of the quantum of the early twentieth century have all served this purpose.[25] Most of these historians implicitly assume that the engine of change in physics lies only in hypotheses, methods rarely figure as key indicators or explanations of change.[26]

The fundamental limitations of the conceptual measure of change are structural. General principles and reasonable expectations of their outcome do not suffice to define theory, or theoretical physics. General notions, such as the idea that heat is a substance, caloric, conserved through thermal change, led to a plethora of theories of heat. The assumption that light was a wave in the ether also led to a range of detailed speculations on the nature of those waves and that of the ether itself.[27] Theory includes general hypotheses and other assumptions used to incorporate specific cases into the explanatory scheme. In the case of caloric it could be a fluid that flowed into and out of material bodies, or made up of particles that interacted with those of matter. We also need to consider standards of argument. Were these particular conceptions of caloric used consistently from one thermal phenomena to another? Were changes that were introduced those of the development of a consistent imagery or merely imagery changed to suit a particular example? How was the experimental evidence incorporated into the body of these theories, if at all?

In earlier centuries speculations on the behavior of nature, and standards of demonstration were very different from those of the twentieth century. literary devices, analogy, metaphor, illustrative example all served the purpose of establishing the viability of an explanation. The social and cultural setting of such speculations also impacted how practitioners defined solutions to problems of natural philosophy. In caloric theories of heat, and eighteenth-century speculations in general, none of the standard explanatory practices that we expect in the twentieth century were in use. In many of these theories caloric remained a metaphysical entity, while the results of experiment were the subjects of a separate discourse.

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25 For recent examples see H. Floris Cohen, *The Scientific Revolution: A Historiographical Inquiry* (Chicago: University of Chicago Press, 1994), in which the sciences are still defined intellectually such that, "the seventeenth century marks the origin of modern science." For use of the concept revolution to explain change in the sciences see, I. B. Cohen, *Revolutions in Science* (Cambridge MA: Belknap Press, 1987). Here revolutions are related to conceptual change only.

26 There are exceptions. See Geoffrey Cantor, "The Reception of the Wave Theory of Light in Britain: A Case Study Illustrating the Role of Methodology in Scientific Debate," *Hist. Stud. Phy. Sci.* 6 (1975): 109–132 and some of the recent literature on practices.

27 See Robert Fox, *The Caloric Theories of Gases from Lavoisier to Regnault* (Oxford: Clarendon Press, 1971). See also *Conceptions of the Ether: Studies in the History of Ether Theories, 1740–1900* G. N. Cantor and M. J. S. Hodge, eds. (Cambridge: Cambridge University Press, 1981).

If ideas about caloric were connected to the results of experiment, it was through illustrative examples, figures of speech, and arguments about reasonable expectations, not through any species of logical strings, even excluding any forms of mathematics.

The usual starting place for the development of theory in modern physics is not from postulating the existence of a general principle or substance but through the solutions to problems. A particular set of physical circumstances, that is, particular problems associated with a narrowly defined physical system, is the locus of the theorist's attention. Upon these specific cases general principles may be brought to bear. However, these principles are in a form that relates directly to the specific case at hand. The starting point for Einstein's theory of relativity was the motion of a charged particle moving through an electromagnetic field, not the nature of space and time. In modern theoretical physics specific physical circumstances are joined to mathematics, then judgments are made about how much of the latter to use to reveal the workings of a particular physical system. The range of interpretations is limited by the structure and use of the available language, the mathematics. These practices were not used in physics until well into the nineteenth century. Forms of argument need integration into the history of physics.

The shortcomings of histories that deal only with foundational assumptions are also compounded by another. Intellectual historians of science usually assume that all problems, ideas, and methods that are now accepted as part of theoretical physics have been used consistently within physics since their first appearance.[28] The usual assumption is that mathematics has been a permanent aspect of physics ever since Galileo's mathematization of the fall of terrestrial bodies. Isaac Newton in his *Principia* was dealing with a problem in physics, and we can expect amidst the struggle to develop his ideas the same mix of observation and mathematics that we find in theory in the twentieth century. The usual account of the history of physics narrates the spread of this method from mechanics to the study of electricity, magnetism and light. Extension of the use of mathematics has followed the successful development of physical ideas.

Because mathematics is now a permanent factor within physics, intellectual historians tend to regard the work of Newton, the Bernoullis, Denis Poisson, or George Gabriel Stokes as forming a continuum with our own. For these historians, the issue is not whether these men produced physical solutions to problems we take to lie within physics, but what are the physical constituents of those solutions. This approach is apparent in recent histories of nineteenth-century physics. Authors assume the relationship between physical imagery, mathematics, and experiment in the early nineteenth are those of the twentieth century. They also treat theories

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28 Kuhn's descriptions of the development of physics depends on this assumption. See Kuhn *Structure* passim.

as having the same function within physics as in the twentieth century.[29]

These suppositions mean that intellectual historians presume that mechanics has remained within physics since the seventeenth century. Implicit in this is the premise that the combination of physical imagery and mathematics that Newton brought to bear on the problem of the planets was read as theoretical physics in the seventeenth, eighteenth, and nineteenth centuries. This approach is accompanied by the preconception that, since the seventeenth century, anyone considering the same problem was also a theoretical physicist having the same general goals and concerns as modern physicists.[30] As recent studies have shown, physics in the eighteenth and early nineteenth centuries was based in experiment, and speculations about nature were expressed in the vernacular. There are strong indications that Newton's *Principia* was read as mathematics, not physics.[31] This being the case, historians of physics must examine the implications of the reactions of Newton's contemporaries, for the history of their field.

Historians of mathematics have shown that mechanics was the means for developing much of the calculus in the eighteenth century, not for deciphering nature. Few historians of physics have asked whether work in mechanics through the eighteenth century lay within physics, or whether Jean Le Rond d'Alembert, Joseph Louis Lagrange or Pierre Simon Marquis de Laplace might be mathematicians who chose problems of mechanics as a means to explore the calculus.

Recent studies show us forcefully that throughout the eighteenth and the nineteenth century, the disciplinary core of physics was experiment. Experiment functioned to broaden the reach of knowledge, to enlighten. Until the mid-ninteenth century interpretations of experiments or explanations of the behavior of nature that joined the results of experiments together were a combination of metaphysical commitments resting on slim evidential ground. They joined the evidence through analogy, illustrative example, plausibility and other rhetorical devices. Theories as systematic strings of argument were rare. Rarer still were theories that enmeshed experiment within the body of its development. More often, experiment remained on the margins of metaphysical speculations. Metaphysical principles

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29 See, Crosbie Smith and Norton Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989), Nahum Kipnis, *History of the Principle of Interference* (Boston: Birkhäuser, 1990). Jed Buchwald, *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century* (Chicago: University of Chicago Press, 1989).

30 This is the approach of René Dugas, *Histoire de la Mécanique* (Paris: Édition Dunod, 1950) that set the pattern of discussion by historians of physics.

31 See Clifford Truesdell, "Reactions of Late Baroque Mechanics to Success, Conjecture, Error, and Failure in Newton's *Principia*," *Texas Quart.* 10 (1967): 238–258, *The Annus Mirabilis of Sir Isaac Newton, 1666–1966* Robert Palter ed. (Cambridge MA.: MIT Press, 1967), and Henry Guerlac, "The Early Reception of his Physical Thought," in Guerlac, *Newton on the Continent*, 41–73.

and schematic systems were plentiful. Theories that centered upon experimental observations were phenomenological, or classified phenomena into rational lists that reduced large bodies of data to manageable size.[32] Within physics, speculations about the operations of nature were expressed in language and images available to a general, literate audience using devices of argument familiar from sources outside of science. This kind of physics does not fit into twentieth century categories. Harnessing metaphysics to logic, even in the vernacular, did not appear in physics until the nineteenth century, after physicists had definitively narrowed the forms and focus of the discipline.

The shortcomings of some histories spring partly from being unaware that the label may stay the same, but the discipline can change profoundly. If we do trace the changes in the meaning and content of the terms "physics" since the seventeenth century, we find that mathematics only enters into its methodology in the latter half of its history.[33] How then, did modern physics develop out of its experimental and metaphysical practice? We can no longer see physics as the residue left after chemistry and other specialities separated from the general body of natural philosophy. Nor can we argue that modern physics is the fusion of mathematics and physics without some understanding of the process of fusion.[34]

The one extensive recent work on the development of modern theoretical physics leaves us with some dilemmas. This study by Christa Jungnickel and Russell McCormmach is one for which other historians should be extremely grateful.[35] However, the subject of their study, theoretical physics, is never defined. While appreciating that leaving the definition moot avoids the temptation of whiggish

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32 Examples are of Charles Dufay in electricity, Karl Linnaeus in botany and William Herschel in observational astronomy.

33 For changes in the meaning of the term physics see, Hans Schimank, "Die Wandlung des Begriff 'Physik' wahrend der ersten Hälfe des 18 Jahrhunderts," *Wissenschaft, Wirtschaft und Technik: studien zur Geschichte* Munich (1969): 453–468, Fritz Krafft, "Der Weg von den Physikern zur Physik an deutschen Universitäten," *Ber. Wissen.* 1 (1978): 123–167, and, "Alte und neue Physik," in *Disciplinae Novae* Christoph Scriba, ed. (Göttingen: Vandenhoek und Ruprecht, 1979), 45–63, R. Hookyas, "Von Physica zur Physik," in *Humanismus und Naturwissenschaften* Rudolf Schnitz and Fritz Krafft, eds. (Boppard am Rhein: Harald Bopt, 1980), 9–38, and John Heilbron, "Experimental Natural Philosophy," in *Ferment of Knowledge: Studies in Eighteenth Century Science,* R. S. Rousseau and Roy Porter, eds. (Cambridge: Cambridge University Press, 1980), 357–387. The same is true of the term "element" in chemistry where Boyle's definition can be taken over into a modern, elementary textbook without change, until the reader becomes aware of Boyle's theory of matter. There are of course numerous others.

34 This fusion is assumed in Kuhn, *Structure,* "Postscript," 179. Kuhn later posited two traditions within physics, without considering that mathematics might also be a problematic category. See, Kuhn, "The Mathematical and Experimental Traditions in Physics," *J. Interdic. Hist.* 7 (1976): 1–31.

35 Jungnickel and McCormmach, *Intellectual Mastery of Nature.*

purpose, we are left with that group appointed to positions as theoretical physicists in the universities of Germany and middle Europe. The institutional and social definition does not touch on the intellectual content of physics. The authors accept the content of the research of these men, appointed to certain chairs in the above universities, as physics wherever it was placed for publication. Some of their research appeared in mathematics journals and yet counts as physics because the authors are physicists from their position in the university structure. And, the measure of the development of theoretical physics is in terms of ideas and theories, not practices. They miss the ways in which criteria of solutions and standards of argument change across time. They are assumed to be those we accept today. Mathematics is simply a "tool" although central to what theoretical physicists did even in the last decade of the nineteenth and the first decade of this century when the required tools were changing.

These omissions mean that we cannot probe crucial differences between mathematics and physics, mathematicians and physicists. They leave unexplained how the new mathematical "tool" of absolute differential calculus could transform the physicists' image of the world. This crucial connection between mathematics and physical imagery needs more investigation. While noting that some physicists did refer to mathematics as a language, they probe no further. However, this is an important key to exploring how theoretical physics came into existence as an intellectual discipline. To explore this process we need criteria that are broader than subject matter and ideas and to follow them across space and time to trace how physics and mathematics were distinguished.

It seems necessary at this juncture to ask how mathematics has been used in physics. Most historians of physics ignore mathematics in their narratives, be they scholarly studies or textbooks. At best they cite formulae and derivations without paying any attention to their origins. Ivor Grattan-Guinness attributes this to a dislike of mathematics on the part of historians of science.[36] However, there is a more pervasive problem here. Most historians and philosophers of physics treat mathematics as a tool, an instrument to be applied to a task, like a hammer to a nail, then replaced on the shelf. It is unchanging and always available. And even if mathematics has a history, the type of mathematics used has no impact upon possible physical interpretations drawn from the mathematics. Thus, the mathematics in physics can be rewritten in modern form; the physics remains the same. Edmund Whittaker complained that physicists for whom vector analysis was not available did not see certain implications of their work. Whittaker and later historians then translated physicists' mathematics into vector form and drew implications about their work directly from the modern mathematical forms, not the original ones.[37]

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36 See Ivor Grattan-Guinness, "Does History of Science Treat of the History of Science? The Case of Mathematics," *Hist. Sci.* 28 (1990): 149–173.

37 This is the assumption in Buchwald, *The Rise of the Wave Theory* and *The Creation of*

However, until the end of the nineteenth century, the mathematics of physics was in algebraic form and Cartesian coordinates. Historians have not considered how physicists have proceeded through mathematical forms to conclusions, and how the available mathematics might shape, or limit, those conclusions.

Mathematics has interacted with, and become integral to physics. And, as in other cases, this transformation became invisible. In retrospective examinations, eighteenth-century mathematics was subsumed by physicists as physics. However, problems that arose in mechanics and astronomy were the means by which mathematicians developed the calculus itself in the eighteenth century. The resulting papers were mathematical, not physical. Mechanics in its mathematical form was a field within mathematics. Yet, in the mid-nineteenth century mechanics became the explanatory core for physics. For mid-nineteenth century physicists, eighteenth-century rational mechanics became physics. Mathematical expressions were given physical meanings derived from methodological and conceptual commitments not present in the eighteenth century. In their definitive textbook on mechanics of the 1870s, William Thomson and Peter Guthrie Tait used Lagrangian generalized coordinates extensively. They accomplished the considerable task of explaining Lagrangian methods to engineers and physicists and of harnessing them to solve physical problems. In the process, Lagrange's original mathematical goals were largely lost.[38] This retrospective treatment of mathematics in the history of physics has appeared consistently since the mid-nineteenth century because mathematics is such an integral part of modern physics.

New mathematical languages such as non-arithmetic algebras or non-Euclidean geometries had profound effects on theoretical physics, including the ways in which physicists envision reality. Seeing the vector quality of physical entities makes visualizing their behavior in space immediate in ways that their expression in algebraic form do not. Vectors make visible the spatial aspects of the motion of a wave front and of moving particles but were available only after waves became a central feature of nineteenth-century physics. The same is true for electrodynamics.[39] Aspects of the theory of relativity became apparent after Hermann Minkowski put Einstein's equations into non-Euclidean geometrical form. Mathematics is repeatedly claimed by physicists as their language. It is also seen as the one characteristic

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Scientific Effects: Heinrich Hertz and Electric Waves (Chicago: University of Chicago Press, 1994) among many others. It began with Edmund Whittaker *A History of the Theories of Aether and Electricity* (New York: Thomas Nelson and Sons, 1962), 2 vols.

38 William Thomson and Peter Guthrie Tait, *Treatise on Natural Philosophy* (New York: Dover reprint of 1879 edition, 1962) 2 vols. For the development and writing of this volume see Crosbie Smith and Norton Wise, *Energy and Empire.*

39 Vector algebra developed in the 1880s. See Michael Crowe, *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System* (Notre Dame: Notre Dame University Press, 1967).

of the discipline that provides any unity to the current sprawl of physics.[40]

Mathematics also has a history, as complex and contested as any other scientific discipline. Meanings of the fundamentals of mathematics, and in particular, the foundations of the calculus, have changed through time and across space.[41] As the content of mathematics mutated, the mathematics available to physicists has also changed. The language forms with which physics is expressed have become dependent on available mathematics. Yet issues that arose in the physical world established the problems that mathematicians used to explore mathematics, extend its range, change its character and reach. Physics has also been a source for mathematics. For mathematicians of the eighteenth century, their starting point in problems in the real world guaranteed that solutions existed. Mathematicians needed physics for their own purposes as surely as physicists have molded mathematics to their own uses. For a more successful history of physics to emerge, we need to scrutinize this interdependence.

The intellectual questions surrounding the relationship between mathematics and physics are inevitably coupled to social ones. New institutional forms were as integral to the creation of modern physics as its new intellectual ones. While one transformation need not involve the other, in this case both social context and intellectual content changed almost simultaneously. However, the immediate causes for both were not the same. In Britain, industrialization opened up opportunities for livelihoods in science, yet this did not dictate changes in the methodology, form of expression or content of physics. Those came later, under the social pressures of successful industrialization on older academic institutions. The changes in practice in the middle third of the nineteenth century followed the intellectual leadership of Paris.[42] In the German States the social opportunities brought by the reform of the Prussian universities, driven by an educational ideology serving the political purposes of the state, involved the transformation of faculty from collegial teaching bodies of local importance into scholars with international research reputations. In this case, research into esoteric questions and issues without visible connection to

40 James A. Krumhand, "Unity in the Science of Physics," *Phys. Today,* March (1991): 33–38, and David Mermin, "What's Wrong with these Equations," same journal (1989): 9–11, p. 9.

41 See Judith Grabiner, "Is Mathematical Truth Time-Dependent," *Amer. Math. Monthly* 81 (1974): 354–363. See also Joseph Dauben, "Conceptual Revolutions and the History of Mathematics,"in *Transformation and Tradition in the Sciences: Essays in Honor of I. B. Cohen,* Everitt Mendelsohn, ed. (New York: Cambridge University Press, 1984) 81–103. For a more extended discussion see, Ivor Grattan-Guinness, *The Development of the Foundations of Mathematical Analysis from Euler to Riemann* (Dordrecht: Reidel, 1970). See also Hans Niels Jahnke, "Mathematics and Culture: The Case of Novalis," *Sci. Context* 4 (1991): 279–295.

42 For social transformation without intellectual change see Rachel Laudan, "Ideas and Organization in British Geology: A Case Study in Institutional History," *Isis* 68 (1977): 527–538.

social use, led to particular forms of theoretical discourse in physics that depended on prior philosophical commitments. The educational ideology of the state did not, however, predetermine that Parisian scholarship in the sciences and mathematics be the model for German academics. Nonetheless, the French offered the most successful model to emulate and then transcend. During this process in Britain and the German States physics, as discipline and profession, was put into the forms we have inherited and hence labelled here, for convenience, as modern.

Mathematics as Language

Recently, some philosophers have discarded more traditional foundations for the philosophy of mathematics and have returned to the analysis of mathematics as a series of languages and the rules of translation between them.[43] There is a group that claims that the history of mathematics must be understood before a philosophy of mathematics can be constituted for whom Ludwig Wittgenstein's philosophy of mathematics is the starting point. Clearly Wittgenstein's assumption that meanings are established through the consensus of its practitioners makes its history central to an understanding of his philosophy.[44] The semiotics of mathematics is also growing.[45] However, long before mathematics or physics were regarded as text, mathematicians and philosophers understood that mathematics was about signs (symbols) whose actual meanings were so abstract as to not specify any real entity, thus functioning as metasymbols.[46]

Symbolism such as mathematics is open to annexation and interpretation along with the syntax and grammar of the language. This vulnerability is broader than with other languages, even in the language most often compared to mathematics, music. Both are ruthlessly efficient, have well developed formal structures that have been transformed from which emerged new kinds of music and mathematics. And, these changes came from explorations within very particular problems to formal considerations of definitions, structure, and syntax. Notation in music and mathematics is important for understanding and interpreting their content that also

43 For our purposes we are exploring in detail only those meanings of the languages of mathematics as they affected and were used by physicists rather than mathematicians.

44 Ludwig Wittgenstein, *Remarks on the Foundations of Mathematics*. R. Rhees, G. H. von Wright and G. E. M. Anscombe, eds. (London: Blackwell, 1967). See *New Directions in the Philosophy of Mathematics*, Thomas Tymoczko, ed. (Boston: Birkhäuser, 1986), for the range of views on this point.

45 Brian Rotman, "Towards a Semiotics of Mathematics," *Semiotica* (1988): 1–35, Rotman, *Signifying Nothing: The Semiotics of Zero* (New York: St Martins Press, 1987), and David R. Lachtermann *The Ethics of Geometry: A Genealogy of Modernity* (New York: Routledge, 1989).

46 However, see, P. Hugly and C. Sayword, "Can a Language have Indenumerable Many Expressions?" *Hist. Phil. Logic* (1984).

changes through time. Aesthetics is also a major factor in judging any particular product. Subjects and forms can be reworked any number of times, as aesthetics and meanings attached to those subjects and themes change.[47] Within both disciplines, individuals can be spotted in their products. When solving the same problem, or using the same musical form, individuals express their own "style."[48] Even as d'Alembert, Euler, and Lagrange all wrote on the wave equation and its solution, their style is apparent in their characterization of the equation and its solution. This is in addition to national traditions that are obvious in mathematics throughout our period.[49]

In regarding mathematics literally as language one can draw parallels between sentences and mathematical expressions, the linguistic roles played by nouns and terms, adjectives and coefficients, verbs and operators.[50] As in ordinary language much is left understood and is a major source of ambiguity. There are implicit assumptions that one writer may treat as shared among all practitioners but are not. There may be deliberate omissions and ambiguities for moral or political purposes. Careers and reputations depend on the judgments of other mathematicians with known preferences and prejudices. Lacuna or obscurity may also hide difficulties, or spring from not understanding the significance of aspects of the problem under discussion, or from implicit assumptions of a deeper kind that make discussions of certain issues impossible as they may destroy the foundations of the discourse. This of course is true of eighteenth-century calculus.

Ambiguity and omission in mathematics can serve the same purposes as in any other languages. Similarly, delaying clarification may simply be a way of getting something expressed and out in the open. Linguistic niceties can wait. Such obscurities are cleared up only when implicit assumptions are forced into the open. The calculus was one such language that developed this way. Initially calculus was the algebraic description of geometric relationships: It was a language whose roots lay in another language. The differential represented the tangent to a curve, the integral the area under the curve. Calculus began to solve problems that older languages were inadequate to tackle. The mode of its development was the solution of problems, not the formal exploration of the structure and function of the calculus

47 For example, the same musical forms have been reworked and Euclidean geometry reformulated many times.

48 The concept of style is problematic. However, it is still useful. For its problems see *Sci. Context* 4, no. 2 (1991). For style in physics see, N. David Mermin, *Boojums All the Way Through* (Cambridge: Cambridge University Press, 1990).

49 Joan Richards, "Rigor and Clarity: The Foundations of Mathematics in France and England, 1800–1840," *Sci. Context* 4 (1991): 297–319.

50 For short introductions to mathematics as language and its uses in literature and as literature see Helen M. Pycior, "Mathematics and Prose," and John Fauvel, "Mathematics and Poetry," in *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences* Ivor Grattan-Guinness, ed. (New York: Routledge, 1994).

itself; that came later.

Any discussion of mathematics as language has to address the question of plot and narrative.[51] Mathematics has a highly compressed narrative form. Plots lie in the progression of proofs, theorems and solutions. There are even minor characters introduced along the way in the form of demonstrations or lemmas that do not seem to have much function in the narrative until they reappear later to resolve some impasse. A major demonstration may require reference back to the previous ones, including those that seem unconnected or out of place. There may be complexity, theorems within other demonstrations. Plots can become hopelessly muddled and convoluted, and the material gets beyond the technical reach of the author, as series are truncated, functions transformed and coefficients become constants. The author can only bring a simplified plot to completion, that is solve the problem in particular but not the general case.

In the eighteenth century plots begin in the establishment of a problematic expression (situation) of the equations of motion or equilibrium of a mechanical system. Resolution occurs through the highly structured form of the demonstration. Establishment of the appropriate mathematical equation was the arena for moral contests over metaphysical issues as well as technical understanding. The narrative was also an arena for contests over mastery of the form. As eighteenth-century mathematicians developed the calculus they were expected to express their solutions in their most general form, then to consider particular solutions. Acceptable practice precluded the use of trigonometrical series except in particular cases. Functional equivalents were preferred as the convergence of these series in general was not demonstrated satisfactorily. Truncating series, reducing functions to constants were also allowed although the goal of generality might be sacrificed. The ultimate display of technical ability came with the general solution to an equation previously only solved in particular cases.[52] However, what counted as a demonstration or that a general solution had been reached changed considerably in the nineteenth century. Mathematical modes of proof (narrative) changed.[53]

Philosophers and linguistic scholars concerned with the structural differences

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51 What is stated here about mathematics can be applied to theoretical physics. Most works on physics as literature focus upon the rhetorical purposes, deduced from conceptual presuppositions.

52 In the eighteenth century language was regarded as a form of calculus, Turgot, "Etymologie," in *Encyclopedie* in *Oeuvres* vol. 2, 473–516, 506–507. For mathematics and language in the eighteenth century, see Robin E. Rider, "Measures of Ideas, Rule of Language: Mathematics and Language in the Eighteenth Century," in *The Quantifying Spirit in the Eighteenth Century,* Tore Frängsmyr, John Heilbron and Robin Rider, eds. (Berkeley CA: University of California Press, 1990).

53 Other narrative forms developed in the nineteenth century, including that of the metaphysical novel, i.e., the search for an understanding of foundations of mathematics. Disputes over these issues sometimes made, broke, or redirected careers.

between mathematical and "natural" languages emphasize the syntactical richness of mathematics, while in "natural" languages contexts of meaning are far more important. When considering the annexation of the calculus by physicists in the nineteenth century, this neat distinction between syntax and semantics becomes somewhat clouded, yet remains useful. In the eighteenth century, mathematicians had developed the calculus through the solution to physical and other problems, themselves fraught, as we shall see, with semantic issues. For these mathematicians some semantic issues became battles over words as they transformed semantic problems into syntactical ones. A century later physicists set the developed calculus within a context of meaning, mechanics, related to that from which the calculus first emerged. Calculus provided them with a syntax developed, from their point of view, for their semantic needs. We cannot separate structure from meaning.

Sometimes the structure of the language carried with it implications for the behavior of the physical world. Elsewhere, the syntax of the language shaped ideas about the behavior of physical systems. In other instances contexts of meaning overrode the stricter syntactical concerns of the mathematicians who had created the language, the calculus. For physicists, the structure of the language and the context of meaning converged and interacted with each other. In their treatment of mathematics, physicists rendered these languages closer to a "natural" language.

None of the recent studies on the rhetoric and literary devices used in science, including mathematics, can account for its content. While some philosophers argue that metaphor is central to explanation, most recent studies of the languages of science focus on rhetorical issues either as social or cultural artifact.[54] Content is secondary. Yet content matters if we are to understand how rhetoric shaped ideas and the development of theory. In the case of physics, content is necessary to examine why and how mathematics became the language of physics; what in the content of the calculus enabled physicists to develop their ideas about nature that were so much more difficult to accomplish when expressed in the vernacular.[55]

The above is only suggestive and not offered as a formal theory but as part of a heuristic explanation to understand some of the developments in physics in the eighteenth and nineteenth centuries. The process of transforming the methodologically defined experimental physics of the eighteenth century into modern physics was, intellectually, one of annexation. It was the appropriation of the most developed of the languages of mathematics, the calculus, as syntax and grammar for the creation of another language, that of theoretical physics.

This appropriation was eased by the process mathematicians had used to create the calculus in the first place. Beginning with the specific imagery of mechanics as

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54 See the references in note 5.

55 For metaphors see Mary Hesse, *Models and Analogies in Science* (Notre Dame: Notre Dame University Press, 1966). For a more recent study on language see Richard Rorty, *Relativism and Truth* (New York: Cambridge University Press, 1991).

a springboard into the exploration of the more general linguistic structures latent in the particular algebraic expressions of mechanics, mathematicians constructed, then explored those general linguistic structures. They used meanings and forms familiar later to nineteenth-century physicists. However, mathematicians did not use the results of the mathematical enquiries to reflect back upon interpretations of the physical entities with which they began their problem solving. In the eighteenth century interpretations and reinterpretations of force, and *vis viva*, for example, were metaphysical matters. Their existence as theoretical entities might emerge from experiment, observation or philosophical contemplation but having mathematical expression they lost all physical significance. Their function was mathematical, not physical. Experiment set up the terms from which mathematicians linguistic explorations began, and to confirm the correctness of the syntax and grammar that developed out of those explorations: experiment confirmed the mathematics, not the physics.

Yet, the notion of mathematics as language is insufficient to explain the timing of the transformation process in the middle third of the nineteenth century. Why did this not happen in the eighteenth century? Both physicists and mathematicians had a common interest in problems of mechanics. However, in this era disciplines were defined through practices exploited in the study of nature. Physicists used experiment and observation to explore nature and these defined their terms of engagement with problems, and of their solutions. Mathematicians used their own terms of engagement and methodologies to explore the same phenomena, and defined the solutions to those questions using criteria developed within mathematics. At the end of the eighteenth century, within the French scientific community, this methodological core for physics was coupled with an attempt to repudiate the search for essences. This predisposition truncated any search for meanings and confined explorations of nature to description. Therefore, *physique-mathématique* was an extension of the calculus as a descriptive, not an interpretative, language for understanding the operations of nature. Given the standards and practices of mathematicians it was also mathematics, not physics. In these same decades, the implicit assumptions at the foundations of the calculus were forced into the open. This process of disclosure was initiated partly by external criticism and internal dissatisfactions with the syntax of the calculus, and challenges to the implicit limits of the language imposed by its practitioners. The foundations were reworked and the first formal and logically defensible form for the calculus emerged from that work. In France at least, these changes were not accompanied by the redrawing of the boundaries between mathematics and physics.

Within the scientific communities of the German States and Britain, a search for essences was legitimate. Mechanics carried physical meanings deliberately avoided by the French. Because of these predispositions *physique-mathématique* seemed to physicists in Britain and Germany to promise solutions to physical problems. Learning how to understand, use, then develop this language for the purposes of exploring nature viewed as a mechanism, meant the exploitation of this language in domains outside of its own self-reference. Learning included misunderstanding then replicating the standards of demonstration in *physique-mathématique.* Syntax, grammar and the method of extracting meaning from the language remained the same. The meanings themselves were changed by the needs of the new mechanical views of nature. The solutions offered by French mathematical methods were finally unsatisfactory in Germany and Britain. Practitioners of physics required explanations rather than descriptions of appearances. Mathematical solutions were given physical meaning.

In other contexts historians have noted the transformation of language from one set of purposes into another.[56] In physics, the purposes of the language changed, from the expansion of the language to the interpretation of nature. The language, the calculus, was redirected outwards from the solution of problems set up within to the description of processes external to itself. The terms of expression were established by physical processes that also set the limits of development of the language. (This in turn opened to mathematicians further opportunities to explore aspects of the calculus that they had previously bypassed.) These borrowings between languages have continued into this century.

From this process of annexation emerged a new language and literature, that of theoretical physics. Plots had changed. External elements, experiments, added sometimes unexpected twists to the narratives and as they developed the meanings of the initial elements could also change. Many of the characteristics of the mathematical scheme of argument were carried over to the physical one. Mathematics could also obfuscate as well as clarify, intentionally at times, and used to claim territory rather than solve problems or answer questions. And as with any language, the issues of precision have to be separated from those of certainty.

Organization of the Text

The process of the transformation of physics is ordered but not completely explained by the annexation of mathematics. Experiment must be considered and how it became enmeshed in this new linguistic net. Some sense of the social opportunities that gave rise to this extraordinary change also needs attention.

The narrative is divided into three parts. In the first, a particularly revealing example, the first solution of the wave equation illustrates eighteenth-century distinctions between the disciplines of mathematics and physics and differentiates the

56 Garber, "Thermodynamics and Meteorology, 1850–1900," *Ann. Sci.* (1976): 51–65. In a more dramatic transformation, Darwin took language "used for hierarchical purpose and expression of dominance" and transformed "the same language and metaphor for purposes of integration rather than dominion." Gillian Beer, *Darwins Plots: Evolutionary Narrative in Darwin, George Eliot and Nineteenth Century Fiction* (London: Routledge and Kegan Paul, 1983).

areas of competence of both disciplines in mechanics. These differences are then traced through other sciences as eighteenth-century meanings of mathematics and physics are explored in their intellectual and social manifestations. In the eighteenth century, the disciplines of science were defined by their methodologies, then by phenomena, and lastly by theoretical elements which were often metaphysical. These two chapters highlight the differences between eighteenth-century physics and mathematics and their modern counterparts. This approach serves to illuminate those characteristics of modern physics that were absent or that needed changing and then fusing together in the nineteenth century to create modern, theoretical physics. For our purposes the eighteenth century closes about 1790.

The second section is devoted to exploring the period between 1790 and 1830. In this era Europe was redefined politically and the pursuit of science followed new patterns. This was especially true in France, or rather Paris, which became even more emphatically the center of European scientific life. In Paris the sciences changed in their social structure, the purposes for which the study of nature were pursued and the practices of research. While the foundations of mathematics were transformed, and the social world of the sciences changed, the boundaries drawn between mathematics and physics still followed methodological lines. These changes had profound effects on disciplines in other societies and on scientific traditions elsewhere that were crucial for the later development of modern physics. Later we draw some conclusions about the "decline" of science in France during the nineteenth century. The narrative also looks at the communities and disciplines of physics and mathematics on the margins in Britain and the German States. This is to assess the boundaries and methods used in these disciplines and the impact of *physique-mathématique* on practitioners within the specific, yet fluid, social contexts.

The final third of the text traces the development of theoretical physics in the universities of the German states and in Britain, 1830 to 1870. There was no one path to theoretical physics, and several versions were in place at the close of this era. It is as well to remember that physicists did not set out to create theoretical physics in our image. We recognized what they accomplished as theoretical physics after the fact.

In an epilogue we trace the changes within theoretical physics from the establishment of the German Empire to the first world war. Initially the disciplines of mathematics and physics drew apart. Some physicists claimed that all the mathematics physicists required was established by 1830. Mathematicians no longer did mathematics of interest to physicists. Face with the indifference of most mathematicians to the mathematics of physics, physicists developed forms of mathematics for their own purposes, i.e., vector algebra and statistical mechanics. We then examine the impact on the relationships between mathematics and physics, mathematicians and physicists, of Felix Klein's long-term campaign to change his own discipline of mathematics. We conclude with a reassessment of the confrontation of Albert Einstein and David Hilbert over the theory of general relativity.

What was meant by mathematics, its standards of proof, and definition of a solution to problems are discussed and contrasted with the standards and solutions of physics during the three periods of this narrative. The dynamic character of mathematics, and its meanings and its narratives, are as much a part of this interpretation of the development of modern physics as are the concepts that give shape to physicists' images of nature.

The method is that of choosing salient examples through which to examine the transformation of experimental philosophy into physics. These range from the wave equation through Fourier analysis and elasticity, to papers in electrostatics, electrodynamics and electromagnetism. Besides keeping the narrative finite in length, limited examples keep the issues of a necessarily complex tale more clearly focussed. This approach also leaves space to include mention of essential social context. The sources are primarily printed, not private, papers. What we are exploring is the public face of mathematics and physics and how a group of practitioners created a new discipline out of its technical inheritance. In particular, it is important to follow the reactions of practitioners to solutions of problems in order to trace the grounds for dissatisfaction and improvement and how these were broadcast within the disciplines. Standards, methods and expectations of the members of those disciplines were communicated through public disputes and accepted solutions. Critiques and the terms of disagreements were crucial in separating disciplines and pinpointing violations of boundaries. All of these activities were done through the public voice in public forums. The private development of an individuals ideas is not an issue, unless letters were exchanged that show with some force disciplinary differences or boundary violations and reactions to those violations. We will find that there were and are important differences between the disciplines of mathematics and physics that altered over time. And, that the incorporation of mathematics into physics was and is historically contingent, dependent on a changing discipline of mathematics and hence on fluid languages of exploration and solution available to physicists to probe and interpret nature.

Part I

Eighteenth-Century Science

Earlier generations pursued their own problems with their own instruments and their own canons of solution.

— Thomas S. Kuhn
The Structure of Scientific Revolutions, p. 141.

Chapter II

Vibrating Strings and Eighteenth-Century Mechanics

Mathematics from Physics

In the late 1740s an acrimonious dispute broke out between Jean le Rond d'Alembert and Leonhard Euler over the solution of the two-dimensional wave equation. The issues were the nature of legitimate mathematical functions, the definition of continuity, and differentiability. A description of this overheated exchange illustrates the relations between mathematics, metaphysics and experimental philosophy during the eighteenth century.[1]

D'Alembert was not the first mathematician to focus on establishing mathematically, then solving, the equation of motion for a string under tension. In examining the problem, d'Alembert exploited the newly developed partial differential equations. Apart from the general physical treatment he needed to establish the equations, d'Alembert's work was an exercise in the development of the calculus, an exploration of solutions to partial differential equations of a certain type. It

1 For details of the mathematical issues involved and the opinions of Euler and d'Alembert see, Ivor Grattan-Guinness, *From the Calculus to Set Theory* (London: Duckworth, 1980), chap. 1, Jerome R. Ravetz, "Vibrating Strings and Arbitrary Functions," in *The Logic of Personal Knowledge*, (Glencoe Il.: Free Press, 1961), 71–88, and Clifford Truesdell, "The Rational Mechanics of Flexible or Elastic Bodies, 1638–1788," in Euler, *Opera Omnia*, series 2, vol. 11, part 2, (Bern: 1957), 240–262, although Truesdell is too partial to Euler. Henk Bos also noted this partiality in H. J. M. Bos, "Mathematics and Rational Mechanics," in *Ferment of Knowledge: Studies in the Historiography of the Eighteenth Century,* G. S. Rousseau and Roy Porter, eds. (Cambridge: Cambridge University press, 1980), 327–355. For d'Alembert see Thomas Hankins, *Jean d'Alembert: Science and the Enlightenment* (Oxford: The Clarendon Press, 1970). Michel Paty, *d'Alembert et son temps: Éléments de biographie* (Strasbourg: Université Louis Pasteur, 1977) characterized d'Alembert's previous work in mechanics more as "a branch of mathematics than as an experimental science," in reference to d'Alembert, *Traité de Mécanique* (Paris, 1743). See also Hankins, "Introduction," to *Traité*. See also, G. F. Wheeler and W. P. Crummet, "The Vibrating String Controversy," *Amer. J. Phys.* 55 (1987): 33–37.

was not the physical expression, in mathematical form, of the motion of a vibrating string. What d'Alembert proposed to demonstrate for a string under tension, using Taylor's conditions, was that "there are infinitely many curves other than the companion of the elongated cycloid [sine curve] that satisfy the problem in question."[2] Starting from Brooke Taylor's work, d'Alembert focussed on Taylor's expression for the accelerating force on an element of the string, $T\beta$, where β was the curvature and T the tension in the string. Using Newton's second law, d'Alembert constructed solutions to expressions for the accelerating force on the string that were equivalent to solving the wave equation,[3]

$$d\,\frac{dy}{dt^2} = \frac{T}{\rho}\,d\,\frac{dy}{ds^2},$$

using eighteenth-century notation. There were obscurities in d'Alembert's argument but this was an attempt to introduce time into the solution to the problem of the motion of a vibrating string. While the physical reasoning was vague, d'Alembert concentrated on the functional solution,[4]

$$y = \psi(t+s) + \Delta(t-s),$$

This expression included the "infinity of curves" d'Alembert promised. He proceeded to demonstrate their existence and explore the mathematical nature of the functions, ψ and Δ and their relationship for a string held taut and fixed at both ends, for which, [5]

$$y = \psi(t+s) + \psi(t-s).$$

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2 Taylor's condition was that the acceleration of the oscillating body was proportional to its distance from its equilibrium position, i.e., simple harmonic motion. Jean le Rond d'Alembert, "Recherches sur la courbe que forme une corde tendue mise en vibration," *Mém. Acad. Sci. Berlin* 3 (1747) [1749]: 214–219, p. 214, and "Suite des recherches sur la courbe que forme une corde tendue, mise en vibration," same journal, 220–249.

3 D'Alembert did not use modern notation but he manipulated his expressions as the partial differential equation,

$$\frac{\partial^2 y}{\partial t^2} = \frac{T}{\rho}\frac{\partial^2 y}{\partial s^2}.$$

See S. S. Demidov, "Création et développement de la théorie des équations différentielles aux dérivées partielles dans les travaux de J. d'Alembert," *Rev. Hist. Sci.* 35 (1982): 3–42 and Steven B. Engelmann, "D'Alembert et les équations au dérivées partielles," *Dix-huit. siècle* 16 (1984): 7–203. Notation remained remarkably fluid until the late nineteenth century. See Florian Cajori, *A History of Mathematical Notations*, (Chicago: Open Court Press, 1929.)

4 For a discussion of d'Alembert's derivation of the equation of motion and its problems, see Hankins, *d'Alembert,* and Ravetz, "Vibrating Strings."

5 D'Alembert obtained this solution through a change of coordinates, not the method of separation of variables. D'Alembert called this "the method of multipliers." For

Initially Leonhard Euler critiqued d'Alembert's assumptions that the vibrations were infinitely small so that the length of the string stayed constant and that, as it moved, the string formed one continuous curve. Simultaneously, Euler was intent on establishing the equation of motion with his own method by examining the balance of forces in an element of string under tension. He then turned to d'Alembert's handling of the initial shape of the curve. For Euler, this shape determined all future motions and subsequent shapes of the curves that formed solutions to the equation of motion. The first motions given the string were continued indefinitely. By restricting this to analytical, i.e., geometrically continuous curves, d'Alembert omitted curves. At this point Euler could not offer a reasonable, general mathematical argument to counter d'Alembert's restriction. He could only restate his assumption that at any time, "the state of vibrations following depend on those preceding, and are determined through them; reciprocally by the state of those following, one can conclude the disposition of those which preceded." Therefore, if

$$y = f(x + t\sqrt{b}) + f(x - t\sqrt{b}),$$

then, in Euler's general solution, the form of the curve also "will represent the figure given to the cord at the beginning of the motion."[6]

Euler rederived d'Alembert's solution but stated that it was defined only within the interval $0 \leq x \leq \ell$. He then explored the properties of his function f, finding it periodic and odd, and extended the range of his solution to $\ell \leq x \leq 2\ell$, and so on. If the curve was continuous, "eel-like," it would cut the axis at an infinite number of points. One such curve was

$$y = \alpha \sin\frac{\pi x}{\ell} + \beta \sin\frac{2\pi x}{\ell} + \gamma \sin\frac{3\pi x}{\ell} + \cdots$$

where 2ℓ was the length of the cord. However, a trigonometric series was not the most general solution to this partial differential equation since it could not duplicate the initial shape of the plucked string.[7] In this statement Euler was addressing the solution to the wave equation of a third protagonist, Daniel Bernoulli.

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discussions, see Demidov, "Création," J. R. Ravetz, "Vibrating Strings," and Grattan-Guinness, *From Calculus*. D'Alembert was not the first to refer to families of curves rather than a generalized functional solution to a partial differential equation. See Steven B. Engelmann, *Families of Curves and the Origins of Partial Differentiation* (Amsterdam: North-Holland, 1984).

6 Leonhard Euler, "Sur la vibration des cordes," *Mém. Acad. Sci. Berlin* 4 (1748) [1750]: 69–85, trans. from *Nova Acta Eruditorum* (1749): 512–527, reprinted in Euler, *Opera Omnia* series 2, vol. 10: 63–78, p. 64 and p. 72 respectively.

7 See Louise Ahrndt and Robert William Gollard, "Euler's Troublesome Series: An Early Example of the Use of Trigonometric Series," *Hist. Math.* 20 (1993): 54–62, and Victor Katz, "The Calculus of Trigonometric Functions," *Hist. Math.* 14 (1987): 311–324.

D'Alembert's paper stimulated Euler to reconsider his ideas on continuity and on the nature of mathematical functions. For d'Alembert, an analytic representation of the string may not always be possible. If it was possible, this "generating curve" in his terminology was arbitrary, periodic, and odd, which meant that it must represent some real mechanical case. The string was continuous. If it was not analytically representable d'Alembert claimed "the problem was insoluble." To be analyzable all the curves representing the string had to be differentiable.

Euler never questioned the form of the wave equation, only the necessity of limiting the initial, arbitrary form of the curve and hence its final solution to such "eel-like" functions. In this debate Euler explored discontinuous functions. In this case he extended functions beyond their original intervals of validity. However, neither mathematician could definitively demonstrate the validity of their approach.[8] Within the language and concepts available to the calculus in 1750, things were at an impasse. The polemic became a restatement of successful extensions of the notion of the function by Euler and the less successful ones on the part of d'Alembert.

The dispute was complicated by the role d'Alembert was beginning to play in the politics of the Berlin Academy of Sciences. Euler was acting, unpaid head of the Academy, a position in which Frederick II would not confirm him. Frederick the Great's francophilia was a disadvantage the Swiss Euler could never overcome. While Euler's letters to bureaucrats, diplomats, and the politically powerful were duly deferential and contain the leaven of Academy gossip, they are straightforward, if sometimes prejudiced. However, there was no lightness of touch, wit or bite in his comments, virtues which d'Alembert had in abundance. Euler's position in Berlin became more precarious with the König affair. When d'Alembert was approached about becoming the next president of the Berlin Academy, Euler's comments about him became more strident, and he began the negotiations that would lead him back to St. Petersburg. The thought of d'Alembert descending on Berlin, gaining the ear of Frederick the Great and disposing of the Academy at will was too much to bear. From Euler's point of view, even though d'Alembert did not become president, "some other Frenchman would."[9]

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8 See Euler, "Sur la vibration." D'Alembert's reply to Euler's initial critique is in d'Alembert, "Addition au mémoire sur la courbe que forme une corde tendue, mise en vibration," *Mém Acad. Sci. Berlin* 6 (1750): 355–360, where he in turn critiqued Euler's first paper. He reiterated his definition of the necessary geometric continuity for the curves in d'Alembert, "Recherches sur les vibrations des cordes sonores," in *Opuscules Mathématiques* (1761), vol. 1, 1–73. Euler's rejoinder appeared as Euler, "De motu fili flexilis, corpusculis quotcumque onusti,"*Novi Comm. Acad. Sci. Petropolitanae* 9 (1762–63): 215–245.

9 See Euler to Müller, April 1763, in Paul Heinrich von Fuss, comp. *Correspondance mathématique et physique de quelques célèbres géomètres du xviii*[e] *siècle* (New York; Johnson Reprint of 1843 edition, 1968), vol. 2, p. 215. For Euler on d'Alembert, see vol.

The importance of this dispute here is that the arguments were over mathematical questions raised in solving a partial differential equation. The origins of the equation lay in a physical problem, but its solution did not. The focus of d'Alembert's and Euler's attention, the fights over authority, and jostling for position were all in the discipline of mathematics. The social locus of the solution lay within the Berlin Academy and patronage of the Prussian state. From this time on most of d'Alembert's and some of Euler's mathematical energies, apart from occasional skirmishes over the vibrating string problem, were turned to exploiting partial differential equations, the new area of the calculus so vividly illustrated by the vibrating string problem.[10]

Ignoring Physics

D'Alembert, Euler, and later Lagrange continued to argue over the mathematical issues surrounding the wave equation. Unanimously they ignored an alternative vision of the problem and its solution put forward by Daniel Bernoulli. Bernoulli stood apart from the other mathematical members of his clan, and from other mathematicians and physicists of his generation. He also had a long, and complicated relationship with Euler. Bernoulli's work encompassed experiments on vibrating bodies of many kinds from which he teased their equations of motion. In this he was more successful as an experimentalist than as a mathematician. As his experiments developed, they provided him with solutions to the equations of motion of some elastic bodies that were deeply at odds with those of his mathematical peers. Despite these differences, Bernoulli's work on vibrating strings cannot be separated from that of mathematicians concerned with figures of equilibrium and the motion of flexible bodies in general, or from the work on the mathematical analysis of musical instruments.

Early in their mathematical careers Bernoulli and Euler published and corresponded with one another on the catenary problem. They both tried to find the principle analogous to the isoperimetrical solution to the catenary problem for other flexible bodies. Bernoulli suggested that for "elastic lamina," some power of the radius of curvature must be a minimum for the lamina to hang in equilibrium.

2, p. 71. For his style, see also his letters to Müller on candidates for the St. Petersburg Academy, and his letters to the chaplain to Frederick, Prince of Wales in vol. 3.

10 D'Alembert's later mathematical work appeared in nine volumes, d'Alembert, *Opuscules Mathématiques*, (Paris, 1761–1780). See Demidov, "Création," for d'Alembert and partial differential equations. Euler's work on partial differential equations culminated in Euler, *Institutiones Calculi Integralis*, (St. Petersburg, 1770). See Demidov, "The Study of Partial Differential Equations of the First Order in the Eighteenth and Nineteenth Century," *Arch. Hist. Exact Sci.* 27 (1982): 325–350. A short account of the history of partial differential equations is in J. Lützen, "Partial Differential Equations," in *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences*, Grattan-Guinness, ed. (London: Routledge, 1994), 2 vols., vol. 1: 452–469.

The discussion led him to propose that the *vis viva* of the body must be a minimum. Not being able to demonstrate this, Bernoulli asked Euler for help.[11]

From his experiments Bernoulli had already deduced the oscillations of a vertically hung chain. Through a geometrical analysis of their oscillating forms, he reached the idea of simple modes and proper frequencies.[12] His first published account of this work, a mathematical paper, contained a number of theorems on the oscillations of bodies hung along a horizontal, flexible thread. He assumed Taylor's condition and expressed his results in the form of the length of a simple pendulum oscillating with the same period as the body under examination. Taking two bodies hung on a string, Bernoulli illustrated two possible types of motion. This expanded to three with three bodies suspended along the string. He then leaped from this case to a heavy chain, arguing that the chain moved in an infinity of ways to execute the vibrations he observed in his experiments.

These and his other "theorems" were assertions, without proofs, along with "Scholia" that were paragraphs on his experiments that illustrated his assertions. This method of argument was a less rigorous form of Newton's techniques in the *Opticks*. For the chain, Bernoulli characterized each motion by the number of points of intersection of the chain with the vertical during its motion when the excursion of the free end is at its greatest. He also drew an analogy between this motion and that of a taut string, concluding that "experiment shows that in musical strings there are intersections similar to those of vibrating chains."

Bernoulli's theorems were geometrical descriptions derived directly from experimental data, although they were written as if they were affirmations of theoretical results. In his second paper Bernoulli used these geometrical results and applied them to the case of two, then three bodies. He assumed the system was constrained and used the balance of forces to find their accelerations. From this and Taylor's condition, Bernoulli deduced their displacement from the vertical. At any point along the string, measured from the top down, the angle of contact was d^2y/ds^2, where y was the horizontal displacement. The accelerating force became

$$\int d\frac{dy}{ds} - (\ell - s)d\frac{dy}{ds^2}$$

which was set proportional to y. Changing variables from s to $x = \ell - s$, where ℓ was the length of the string, Bernoulli obtained a linear differential equation

$$n\ dy\, dn + nx\ d\,dy = -y\ d\,dx,$$

11 This discussion began in correspondence after Daniel Bernoulli left St. Petersburg and returned to Basel. See Fuss, *Correspondance,* vol. 2 and Eneström, "Der Briefwechsel zwischen Leonhard Euler and Daniel Bernoulli," *Bibliotecha Mathematica*, series 3 (1906–1907): 126–156.

12 Daniel Bernoulli, "Theoremata de oscillationibus corporum filo flexili connexorum et catenae verticaliter suspensae," *Comm. Acad. Sci. Imp. Petropolitanae*, 6 (1732–33): 108–122, and, "Demonstrationes theorematum suorum de oscillationibus corporum filo flexili connexorum et catenae verticaliter suspensae," same journal, 7 (1740): 162–173.

where n was a proportionality constant. This was the form of the curve. There was no sense in his solution that the shape of the curve changed in time.

Even before the publication of Bernoulli's second paper, Euler became interested in the problem and analyzed the case of a flexible, loaded string in the same way. He reiterated Bernoulli's results and mathematically went beyond them.[13]

Thus far, neither Bernoulli nor Euler caught the dynamical character of the phenomenon. Although Euler's mathematical vision was clearer, Bernoulli understood that the principle of proper modes applied to all vibrating systems. He was already concentrating on experiments with musical instruments. By September 1736 he claimed that, "I have quit mathematics almost completely, and if it was not demanded by my relationship with the Academy [St. Petersburg] I would abstain completely." Even allowing for hyperbole, this reflects the direction of his work. In his letters to Euler he discussed experiments and mathematical expressions that emerged from them rather than purely mathematical issues. In the eighteenth-century experimentalists, such as Bernoulli and their results established the expressions from which emerged algebraic relationships that mathematicians then manipulated into "equations of motion." These equations of motion offered new opportunities to explore and extend the calculus.[14]

Bernoulli continued to report his own mathematical findings on the oscillations of strings, while Euler attacked the problem of elastic bodies through their equations of motion. At each point Bernoulli asked Euler's opinions of his ideas and Euler's solutions to problems of mutual interest. Initially, the tone of the letters was informal and an easy continuation of their relationship in St. Petersburg. Gossip was in French, with German as the main language that merged into Latin for technical discussions. However, their interaction was more complex than one of long-distance cooperation. Both were intensely ambitious and rivals for the same prizes, literally and figuratively. The correspondence contained statements of problems and results, no methods. The latter were matters for publication.[15]

The increasingly acrimonious rivalry between Daniel Bernoulli and his father

13 Euler, "De oscillationibus fili flexilis quotcumque pondusculis onusti," *Comm. Acad. Sci. Imp. Petropolitanae*, 8 (1736): 30–47. Details of Euler's derivation are in Truesdell, "Rational Mechanics," 162–165, and H. F. K. Burckhardt, "Entwicklungen nach oscillirenden Functionen und Integration der differential Gleichungen der mathematischen Physik," *Jahresber. dtsch. Math. Verein,* 10 pt. 2 (1901–1908).

14 Bernoulli to Euler, September 1736, in Fuss, *Correspondance,* vol. 2 p. 434. See also Bernoulli to Euler, March 1739, Fuss, vol. 2, p. 456. In his November 1740 letter to Euler Bernoulli discussed the fundamental frequencies of pipes of different lengths, Fuss, vol. 2, p. 465. O. B. Sheynin also argues that Euler treated physical problems as purely mathematical ones. See Sheynin, "Euler's Treatment of Observations," *Arch. Hist. Exact Sci.* 9 (1972): 45–56.

15 This correspondence is important for establishing directions of research and when each participant actually reached certain results. Publication of papers could be delayed up to a decade depending on the sponsoring Academy. Their order of publication does not

Johann complicated matters further, especially after Daniel learned through Euler of some his father's hydrodynamic work. Father and son sought Euler's support in their dealings with one another and Euler tried to treat them evenhandedly. Scientifically, they were both useful to him. Although Daniel Bernoulli's treatise on hydrodynamics had appeared years earlier, Euler sent his opinions on Daniel's *Hydrodynamica* to him within days of doing the same for Johann on his *Hydraulica*.[16] Daniel Bernoulli respected Euler and usually accepted his mathematical opinions without demur. However, he considered Euler's work "abstract." It did not describe the real world. And, over the theory of the oscillations of vibrating systems and musical instruments, there was a profound rupture between the two men.

Much of the Euler–Daniel Bernoulli correspondence was on the mutually interesting problem of the vibrations of "elastic lamina" fixed horizontally at one end. Bernoulli asked Euler whether the *vis viva* for elastic lamina must be a minimum or would the body move spontaneously? Euler replied that such curves must follow an extremum principle but what he wanted from Bernoulli was how to determine "the quantity of potential forces that lie in the bendings" of the lamina.[17] It is clear here, and elsewhere, that the two depended on one another, Bernoulli on Euler for guidance with his mathematics, Euler on Bernoulli for his understanding of the physical phenomenon from which he began his mathematical explorations. However, the two together did not produce a mechanical understanding of the vibrations of "lamina," in which physical theory, expressed in the language of mathematics, was joined to experiment, and where the direction and depth of the mathematical development was bounded by the needs of the physical imagery. As their work developed they saw each other as rivals and stood more and more in opposition to each other. They offered alternative solutions, each of which excluded the other.

By 1740 Bernoulli experimentally had distinguished the simple motions of

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reflect the development of the research.

16 In his treatise on hydrodynamics, *Hydraulica*, Johann Bernoulli claimed that his son's hydrodynamics was based on his idea of *vis viva*. He also claimed to have composed his treatise before Daniel published the *Hydrodynamica*. Daniel Bernoulli on his part complained bitterly to Euler that his father's mathematics was based on his unacknowledged physical work. Indeed where Johann Bernoulli needed physical intuition, he argued incorrectly. See Daniel Bernoulli, *Hydrodynamica* and Johann Bernoulli, *Hydraulica*, trans. Thomas Carmody and Helmut Kobus (New York: Dover, 1968). Johann Bernoulli's comments on the shortcomings of his son's work are in his letter to Euler, March 1739, that included the first part of his own treatise. In his letter accompanying the second part, August 31, 1740 he again discussed the superiority of his methods, and in September 1741 continued his litany of complaint. See Fuss, *Correspondance*, vol. 2, 19–20, p. 43, and p. 73. Daniel Bernoulli's letters to Euler on this issues are, November 1740, September 1741, September 1743, in Fuss *Correspondance*, vol. 2, p. 463, p. 475, and p. 530.

17 Bernoulli to Euler, 7 March 1739, Fuss, *Correspondance*, vol. 2, p. 457 and Euler to Bernoulli 5 May, 1739, p. 459. See also Eneström, "Der Briefwechsel."

strings (fundamentals or overtones) from motions made up of "irregular vibrations." Only the former lasted any time, the latter dying away as the string "soon composes itself to the curvature necessary for isochronous motion."[18] The following year Bernoulli connected the sounds emitted by elastic lamina to their isochronous motions. Isochronous motions were connected with clear sounds that lasted, irregular motions with obscure sounds that die away quickly, sometimes to "clear, free sound." By this date he also noted:

> It is extraordinary that elastic lamina give out different tones depending on how they are supported; they have their nodes, upon which they must be supported in order to emit clear tones etc. Otherwise these tones are indeed as the inverse squares of the lengths of the lamina of different lengths and similarly supported. But not only the ratio of the sounds but also the absolute sound may be derived for a lamina of given length, weight, elasticity.

Bernoulli also asked Euler's help with a mathematical problem, to find the "potential *vis viva*" for a lamina bent into a given curve, or the motions that would restore it to equilibrium. The question Bernoulli wanted answered was the curve, such that the lamina had the least "potential *vis viva*." Euler answered this and other mathematical questions, and, because of delays in the publication of the journals of the St. Petersburg Academy his solution, published in Geneva, appeared in print before Bernoulli's statement of the problem.[19]

Bernoulli did not see his own papers of 1741 until after October 1753, by which time he was writing to Euler through an intermediary. The politics that delayed the appearance of the St. Petersburg Academy's journals also led to Euler's departure for Berlin. He thus missed the treatise on hydrodynamics and papers on sound sent by Daniel Bernoulli. At the same time Daniel Bernoulli was trying to deal with his father's upstaging of the same treatise. While stiffly assuring Euler of his continued esteem, Bernoulli complained of the delay in the appearance of his work, and of others (Euler and d'Alembert) that had already appeared in the *Mémoires* of the Berlin Academy. If Euler no longer wanted his work for St. Petersburg, he would send them elsewhere. In addition, Bernoulli had ready a memoir on the vibrating strings problem that would "explain everything difficult or in any way mysterious in this subject, making it in fact very simple." This was a jab at the

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18 Bernoulli, "Commentationes de oscillationibus compositis praesertim iis quae fiunt in corporibus ex filo flexili suspensis," *Comm. Acad. Sci. Imp. Petropolitanae,* 12 (1740): 97–108.

19 Bernoulli to Euler, 28 January, 1741 in Fuss, *Correspondance*, vol. 2 p. 469. Euler published his solution as an appendix in Euler, *Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes* (Geneva: 1744), "Additamentum I De curvis elasticis." For details of Euler's work see Truesdell, "Rational Mechanics," 192–219. Truesdell sees the exchange purely in terms of the mathematics involved and characterizes Bernoulli as "whining" about Euler's mathematical success while slacking off himself.

already available mathematical solutions of Euler and d'Alembert. He was sending this paper to the Berlin Academy.[20]

There were other differences. Euler wanted to deduce the curve of a lamina under load, a static problem, and trace its elasticity until its breaking point. Bernoulli was interested in the motions of laminae, not their equilibrium properties. Mathematically he followed their motions using the balance of moments. The goals of his analysis were ultimately to deduce the form of the curve they traced as they moved isochronously, and hence obtain their frequencies of oscillation and compare them with the frequencies of a simple pendulum. Bernoulli deduced the form of the curve from the differential form,

$$d^4y = \frac{G}{m^4c}\, y\, dx^4,$$

where G was the force on the lamina, c was the amplitude of the oscillations at the free end of the lamina, and m a constant determined by experiment. The y axis ran along the lamina, with displacements in the x direction. Bernoulli gave two solutions to this differential equation, one in series form,

$$\begin{aligned} y = \alpha\Big(1 + \frac{x^4}{4!f^4} + \frac{x^8}{8!f^8} + \cdots\Big) + \beta\Big(\frac{x}{f} + \frac{x^5}{5!f^5} + \frac{x^9}{9!f^9} + \cdots\Big) \\ + \gamma\Big(\frac{xx}{ff} + \frac{x^6}{6!f^6} + \frac{x^{10}}{10!f^{10}} + \cdots\Big) + \delta\Big(\frac{x^3}{f^3} + \frac{x^7}{7!f^7} + \frac{x^{11}}{11!f^{11}} + \cdots\Big) \end{aligned}$$

where α, β, γ and δ were arbitrary constants.

Initial conditions and other limitations on the problem allowed Bernoulli to express this in a form already known from Euler's work,

$$y = ae^{x/f} + be^{x/f} + h \sin \text{arc} \left(\frac{x}{f} + n\right)$$

where $1/f^4 = G/m^4c$. Using special conditions, Bernoulli obtained values for the coefficients in each term and from these deduced the result that the frequency of the oscillations of the lamina varied inversely as the length of the lamina squared. He then compared his mathematical results to the clear tones emitted by lamina

20 The delayed papers were, Bernoulli, "De Vibrationibus et Sono Laminarum Elasticarum, Commentationes Physico-Geometricae," *Comm. Acad. Sci. Imp. Petropolitanae,* 13 (1741) [1751]: 105–120, and "De Sonis Multifariis quos Laminae Elasticae Diversmode edunt disquisitiones Mechanico-Geometricae Experimentis Acusticis Illustratae et Confirmatae," same journal, 167–196. While both Bernoulli and Euler continued to publish in the St. Petersburg journals, Euler was always careful to keep in close touch with the remaining members of the Academy. Bernoulli's comments on the vibrating strings problem are in Bernoulli to Johann III Bernoulli (probably), 7 October 1753, in Fuss *Correspondance*, vol. 2. See also Truesdell, "Rational Mechanics," p. 254.

themselves. That is, he compared his mathematical results to the fundamentals for lamina of various lengths and the frequencies of the emitted tones. Bernoulli recognized that the equation of condition from which his frequencies were deduced had an infinite number of roots each of which corresponded to a different L, where $L = gc/G$, g is the acceleration due to gravity, and L the length of the equivalent, simple pendulum oscillating with the same frequency.[21]

In his second paper Bernoulli explored the implications of these results for the motions of lamina and the sounds they could emit. He concluded that clear sounds coexisted, and their motions never interfered with one another. He claimed to have seen and heard three or four simultaneous sounds and their motions in one lamina. Bernoulli thus expressed the principle of superposition.[22] He understood the physical importance of this result and later called it a new mechanical principle and used it to defend his trigonometric series expression for the motion of the vibrating string. His evidence was experimental, not mathematical. His experiments made his analysis plausible.

Subsequently, Bernoulli reconsidered the simple modes of a weighted string using a static method. The modes were combined to fit arbitrary initial displacements with zero velocities. He succeeded in solving the problem only for two and three masses. From this he extended the analysis to all small, reciprocal motions. He clearly treated his solution for the form of the curve, a trigonometrical series, as including all possible curves. By using arbitrary constants the curve could be made to go through any assigned points. At the same time the equation displayed the isochronous vibrations of the string. In a letter to his brother Johann, Bernoulli first expressed his solution to the equation of motion of the vibrating string as a sine series

$$Y = \sum u_n \sin n\pi \frac{x}{\ell}.$$

In his letter he explicitly asked whether this solution contained all possible "curves." It is clear that Daniel Bernoulli thought so because through the use of arbitrary constants the curves would pass through "any points we please." Here was a combination of a physically determined, mathematically flexible solution. Inelegant, clumsy but in this case it worked.[23]

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21 Bernoulli, "De Vibrationibus."

22 Bernoulli, "De Sonis Multifariis."

23 The series appeared in Bernoulli to Johann II Bernoulli (probably), undated, Fuss, *Correspondance*, vol. 2. 653–655. Fuss dated this letter between 1754 and 1766. Truesdell narrowed the dates to between 1754 and 1755. See Truesdell, "Rational Mechanics," p. 257. Truesdell argues that this letter was meant for Euler. Bernoulli's series appeared in Bernoulli, "Réflexions et éclaircissemens sur les nouvelles vibrations des cordes exposées dans les mémoires de l'Académie de 1747 et 1748," *Nouveaux Mém. Acad. Sci. Berlin* 9 (1753) [1755]: 147–172. In a second paper Bernoulli applied the principle of superposition. Bernoulli, "Sur le mélange de plusieurs espèces de vibrations simples isochrones,

The virtues of Bernoulli's work were experimental and physical, while the mathematical analysis was unsatisfactory. One can sympathize with Euler's neglect of Bernoulli's mathematical work. Bernoulli's papers were usually in the form of theorems, whether the subject matter of the theorem was mathematical or experimental. Proofs might or might not follow. On the theorems he then hung a series of corollaries in which he explored the implications of the original theorem. The scholia were descriptions, tantalizingly brief, of experiments where he demonstrated the results of his theorems and their implications. Bernoulli gave no data or the formulae that transformed that data into the results confirming the theorems. He sketched a general method and the confirmation of the theoretical results. The "geometrical" aspect, the "mathematical proof" of the theorems were in separate, usually later papers and mathematically as incomplete as the descriptions of the experiments.[24] His mathematical abstractions are geometrical descriptions of experimental situations. In all his proofs the concrete particulars of the experiment are clearly before the reader. His mathematical methods are clumsy, limited, and inadequate to the task, although suggestive and physically correct.

His father Johann, brother Nicholaus, and Euler clearly were in a different mathematical league. Time and again Daniel Bernoulli could present physical problems to them in a form that highlighted the mathematical implications of the physical experiments he was currently engaged in. At the same time he would describe his experiments that impinged on the mathematical solutions to the equations of motions that emerged from his earlier experiments that demonstrated the truth of his mechanical ideas. Often he left the mathematics in mid course, as in the case of a vibrating chain, and asked Euler to complete it. However, Bernoulli's usual deference to Euler's authority broke down in the case of vibrating strings.

On his part, in the case of the vibrating string, Euler could not reconcile Bernoulli's physical insights with the promise of d'Alembert's mathematics. Euler tried to retain all the abstract, general, yet physically arbitrary quality of his own and d'Alembert's work, while admitting that it would be embarrassing to be "pulled down by a simple physical consideration." To retain his own mathematical solution, he argued that Bernoulli's solution, in terms of a trigonometrical series, was incomplete. He also indicated that, when the number of terms in Bernoulli's trigonometrical series became infinite, it seemed doubtful that the curve of the string would consist of an infinity of sine waves. Each term clearly was independent of the others. Euler also could not accept a physical limitation to the solution, of what was to him, a mathematical problem. Bernoulli's infinite sine series,

$$y = \alpha \sin\frac{\pi x}{a} + \beta \sin\frac{2\pi x}{a} + \gamma \sin\frac{3\pi x}{a} + \cdots,$$

did not include all the possible curves. In addition, the initial curve of the string,

qui peuvent coexister dans un même système de corps," same journal: 173–195.

24 For example see, Bernoulli, "Demonstrationes theorematum."

as it was plucked, was quite arbitrary and the above equation might never reduce to that initial form.

These remarks followed Bernoulli's papers in the same Academy journal where Bernoulli set out his sine-series solution, his experiments and illustrations of superposition. Bernoulli declared that a series made up of sine curves was the only expression that represented simple, isochronous, regular vibrations of the kind Euler and d'Alembert claimed to establish in their general, mathematical solutions. All vibrating strings, fixed at both ends, must move either in their fundamental mode or in an overtone, of which there were an infinite number, or in a mixture of these motions. As he understood them, Euler's and d'Alembert's solutions were simply made up of mixtures of the type that all conform to the conditions set out by Taylor. However, Bernoulli could not demonstrate these contentions mathematically and could only argue by analogy, using the evidence gleaned from musicians and his own work on the behavior of musical instruments.[25]

While claiming to admire the mathematical abilities of both d'Alembert and Euler, Bernoulli described their analyses as "arbitrary" and apt to surprise rather than to enlighten. They paid no attention to the simple vibrations of cords, or to the actual motions of real cords. The latter die away quickly unless they move as he, Bernoulli, had described. In particular he could not grasp what d'Alembert "intended to say with his infinitely, isochronous vibrations and curvatures," particularly since he was always so abstract and never gave specific examples. While finding in the two mathematicians' papers the "most profound analysis," they appeared to Bernoulli arbitrary, "without synthetic examination of the question, and they have not led to its clarification." The actual vibrations of strings confirmed his trigonometric solution to the problem and by experiment he had confirmed the "mixture of vibrations in one and the same sonorous body which are absolutely independent of one another." He went on to compare this principle with that of the "composition of motion."

For Bernoulli, mathematics had become an adjunct to experiment. In a letter to Euler during this controversy, Bernoulli closed the matter by saying that he "took his proofs from nature, not some principle of analysis." His understanding of the phenomena restricted the possible mathematical solutions of the equation of motion for the vibrating string.

Such restrictions never entered into the mathematical discussions of d'Alembert or of Euler. Bernoulli could not make his assertions about the results of his experiments, the motions of strings, and the sounds of musical instruments mathematically sensible. He could not, therefore, change the terms in which the two

25 Euler's critique of Bernoulli and further remarks on discontinuous functions appear in Euler, "Remarques sur les mémoires précédentes de Mr. Bernoulli," *Nouveaux Mém. Acad. Sci. Berlin* 9 (1753) [1755]: 196–222. The papers to which Euler refers are Bernoulli, "Réflexions," and "Sur le mélange."

mathematicians carried on their debate. They could both safely ignore him. In later papers Bernoulli could only reiterate his belief in his trigonometric series solution, and that it was the general one since it contained "an infinity of arbitrary quantities that make the curve pass through any given points that you wish, and one can identify this curve to one proposed by them to any degree of precision as one wishes."[26]

The different approaches of Euler and Daniel Bernoulli are illustrated further in their exchanges over the science of music. Bernoulli thanked Euler for his treatise on the subject but noted that his mathematical deductions were not recognized by musicians or experimentalists, like himself, and did not represent the harmonies actually in use. He then went on to give Euler a lesson on the actual tuning of musical instruments.[27]

For Euler and d'Alembert, the solution to the problem of the vibrating string revolved around the meanings of the terms "continuity" and "function." D'Alembert required the function representing the curve of the string to be geometrically continuous and analytical in form. Only such a function could be a "general" solution and contain all other possible solutions. Euler's criteria for the function was less rigorous, but mathematically more imaginative. The functions might be continuous only in certain intervals, or might be any arbitrary curve that might not meet d'Alembert's criterion of differentiability.[28] The debate was argued solely in mathematical and metamathematical terms. They both abandoned further physical considerations of the vibrating string as a guide to the solution of the equations of motion. Reference to the physical problem that was the seed bed for this mathematical challenge was abandoned. Daniel Bernoulli's suggestion of using a trigonometric series solution to the wave equation already carried mathematical

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26 Bernoulli, "Lettre de M. Daniel Bernoulli à M. Clairaut au sujet des nouvelles découvertes faites sur les vibrations des cordes tendues," *J. des Sçavans* (March 1758): 157–166, p. 157. See also, Bernoulli, "Mémoire sur les vibrations des cordes d'une épaisseur inégale," *Mém. Acad. Sci. Berlin* 21 (1765): 281–306, p. 283.

27 H. Floris Cohen, *Quantifying Music: The Science of Music in the First Stage of the Scientific Revolution* (Boston MA: Reidel, 1984) notes that Euler's theory of consonance and those of other mathematicians was "largely abstracted from physical and physiological considerations and went back to operations with number," p. 237. There was no physical theory of consonance until the work of Hermann von Helmholtz in the nineteenth century. See also Albert Cohen, *Music in the French Royal Academy of Sciences* (Princeton NJ: Princeton University Press, 1981) and Jamie C. Kassler, "The 'Science' of Music to 1830," *Arch. Int. Hist. Sci.* 30 (1990): 111–135.

28 Historians have traced mathematicians' understandings of functions in Jerome R. Ravetz, "Vibrating Strings," Grattan-Guinness, *Foundation,* A. P. Iushevich, "The Concept of the Function," *Arch. Hist. Exact Sci.* 16 (1976): 37–85, Pierre Dugac, "Des fonctions comme expressions analytiques aux fonctions représentables analytiquement," in *Mathematical Perspectives: Essays on Mathematics and Its Historical Development* Joseph Dauben, ed. (New York: Academic Press, 1981), 13–36.

baggage that, in the context of eighteenth-century mathematics, demonstrated to mathematicians his lack of mathematical sophistication.[29]

Why is this not a simple example of Mathematician meets Experimentalist? The familiar misunderstandings of goals and methods presented here are met in such disputes in the late nineteenth and twentieth centuries. However, in the eighteenth century the space between mathematician and experimentalist was empty. Theoretical physicists did not flourish. There was no audience for the kind of analysis Daniel Bernoulli was suggesting, and no community of researchers sharing the same approaches to problems in experimental physics with his expectations and standards for their solution. His experimental work and results on vibrating strings, lamina, heavy chains, and musical instruments found a ready audience within the discipline of experimental natural philosophy.[30] It was Bernoulli's mathematical deductions that caused critical comments and rebuttals. His mathematical approach to solving the vibrating strings problem found no such ready audience. It drew experimentalists beyond the simple, algebraic or geometrical expressions drawn from experimental results, that is if the experimentalists were even willing to go beyond putting their numerical data in more than tabular form. However, in the 1740s, there were not many doing even that. His solution was unsatisfactory for mathematicians because it fell within the boundaries of the calculus. In the technical terms of that discipline, the solution seemed to limit the very generality of solution mathematicians sought. Bernoulli transgressed disciplinary boundaries trying to open up a middle ground between the two.

Daniel Bernoulli's work is isolated among both experimental philosophers and mathematicians. Mathematicians could safely ignore his remarks on the limited, physically possible, motions of vibrating bodies. D'Alembert and Euler, whatever they quarreled over, and their differences were deep, agreed on methodology. Neither could conceive of the kind of solution that Daniel Bernoulli laid claim to in using his experimental results. Implicit in their comments was a continuity of solutions, and hence a continuous spectrum of mathematically possible vibrations. Bernoulli was squeezed out of this particular dispute. However, almost as quickly, their own solutions were upstaged by the magisterial mathematical solution to the problem of vibrating strings by the young, ambitious mathematician, Joseph Louis Lagrange.[31]

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29 During the eighteenth century there was no general proof for the convergence of infinite series of sines or cosines. Mathematicians sought functional equivalents to such series. See Grattan-Guinness, *Foundations*, Victor Katz, "The Calculus of Trigonometric Functions," *Hist. Math.* 14 (1987): 311–324, and L. A. and R. W. Golland, "Euler's Troublesome Series."

30 See Cohen, *Music in the French Royal Academy*.

31 Joseph Louis Lagrange, "Recherches sur la nature, et la propagation du son," *Misc. Taurin,* 1 (1754): 1–112, and "Nouvelles recherches sur la nature et la propagation du son," same journal, 2 (1760–1761): 11-172. Reprinted in *Oeuvres de Lagrange* (Paris:

Lagrange's work on the problem was the culmination of over a century of mathematical attention to vibrating strings. Historically, however, investigations of vibrating strings cannot be isolated from mathematicians' attention to the motions of elastic bodies in general or the development of the calculus.[32] Lagrange's work encompassed the motions of elastic bodies, one of the mathematically simplest of which was that of the vibrating string. The roots of the modern problem lay in Marin Mersenne's experiments and his deduction from those experiments of the algebraic relationship between frequency of sound emitted by the string, its length, and the tension in the string. Brook Taylor was the first mathematician to deduce Mersenne's expression from the geometry of the string and the mechanics of considering it as a row of rigid particles. Arguing, reasonably, without reference to any specific experiments, Taylor established that when the string's displacement from the equilibrium position was small, the vibrations of the string were isochronous, i.e., the acceleration of the string towards its equilibrium position was as its distance from that position. From this he deduced the "time of vibration," ν,

$$\nu = \frac{1}{2\ell}\sqrt{\frac{T}{\sigma gma}}$$

where 2ℓ was the length of the string, T the tension, and σ a constant. He then constructed the equation of motion for the midpoint of the string, treating it as a simple pendulum of length U. Using fluents and neglecting higher terms, Taylor found the equation for the form of the string,[33]

$$y = U \sin \frac{x}{a}.$$

As well as deducing a form for the curve of the string, Taylor laid down the assumptions for subsequent mathematical derivations. However, there was no search for a curve that changed in time. The derivation was static.[34]

While Taylor remarked that the shape of the curve of the string was a sine curve, he did not use this physical result in any way to restrict his mathematics. Yet

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Gauthier-Villars, 1867), 14 vols., vol. 1, 39–148 and 151–316, respectively.

32 For the role of elasticity in the development of the calculus see, Clifford Truesdell, "The Influence of Elasticity on Analysis," *Bull. Amer. Math. Soc.* 9 (1983): 293–310. This is also implicit in discussions by historians of eighteenth-century mathematics; see, Grattan-Guinness, *From Calculus*, and *Foundations*.

33 Brook Taylor, "De motu nervi tensi," *Phil. Trans. R. Soc. London* 28 (1713): 26–32. See L. Feigenbaum, "Brook Taylor and the method of Increments," *Arch. Hist. Exact Sci.* 34 (1985): 1–140.

34 In 1716 Jakob Hermann (1678–1733) developed an alternative derivation. However, Taylor's was influential in the subsequent mathematical history of the problem. For Hermann, see Truesdell, "Rational Mechanics," p. 86.

significantly, and separately, Taylor apparently performed a number of experiments that agreed with this result.[35] These experiments confirmed his mathematical, not his physical results. Experiments substantiated the results of mathematics, not a physical theory.

Further mathematical development that set the pattern for thinking about vibrating strings came from Johann I Bernoulli. He accepted Taylor's basic conditions for setting up the problem and his assumption of a weightless string hung with mass points. Bernoulli complained that Taylor's derivation was not rigorous enough. He had only considered a finite number of weights and had not taken this number to infinity, that is, he was not careful in considering a continuous string. Bernoulli looked at the force on the kth particle for $k = 1$ to $k = 6$ obtaining the *vis viva* in each case. Redoing the problem from the point of view of statics, Bernoulli generalized to the continuum case to arrive at Mersenne's rule, and recognized the resulting form of the curve as sinusoidal, "an elongated trochoid."[36]

While both Taylor's metaphysics and mathematics were unsatisfactory to Bernoulli, neither could tackle the dynamics of the problem to establish the equation of motion of the string. To do so required that the mathematician handle a differential equation in more than two variables, to be bold enough to create and solve partial differential equations.[37] In addition, some mathematician needed to translate the static analysis of a chord under tension into the dynamics of vibrating motion. That

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35 See, John T. Cannon and Siglia Dostrovsky, *The Evolution of Dynamics: Vibration Theory from 1687 to 1742* (New York: Springer-Verlag, 1981).

36 Johann I Bernoulli, "Theoremata selecta pro conservatione virium vivarum demonstranda et experimenta confirmanda excerpta ex epistolis datis ad filium Danielem," *Comm. Acad. Sci. Imp. Petropolitanae,* 2 (1727): 200–207. Proofs appeared in the following volume in Bernoulli, "Meditationes de chordis vibrantibus, cum pondusculis aequali intervallo a se invicem dissitis, ubi mimirum ex principio virium vivarum quaeritur numerus vibrantionum chordae pro una oscillatione penduli datae longitudinis," same journal, 3 (1728): 13–28.

37 The history of the calculus up to the work of d'Alembert is in Florian Cajori, "The Early History of Partial Differential Equations," *Amer. Math. Monthly* 35 (1928): 459–467. John L. Greenberg, "Mathematical Physics in Eighteenth-Century France," *Isis,* 77 (1986): 59–78 discusses the early hostility then acceptance of the calculus in Paris, and in Greenberg, "Alexis Fontaine's Integration of Ordinary Differential Equations and the Origins of the Calculus of Several Variables," *Ann. Sci.* 39 (1982): 1–36 explores the development of the calculus from Johann I Bernoulli and Maupertuis to Alexis-Claude Clairaut. S. S. Demidov examines the history of partial differential equations in Demidov, "La naissance de la théorie des équations différentielles aux dérivées partielles," *Proceedings of the xiv International Congress of the History of Science* 2 (1974): 111–113 and in Demidov, "The Study of partial differential Equations of the first Order in the Eighteenth and Nineteenth Centuries," *Arch. Hist. Exact Sc.* 26 (1982): 325–350. For the overall development of the calculus see, Carl B. Boyer *A History of the Calculus* (New York: Dover, 1959).

mathematician was d'Alembert.[38] D'Alembert had already exploited partial differential equations in his *Traité de dynamique* and in his essay in the causes of the winds, which captured the prize of the Berlin Academy for 1746. Euler, already in Berlin, had access to this essay but did not see the significance of its mathematical methodology for other areas of mechanics. Neither did Daniel Bernoulli.

Having worked on problems in the motions of elastic bodies, both Euler and Bernoulli were overtaken by d'Alembert's fresh approach in a paper that was significant for their work on a particular problem and for the development of the calculus. Given the interest taken by mathematicians in the problems of elastic bodies, any solution tendered to the vibrating strings problem was bound to lead to disputes. What is of relevance here is that all the issues that emerged and the battles waged were totally within the domain of the discipline of mathematics. Once issues were accepted as belonging to the discipline of mathematics, physical considerations could be and were ignored, to the detriment of Daniel Bernoulli's reputation.

Lagrange's papers cemented the mathematical approaches to solving the vibrating string problem and established the basis for any future work. Physics was firmly excluded. And there for decades, the matter rested. For Lagrange, as for Euler and d'Alembert, sound and the vibrating string was a means of furthering the calculus by solving the wave equation. Lagrange also had to persuade his colleagues that his solution encompassed more than previous ones, and that his methods of arriving at that solution, were more judicious than those of his predecessors. However, Lagrange was more ambitious than this. In his papers on sound, he intended to establish a new foundation for the calculus using Taylor's theorem.[39] The new foundation would allow him, so Lagrange contended, to solve a number of equations previously unsolvable and to extend his work to discontinuous functions.[40] The means to achieve these mathematical goals were the mathematics

38 See d'Alembert, "Recherches sur la courbe," "Suite des Recherches." For how these papers fit into the development of the calculus see Demidov, "Création et développement de la théorie des équations différentielles aux dérivées partielles dans les travaux de J. d'Alembert," *Rev. Hist. Sci.* 35 (1982): 3–42.

39 For details of Lagrange's goals, see Grattan-Guinness, *Foundations*. For Lagrange's continuing interest in the foundations of the calculus see Judith Grabiner, *The Origins of Cauchy's Rigorous Calculus* (Cambridge MA.: MIT Press, 1981). See also Craig Fraser, "Joseph Louis Lagrange's algebraic Vision of the Calculus," *Hist. Math.* 14 (1984): 38–53, "The Calculus as Algebraic Analysis," *Arch. Hist. Exact Sci.* 39 (1989): 317–335, and "Lagrange's Analytical Mathematics," *Studies Hist. Phil. Sci.* 21 (1990): 243–256.

40 Lagrange's early interest in discontinuous functions also emerged in his correspondence to Euler. See Lagrange, *Oeuvres* vol. 14, *Correspondance*, letters to Euler of August 1758 and October 1759. After Lagrange's paper on vibrating strings Euler's work on discontinuous functions appeared in Euler, "De motu vibratorio fili flexilis, corpusculis quotcunque onusti," *Novi Comm. Acad. Sci. Petropolitanae*, 9 (1762–1763): 215–245, "Ëclairissemens sur le mouvement des cordes vibrantes," *Misc. Taurin*, 3 (1762–1763):

of sound and in particular, the mathematics of the motions of vibrating strings. His paper on the subject was thus very complicated as well as discursive. Lagrange located in Turin, was young, and yet to make his mark in mathematics. He had to demonstrate that he understood the history of the problem, the points of view of his predecessors, and their limitations. That is, Lagrange needed to take care of all his footnotes. To make his new approach to the calculus plausible, Lagrange needed to transcend d'Alembert's restrictions on functional solutions to the wave equation and go further than Euler in developing alternative, functional solutions.

Lagrange began, as did his predecessors, by considering a number of weights strung along a weightless string which was then plucked. Starting with the case of one body then two then n bodies and letting $n \rightarrow \infty$, he obtained the wave equation.[41]

With a new method of integration Lagrange reached a general solution which he gave in the form of two integrals of a series of products of sines and cosines. This result allowed Lagrange to reject d'Alembert's solution. His own ideas on functions were less restrictive than d'Alembert's. However, Lagrange was also convinced that series solutions were not legitimate. Therefore, he argued for Euler's approach, on which he expanded at some length. He was also confident that Euler had gone only part of the way in exploring discontinuous functions. He needed to construct a functional solution that would include his series solutions. In the process he hoped to confound Daniel Bernoulli. His ultimate functional solutions in the form of exponentials were reached through exuberant displays of algebraic manipulation.

Both Lagrange and Euler elaborated on their interest in discontinuous functions. In both cases, pulses running along cords was the illustrative example that launched these mathematical explorations.[42] In this relationship both gained a powerful ally. For Lagrange, the support of Euler, the premier mathematician of Europe, was important at the beginning of his career. For Euler, the support from a mathematician of such potential was welcome in his battles against the French in Berlin. Lagrange

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27–59, "Sur le mouvement d'une corde, qui au commencement n'a été ébranlée que dans une partie," *Mém. Acad. Sci. Berlin,* 21 (1765): 307–334, "De chordis vibrationibus disquisito ulterior," *Novi Comm. Acad. Sci. Petropolitanae,* 17 (1772): 381–409, "Consideratio motus plane singularis qui in filo flexili locum nature potest,"*Novi Acta Acad. Sci. Petropolitanae,* 2 (1784): 103–120. See also, Lagrange, "Recherches sur la nature et la propagation du son."

41 Lagrange was not just considering a vibrating string but the propagation of a pulse through an elastic medium. The vibrating string was a simple example of this general problem.

42 Lagrange was already known to Euler through correspondence. In 1754 they began corresponding on new methods of calculating maxima and minima as well as other issues in the calculus. See Craig Fraser, "J. L. Lagrange's Early Contributions to the Principles and Methods of Mechanics," *Arch. Hist. Exact Sci.* 28–29 (1983–84): 197–242. See also, Jean Itard, "Lagrange, Joseph Louis," in *Dict. Sci. Bio.* Charles C. Gillispie, ed. (New York; Scribners, 1971), 14 vols., vol. 7, 559–573.

was a mathematician whose talents Euler recognized early.

In Lagrange's opinion, Daniel Bernoulli's offense was that of solving a mathematical problem "by a kind of induction," but never demonstrating the result rigorously. While Bernoulli's analysis might work for a finite number of bodies strung along a weightless cord, it was insufficient for an infinite number of such bodies, the continuum case of the cord itself.

> The change that this formula undergoes in passing from one case to the other is such that the simple motions which make up the absolute motion of the whole system destroy each other for the most part, and those that remain are so disfigured and altered as to become absolutely unrecognizable.[43]

Although he offered no evidence for such an inference, Bernoulli's theory of superposition, while ingenious and somewhat useful, was "false in the principal case." Lagrange was not the first mathematician to dismiss the principle of superposition. D'Alembert had argued that the secondary oscillations were not truly isochronous with the primary. Bernoulli was wrong.[44]

Bernoulli's standards of demonstration were irrelevant to mathematicians. And, given the contemporary state of understanding of infinite series, Bernoulli had no method of demonstrating, that his trigonometric series would converge, or that it was the most general solution available in terms acceptable to mathematicians. Bernoulli was left with an intuitive argument that, if particles of the string moved sinusoidally, how could we describe their motion other than with the same kind of series.

John Greenberg has documented in great detail a case analogous to that of the vibrating string in the disputes over the shape of the earth.[45] The original mathematical problem arose within Newton's *Principia*. Newton reasoned that measurements taken with a seconds pendulum indicated that the earth was not a sphere. His own ideas on how to tackle this problem were scattered through the theorems and lemmas of the *Principia* and did not constitute a coherent attack on the central mathematical problem. By the time it was of interest to the mathematicians of the Académie des Sciences in Paris, it was also enmeshed in metaphysical issues, forced by Newtonians in the Académie. The Académie mathematicians transformed the problem into a mathematical one through the intermediary of mechanics, considering as had Newton, the equilibrium shape of a rotating mass

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43 Lagrange, "Nouvelles recherches sur la propagation du son," p. 107.

44 Most historians of mathematics see Lagrange's 1759 paper as closing mathematical discussion on the vibrating strings question for some twenty years. See Grattan-Guinness, *Foundations* and Ravetz, "Vibrating Strings," p. 86.

45 John Greenberg, *The Problem of the Earth's Shape from Newton to Clairaut: The Rise of Mathematical Science in Eighteenth-Century Paris and the Fall of "Normal" Science* (New York: Cambridge University Press, 1995).

of fluid. In addition, the struggles of mathematicians to understand and then solve the equations of this problem were materially involved in the development of the calculus in the 1730s and 1740s. Not the least accomplishment was the translation of some of Newton's geometrical proofs in the *Principia* into the language of the continental calculus.[46]

Once mathematicians had translated the problem into their terms, the solution and debates over those solutions stayed within mathematics. Pierre-Louis Moreau de Maupertuis attacked Pierre Bouguer's metaphysics (Cartesian) as well as his mathematics. Clairaut returned to Newton's original work. The case of the shape of the earth differs from that of the vibrating string in that values for the ellipticity of the earth were known. Indeed Maupertuis had led a highly publicized expedition to Lapland to measure the length of the angular unit of length along a meridian in 1735. Before the expedition to Lapland, Maupertuis published articles on how the shape of the earth could be deduced from the rate of increase of the arc of latitude along a meridian. He also published on the implications of the measurements, but these were colored by the polemic over whether nature was Newtonian or Cartesian.[47] These were distinct from his mathematical papers on the shape of the earth. Maupertuis' mathematical work on the problem was considerably less successful than his observations. He defined the mathematical problem as the equilibrium shape of a rotating two-dimensional fluid. His solution was in terms of arbitrary constants that led to a generating curve for two ovals.[48] However, the results of measurement and those of mathematics were hardly compatible. The mathematicians set the observations aside lightly. Maupertuis did not connect this mathematical problem with his astronomical and geodesic measurements.

For Clairaut, also on the Lapland expedition, the problem of the shape of the earth opened a mathematical gold mine. He considered the equilibrium of fluids in general and the equilibrium form of a rotating mass of fluid in particular. Clairaut assumed the equilibrium figure to be an oblate spheroid and examined a series of such nested spheroids of different densities rotating about a common axis. He

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46 On Newton see Greenberg, *Shape of the Earth,* 119–120. On the more general issue of the development of the calculus in this era, see chaps. 7, 8.

47 See Maupertuis, "Sur la figure de la terre et sur les moyens que l'astronomie, et la géographie fournissent pour la déterminer," *Mém. Acad. Sci. Paris* (1733): 153–164, "Sur la figure de la terre," same journal, (1735): 98–105, and *Examen désinteressée des différents ouvrages qui ont été faits pour déterminer la figure de la terre* (Oldenberg, 1738).

48 Maupertuis, *Discours sur les différentes figures des astres* (Paris, 1732), Maupertuis, "De figure quas Fluida rotata induere possant," *Phil. Trans. R. Soc. London* (1733): 240–256, and "Loi du repos des corps," *Mém. Hist. Acad. Sci.* (1740): 170–176. For a detailed discussion of Maupertuis' mathematics, see Greenberg, "Mathematical Physics in Eighteenth-Century France," *Isis,* 77 (1986): 59–78, and *Shape of the Earth*, chap. 5, and chap. 7, 243–258.

also assumed that these mathematical ellipsoids represented the earth.[49] Clairaut worked on the problem over a long period of time and his physical understanding of how to establish his initial equations grew. While in this sense "mechanics and mathematics interacted in his work," he treated the equations after they were established solely as problems in the calculus. At the same time he developed solutions to partial differential equations, opening up more of the possibilities of the calculus.

That the results from the Lapland expedition did not agree with his mathematical deductions did not interfere with Clairaut's assumptions or the exercise in mathematics that this problem presented to him. Clairaut could only compare the two through various technical devices. In his opinion there were inevitably errors of observation which, if properly attended to, would bring the empirical results closer to his mathematical deductions. He seemed unperturbed by the discrepancies.

This hardly seems the reaction of physicists. Maupertuis saw the issue as metaphysical and mathematical. Clairaut soft peddled the metaphysics, yet the problem remained mathematically bounded. Neither tried to find a physical explanation for the discrepancies their mathematical methods had uncovered. Neither wondered how the their physical model, a rotating mass of fluid, could be made plausible as well as mathematically possible. Greenberg sees the problem as physical and Clairaut as solving a physical as well as a mathematical problem. However, he notes that Clairaut's practices do not fit either Kuhn's description of belonging to the mathematical or experimental traditions in physics. Nevertheless, as he demonstrates, the mathematical solutions to the problem have no bearing on its solution as physics. That was indeed observational and he does not follow the observational history of the problem throughout the remainder of the eighteenth century. Neither of the main actors behaved as we might expect from a physicist, if we can impose the standards of twentieth century behavior on eighteenth-century figures.[50] Points of dispute between the contestants here, along with others such as d'Alembert, were mathematical.

Clairaut reverted to similar arguments in his later commentary on Newton's law of gravitation. He criticized Buffon's defense of Newton as phenomenological and dependent on observation. As a mathematician, he was free to explore several laws

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49 Clairaut, *Théorie de la figure de la terre, tirée des principes de l'hydrostatique* (Paris, 1743). See also, Greenberg, "Breaking a "Vicious Circle." Unscrambling A.-C. Clairaut's Iterative Method of 1743," *Hist. Math.* 15 (1988): 228–239 and *Shape of the Earth*, chaps. 6 and 9. Greenberg considers Clairaut's place in the development of the calculus in chaps. 7 and 8. It is well to remember that the reduction of the data was not done by Clairaut.

50 Greenberg, *Shape of the Earth*, 626. He likens Clairaut's work to that of Heisenberg and Feynman in the twentieth century, both of whom reinvented aspects of mathematics for their physics. However, the physical implications explicit in the twentieth century examples are absent in Clairaut.

of attraction not open to "physicists." After arguing the impossibility of gravitation as a property of matter, Clairaut introduced reasoning used by other mathematicians later in the century. He accepted Kepler's laws as observationally grounded and from them deduced Newton's law of gravitation, thus using observation both ways to satisfy his own metaphysical position.[51]

Eighteenth-century quarrels over the shape of the earth do not reduce easily to a problem within theoretical physics with the neat outcome of justifying Newtonian gravitational theory. No set of ideas was expressed mathematically, then explored along lines dictated by the limitations of the physical model. In the case of mathematicians, when mathematical results were compared to observational ones, and from our point of view found wanting, mathematical methods were relegitimated and those of the opposing discipline, experimental physics, devalued, to maintain the disciplinary autonomy of mathematics.

Eighteenth-Century Mechanics and the History of Physics

There is, of course much more to the history of mechanics in the eighteenth century than the vibrating string problem and its solution. As metaphysics, mechanics gave both mathematicians and experimental philosophers the principles on which to argue the validity of their solutions to mathematical and experimental problems.

Historians of the calculus agree that, in the eighteenth century, physical problems were the starting point for a great deal of the construction of new differential and partial differential equations.[52] The solution of these equations were the means by which mathematicians developed the calculus.[53]

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51 Philip Chandler, "Clairaut's Critique of Newtonian Attraction: Some Insights into his Philosophy of Science," *Ann. Sci.* 32 (1975): 369–378.

52 As well as Greenberg other historians of mathematics have stated this explicitly such as, Henk Bos, "Mathematics and Rational mechanics," in *Ferment of Knowledge* Rousseau and Porter, eds. 327–355, Grattan-Guinness, *Foundations*, and *From Calculus to Set Theory*, Grabiner, *Origins*, and, Clifford Truesdell, "The Influence of Elasticity on Analysis."

53 This begins early. Newton's fluxions emerged from his mechanics. Questions of mechanics exercised Johann I Bernoulli and other early developers of the calculus. Of the mathematicians mentioned in this chapter see, Euler, *Mechanica sive motus scientia analytica exposita* (St. Petersburg, 1736), 2 vols. The calculus of variation began similarly in the consideration of mechanical problems by Newton, Leibniz, Brook Taylor, Johann I Bernoulli then Daniel Bernoulli amongst others. As a branch of the calculus Euler systematized it in Euler, *Methodus Inveniendi Lineas Curvas Maximi Minimive Proprietate Gaudentes* (St. Petersburg, 1744). For a brief account see Craig Fraser, "Calculus of Variations," *Encyclopedia of Hist. Philos. Math.* vol. 1, 342–350. See also H. H. Goldstine, *A History of the Calculus of Variations* (New York: Springer Verlag, 1980). For the technical developments in mechanics see, A. T. Grigorian, "On the Development of the Variational Principles of Mechanics," *Arch. Int. Hist. Sci.* 18 (1965): 23–35.

Once mathematicians developed a sure grasp of a new extension of the calculus, they would explore it solely as a branch of the calculus; in our case, discontinuous functions.[54] In a short time mathematicians could formulate questions within the calculus without reference to physical problems at all. Most eighteenth-century mathematicians continued to find inspiration in the puzzles set by physical situations and processes while also developing the calculus on more abstract grounds.[55] Both setting up the equations of motion for a broad range of problems in mechanics and then solving them defined much of the activity of eighteenth-century mathematicians. These two activities afforded the mathematician an opportunity to display technical proficiency and the occasion to establish a career or enhance a reputation. However, both steps were subject to technical and metaphysical challenges from other mathematicians. The important characteristic of eighteenth-century mathematics here is that both steps lay in mathematics, once the mathematical terms of the problem were determined.

The principles of mechanics were never fixed during the eighteenth century and became a metaphysical battleground for mathematicians. Mathematical problems derived from real physical situations guaranteed that solutions existed. Experiments, therefore, validated mathematical deductions. Proper derivations of equations of motion and hence their solution depended upon establishing the truth of certain physical principles. However, these metaphysical arguments were necessarily without closure. The metaphysical terms of mechanics became the opportunity for mathematicians to parade alternative conceptual approaches to reaching the same equations of motion, or the terms of their solution. This accounts for much of the discursive nature of many mathematical papers in this period and into the early nineteenth century. Many mathematicians began the problem from scratch, discussing the merits of this or that principle versus some other approach, thus demonstrating to his satisfaction that earlier attempts to arrive at the sought after equations of motion were flawed fundamentally. Metaphysical quarrels could be over whether Newtonian force, conservation of *vis viva*, or various minimum

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54 Both Lagrange and Euler did both. Their work discussed here led them into new channels of mathematics which they then explored as mathematics, not as outcomes of the problems of mechanics. See Fraser, "J. L. Lagrange's Changing Approach to the Foundations of the Calculus of Variations," *Arch. Hist. Exact Sci.* 32 (1985): 151–191. Lagrange, *Mécanique Analytique* (Paris: 1788) was a text on the the calculus and partial differential equations pertinent to mechanics. See also S. B. Engelmann, "Lagrange's Early Contributions to the Theory of First Order Partial Differential Equations, " *Hist. Math.* 7 (1980): 7–23. Daniel Bernoulli's and Euler's work on the oscillations of lamina led Euler to a general framework for the solution of n-th order differential equations. See Demidov, "On the History of the Theory of Linear Differential Equations," *Arch. Hist. Exact Sci.* 28 (1983): 369–387. See also Demidov, "The Study of Partial Differential Equations of the First Order."

55 The first mathematician historians accept as working completely with problems set by mathematics alone without reference to physical problems was Adrien Marie Legendre.

principles were the legitimate starting point for understanding the motions under discussion.

Because the disputes were metaphysical, their terms lay outside of the particular problem under discussion, became bitter, and led nowhere. d'Alembert dismissed the *vis viva* controversy as a war of words. However, they were words conveying meanings beyond their supposed descriptions of physical reality.[56] The idea of the Principle of Least Action was based on characteristics of God's creation, not on any analysis of how bodies move. In Pierre Maupertuis' words, "the quantity of action necessary to cause any changes in nature are the smallest necessary." And Maupertuis proceeded to apply this principle to the whole of animate and inanimate nature.[57] In these metaphysical disputes the physical foundations for establishing the mathematical problem connected the mathematical solution to the real world and on this hinged the fundamental correctness of the solution. Metaphysics mattered.

Once a particular form of an equation of motion was established, mathematicians would reach that same equation despite differences in metaphysics. However, the physical content of these deductions of the equations of motion was a hit and miss affair. Physical arguments might–and at times were–beautifully succinct and clear. Sometimes they were obscure and difficult to follow, other times they were fudged. The main point of the exercise was attainment of the mathematical form of the equations of motion.

The derivation of the equations of motion and their solution were contests over technical issues as well. Confrontations erupted over the path a mathematician took from experiment or observation to the equations of motion, rarely the form of the resulting equations. The next step was to solve them, technically bettering one's predecessors. And here, as we have seen with Euler, d'Alembert, and Lagrange, metamathematical principles could enter into determining the completeness of solutions. In both steps, metaphysical and technical considerations lay within mathematics. No other kinds of considerations were needed in setting up or in solving differential equations of all kinds. Above all, there was no return to the

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56 For d'Alembert on the *vis viva* controversy, see Hankins, "Eighteenth Century Attempts to solve the *vis viva* Controversy," *Isis,* 56 (1965): 281–297, and *d'Alembert.* See also David Papineau, "The Vis Viva Controversy," *Stud. Hist. Phil. Sci.* 8 (1977): 111–142, and J. Morton Briggs, "d'Alembert: Philosophy and Mechanics in the Eighteenth Century," *University of Colorado Stud.* (1964): 38–56. For Euler's metaphysics, see Jean Dhombres, "Les présupposes d'Euler," *Rev. Hist. Sci.* 40 (1987): 179–202, and Stephen Gaukroger, " Euler's Concept of Force," *Brit. J. Hist. Sci.* 15 (1982): 132–154.

57 For the principle of Least Action see Pierre Brunet, *Etude historique sur le principe de la moindre action* (Paris: Hermann et Cie, 1938), p. 6, Philip Jourdain, "The Nature and Validity of the Principle of Least Action," *Monist,* 23 (1913): 277–293. See also A. Kneser, *Das Prinzip der Kleinsten Wirkung von Leibniz bis zur Gegenwart* (Leipzig: Teubner, 1928).

mechanical problem that sparked the whole discussion. In generating their general solutions, mathematicians were not required to interpret the physical implications of any of their intermediate or final mathematical results. For mathematicians the equations of motion were mute on what they contained in terms of how mechanical systems moved. Their solutions contained nothing on the characteristics of physical systems that they chose to explore.

Daniel Bernoulli's appeals to experiment to guide the solution to the problem were simply irrelevant. The annexation of the problem and solution by mathematicians is emphasized by their actions once they grasped the mathematical principles behind linear differential and first order partial differential equations and variational calculus. They reversed the order of the methods of solution and the problems that gave rise to them both in the sense of their primacy and their place in their accounts. Mathematicians produced texts that treated the equations that related to physical problems as derivative of the mathematical techniques these problems had given rise to initially. Lagrange's examination of vibrating strings ended debates on some mathematical aspects of the legitimacy of discontinuous functions. Mathematicians then focussed on discontinuous functions without reference to vibrating strings except as an illustrative example.

Many mathematicians working on so-called physical problems had as their goal was the reduction of physics to mathematics. The most well-known of these was Lagrange, who wanted mechanics to become a new branch of analysis.[58] For d'Alembert, mechanics was a completely deductive mathematical system untainted by any hint of experiment. Euler was far more tolerant of experiment and more attentive to the results of experimentation, but his work on physical, astronomical, and engineering problems was mathematical and deductive.[59]

Euler and other mathematicians used experiment to determine the initial conditions of a problem from which the equations of motion would then be derived. In the example chosen here, the experimental starting point was Mersenne's relationship between frequency of sound, tension in a plucked string, and its length. However, the derivation of the equations of motion were matters of analysis, based on metaphysical principles that were less, rather than more, connected to observations of the actual motions of bodies. The instance in which experiment had the most effect on eighteenth-century mathematicians were the collision experiments of Willem Jakob van 'sGravesande. Yet the mathematicians' discussions of how to express the results of those experiments in mathematical form focussed on the metaphysical issue of the notion of "force" and its mathematical expression.[60]

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58 Lagrange *Mécanique Analytique*, "Introduction."

59 For d'Alembert on the relation of mathematics and mechanics, see T. Hankins, *d'Alembert*, Briggs, "d'Alembert," and Gary Brown, "The Evolution of the Term 'Mixed Mathematics'," *J. Hist. Ideas,* (1991): 81–102, 87–94.

60 For Leonard Euler see Truesdell, "The Rational Mechanics," and, "Euler's Contribution

Abstract physical problems of bodies in motion, at rest, or in collision, were not the only launch pad into the new mathematics. More practical problems of fortifications, annuities, the probability of events being accidental versus the result of imminent causes, the actual paths of the planets, the shape of the earth, were all used to display mathematical ingenuity and to extend the calculus. How much understanding of the behavior of real bodies in flight, at rest, in collision, or the construction of annuity tables, or of empherides came from all this mathematical activity is another matter. This was not a question of striking the correct balance between mathematical abstraction and the concrete needs of a solution designed to examine real conditions. Solutions to the equations were mathematically the most general possible the particular mathematician was able to deduce, unhindered by the special conditions presented by the physical assumptions or model used to set up the equations in the first place. Nor, in the case of empherides were mathematicians hindered by the needs of observational astronomers. The quality of those solutions were judged by mathematical not physical criteria or the practical implications of the solutions. All kinds of mathematical devices were used to squeeze out solutions: Changes of variables, the use of arbitrary functions or constants, lopping off series when mathematically convenient, without a hint of how these techniques related to physical processes or imagery. Conspicuously absent were discussions of specific restrictions to solutions that suggested possible experiments. If observations or experiments led to results contrary to mathematical analysis, arguments were presented so that they could be discounted.

The physical problem of the vibrating string is just one example among many in the eighteenth century that lead to a number of conclusions about the disciplines of physics and mathematics. Once a problem was expressed mathematically, such as an algebraic relation between measurable experimental quantities, the problem was lost to physics and experiment. All subsequent investigation of the problem lay within the discipline and competencies of mathematicians. Such research was subject only to the values, methods and tests of mathematicians as to sufficiency and completeness of any solutions. Mathematics was self-sufficient for exploring nature. In the developing calculus lay the answers to physical problems. Input from experimentalists, such as Daniel Bernoulli, indicating how the physical solution to the problem might run were irrelevant. Also, no physical interpretation of the results of the integration process of the equations of motion were necessary. Physical meanings were not offered for mathematical results.

Equally important, these results were read, critiqued and developed as mathe-

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to the Theory of Ships and Mechanics," *Centaurus*, 26 (1982): 323–335, O. B. Sheynin, "Euler's Treatment of Observations." For an account of Willem Jakob van 'sGravesande's experiments see Pierre Brunet, *Les physiciens hollandais et la méthode expérimentale en France au xviiie siècle*, (Paris: Albert Blanchard, 1926), and Edward G. Ruestow, *Physics at Seventeenth and Eighteenth Century Leiden: Philosophy and the New Science in the University* (The Hague: Nijhoff, 1973).

matics, not physics. Thus, a whole series of considerations, tasks and skills taken for granted as physical theory in the late nineteenth and twentieth centuries were absent. They were clearly not part of the technical discourse of the eighteenth or the early nineteenth-century physics.

In the eighteenth century, despite the excitement over electrostatic and other phenomena, mechanics also had a place within experimental physics.[61] In the experimental sciences, mechanics was the focus of some steady attention from Daniel Bernoulli, Ernest Florens, Friederich Chladini, Jean Théophile Desaguliers, van 'sGravesande, James Riccati and George Atwood, among others. However, the purpose of these experimenters and lecturers on mechanics was not to produce physical theory. Of these, Desaguliers, van 'sGravesande, and Atwood intended to reach audiences beyond the community of other experimental practitioners. Desaguliers and later Atwood worked to demonstrate the principles of Newton's mechanics. Desaguliers went further. From demonstrating Newton's principles, he progressed to mechanical devices and hence to manifesting the usefulness of Newton's mechanics.[62]

Atwood was not the first to try and develop a purely experimental approach to mechanics, nor was he unique in Europe in offering university courses on Newton's mechanics featuring experiments demonstrating Newton's laws of motion. His lectures in natural philosophy were well attended and leavened by experiments in mechanics, optics, and electricity. However, there was no indication of linking his experiments to physical theory in the modern sense.[63] Atwood's texts were not mathematical except when analyzing practical problems. His was a mathematics to display, not to explain, nature as were his experiments. With display the experimental and mathematical enterprise ceased. However, he did try to develop a general theory of measurement and mathematics as an adjunct to experiment, not

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61 For the limitations of seeing eighteenth-century mechanics simply as rational mechanics, see Henk Bos, "Mathematics and Rational Mechanics," in *Ferment,* Rousseau and Porter eds., 327–355.

62 On Desaguliers and his imitators, see Larry Stewart, *The Rise of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660–1750* (New York: Cambridge University Press, 1992).

63 See, George Atwood *Treatise on the Rectilinear Motion and Rotation of Bodies* (London: 1784). On Atwood's machines and his teaching see Simon Schaffer, "Machine Philosophy: Demonstration Devices in Georgian Mechanics," *Osiris,* 9 (1993): 157–182, 159–163. Gerard L'E. Turner, "Physical Science at Oxford in the Eighteenth Century," in *The History of the University of Oxford* vol. 5 *The Eighteenth Century,* L. S. Sutherland and L. G. Mitchell, eds. (Oxford: Oxford University Press, 1986), 659–681. On the place of experiment in general see, John Schuster and Graeme Watchins, "Natural Philosophy, Experiment, and Discourse in the Eighteenth Century," in *Experimental Inquiries: Historical, Philosophical, and Sociological Studies of Experiment,* Homer Le Grand ed. (Dordrecht: Kluwer Academic, 1990),

a means of developing theories based in experimental evidence.[64]

George Atwood is used here as an illustrative example of a trend in the teaching of experimental physics towards the end of the eighteenth century. More organized, with an abundance of textbooks to choose from, university lecturers could display the principles of experimental philosophy with drama and great success. The explanations offered for the meanings of these displays were vernacular, metaphysical, and non-mathematical.[65] Others also used demonstration experiments in mechanics in lecture courses in natural philosophy that included other experiments in optics, sound, and electrostatics.[66] These lectures were aimed at general audiences, from undergraduates broadening the liberal foundation of their education, to lectures behind coffee houses or, later in the century, in meeting rooms in provincial towns across Europe. And the setting for such lecture series could shift radically through the century. In the Dutch republic, interest in the natural sciences lay in the universities at the beginning of the century. However, the universities declined in importance against growing memberships in volunteer scientific societies which catered to broader audiences.[67]

Although 'sGravesande established the experimental foundations for the metaphysical and mathematical arguments over collisions, he did not use those results himself to develop a mathematical theory of collisions. His own text on mechanics did not, despite its English title, attempt to mathematize the experimental mechan-

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64 See Simon Schaffer, "Machine Philosophy," for experiment as display.

65 See W. D. Hackmann, "The Relation between Concept and Instrument Design in Eighteenth-Century Experimental Science," *Ann. Sci.* 36 (1979): 205–224.

66 In France such experimental forms of mechanics were more likely to be met in courses designed for engineers. See, Grattan-Guinness, "Varieties of Mechanics by 1800," *Hist. Math.* 17 (1990); 313–338, 321–322, see also, C. C. Gillispie *Lazare Carnot, Savant*: (Princeton NJ: Princeton University Press, 1971).

67 On Holland, see H. A. M. Snelders, "Professors, Amateurs and Learned Societies: The Organization of the Natural Sciences," in *The Dutch Republic in the Eighteenth Century*, M. Jacob and W. W. Mijnhardt, eds. (Ithaca NY: Cornell University Press, 1992), 308–328. For public lecturing in Britain see, *British Journal for the History of Science* March 1995 issue on this topic, Larry Stewart, *The Rise of Public Science* and Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760–1820* (New York: Cambridge University Press, 1992). On London see, A. Q. Morton, "Lectures on Natural Philosophy, 1750–1765: S. C. T. Demainbray (1710–1782) and the 'Inattention' of his Countrymen," *Brit. J. Hist. Sci.* 23 (1990): 411–434 and the economic hazards of a career in public lecturing and on the patrons of such lecture courses, John R. Milburne, "The London Evening Courses of Benjamin Morton and James Ferguson, Eighteenth Century Lecturers on Experimental Philosophy," *Ann. Sci.* 40 (1983): 437–455, and "James Ferguson's Lecture Tour of the English Midlands in 1771," same journal, (1985): 597–415, and Colin Russell, *Science and Social Change in Britain and Europe, 1700-1900* (New York: St. Martin's Press, 1983).

ics within its covers.[68] Mathematics was the means only of displaying the results of experiment more precisely. Mathematized theory is remarkable for its absence rather than its presence in this or any other form.

Interpretations culled by experimentalists from the results of their work were untouched by higher mathematics. The language of explanation was the vernacular. While using the same explanatory terms, force, *vis viva* etc., these explanations could conflict with the constructions of mathematicians using the same terms. Meanings, expressed in the vernacular and explanations ranged from the phenomenological to the metaphysical were only tenuously connected to the results of experiment through various rhetorical devices. Illustrative examples of how a mechanical system might work in situations that through analogy mirrored those of the experiment sufficed as the connection between the minute particles or the various ethers that were the source of mechanical activity and the real bodies of the laboratory. The two levels, one of the imagined world of theoretical entities and the material world of the laboratory were tied together only tenuously. In addition to illustrative example, metaphor and analogy served the function of connecting what later would be bound together by the ties of mathematics. And the density of experimental evidence drawn into this explanatory net could be very thin.

Daniel Bernoulli was rare because he was an experimentalist who could follow through the maze set by the mathematicians and establish, using his continuing work on vibrating bodies, whether or not, that mathematics spoke to the results of his experiments. Most experimentalists could not. Their methods defined a methodological and explanatory space that precluded such mathematics. When used, mathematics was subordinate to the primacy of experiment and vernacular explanation. There was, therefore, an experimental mechanics whose practitioners worked independently of the mathematicians.

Here we have distinguished experiments and experimentalists in mechanics from mechanical philosophy and philosophers which, in the eighteenth century, is well nigh impossible. Metaphysical messages within the interpretations of experiments were to the fore. This was especially true in lectures addressed to students and the socially broader audiences of the experimentalists. Discourse within the world of practitioners was about metaphysical and technical matters. The urgency of the metaphysical truths over which they battled referred to the world beyond that of the practitioners themselves. For some experimental craftsmen, the realm of explanation of their metaphysics stayed within experimental philosophy. For others, the implications of their ideas was far broader and the rightness of their ideas more urgent. As has been argued elsewhere, it was through experimental demonstrations that the new philosophy entered the reconstituted cultures of late seventeenth and

68 Wilhelm Jacob van 'sGravesande, *Mathematical Elements of Natural Philosophy Confirmed by Experiment*, 4th ed. (London: 1731) 2 vols. For 'sGravesande and physics as experiment, see Pierre Brunet, *Les physiciens hollandais*.

early eighteenth century Europe. However, the ideological content of these experiments was not inherent in the metaphysics and experiment until those connections were stated explicitly. Whether these connections were ever made depended on both lecturer and audience. And, such connections led in opposite explanatory directions ranging from the proof to the denial of God's existence as one example. Nature and the natural had functioned before the seventeenth century, and still did, to justify and even constitute a theory of the political, social, or economic order. Whether those interpretations were imposed on nature depended upon the expectations of both audience and speaker.

Therefore, mechanical interpretations aimed at other practitioners or broader audiences could range from descriptive, phenomenological discussions of experiment to mechanical philosophies arranged independently of mathematics or experiment for extra-scientific purposes. Many of these speculative metaphysical essays were written to give voice to God's structure of his universe, to prove God's existence, or to develop a metaphysics without calling on the necessity for a creator. The "systems" ranged from the radical materialism of Julien Offray de la Mettrie, Denis Diderot, or Baron d'Holbach to those of Christian Wolff and the Scottish Common Sense philosophers. There were also the excursions into natural theology by British Newtonians and the continental materialists George Louis Buffon and Albrecht von Haller.[69] Mathematicians could join in as well. Euler's letters on natural philosophy used non-technical language free of mathematics. The little mathematics that did enter, for example, the geometry of optical systems, was descriptive. It was not used in interpretation of the nature of light.[70] In these works on mechanical philosophy, mathematics had no place. A description of the operations of nature may or may not depend on evidence from experiments.

Mathematics could also be put to use for extra scientific purposes, although with less success than that of experimental mechanics.[71] What therefore was meant by

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69 This seems like a mixed bag but these authors fit the criterion of being more concerned with developing a speculative system to meet extra scientific goals than bringing in the details of experimental or observational data. The list could be extended. The literature on such important thinkers for the eighteenth century is immense and in a study of this size they are reduced to the status of name dropping.

70 Leonhard Euler, *Letters on Different Subjects in Natural Philosophy, addressed to a German Princess* 2 vols. (New York: Arno reprint of 1833 edition). This was not a popularization as depicted in Walter D. Wetzels, "Popularization of the New Physics: Euler's *Letters*," *Stud. Voltaire Eighteenth Cent.* 264 (1989): 796–800. Their metaphysical and "antique" character is discussed in John Heilbron, *Electricity in the Seventeenth and Eighteenth Centuries: A Study of Early Modern Physics* (Berkeley, CA: University of California Press, 1979), p. 72–73. See also Casper Hakfoort, *Optics in the Age of Euler: Conceptions of the Nature of Light* (Cambridge: Cambridge University Press, 1995).

71 See Tore Frängsmyr, "Mathematical Philosophy," in *The Quantifying Spirit in the Eighteenth Century*, Frängsmyr, Heilbron and Robin Rider, eds., 27–44.

mechanics depended on the author and his audience. If, as with Euler, the author used mechanics for a variety of purposes, the ideas expressed to one audience are not necessarily traceable in the ideas labeled mechanics addressed to another audience. However much we might try to find unity in this situation, we have to settle for fragmentation.

Despite the range of audiences and purposes to which it was put, mechanics developed within two distinct communities of practitioners; experimentalists and mathematicians. When they addressed other colleagues within their communities, separate sets of criteria were used to define problem choice, methods, language and terms that defined when a solution had been reached. Overlap between these communities was minimal. Both disciplines were self-sufficient. If boundaries were transgressed, the violations were either ignored or invalidated, the violator of borders ridiculed, their work discounted.

This is just one example of the eighteenth-century division of labor within the sciences. We now need to examine the depths of this partition, how it functioned across other areas of experimental philosophy, and its social manifestations. We can then examine the implications of this different disciplinary geography of the study of nature for the development of modern theoretical physics.

Chapter III

Eighteenth–Century Physics and Mathematics: A Reassessment

We now need to examine the plausibility of this demarcation of physics and mathematics in the eighteenth century. Methods indeed defined other disciplinary fields such that there were multiple disciplines of astronomy, celestial mechanics and observational astronomy, as well as mechanics. There was even an attempt to establish a mathematical chemistry. Eighteenth century practitioners narrowed their specialties by the particular phenomena that yielded results to well defined practices. In this way chemists began to differentiate their specialty from the broad field encompassed by observation and experiment. Methods became a means of investigation and an explanation of the phenomena those methods uncovered. Interpretation, especially as natural philosophy, was more speculation than an aspect of the investigation of the structure and functioning of nature. Regarding the disciplines through the lens of a methodological definition, some of the important unique social as well as cognitive characteristics of eighteenth century science begin to coalesce. Histories of the sciences written in the eighteenth century by practitioners in their fields of specialization confirm the divisions of the sciences by method, as does the social structure of major scientific societies and the content divisions of their journals.

This methodological understanding of the sciences in the eighteenth century changes the search for the social and intellectual origins of modern physics and propels it definitively into the nineteenth century.

Physics as Experimental Philosophy

In the eighteenth century physics encompassed much more than experimental mechanics. Physics as a term had two definitions. In its broader meaning Physics encompassed every discipline from observational astronomy to chemistry and the natural sciences to physiology.[1] Physics was the whole domain of research and natural knowledge gained through experiment and observation. Experimental philosophy, or physics, included, yet was distinguished from, physics "narrowly" defined as the experimental investigation of light, sound, electricity, magnetism and mechanics. This narrower definition of physics did not confine the investigator to the twentieth-century meaning of these terms. Research in light, for example, could include the anatomy of the eye and the perception of color. For Johann Heinrich Lambert, the study of light included the physiology of the eye and the psychology of vision. The study of sound included music, the structure of the ear, and the perception of sound.[2] Electricity was investigated in all its manifestations, atmospheric and physiological, as well as those effects produced in the laboratory. Henry Cavendish's research into electricity also included the investigation of electric eels and "fishes."[3] The study of lightning was central to the developing understanding of quantity of electricity and then of potential in the middle of the century. Luigi Galvani's focus on the reactions of frogs legs to atmospheric electricity was not unique.[4]

These interests and their place in physics are confirmed by later work in the nineteenth and early twentieth centuries. As uncovered by Susan Cannon, the study of the earth became an organizing principle for some physicists in the 1830s. This movement is a continuation of the earlier, broader understanding of physics. Hermann von Helmholtz's work on sound, accepted by physicists, was encompassed within this broader sense of physics. In the Cavendish Laboratory, under J. J. Thom-

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1 See Chapter I, note 25 for the changes in meaning of the term Physics in the seventeenth and early eighteenth centuries.

2 Johann Heinrich Lambert, *Photometria,* 3 vols., E. Anding, trans. in Ostwald's Klassiker series (Leipzig: 1892. See also the work of Thomas Young. See Chapter VI for more details on these aspects of physics in the late eighteenth century.

3 Henry Cavendish, "Some Attempts to imitate the Effects of the Torpedo by Electricity," *Phil. Trans. R. Soc. London,* 66 (1776): 196–225, in *Electrical Researches of Hon. Henry Cavendish* James Clerk Maxwell, ed. (Cambridge: Cambridge University Press, 1879), 194–215.

4 John Heilbron in his important study, *Electricity,* constrains this narrower meaning of the term physics by including much of the study of meteorological, and physiological electrical phenomena and electricity in animals and fish only as they were brought into the laboratory and were necessary to narrate the conceptual developments in the field. His physics is too modern. Luigi Galvani and his work on frogs appears from a narrative vacuum. This has been noted also by Robert Palter, "Some Impressions of Recent Work in Eighteenth-Century Science," *Hist. Stud. Phys. Sci.* 19 (1989): 349–401. As an antidote see, Marcello Pera, *The Ambiguous Frog: The Galvani-Volta Controversy on Animal Electricity,* trans. Jonathan Mandelbaum (Princeton NJ: Princeton University Press, 1992) and Giuliano Pancaldi, "Electricity and Life: Volta's Path to the Battery," *Hist. Stud. Phys. Biol. Sci.* 21 (1990): 123–160.

son and even Ernest Rutherford, work on meteorological phenomena continued.[5]

Experiment was open-ended, and it was in a constant process of development both as an investigative procedure and an explanatory form.[6] Practitioners assumed that experiments demonstrated the workings of nature. This is visible in the structure of Newton's *Opticks*, his geometrical treatise on light, where experiments replaced mathematical demonstrations as the theorems demonstrating the nature of light. Questions asked of nature, answered through experiment, also led to the improvement of experiment as a "language" for the description of nature. In the eighteenth century the most dramatic of these improvements was the development of quantitative experiments. Less dramatic but of the same kind were improvements in the sensitivity of instruments and the power of equipment.[7] As experiment became an explanatory form, practitioners constantly criticized and changed techniques, standards of observation, criteria for differentiating phenomena from ephemera. There were also experimental philosophers bent on improving their instrumentation and equipment, the language of experimental philosophy, without performing any experiments. The fascination with equipment and instrumentation took over from addressing nature.[8] There were parallels to this later in the century when mathematicians improved and developed the language of the calculus without reference to problems external to the calculus.

As experiment became quantified, practitioners used mathematics to reduce data and assess results. However, the quantification of experiment and the expression of results in tabular form or even compressed into a general algebraic expression did not lead the community of experimentalists to develop of theories expressed in the languages of mathematics.[9] Mathematical expressions were deduced directly from the measurables of the experiments. No theoretical structure developed from

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5 See Susan Faye Cannon, *Science in Culture: The Early Victorian Period* (New York: Science History Publications, 1978), and Peter Galison and Alexi Assmus, "Artificial Clouds, Real Particles," in *The Uses of Experiment: Studies in the Natural Sciences* David Gooding, Trevor Pinch and Simon Schaffer, eds. (Cambridge: Cambridge University Press, 1989), 225–274.

6 The beginnings of experiment in physics as an explanatory form are in R. H. Naylor, "Galileo's Experimental Discourse," in *Uses of Experiment,* Gooding, Pinch and Schaffer, eds. 117–134. In the same volume Willem D. Hackmann, "Scientific Instruments: Models of Brass and Aids to Discovery," 31–65, discusses the explanatory role of experiment and the function of instruments in that role. See also Jan Golinski, "Precision Instruments and the Demonstrative Order of Proof in Lavoisier's Chemistry," in *Osiris* 9 (1994): 30–47.

7 For the case of electrostatics, see Heilbron, *Electricity*, chap. XIX.

8 See Heilbron, *Electricity,* on van Marum's work on electrostatic machine.

9 The urge to quantify in the eighteenth century is examined in *The Quantifying Spirit in the Eighteenth Century* Tore Frängsmyr, John Heilbron and Robin Rider, eds. (Berkeley: University of California Press, 1990).

there.[10] The measurement of a quantity required the clear understanding of how that quantity expressed itself within the experiment.[11] This kind of understanding seems to imply some prior theoretical comprehension of the phenomena or, at least, the instrumentation and methods used in the experiment. However, this understanding did not necessarily depend on any prior theory expressed in even simple algebraic form. Conceptual clarity did not require a mathematized theoretical base. The source of understanding of what was being measured did not depend on speculations about the operation of nature in specific circumstances. Sometimes they sprang from an understanding of instrumentation and measurement methods themselves.[12] In the eighteenth century instruments and experiments were useful in constructing the meanings of theories rather than the reverse.[13]

Experimental demonstrations of Newton's laws of motion did not lead the demonstrator to take his audience through Newton's mathematical theory. That was a matter for mathematicians.[14] This did not preclude speculations about the operation of nature. However, interpretations were in the vernacular. Even as experiments became quantitative, explanations remained qualitative. This is the case even in Ulrich Theodosius Aepinius' work on electrostatics.[15] Aepinius made one of the first attempts to develop a theory of the Leyden jar expressed in mathemat-

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10 For the simplicity of the mathematics used and what experimentalists actually did with it in the eighteenth century see, Theodore S. Feldman, "Applied Mathematics and the Quantification of Experiment: The Example of Barometric Hypsometry," *Hist. Stud. Phy. Sci.* 15 (1985): 127–195, and John Heilbron *Weighing Imponderables and other quantitative Science about 1800* Supplement *Hist. Stud. Phy. Sci.* 24 (1993), chap. 2.

11 See Heilbron, *Electricity* chapter XIX.

12 See W. D. Hackmann, "The Relationship between Concept and Instrument Design in Eighteenth Century Experimental Science." and "Instrumentation in the Theory and Practice of Science: Scientific Instruments as Evidence and as a Aid to Discovery," and Albert van Helden and Thomas L. Hankins, "Introduction: Instruments in the History of Science," *Osiris*, 9 (1994): 1–6. Allan Franklin, *The Neglect of Experiment* (New York: Cambridge University Press, 1986). For a thorough examination of experimental practices see Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987). Heilbron, *Electricity* assumes such prior theoretical structures always exist. This issue touches upon the question of "tacit knowledge," or whether there are other ways of investigating nature that yield rational order but are not subject to the limited methodologies of philosophers. See, Edwin T. Layton, "Mirror Image Twins: the Communities of Science and Technology," *Techn. Cult.* 12 (October 1971): 562–580 on different ways of "knowing."

13 Simon Schaffer, "Natural Philosophy and Public Spectacle in the Eighteenth Century," *Hist. Sci.* 21 (1983): 1-43.

14 See Schaffer, "Machine Philosophy."

15 Heilbron, *Electricity*, has shown the crucial role of electrostatics in the development of the domain of physics. This is the reason for taking it as an example here. From the masses of this material in the eighteenth century, we focus only on those examples usually seen as steps in the development of mathematized, physical theory.

ical form, developing an algebraic expression for the repulsive force of a charged plate on a particle of electric fluid. His mathematics remained largely as algebra and appeared *after* his own vernacular argument explaining the meaning of the phenomena under discussion. The mathematics illustrated his physics. It was not the means for developing his physical argument.

Aepinius' attempt illustrates some of the missing steps that were needed to join theoretical ideas, expressed in mathematical form, and to develop those ideas through the mathematics into a physical theory whose deductions can be joined to physical implication about the processes through which the physical system had passed. In addition, this theoretical state must be related to measurable quantities.[16] He never took the algebraic expression for force, and through an argument expressed and developed by mathematical rules joined the mathematical expression to the physical theory with which he began his mathematical exploration. How he expected to connect his mathematical expression to his experimental results was left moot. The solution of the mathematical problems posed by electrostatics and magnetism occurred after 1800. A physical theory expressed in mathematical form did not appear until the middle of the nineteenth century.

The difficulties of constructing such arguments are also seen in Henry Cavendish's attempt to argue, using geometry and fluxional calculus, that the particles of the fluid of electricity repulsed every other electric particle and those of matter with a force that was the nth power of the inverse of their distance apart, where $n < 3$. His propositions were physical, his theorems geometrical or fluxional. Cavendish took a cone of electric fluid, each particle of which repulsed the other electric particles and acted on a particle at its vertex with a force that was $1/r^n$. He then considered the repulsion on the particle from the fluid up to a plane at a distance from the vertex and that from the fluid beyond the plane. He argued that if $n \geq 3$ the repulsion from beyond the plane was infinitely small compared with that from the plane to the vertex. The mathematics at his disposal, geometry and fluxions were unequal to his task. He could only demonstrate that $n = 2$ for a series of particular cases. He was far more successful in demonstrating his case for a repulsive force of $n = 2$ experimentally.[17]

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16 Ulrich Theodosius Aepinius, *Aepinius' Essay on the Theory of Electricity and Magnetism*, trans. P. J. Connors, Introduction, R. W. Home (Princeton NJ: Princeton University Press, 1979). Home seems uncertain of the value of this essay, stating that it is "the first example of genuinely mathematical physics in which mathematical analysis is united to experiment in a highly fruitful alliance," although he does not investigate the nature of this alliance. At the same time he notes that that Aepinius' theory "as a whole never became more than semiquantitative at best," p. 114.

17 Henry Cavendish, "An Attempt to explain some of the Phaenomena of Electricity by means of an Elastic Fluid," *Phil. Trans. R. Soc. London,* 61 (1771): 584–617, also in *Electrical Researches*, 140–174. Although Cavendish cites Aepinius' work here its is unclear what he derived from it. For his experiment see *Electrical Researches* , 104–

Coulomb's experimental study of electrostatics and magnetism brought into experimental philosophy a new element of precision and systematic measurement using sophisticated instrumentation. Having mastered the phenomenon of torsion in hanging threads, he turned his torsion balance into an instrument to explore the laws of attraction and repulsion in electrostatics and magnetism.[18] Coulomb gave no mathematical deduction of his force law for electricity or magnetism, they were experiential relationships. His conclusion that there was no charge within a hollow conductor was also demonstrated experimentally rather than mathematically.[19] He used mathematics to establish relationships between the moment of the torsion in the suspension system to those of the magnetic or electric forces. Similarly he established ratios between the electricity distributed over electrically charged surfaces of different radii. In both cases the mathematics was not rigorous and the variables were directly measurable quantities.

In Coulomb's work, there were no speculations about the nature of electricity other than the generic "fluid" that flowed, although with no other details. He also chose not to investigate the internal state of matter under stresses or forces. The physical states of matter produced by electricity were not part of his mathematical investigations. This is not to devalue Coulomb's work but merely to illustrate what he accomplished experimentally and mathematically, but not necessarily in physical terms. He reduced the coefficients attached to his unknowns to constants that were measured and hence established through experiment. He also used mathematics to explore possible sources of experimental error.

His experiments were quantitative and concisely yet clearly described and the results tabulated, then expressed in algebraic form. Coulomb also connected the measured quantities to clearly stated problems through a series of theorems. He was systematic in his experimental pursuit of his variables, length of the wire, tension, etc., in his experiments on torsion, each one varied in its turn in a distinct set of experiments. His method was that of the engineer.[20] Speculation was at a

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113. Cavendish's work remained largely unknown until it was too late to affect the development of the field. For a fuller discussion of his theory see Heilbron, *Electricity*, 477–479.

18 This is an example of Hackmann's contention of instrumentation as a source of new physical insight and experiments, rather than theory. See, W. D. Hackmann, "Scientific Instruments," in *The Uses of Experiment,* Gooding, Pinch and Schaffer, eds., 31–65. Richard Sorrenson, "The Ship as Scientific Instrument in the Eighteenth Century," *Osiris* 11 (1996): 221–236, argues that the ship became a scientific instrument in the voyages of discovery of Captain Cooke. He also notes that geographers defined their science through observations and facts, not theories which they disdained.

19 Charles Coulomb, "Sur l'électricité et le magnétisme, quatrième mémoire, où l'on démontre deux principales propriétés du fluide électrique," *Mem. Acad. Sci., Paris* (1786): 67–77, in *Mémoires de Coulomb* A. Potier, ed. (Paris: Société française de physique, 1884–1891) 5 vols., vol. 1, 173–182.

20 For a comprehensive study of Coulomb that links his work in physics with his experience

minimum, systematic exploration at a premium with only just enough mathematics to bring his experiments to fruition. He quantified certain aspects of electrostatics. However, mathematical demonstrations of results were not established rigorously, and were only pursued in the simplest of cases.[21]

In venturing beyond the results of his experiments, Coulomb's deductions were in the form of simple, illustrative examples, taking his assumption of electricity as a fluid literally. He could use a geometric argument to demonstrate that the action of electricity was perpendicular to the surface of a conductor, and that the electric fluid lies within the surface, not throughout the body. While this model is suggestive he took it no further.[22]

Mathematics even in electrostatics, the most systematically developed area of physical experiment, was not used to develop theoretical ideas. In the best example, that of Coulomb, mathematics replaced physical imagery. At the conclusion of his argument, mathematical results and physical imagery were not joined to form any closure. Proofs and demonstrations remained within the domain of experiments. Mathematics expressed the results of these experiments. However, he did not use mathematics to explore the physical implications of his experiments, or anticipate any of their results.

Experiment was the method from which systematic understanding of nature was teased, and then demonstrated to the world.[23] In the cluster of sciences methodologically defined by experiment and observation, new phenomena marked the spread of the methods of experimental philosophy into new areas of experience. The expansion of experiment and observation was epistemologically more important than the introduction of new concepts to interpret their meaning or to reconceptualize an existing field. Theory did not hold a central, defining place within the discipline. Progress was the broadening of the field through the exercise of the method of observation and experiment. The broadening of experimental philosophy into socially useful domains legitimated that method, physics, as knowledge. The later

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as a military engineer see, Stewart Gilmor, *Charles Augustin Coulomb and the Evolution of Physics and Engineering in Eighteenth-Century France* (Princeton: Princeton University Press, 1971).

21 See Coulomb, "Recherches théoriques et expérimentales sur la force de torsion et sur élasticité des fils de métal," *Mem. Acad. Sci., Paris* (1784): 229–269, *Mémoires*, vol. 1, 64–103, and, "Sur la manière dont le fluide électrique se partage entre deux corps conducteurs mis en contact, et de la distribution de ce fluide sur les différentes parties de la surface de ces corps," *Mem. Acad. Sci., Paris* (1787): 421–467, *Mémoires* vol. 1, 183–229.

22 Coulomb, "Sur l'électricité et le magnétisme, quatrième mémoire, où l'on démontre deux principaux propriétés du fluide électrique," *Mém. Acad. Sci., Paris* (1786) [1788]: 67–77. See also Heilbron, *Electricity,* 495–498.

23 See Simon Schaffer, "Machine Philosophy,"and Jane Weiss,"Lecture Demonstrations and the Real World: the Case of Cartwheels," *Brit. J. Hist. Sci.* 28 (1995): 79–90.

boundary between "pure" and "applied" knowledge did not exist.[24]

Explanations of phenomena even within physics were, by later standards, only loosely connected with the experimental base. Eighteenth century experimentalists did not develop theory as an extended argument encompassing known experimental results leading to the prediction of new ones from the body of the theory. Logical connection between presuppositions about nature and results from the laboratory were tenuous at best, and sometimes completely separated one from the other.[25] Analogy, metaphor, and illustrative example were accepted methods for establishing connections between the phenomena uncovered by the practices that defined experimental philosophy and the interpretations of those phenomena. These connections were often suggestive, never rigorous. They also allowed for a multiplicity of particular details of the theoretical elements tied to the same, usually small, empirical base. These can be seen in the range of "theories" of the ether, electricity, and caloric.[26]

No single metaphysical stance united experimentalists, except as small communities of practitioners within a field. Fundamental concepts were contested throughout the era. As metaphysics, natural philosophy served many social functions that went well beyond the needs of experimentalists. The social uses to which these presuppositions were put ranged from theology to economics. The formulations of these metaphysics were pursued before many audiences for a multiplicity of purposes. The utilitarian goals of mechanical philosophy were embedded in some forms of eighteenth century Newtonianism and Common Sense philosophy. The economic utility of these mechanical philosophies emerged in the travelling lecturers' series given throughout Britain. Metaphysically the mechanical philosophy also bolstered aspects of the official ideology of Cambridge university.[27]

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24 See Donald deBeaver, "Textbooks of Natural Philosophy: The Beatification of Technology," in *From Ancient Omens to Statistical Mechanics,* J. L. Berggren and B. R. Goldstein, eds. (Copenhagen: University Library, 1987). This connection has recently been invested with immense epistemological and economic importance in Larry Stewart, *The Rise of Public Science*, chap. 1. See also, Margaret Jacob, *The Cultural Meaning of the Scientific Revolution* (New York: Knopf, 1988).

25 The separateness of "theory" from experiment and "instrumental" levels of discourse is also noted in Hackmann, "Scientific Instruments," in *Uses of Experiment,* Gooding, Pinch, and Schaffer, eds. p. 58.

26 See, Geoffrey Cantor, *Optics After Newton: Theories of Light in Britain and Ireland, 1704–1840* (Manchester: Manchester University Press, 1983), Hakfoort, *Optics in the Age of Euler, Conceptions of Ether: Studies in the History of Ether Theories, 1700–1900* Cantor and M. J. S. Hodge, eds. (Cambridge: Cambridge University Press, 1981), Heilbron, *Electricity*, and Robert Fox, *Caloric Theories of Gases from Lavoisier to Regnault* (Oxford: Clarendon Press, 1971).

27 See chapter II. For Cambridge see John Gascoigne, *Cambridge in the Age of Enlightenment: Science, Religion, and Political Reform from the Restoration to the French Revolution* (Cambridge: Cambridge University Press, 1989).

However, it is simplistic to reduce any ideology as inherent in particular theories of nature.[28]

As historians have become aware of the easy standards of demonstration and the range of available metaphysical choices, the eighteenth century is conceptually no longer Newton's.[29] However, it still might be his methodologically. In the early part of the century Newton's *Opticks* delineated standards of experimentation that demonstrated the operations of nature. His successors in the emerging discipline of experimental philosophy copied his use of experiments as the epistemological equivalent of geometrical demonstration.[30] Even as alternative metaphysical principles emerged, the pattern of Newton's *Opticks* proved a long lived standard. Experimental demonstrations were published along with separate metaphysical speculations.[31]

Histories of physics, especially of physics since Newton, written by experi-

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28 For the case of ideology and Edinburgh, see Steven Shapin, "The Audience for Science in Eighteenth Century Edinburgh," *Hist. Sci.* 12 (1974): 95–121. Modifying this view by noting the care with which utility entered into the social language of the experimental philosopher in Edinburgh, see Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760–1820* (Cambridge: Cambridge University Press, 1992). For broad claims for the mechanical philosophy and the ideological foundations of the industrial revolution, see Margaret Jacob, *The Cultural Meaning of the Scientific Revolution* and Larry Stewart, *The Rise of Public Science.* For a specific example of mechanical demonstration and economic problems, see Jane Weiss, "Lecture Demonstrations and the Real World: The Case of Cart Wheels," *Brit. J. Hist. Sci.* 28 (1995): 79–90.

29 Arnold Thackary *Atoms and Powers* (Cambridge MA: Harvard University press, 1970) liberated chemistry from Newton's grip. See Robert E. Schofield, *Mechanism and Materialism: British Natural Philosophy in the Age of Reason* (Princeton NJ.: Princeton University Press, 1970). On natural philosophy in the eighteenth century, see also, Simon Schaffer, "Natural Philosophy," in Rousseau and Porter, *Ferment*, 55–91. Clifford Truesdell, "Reactions of late Baroque Mechanics to Success, Conjecture, Error and Failure in Newton's *Principia,*" *Texas Quart.* 10 (1967): 238–258, undermines Newton's influence on eighteenth-century mechanics and mathematics, Henry Guerlac, "Newton on the Continent: The Early Reception of his Physical Thought," in *Newton on the Continent,* (Ithaca NY.: Cornell University Press, 1981), 41–73, R. W. Home, "Out of a Newtonian Straitjacket," in *Studies in the Eighteenth Century*, R. F. Baissenden and J. C. Eade, eds. (Canberra: Australian National University Press, 1979), and Colin Russell, *Science and Social Change*, chap. 3 "Alternatives to Newton." Peter H. Reill has argued that the alternatives to Newtonianism attracted a large percentage of Europe's educated elite and was a discourse "within" enlightenment scientific thought not against it. Reill, "Between Mechanism and Hermeticism: Nature and Science in the Late Enlightenment," in *Frühe Neuzeit–Frühe Moderne?* R. Vierhaus, ed. (Göttingen: Vandenhoek and Ruprecht, 1992), 393–421.

30 On the reading of the *Opticks* see, Henry Guerlac, "Early Reception."

31 Although Newton's work on optics was patterned after his mechanics he never succeeded in complete closure. For Newton's attempts to develop a mathematical theory of color, see Alan Shapiro, "Experiment, and Mathematics in Newton's Theory of Color," *Phys.*

mental practitioners made visible the methodological sense of eighteenth-century physics. Johann Karl Fischer's history of physics was an Enlightenment piece on physics from "the restoration of learning to the present time." His concern was in narrating the development of this experimental science. The boundaries of the experiments spilled over into chemistry and other fields. One of his major topics was the development of instrumentation. He gave very short shrift to the hypotheses developed to explain experiments. The phenomena were the core of his discipline and narrative. As the method of experiment and physics expanded, so did that of the phenomena encompassed within physics, Fischer's narrative slowed and became less and less structured. Yet he maintained his discussion with only one major division throughout, the distinction between physics in general (broadly defined) and physics in particular (narrowly defined).[32] The most familiar history in this genre written in the eighteenth century is that of Joseph Priestley on electricity. Although on a more narrowly defined topic his history, like Fischer's, consisted of paraphrases of the work of the authors he described. Priestley's narrative was partly a partisan history, and partly a description of the state of the field.[33] For Priestley, the study of electricity was experiment. The focus of his attention was the instruments that "exhibit the operation of nature." Thus Benjamin Franklin's work was not discussed in terms of his ideas but of his "discoveries" through experiments of "facts." The closest Priestley came to ideas was in his discussion of lightning, but only through analogy. Ideas about electricity were separated from "facts" in the second volume of his chronicle.[34]

The Practice of Mathematics

Experiment was only one of the two methods that reoriented the study of nature during the seventeenth century. The second was mathematics.[35] As in the case of experimental philosophy, mathematics was a language of explanation, developed

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Today, (September 1984): 32–42 and *Fits, Passions and Paroxysms: Physics, Method, and Chemistry and Newton's Theories of Colored Bodies and Fits of Easy Reflection* (Cambridge: Cambridge University Press, 1993).

32 Johann Karl Fischer *Geschichte der Physik seit der Weiderherstellung der Künste und Wissenschaften bis auf neuesten Zeiten* (Göttingen, 1801–1808), 8 vols. For a similar, though condensed account, see F. A. C. Gren, "Geschichte der Naturwissenschaft," *Ann. Phy.* 1 (1799): 167–204.

33 He was also involved in the construction and sale of equipment. See, Simon Schaffer, "The Consuming Flame: Electrical Showmen and Tory Mystics in the World of Goods," in *Consumption and the World of Goods,* John Brewer and Roy Porter, eds. (London: Routledge, 1993), 489–526. See Heilbron, *Electricity,* for an alternative interpretation.

34 Joseph Priestley, *History and Present State of Electricity with Original Experiments* 2 vols. (New York: Johnson Reprint of the third edition, 1966) and Robert Schofield, "Introduction".

35 These were not independent of one another. See Peter Dear, *Discipline and Experience:*

and improved through solutions to the problems posed by nature and addressed through the languages of mathematics. At the same time mathematicians developed a new language of explanation, the calculus. As problems were tackled and successfully solved, techniques and standards of solution changed and expanded the range of this new language. Demonstrable success legitimated the techniques and extensions and reenforced the power of the calculus to display the workings of nature.[36]

For mathematicians, the point of mathematics was to extend the domain of their method into new fields, gaining access to those fields through the consideration of particular problems. Nature both provided the problems that needed solution and guaranteed that solutions existed. As problems were expressed in mathematical language, they were annexed to mathematics and their solutions subject to the techniques and standards of that developing discipline. Disputes arose over the nature of functions and the legitimacy of certain solutions but seldom got further than reiterations of preferences for certain definitions of functions. Importantly, solutions were judged by criteria being developed within the calculus itself; they were mathematical not physical. Experiments in mechanics posed the problems for mathematicians to solve, but gave no clues to their solution. Those solutions lay in the new, and developing, language of the calculus. The domain of mathematics also grew larger as methods were improved through solutions being reworked. The discipline also grew as solutions became more general as well as solutions to different types of differential and partial differential equations succumbed to the technical ingenuity of mathematicians. The invention of methods to accomplish this work of expansion were as important as demonstrating that such solutions existed.

In the course of addressing the problems of mechanics, the metaphysical principles on which the mathematical solutions were based came under close scrutiny and new ones entered the mathematician's repertoire. Discussions on such hypotheses were directed to mathematical ends, not those of producing a physical theory of mechanics. The appearance of a concept within a mathematical context did not signal, necessarily, a growing physical understanding of mechanics. At least these physical insights did not impact the work of the small group of experimentalists interested in mechanics.[37] Using a physical principle to set up an equation of motion

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The Mathematical Way in the Scientific Revolution (Chicago: University of Chicago Press, 1995).

36 The argument here is that mathematics and physics were distinct disciplines, not merely two traditions within physics as in Thomas S. Kuhn, " Mathematical versus Experimental Traditions in the Development of Physical Science," *J. Interdis. Hist.* 7 (1976): 1–31, reprinted in Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 31–65.

37 Perhaps we should also keep in mind that the theoretical fluids of natural philosophy were metaphysical and did not need the mathematical expression of the properties and

might or might not include an understanding of how a physical system actually functioned. The validity of these physical principles was recognized in retrospect in the nineteenth century, or read into the mathematics of the previous century.[38]

The use of minimum principles, especially the principle of least action, in the context of problems in mechanics was the path for the development of variational calculus. If we accept that in the eighteenth century, the principle of least action also was important for understanding the motions and characteristics of physical mechanical systems, we run into evidential difficulties. There is no evidence from that era that variational calculus renewed or changed the physical analysis of the motions of particles or systems of particles using minimum principles. Mathematicians worked on developing variational calculus, using the principle of least action. Mechanics was the source of problems and the vehicle for exploring this new branch of analysis. They did not connect these solutions within a physical context that might relate to issues and problems of interest to experimental physicists. Eighteenth-century experimental physicists did not use these ideas or develop physical theories of mechanics with them. To see eighteenth-century mathematics as solutions to physical problems requires us to read physical interpretations into that mathematics that do not exist in the original. Physicists appreciated and used this mathematical research only after the mid-nineteenth century. By that date a mathematical mechanics became central to the research, then to the training, of physicists.

The mathematical character of eighteenth century mechanics becomes clearer when we look for indications in mathematical results of how they are related to the physical processes the mechanical system supposedly undergoes. There was no return to such physical considerations after the problem was expressed mathematically. No more physical information was extracted by the author from the solution than entered into its initial mathematical expression. The situation in the case of the vibrating string was typical. There was no return to the physics of forces, *vis viva,* or bodies moving in particular ways, and hence images of nature interpreted from the mathematical terms and variables in terms of the vibrations and motions of bodies, free or constrained. With changes in variables, transformations of equations and other mathematical techniques, the terms of the solutions often bore no visible physical relations to those in which the initial problem was set. Eighteenth-century mathematicians' investigations into mechanics did not set experimentalists upon new avenues of research.

Physics might still lie, potentially, even pregnantly in the mathematical forms

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flow of real fluids developed by mathematicians.

38 The conflation of mathematicians' expression of a principle with insight into the physical meaning of that principle or concept is widespread among historians of mechanics and eighteenth-century calculus. For example see, René Dugas, *Histoire de la mécanique* (Paris: Editions Dunod, 1950). Craig Fraser, "J.-L. Lagrange's Early Contributions," and Grattan-Guinness, "The Varieties of Mechanics by 1800."

of the solution. However, it was rarely extracted. For a physical understanding of the vibrations of strings to emerge from the myriad of solutions

$$y = F(x + at) + F(x - at),$$

required additional physical information. Mathematical reasoning was not enough. To channel mathematical thinking about this general solution into physical understanding required consideration of the motions of strings moving in real time. This entailed physical insight or information on the way bodies actually were observed to move. As we have seen, mathematicians dismissed such information as irrelevant to their solutions to this problem.

In ways parallel to the case of mechanics, mathematicians also turned to observational astronomy as a source of problems for the calculus. In doing so they redefined those problems until they fell within the domain of their discipline and the scope of their methods. They used such problems to develop variational calculus, rather than to meet the needs of observational astronomers in the context of current instrumentation and observational methods. The abstract methods and analytical results of Euler, Clairaut, d'Alembert, and Laplace existed separately from those developed by observers. A profound effort was necessary to shape the analysis of mathematicians to the needs of the observational astronomer. This was done in the nineteenth century, literally through the translation of Laplace's celestial mechanics by Nathaniel Bowditch. He made his work "sensible in terms of an observational context through his extensive notes and expansions of Laplace's arguments."[39]

During the eighteenth century the mathematical prize questions put forward by the Academies of Paris, Berlin, and St. Petersburg were indicators of preferred problems. The prize essays themselves sealed the standards of solution.[40] Most of the questions seem directed to the solution of problems we would not consider as mathematical; the tides, the most effective form for an anchor, the problem of longitude. Solutions to these problems and many others required technically sophisticated mathematics and in some cases led to the invention of new domains in the calculus. The papers that were crowned were all mathematical in content and intent. These included Daniel Bernoulli on the anchor and d'Alembert on the winds. None of the papers addressed the practical requirements that we assume as part of the answer. In 1785 Lagrange was the first to pose a mathematical prize question directed specifically to issues within the calculus not couched as a problem with external references to the physical world. Significantly, Lagrange was the first

39 Curtis Wilson, "Perturbations from Lacaille to Delambre: The Rapprochment of Observation and Theory," *Arch. Hist. Exact Sci.* 22 (1980): 53–304. Bowditch's notes and corrections of Laplace's proofs can be seen throughout the volumes.

40 E. Maindron, *Les fondations de prix à l'Académie des Sciences, les lauréats, 1714–1880* (Paris: 1881).

mathematician to systematically develop the calculus to try and connect definitions and results of technical methods together into a defensible whole.[41]

Lagrange's effort to establish the calculus on a logically defensible footing did not change the eighteenth century standard of "a reasonable demonstration" to rigorous proof. The words "it is easy to see that" usually covered a lacuna within a proof or was a standin where demonstration was necessary. The statement following was anything but easily deduced from the previous one. A quick look through Nathaniel Bowditch's translation and his footnotes shows how much Bowditch needed to fill out or amend many of Laplace's proofs in his *Celestial Mechanics*. Laplace's work reflects the standards of late eighteenth century mathematicians. The eighteenth century understanding of mathematical proof parallels experimentalists' ideas on demonstrations as explanation in physics. Mathematicians replaced such standards of demonstration by more rigorous ones in the nineteenth century. These more rigorous standards emerged from mathematicians' considerations of the foundations of the calculus and the development of stricter demonstration as proof and of results deduced from foundations.[42]

Just as experimental philosophers expanded the reach and legitimated their discipline by claiming social utility, mathematicians considered socially pressing questions.[43] We can again point to the prize questions listed above.[44] Mathematics had its theological and philosophical uses. Theological and philosophical arguments were couched in mathematical forms, as axioms, and theorems. Mathematics offered a firm foundation for logically infallible arguments that no longer resided in Aristotelian logic. Whether such philosophical schema developed in mathematical garb were even seriously applied to empirical evidence is unclear.[45]

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41 Grattan-Guinness, "The Emergence of Mathematical Analysis and its Foundations: Progress, 1780–1830," in *From the Calculus to Set Theory,* Grattan-Guinness ed. On Lagrange see, Judith Grabiner, "Changing Attitudes toward Mathematical Rigor: Lagrange and Analysis in the Eighteenth and Nineteenth Centuries," in *Epistemological and Social Problems of the Sciences in the Early Nineteenth Centuries* H. N. Jahnke and M. Otte eds. (Boston: Reidel, 1979), 311–347.

42 Historians of mathematics usually associate the beginnings of this move towards rigor with Augustin Cauchy's reformulation of the calculus in the 1820s.

43 One of the most pressing was that of longitude in which mathematicians and observational astronomers vied for authority over the solution to this problem. It was solved technologically and John Harrison fought to have his solution recognized.

44 For a recent bibliographic survey on the literature in praise of the utility of mathematics in the eighteenth century, see Robin E. Rider, "Bibliographic Afterword," in *Quantifying Spirit,* Frängsmyr, Heilbron and Rider, eds., 381–396, 384–385.

45 For an account of a theology expressed mathematically see, Richard Nash, *John Craig's Mathematical Principles of Christian Theology* (Carbondale IL: Southern Illinois University Press, 1991). This trend had begun earlier, see Rick Kennedy, "The Application of Mathematics to Christian Apologetics in Pascal's *Pensées* and Arnauld's *The Port–Royal Logic*," *Fides et Historia,* 23 (1991): 37–52.

Christian Wolff's popular philosophy extolled mathematics as the model for acquiring knowledge. Wolff also explored the relationship between mathematics and philosophy. His attempts were finally unsuccessful and later demolished by Immanuel Kant.[46] In a more restricted sense of utility, taken to apply only within the sciences, the titles to many eighteenth-century mathematical works would lead us to classify them as astronomical, physical, geophysical, chemical, and technological. However, their results were geared to the expansion of mathematics not to deepening the understanding of physical phenomena, geophysics, chemistry, or engineering. Only in retrospect has the latter content been discerned, and even here, ambiguously.[47]

Eighteenth-century methodological understanding of mathematics also emerged in the historical narratives of eighteenth-century mathematicians. Jean Étienne Montucla's history of mathematics claimed that the difference between ancient and modern physics lay in the use of mathematics. He then annexed optics, acoustics, music, mechanics, and pnuematics for "mixted" mathematics. Montucla then claimed that any problem of "mixted" mathematics could be reduced to one of pure mathematics, and that "the particular physical circumstances are immaterial to its solution." His account of Newton, whom he eulogized as the "sublime" mathematician, was fragmented to suit Montucla's focus on narrating the development of the calculus.[48] We also find that much of his narrative, like that of Priestley's on electricity, was a paraphrase of the research he was describing. His narrative was an introduction to the state of the field and a narration of the paths taken to that state.

Treating Newton as a mathematician and fitting his achievements within mathematicians' standards was common throughout the eighteenth century. While his physical ideas were described as "sublime," and "incomparable" they were as often either passed over without discussion, or, argued against on metaphysical grounds.

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46 See Christian Wolff, *Matematisches Lexicon,* in Wolff *Gesammelte Werke* J. E. Hofmann, ed. (Hildesheim: Georg Olms) Vol. XI, Pt. 1. For a short account of Wolff, his philosophy and reputation, see Tore Frängsmyr, "Mathematical Philosophy," in *Quantifying Spirit,* Frängsmyr, Heilbron, and Rider, eds., 27–44.

47 For example, see Isaac Todhunter, *A History of the Mathematical Theories of Attraction and the Figure of the Earth* 2 vols., (New York: Dover reprint of 1873 edition, 1962) and *A History of Elasticity and the Strength of Materials: From Galileo to Lord Kelvin.* (New York: one volume Dover reprint of the 1886 and 1893 edition, 1960), Edmund Whittaker, *A History of the Theories of Aether and Electricity,* and Stephen P. Timoshenko, *History of Strength of Materials* (New York: Dover reprint of 1953 edition, 1983). All these authors separate the mathematical explorations from theories that were designed to uncover physical processes and to understand physical phenomena.

48 Jean Étienne Montucla, *Histoire des mathématiques* (Paris: 1799–1802) 4 vols., vol. 1, p. 13. Montucla only completed the first edition that took his history to the end of the seventeenth century. Joseph Jerome Le Français de Lalande finished the third and fourth volumes on the eighteenth century.

Detailed discussions were reserved for his methodological heritage. His *Principia* was discussed as mathematics, or as metaphysics, not as theoretical physics.[49] Reactions to Newton's mechanics revolved around differing notions of a satisfactory foundation for the calculus and acceptable solutions to a mathematical, not a physical problem.[50]

In ways that parallel experimental physicists and their treatment of the *Opticks* as paradigmatic for their discipline, mathematicians accepted the *Principia* as mathematics. What we take as the foundations of his physics, his three laws of motion, his concept of force, and the idea of gravitation, were critiqued, accepted, discussed as the work of some predecessor, or discarded for others. Newton's physical problems became the basis for forays into mathematics using various forms of metaphysics to establish the necessary physical imagery to enter into a discussion of the mathematics. The result was always mathematics.

The Intellectual Geography of Physics and Mathematics

The geography of the disciplines in the sciences of the eighteenth was not that of later centuries. Both mathematicians and experimental philosophers shared the goal of increasing the explanatory range of their method by expanding the phenomena covered by the methods they wielded and hence the reach of their disciplines. For practitioners in both disciplines, there was no distinction between "pure" and "applied." The broadening of the investigation of nature to areas of social importance, from theology to industrial production, was regarded as valuable, as notable as research as any esoteric, abstract result of only intellectual interest. The ultilitarian potential of the sciences, moral, social, or material was part of its definition as knowledge, and were important justifications for their pursuit. Chemistry's utility became part of its definition.[51] Joseph Priestley was equally interested in the chemistry of "airs" and in Josiah Wedgwood's problems with glazes and pottery techniques. There were also his own attempts to exploit carbonated water.[52] Members of the Academies of Sciences were involved in solving technical problems

49 For details of the reactions to Newton's *Principia,* see Clifford Truesdell, "Reactions of late Baroque Mechanics." See also, *The Annus Mirabilis of Sir Isaac Newton (1666-1966),* Robert Palter, ed. (Cambridge MA.: MIT Press, 1967), and Guerlac, "Newton on the Continent."

50 See, Domenico Bertoloni Meli *Equivalence and Priority: Newton versus Leibniz* (Oxford: Clarendon Press, 1993) for a discussion of these competing forms of the calculus.

51 See, Arthur Donovan, "British Chemistry and the Concept of Science in the Eighteenth Century," *Albion* 7 (1975): 131–144. See also the career of Joseph Black in *Joseph Black,* A. D. C. Simpson, ed.(Edinburgh: Royal Scottish Museum, 1982).

52 See Robert E. Schofield, *The Lunar Society of Birmingham; A Social History of Provincial Science in Eighteenth Century England* (Oxford: Clarendon Press, 1963). Roger Hahn,

of states. An academician's position in his discipline was unaffected if he was involved in such work. On the contrary, to demonstrate the utility of science was to confirm its place and that of the academician in society.[53]

Eighteenth-century mathematicians devoted too much of their time to addressing utilitarian problems for the latter to be ignored as a minor aspect of their work. Whether their solutions to what we call problems of engineering, observational astronomy, or of experimental electricity were useful to practitioners in those fields is another matter. It does mean that we have to expect eighteenth-century mathematicians to begin their mathematical work in problems that ranged from the very abstract to the mundane. Whether the subject of investigation lay in number theory, geometry, or the characteristics of real, physical bodies, mathematics defined the standards of solution. Mathematicians could then claim authority over future solutions. In this case we can use Michel Foucault's characterization of episteme. In the eighteenth century, language categories reflected the real world. Mathematicians did not apply the calculus to the solution of engineering or physics. Those problems were a necessary attribute of a mathematics that represented a category of the real world. The study of nature was a discourse, within a very constricted and implicitly known space. This discourse created its own landscape, delineated through language in which legitimate discourse could be carried on, and defined the terms in which discourse could occur. Rather there were two parallel discourses and sets of practices, one of experimental philosophy, of experimentalists and observers, the other of mathematics and mathematicians.[54]

In mathematics the demonstration of a proposition or theorem was Mathematics, even when the initial problem arose within the discipline of physics, engineering, or

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"Science and the Arts in France," *Stud. Eight. Cult.* 10 (1981): 77–93. For Swedish science see, Tore Frängsmyr, "Swedish Science in the Eighteenth Century," *Hist. Sci.* 12 (1974): 29–42.

53 See Roger Hahn, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666–1803* (Berkeley CA.: University of California Press, 1965). Hahn notes that no member of the Académie balked at the duties placed on him by the state. See also, Charles C. Gillispie, *Science and Polity at the End of the Old Regime* (Princeton NJ: Princeton University Press, 1981), chap. 4 and Robin Briggs, "The Académie Royale and the Pursuit of Utility," *Past Present,* 131 (1991): 38–88. For the German states see Henry E. Lowood, *Patriotism, Profit, and the Promotion of Science in the German Enlightenment: The Economic and Scientific Societies, 1760–1815* (New York: Garland, 1991). For the Berlin Academy of Science see, Hans Aarsleff, "The Berlin Academy under Frederick the Great," *Hist. Human Sci.* 2 (1989): 193–206; Mary Terrall, "The Culture of Science in Frederick the Great's Berlin," *Hist. Sci.* 28 (1990): 333-364, and Roland S. Calinger, "Frederick the Great and the Berlin Academy," *Ann. Sci.* 24 (1968): 239–249.

54 See Michel Foucault *The Order of Things: The Archaeology of the Human Sciences* (New York: Vintage Books, 1973). Foucault did not use his "archeology" to examine science; that still existed in a separate category of knowledge.

observational astronomy. Closure, translating or relating that solution to abstract yet physical conditions, or trying to reach a mathematical form that might be translated into an experimental situation, was unnecessary. It was not within the terms of the discourse.

We have therefore twin disciplines of astronomy, celestial mechanics and observational astronomy, as well as two disciplines of mechanics, one mathematical and one experimental, two disciplines of hydrodynamics, and so on. Jean Baptiste Joseph Delambre's history of astronomy had no place for celestial mechanics. He merely reported Newton's work but without comment until it related directly to his main concern, a narration of the development of observational methods and the methods to reduce data to tabular form. Delambre's measure of the state of astronomy was the "state of astronomical tables." There was no celebration of Newton as theorist. Delambre had more to say on Clairaut's "inductive interpretation of Newton." Clairaut had proved by observation and induction, no hypotheses required, the inverse square law of attraction.[55] Experiment and observation belonged to a discipline that, in the Eighteenth century was a rival to mathematics. Each mutually excluded the other from its domain of competence. The potent combination of mathematics and experiment developed in the nineteenth century was precluded in the previous century by the self-sufficiency and closure assumed for these separate methodologies. Since mathematics and experiment were viewed as separate sources of truth, they defined their own criteria for explanations and solutions. They covered separate domains of explanation. When both experimental and mathematical methods were brought to bear on the same problem, disputes over intellectual property broke out as disciplinary boundaries were violated, and livelihoods and reputations were at stake.

Disputes were bound to occur over the legitimacy of solutions developed in competing spheres of discourse, especially before the practitioners had settled the terms within their discourse space. As the terms of the discourses were better defined and understood, the competing solutions from the rival methodological discipline could be safely ignored. Disputes usually began and ended over the ontological legitimacy of the methods within the opposing disciplines to solve the problem in question. No accommodation appeared possible in the terms available during the eighteenth century.

Between the two astronomies, one observational, the other mathematical (celestial mechanics), controversies erupted over the rights to possession of the intellectual property invested in certain problems and the proper methods for their

55 Jean Baptiste Joseph Delambre, *Histoire de l'Astronomie au dix-huitième Siècle* (New York: reprint of the Bachelier 1821 edition, 1969), 2 vols., vol. 1, p. 23. Rachel Laudan has drawn attention to the function of histories of the sciences for the practitioners of those sciences. However, she pays more attention to later eras. See Rachel Laudan, "Histories of the Sciences and their Uses: A Review to 1913," *Hist. Sci.* 31 (1993): 1–34.

solution.[56] These intellectual quarrels also involved issues of institutional prerogatives and status, personal reputation and future prospects. One such ongoing battle was between the observational astronomers, the Cassini's, at the Paris Observatory and the géomètres of the Académie des Sciences. The Académie controlled the Observatory's budget, and was responsible to the state for the Observatory. Furthermore its mathematicians were involved in a case of conflict of interest. J. D. Cassini and his successors argued that the paths of the planets could only be determined through observation. Mathematics was a necessary adjunct to observation for the reduction of data to results. Mathematics was not the method to establish, in the sense of demonstration or proof, the true paths of the planets.[57] The hostility between the mathematicians at the Académie and the Cassinis at the Observatory was over their conflicting claims to the solution of the same problem, the paths of the planets. The mathematicians believed that with the problem reduced to mathematical form, its solution was their prerogative. Throughout the century, as the position of the mathematicians within the Académie rose, the physical state of the Observatory declined, as did the quality of its observational work. The institution sprang back after its independence from the Académie.[58]

The Cassini case brings up some important differences between eighteenth century astronomy and that of the last century and a half. Within the latter the assumption is implicit that an observational matrix and mathematical, theoretical net are complementary facets of a common enterprise. This was clearly not the case in the eighteenth century.

Another matter in which we would expect the methods of mathematics to complement those of observation is in the construction of astronomical tables. D'Alembert, Euler and Laplace used astronomy to extend the range of their mathematical methods, not to solve the problems of contemporary observational astronomers. Euler turned his hand to composing lunar tables that he felt would be helpful to the practicing astronomer. They do not appear to have been useful to eighteenth-century observational astronomers. This was also true of the mathematical work of Euler and Clairaut on the design of optical systems. These results might arise from social circumstances, or the lack of understanding of observational astronomers and of higher mathematics. Even in the latter case ob-

56 This division of astronomy into distinct disciplines, mathematical and physical is noted in Rainier Baasner, *Das Lob der Sternkunst: Astronomies in der deutschen Aufklärung* (Göttingen: Vandenhoek and Ruprecht, 1987).

57 For another example of this use of mathematics by an observational astronomer see, Eric Forbes, "Mayer's Contribution to Observational Astronomy," *J. Hist. Astron.* 11 (1980): 28–49.

58 This decline might be partly explained by the positions at the Observatory being treated as a family prerogative. Seymour Chapin, "The Academy of Sciences during the Eighteenth Century: An Astronomical Appraisal," *French Hist. Stud.* 5 (1968): 371–404. Other disputes within the Academy further weakened support for the Observatory.

servational astronomers developed their own mathematical solutions to problems of data analysis.[59]

The limitations of mathematicians' approaches to these problems were many, not the least being the technical problems of treating equations of many variables. The other was that, while mathematicians needed to achieve solutions compatible with known measurements, they were free to arbitrarily adjust constants within their equations and change variables. The changes in variables did not bring with them any need to understand what these changes implied about the motions of heavenly bodies or astronomical processes. Nor did mathematicians have to worry about what changes in constant might imply for observers.[60]

Mathematicians' calculations added nothing useful to solutions practical astronomers needed. Observational astronomers developed their own methods of dealing with data. Even when observational astronomers announced a program including mathematics, the mathematics under consideration was one of immediate use in observational astronomy, nothing more. Such was the mathematics of mathematical cosmography that was of interest to observational astronomers not mathematicians. The goals of the Mathematical Class of the Cosmographical Society of Nuremberg, were consistent with the mathematical needs of observers, not mathematicians. The only contact between the designers, builders and users of the instruments and observational methods with mathematicians was that of Georg Moritz Lowitz with Euler on problems of projection.[61]

Long before eighteenth century astronomical observers needed mathematical methods to deal with the spread in data points, they understood that the average of several readings was better than one carefully collected reading. After experiments became quantitative, such methods also became critical to experimental philosophers. Observational astronomers and experimentalists developed their own traditions of defining, and then analyzing the problem of error independent of mathematicians' explorations of several kinds of error curves. Daniel Bernoulli analyzed the errors of measurements in several papers. In these papers he clearly draws on his own experience. Initially Bernoulli chose to discuss the more difficult

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59 J. C. Deiman, "Optics and Optical Instruments, 1600–1800," in *Mathematical Encyclopedia,* Grattan-Guinness ed., vol. 2, 1158–1164, and Brett D. Steele, "Muskets and Pendulums: Benjamin Robins, Euler and the Ballistics Revolution," *Techn. Cult.* 35 (1994): 348–382, 367. Umberto Bottazzini, "Lagrange et le problème de Kepler," *Rev. Hist. Sci.* 43 (1990): 27–78, sees the same pattern in Lagrange's solution to the anomalies of planets motions.

60 See d'Alembert, *Recherche sur la précession des equinoxes et sur la nutation de l'axe de la terre dans le système newtonien* (Paris: 1749), and Euler, *Lunar Tables*. D'Alembert saw his solution as vindicating Newton's law of gravitation.

61 See Eric G. Forbes, "Mathematical Cosmography," in *Ferment,* Rousseau and Porter eds., 417–448, p. 428, and Forbes, *Tobias Mayer (1723–1762)* (Göttingen: Vandenhoek and Ruprecht, 1980).

problem involving a very finite number of observations over a finite range of values. His problem was to determine the most likely value for a measurement after all systematic errors were removed. The solution lay solely within the theory of probability. Bernoulli was looking for the value with maximum likelihood which he then assumed explicitly to be the true value. His distribution of values lay within a circle whose diameter spanned observations on both sides of the "true" value. If in the series of observations the smallest is A, the second $A + a$, the third $A + b$ and the most probable $A + x$, the highest probability for x was given by taking the derivative of

$$(r^2 - x^2)(r^2 - (x - a)^2)(r^2 - (x^2 - (x - b)^2)...$$

Bernoulli deduced the value for x for $n = 1, 2, 3...$, noting the tedium in working out such calculations, and ended by illustrating his method with numerical examples.[62]

Taking examples of actual results from the available astronomical literature Euler objected to Daniel Bernoulli's assumption that maximum likelihood was the true reading but offered no improvements on his arguments. Indeed Euler's mathematical excursions lead into territory where the error range was in danger of becoming imaginary and he beat a hasty retreat. Euler had other problems. In his example of deducing the shape of the earth from four measurements of the length of an arc along a single meridian, he eliminated unknown parameters from his initial equations. This left him with two equations in two unknowns, the corrections to the lengths of the arc. However, the equations led to more than two solutions, one of which seemed more "reasonable" than the other, although Euler did not indicate why.[63]

While Laplace's work was important for the development of the mathematical theory of probability, it was less useful for experimentalists and observational astronomers.[64] However, Laplace investigated the "probability that an error should lie between α and γ by $\int_\alpha^\gamma f(z)\,dz$, where $f(z)$ is a known function of the error

62 Daniel Bernoulli, "Dijudicatio maxime probabilis plurum observationum discrepantium atque versimillima inductio indeformand," *Acta Acad. Sci. Petrop.* (1777): 3–23, translated in *Studies in the History of Statistics and Probability,* E. S. Pearson and M. G. Kendall, eds. (London: Charles Griffin, 1970), 155–167. This is noted by statisticians as the first discussion of maximum likelihood.

63 Euler, "Observations on the foregoing dissertation," in *Studies* Pearson and Kendall, eds., 167–172. See also O. B. Sheynin, "Euler's Treatment of Observations," *Arch. Hist. Exact Sci.* 9 (1972): 45–56, "J.-H. Lambert's Work on Probability," *Arch. Hist. Exact Sci.* 7 (1970–71): 244–256, and "Origins of the Theory of Errors," *Nature,* 211 (1966): 1003–1004.

64 For Laplace's work in the history of the development of the mathematical theory of probability see Stephen Stigler *History of Statistics: the Measurement of Uncertainty before 1900* (Cambridge MA.: Harvard University Press, 1986).

z." He redefined the probability of error into a more general and more interesting mathematical one. He examined the Gaussian distribution as well as the behavior of other functions. However, Laplace offered no clues as to which function was useful and under what conditions for dealing with the problems of errors of observation.[65]

Karl Friederich Gauss was the exception. Together with Wilhelm Bessel, he changed the relationship between mathematics and observational astronomy in the crucial period after 1800. He defined the problem as an observational astronomer, "to determine the orbit of a heavenly body, without any hypothetical assumptions, from observations not embracing a great period of time, and not allowing a selection with a view to the application of special methods." Using the method of least squares, he worked out the principle characteristics of a new planet, Ceres, first observed by Giuseppe Piazzi in 1801. Franz Xaver von Zach used these characteristics to relocate the planet later that year.[66] Gauss' account of his methods were directed to astronomers. He noted that "the motions of the heavenly bodies, so far as they take place in conic sections, by no means demand a complete theory of this class of curves,"[67] His work was replete with specific examples taking into account calculations with incomplete sets of observations and completed his text with a set of log tables.[68]

Most forays by mathematicians into the domain of experimental and observational natural philosophy did not stop those philosophers seeking their own solutions within their terms of discourse. While mathematicians appeared to appropriate the problem of the shape of the earth completely, even to making the measurements in Peru and Lapland, they did not decide the issue. Their mathematical explorations of the shape of the earth did not solve the problem. Geodesic measurements continued throughout the eighteenth century. This measurement had political and imperial implications as well as being of metaphysical use. These efforts were, therefore, international and inconclusive. The incompatibilities of these measurements seemed to indicate that the shape of the earth was not the simple oblate spheroid of the mathematicians. Observational closure came only with the measurements of P. F. A. Mechain and J. B. J. Delambre during the revolution-

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65 Laplace, *Mécanique Célèste,* trans. with commentary by Nathaniel Bowditch, vol. 2, book 3 chap. 5.

66 Gauss *Theoria motus corporum coellestium,* Charles Henry Davis, trans. (New York: Dover reprint of 1857 edition, 1963).

67 Gauss *Theoria*, p. 3.

68 On the method of least squares, see Jean-Luc Chabert, "Gauss et la méthode des moindres carrés," *Rev. Hist. Sci.* 43 (1990): 5–26 argues that Gauss' solution was to a mathematical, not an astronomical problem. See also, Elizabeth Garber, "Aspects of the Introduction of Probabilities into Physics," *Centaurus,* 17 (1971): 11–39, and Forbes, "Mathematical Cosmography." Forbes argues that Tobias Mayer implicitly used the same postulate, that the algebraic sum of random errors is zero in any run of independent measurements, that was later the foundation of Gauss' formal theory of errors.

ary period.[69] Chemists successfully repulsed attempts to develop a mathematical chemistry. Chemistry was experiment. The object of its study were real bodies and their reactions. The proofs chemists required for their ideas came from their own observations.[70] Chemists and chemistry belonged on the other side of the methodological divide within that broad, disciplinary swathe defined by experiment and observation, physics.

Defining eighteenth century disciplines methodologically produces an entirely different research landscape from that of the twentieth century. However, disciplines defined by method had their drawbacks. As the method uncovered phenomena, the field, for its practitioners and historians alike, became unwieldy. By the middle of the eighteenth century some practitioners circumscribed their disciplines more closely. The first to be successful were the chemists. Rather than define themselves by adherence to a common set of concepts or presupposition, they demarcated their field by method and the phenomena they worked on. Chemists claimed jurisdiction over phenomena that yielded to their particular set of methods. Chemists postulated that physics, narrowly defined, only dealt with the aggregate properties of matter. Physical properties depended only upon the arrangement of the particles of the substances that were more or less complex. Only chemical methods could break apart these complex particles and reaggregate them into different combinations. Only the methods of chemists could plumb the microscopic structure of matter.[71]

Throughout the century, pinning down precisely the relationship between chemistry and physics was not that simple. Chemists argued over the place of various phenomena and the status of caloric and other imponderables within their discipline. Historians of eighteenth-century chemistry also argue over that relationship. Their dispute focuses on physics, not precisely defined, as a model for chemistry during the era of Lavoisier. While not entering into the debate within the history of chemistry, it is not at all clear that physics, even if it was a model for chemistry, was any closer to being a modern scientific discipline than eighteenth-century

69 Heilbron, *Weighing Imponderables*, 213–242.

70 Michele Sadoun-Goupil, "Les tentatives de mathématisation de la chimie au xviiiième siècle: echecs et oppositions," *Sci. Techn. Persp.* 1 (1981–82): 2-1–2-19. See also Michele Sadoun, *La mathématisation de la chimie en xviiiième siècle* (Strasbourg: Université Louis Pasteur, 1974).

71 Karl Hufbauer, *The Formation of the German Chemical Community, 1720–1795* (Berkeley CA.: University of California Press, 1982), chap. 1, claims that chemistry attained disciplinary autonomy in the early eighteenth century by German chemists anxious to separate their work from that of alchemists as well as physicists. J. B. Gough, "Lavoisier and the Fulfillment of the Stahlian Revolution," *Osiris,* 4 (1988): 15–33, p. 24. Arthur Donovan, *Philosophical Chemistry in the Scottish Enlightenment* (Edinburgh: Edinburgh University Press, 1975) discusses the attempts of William Cullen to define chemistry as an autonomous discipline, more difficult in Scotland than France, given the heavy Newtonian tradition. Cullen's definition was based on phenomena.

chemistry. What could physics offer chemistry in the late eighteenth century except quantified experiment. However, the quantified experiments of Lavoisier and Laplace on heat do not seem to change the terms of the theoretical debate about the nature of heat. It remained in their study in the vernacular. Their elaborate theory of heat as the *vis viva* of the particles of bodies was abandoned for the simpler assumption that heat entered a body as it was heated and left as it cooled. From this they developed their linear, algebraic expression for comparing specific heats.[72] Physics was still a loosely held bundle of practices whose theoretical conjectures were as qualitative as those of chemists in the eighteenth century.[73]

The practitioners of experimental physics were as divided as chemists over foundational ideas, such as ether or caloric. Nor did practitioners in either discipline structure theories in our sense of the term. Arthur Donovan's description of pre-Lavoisian chemistry as a "dispersed and varied set of local didactic, experimental, and explanatory practices" works for experimental physics in the same era and into the next century.[74] Physics in the eighteenth century shared the same characteristics as eighteenth-century chemistry which differentiate them both from their modern namesakes There is indeed more evidence that chemistry itself initiated the move "into science" from natural philosophy.[75] Lavoisier's new chemistry reorganized that science around particular concepts rather than defining chemistry through methods and phenomena. In trying to promote the new chemistry as a revolutionary method, Lavoisier and the other anti-phlogistonists knew their colleagues well.

72 Lavoisier and Laplace, *Memoir on Heat,* Henry Guerlac, trans. (New York: Neale Watson Pub., 1982), on *vis viva* 4–5. Their claims for precision were spurious. See also Heilbron, *Weighing Imponderables,* 101–104.

73 On these points see the exchanges between Evan M. Melhado, "Chemistry, Physics and the Chemical Revolution," *Isis,* 76 (1985): 195–211, C. E. Perrin, "Revolution or Reform," *Hist. Sci.* 25 (1987): 395–423 and "Research Traditions, Lavoisier, and the Chemical Revolution," *Osiris* 4 (1988): 53–81. Essentially, Perrin argues for a self-contained tradition of research within chemistry, with chemistry as a disciplinary equal of physics. Melhado and also Arthur Donovan in Donovan, "Lavoisier and the Origins of Modern Chemistry," *Osiris,* 4 (1988): 214–231 argue that chemistry took as its model the methods of physics, already assumed to be a modern scientific discipline. The debate continued in Perrin, "Chemistry as Peer of Physics: A Response to Donovan and Melhado on Lavoisier," *Isis,* 81 (1990): 259–270, Melhado, "On the Historiography of Science: A Reply to Perrin," *Isis,* 81 (1990): 273–276, and Donovan, "Newton and Lavoisier: Chemistry as a Branch of Natural Philosophy to Chemistry as Positive Science," in *Action and Reaction,* P. Theerman and A. D. Seeff, eds. (Newark DEL: University of Delaware Press, 1993). The issue begins in Guerlac, "Chemistry as a Branch of Physics: Laplace's Collaboration with Lavoisier," *Hist. Stud. Phys. Sci.* 7 (1976): 183–276. For the impact of the methods of physics on Lavoisier see, Donovan *Antoine Lavoisier: Science, Administration and Revolution* (New York: Cambridge University Press, 1996), chap. 3.

74 See Donovan, "Introduction," *Osiris* 4 (1988): 5–12, p. 11.

75 The phrase is taken from Donovan, "Lavoisier and modern Chemistry," p. 219.

While Lavoisier claimed to be introducing a new nomenclature, to use that language required the acceptance of the new chemical principles underlying the language. His critics saw it as a conceptual reorganization that left the basic methods of chemistry intact. They were correct. Lavoisier had changed "the rules of scientific discourse."[76]

Lavoisier and the other anti-phlogistonists successfully isolated chemistry as a discipline because they had joined together concept, theoretical explanations and method in a potent new form. In this new form the organizing principles of the theory held as high an ontological value as its methods.[77] With the adoption of Lavoisier's chemistry, the discipline changed and shed many of its eighteenth century characteristics. Physics, narrowly defined, did not make that transition until well into the nineteenth century.

The Social Geography of Physics and Mathematics

The methodological division of the sciences was both intellectual and social. On the continent, members of Academies of Science were divided into two sections, mathematical and physical. These membership categories were further subdivided. The publications of these societies reflected this same social division. In the Paris Académie this division between the mathematical and deductive, versus the experimental and observational and inductive, sciences was particularly long lived. In the mathematics division was geometry, astronomy and mechanics. Under physics was physics, anatomy, chemistry and botany. These particular subfields came under revision as the methodological divisions of the disciplines became unwieldy. Under Lavoisier's suggested reforms physics, narrowly defined, entered as a specific section within the Académie. This separation of physics from chemistry meant that it no longer encompassed chemistry. Lavoisier reinforced chemists' efforts to establish their discipline as separate and equal to the experimental field of physics.[78]

76 To understand that Lavoisier's chemistry was based upon new principles read Antoine Laurent Lavoisier, *Traité élémentaire de chimie* (Paris, 1789) trans. by R. Kerr as *Elements of Chemistry* (Edinburgh: 1790). The phrase is taken from Jan Golinski, "The Chemical Revolution and the Politics of Language," *Eighteenth Century: Theory and Interpretation* 33 (1992): 238–257. Bernadotte Besaude-Vincent in, "A View of the Chemical Revolution Through Contemporary Textbooks: Lavoisier, Fourcroy and Chaptal," *Brit. J. Hist. Sci.* 23 (1990): 435–460 discusses the differences between Lavoisier's text and those of other chemists, particularly Chaptal who published texts before and after becoming a Lavoisian.

77 The loud protests of chemists in the nineteenth century proclaiming their empiricism does not alter the place of theory within their reorganized discipline. These protests occurred after the acceptance of Lavoisier's revised system. His theory had become so ingrained in practice as to be natural, not theoretical.

78 See Hahn *Anatomy*, for the divisions within the Académie, and Rhoda Rappaport, "The

In the Göttingen Societät der Wissenschaften, whose intellectual reach was wider, the classes were mathematics, physics, history and politics—to the exclusion of the "useful" arts of theology, law and philosophy. The distinctions between mathematics and physics was still a methodological one.[79] The sections of the publications, and the social divisions they reflected in the St. Petersburg Academy of Sciences, were unstable and seem to shift with the political fortunes of the Academy itself.[80] The position of physics changed from the mathematical to the physical section and back again. Looking at the journals of the Academy, the content of the papers reflected the sections to which physics was relegated. When physics was within the physics section, the papers were experimental. When it was shifted to the mathematics division, the papers were mathematical.

The memberships of state-financed academies were carefully controlled, socially and intellectually. And only in such academies could mathematics such as the calculus flourish. Calculus only entered the curricula of military academies of the European states as it became useful for fortification design. Similarly, the mathematics taught at the universities of the German states and elsewhere was useful, to meet the needs of a gentleman on his estates, and the cameralist for a career within the state civil service.

Most eighteenth century scientific societies were local and functioned scientifically as centers for the experimental and observational sciences. While those on the continent at the apex of the cultural pyramid were arms of the state, most of these societies served the interests and needs of local groups. Experimental philosophy drew more people into the study of nature and into active participation in local societies. However, the interests of the individuals that supported such societies and activities were very diverse. This diversity led to tensions within those institutions that reflected the different aspirations of their members.[81] Despite the multiplicity of interests, local scientific societies were as important as lecture series for the

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Liberties of the Paris Academy of Science, 1716–1785," in *The Analytical Spirit: Essays in the History of Science in Honor of Henry Guerlac,* Harry Woolf, ed. (Ithaca NY.: Cornell University Press, 1981), 225–253, and James E. McClellan III, "The Académie Royale des Sciences, 1699–1793: A Statistical Portrait," *Isis,* 72 (1981): 541–567, 543–544.

79 See Otto Sontag, "Albrecht von Haller on Academies and the Advancement of Science: the Case of Göttingen," *Ann. Sci.* 32 (1975): 379–391.

80 David M. Griffith, "The Early Years of the Petersburg Academy of Science," *Canadian-American Slavic Studies,* 14 (1980): 436–445.

81 This was also true in the early years of the Royal Society as later in local scientific societies. See Marie Boas Hall, *Promoting Experimental Learning: Experiment and the Royal Society, 1660–1727* (Cambridge: Cambridge University Press, 1991). This changed as the eighteenth century wore on; see David P. Miller, "Into the Valley of Darkness: Reflections on the History of the Royal Society in the Eighteenth Century," *Hist. Sci.* 27 (1989): 155–166.

dissemination of knowledge of experimental philosophy.[82]

We can recognize disciplines and communities of practitioners within the sciences, perhaps the best example being that of the chemists. Yet these communities did not function as professions. Twentieth-century patterns of behavior, social relations, training, and practices are not consistently detected in the eighteenth century.[83] While some individuals might earn enough doing chemistry, say, to live comfortably, most did not, even when they were members of the Académie des Sciences in Paris.[84] Membership in the academies conferred great social status and opened up opportunities to teach, consult, and even become a paid expert witness in legal cases. Teaching meant teaching in several places, but did not include training the next generation of practitioners. Neither did the institutional setting of teaching become the site for research. Research was pursued privately. As state functionaries, members of the academies were expected to aid the state in solving

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82 The social range of the membership of these societies, their purposes and their fates are the subject of a large literature. Most historians emphasize the provincial character of these institutions and the local purposes they served. For provincial French scientific societies, see Daniel Roche, *Le siècle des lumières en province: Académies et académiciens provinciaux, 1680–1789* (Paris: Mouton, 1978), 2 vols. For Holland, see Snelders, "Professors, Amateurs and Learned Societies," in *Dutch Republic*, Jacob and Mijnhardt, eds. For Scotland, see Donovan, *Philosophical Chemistry*, and R. L. Emerson, "The Philosophical Society of Edinburgh (1737–1747)," *Brit. J. Hist. Sci.* 12 (1979): 154–171; "(1747–1780)," same journal, 14 (1981): 133–176 and "(1768–1785)," same journal, 18 (1985): 255–303 and "Science, Origins and Consensus of the Scottish Enlightenment," *Hist. Sci.* 26 (1988): 333–336, and Kathleen Holcomb, "A Dance in the Mind: The Provincial Scottish Philosophical Society," *Stud. Eight. Cult.* 21 (1991): 89–100. For an early provincial English scientific society see Schofield, *The Lunar Society*. For the German states see Lowood, *Patriotism.*

83 For arguments why twentieth-century sociological categories are useless for understanding the social behavior of scientists even at the end of the eighteenth century, see Dorinda Outram, "Politics and Vocation in French Science," *Brit. J. Hist. Sci.* 13 (1980): 27–43.

84 For the argument that eighteenth-century chemistry was a discipline but not a profession see Karl Hufbauer, *The Formation of the German Chemical Community, 1720–1795*, (Berkeley CA: University of California Press, 1982), and "The Social Support for Chemistry in Germany in the Eighteenth Century: How and Why did it change," *Hist. Stud. Phys. Sci.* 3 (1971): 205–231. John Heilbron illustrates this for "electricians," although he does not address the question directly, in Heilbron, *Elements ofEarly Modern Physics,*. Roger Hahn also argues that even under the patronage of the French State scientists cannot be defined as professionals. See, Hahn, "Scientific Research as an Occupation in Eighteenth-Century Paris," *Minerva,* 13 (1975): 501–513, and "Scientific Careers in Eighteenth-Century France," in *The Emergence of Science in Western Europe* Maurice Crosland, ed. (New York : Science History Books, 1975). For an alternative view, see Crosland, "The Development of a Professional Career in Science in France," *Minerva,* 13 (1975): 38–57, and *Emergence* Crosland ed., 139–159, although he claims the status of profession in France only with the French Revolution. For a more general discussion, see *Professions and the French State, 1700–1900*, Gerald L. Geison, ed. (Philadelphia

its technical problems. Many did and this sometimes ended their ability to do sustained pieces of research.[85] Making a living in science required entrepreneurial skills to establish, consolidate, and sustain.

There was no established, systematic or regulated entry into the scientific disciplines. The study of nature was an integral aspect of polite, and not so polite, cultures throughout the eighteenth century. At the universities of Europe the sciences were taught as adjuncts to medicine, or some other profession, or as an aspect of this male, educational rite of passage into elite, adult society. While not on a par with dancing or fencing classes the sciences added a certain gloss to the universities' offerings to the sons of the rich and the upper classes. For a student the pursuit of science required aptitude, perseverance, and the opportunity to take advantage of local resources. Training to become competent enough to publish and enter the world of research was again a matter of personal commitment, social place, opportunity, and patronage.

The arena of social and intellectual activity for most scientists in the eighteenth century was the local scientific society, rather than the university or college. And some of those studies accepted and lauded as research in the eighteenth century appear from the late twentieth century to be an unending recital of observations, and some decidedly misplaced. Then as now, disciplinary colleagues were geographically scattered and might be addressed in technical terms. Yet the expectations of this "imagined community" for the content of research reports were very different from those of professionalized disciplines of the past century and a half. Examples of those were examined in the preceding chapter. This is not a matter of the concepts of physics having changed, or of its problems being different. That is to be expected. In the eighteenth century, experimentalists and mathematicians had different notions of what constituted a valid research problem and the criteria for their solution. Much that counted as explanation in the eighteenth, does not stand up to scrutiny as science in this century. Similarly disciplinary boundaries are not where we might expect them. What we find is that while names of broad disciplines

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PA: University of Pennsylvania Press, 1984). Charles Coulston Gillispie locates the professionalization of the sciences in France in the nineteenth century. See, Gillispie, *The Professionalization of Science in France, 1770–1850* (Tokyo: Kyoto Doshiha University Press, 1983). An enquiry into mathematicians and their status as "professionals" based on an historically shifting definition of professions is in Ivo Schneider, "Forms of Professionalization in Mathematics before the Nineteenth Century," in *Social History of Nineteenth Century Mathematics*, Herbert Mehrtens, Henk Bos and Ivo Schneider, eds. (Basel: Birkhäuser, 1981), 89–110.

85 See Charles C. Gillispie, *Science and Polity.* Frederick the Great had the same expectations of his academicians in Berlin. See, Friederich II, "Discourse de l'utilité des sciences et des arts dans un état," (read by Thérault Jan. 1772) *Mém. Acad. Sci. Berlin* (1772): 18. Whether he was as successful in mobilizing his academicians as the French state is questionable. See also Calinger, "Frederick the Great and the Berlin Academy of Science."

such as physics and mathematics have remained the same, the expectations and practices of physicists and mathematicians have changed radically. Establishing a theoretical point of view, then developing it to interpret phenomena, or as a valid foundation for a mathematical argument, was a matter of metaphysics. The conduct of disputes, the curve of vocations and the distribution of honors and prizes follow different intellectual as well as social rules.

Historians of the eighteenth-century sciences appreciate these differences and take them as their starting point in analyzing their era.[86] We need to take the next step and ask how then did the modern discipline of physics develop into a profession whose practitioners were located in universities? How was it that the education and practices of these professionals cut off most of the members of society from participation in or even understanding of what those practitioners were doing? How did the modern system of physics come into existence?

86 Symbolic of this realization are the essays in *Ferment of Knowledge* R. S. Rousseau and Roy Porter, eds. This is continued in the review of *Ferment of Knowledge* by G. N. Cantor, "The Eighteenth Century Problem," *Hist. Sci.* 20 (1982): 44–63. See also J. F. Musser, "The Perils of Relying on Kuhn," *Eighteenth Cent. Stud.* 18 (1984): 215–226, and E. M. Melhado "Metzger, Kuhn and Eighteenth Century Disciplinary History," in *Studies on Hélène Metzger*, Gad Freudenthal, ed. *Corpus,* 8/9 (1988). For an earlier discussion of the inherent difficulties of investigating science in the eighteenth century, see Crosland, "Editor's Foreword," in *Emergence of Science* Crosland, ed. The pursuit of the place of science in the growing consumer societies of the eighteenth century continues. See *Consumption* Brewer and Porter, eds., part IV, as well as Golinski, *Science as Public Culture,* and Stewart, *Rise of Public Science.*

Part II

Transitions, 1790–1830

Chapter IV

"Empirical Literalism": Mathematical Versus Experimental Physics in France, 1790–1830 [1]

Well before 1790 Paris had become the social and intellectual center for scientific life in Europe. It remained at the center until after 1830. Because this era in the scientific life of France has been seen as the source of modern physics, we need to examine the workings of Parisian scientific institutions and the practices of mathematicians and experimental physicists. What, precisely, did this band of intensely competitive men change in their mathematical and physical heritage from the eighteenth century? After examining the social and political structures of scientific Paris and their workings, we will turn to a series of problems and prize-essay questions of the era. In France the solutions to these problems were the occasions for fierce contests over the practices and future of both physics and mathematics. The solutions also disclose what was accomplished in this era in terms of changing the relationships between mathematics and experimental physics. The mathematization of electrostatics by Poisson, and Fourier's work on the conduction of heat through solids, will help us distinguish the technical mathematician of the early nineteenth century from the theoretical physicist of a later era. Similarly, the development of the wave theory of light and subsequent work in France on elasticity will separate mathematicians from experimentalists and reveal the changes in physics by 1830.

The place of science in French society and culture was strengthened in the era between 1790 and 1815, despite the arbitrary rule of the radical period of the French revolution, the instabilities of the Directory, and the manipulations of the Napoleonic era. There were even signs of long-term social and intellectual continuity in the scientific community in the context of political change.[2] Institutions

1 Stephen Jay Gould coined this expression in, "The Stinkstones of Oeningen," in Gould *Hens Teeth and Horses Toes* (New York: Norton, 1983) 94-106, 105 to describe Cuvier's methodology in paleontology in his efforts to read the fossil record as it presented itself, with no interpolations or theoretical leaps of the imagination.

2 This does not preclude the short-term dislocations, anxieties and even terror during the

changed, but as institutions they functioned in ways that were continuations of pre-Revolutionary social practices within the sciences. Paris retained its international leadership as the center for research and that leadership was even strengthened and remained unchallenged until after 1830.[3] Scientists became even more important to the State in this era.[4]

Changes in Social Geography, 1790–1830

In these three decades, as in the eighteenth century, political forces dictated the social space of mathematicians and experimental physicists.[5] Scientists proved remarkably adaptable to the sometimes rapidly changing political order.[6] Science was important for all the republican regimes. It was part of the foundation of their rhetoric.[7] Until it was discarded by Napoleon, state scientific institutions had to be remade to conform with the prevailing egalitarian ideology. The "elitist" institutions of the old regime were destroyed without any immediate, official institutions to replace them.[8]

1790s. See Dorinda Outram, "The Ordeal of Vocation: The Paris Academy of Sciences and the Terror," *Hist. Sci.* 21 (1983): 251–274, and Hahn *Anatomy,* chaps. 8 and 9.

3 The issue of the impact of the revolution and of Napoleon on science is yet to be decided. See Henry Guerlac, "Some Aspects of Science in the French Revolution," *Sci. Monthly* 80 (1955): 93–101, René Taton, "The French Revolution and the Progress of Science," *Centaurus,* 3 (1953): 73–89, L. Pearce Williams, "The Politics of Science in the French Revolution," in *Critical Problems in the History of Science,* Marshall Clagett, ed. (Madison WI.: University of Wisconsin Press, 1962), 291–308, and Joachim Fischer, *Napoleon und die Naturwissenschaften* (Stuttgart: Franz Steiner, 1988).

4 See Nicole Dhombres and Jean Dhombres, *Naissance d'un pouvoir: sciences et savants en France, 1793–1824* (Paris: Editions Payot, 1989) and Terry Shinn, *Savoir scientifique et pouvoir social: l'École Polytechnique, 1794–1914* (Paris: Presse Foundation Nationale des Sciences Politiques, 1980).

5 See Charles Coulston Gillispie, "Science and Politics, with specific Reference to the Revolution and Napoleonic France," *Hist. Techn.* 4 (1987): 213–223, and Gerald L. Geison, *Professions.*

6 The epitome for this is Pierre Simon, Marquis de Laplace who flourished under the Old Regime, survived the Revolution, Directory, Napoleon and the Restoration. Others were less fortunate and suffered at certain periods, among them Augustin Fresnel, Joseph Fourier, and Gaspard Monge. The fate of Antoine Laurent Lavoisier lies outside of our story.

7 Gillispie, "The *Encyclopédie* and Jacobin Philosophy of Science," In *Critical Problems,* Clagett, ed., 255–289.

8 Hahn discusses The fate of the Académie des Sciences and the academicians in Hahn, *Anatomy.* Maurice Crosland *The Society of Arcueil. A View of French Science at the Time of Napoleon I* (London, 1967) recounts the attempts of some scientists to continue their research careers during the chaos of the 1790s. However, this represents only a small number of established scientists and their protegées.

Scientists gained a refuge in the new educational institutions established and sustained by the various regimes. It was largely within these new institutions that scientists regained their sense of collective identity, the physical and social space, and resources for the resumption of their research. For our purposes, the most important of these new educational institutions was the École Polytechnique.[9] The establishment of the Grand Écoles and later the University changed the teaching of science.[10] At the École Polytechnique the sciences were taught as subjects in their own right, whether or not the student was destined for a career as an engineer, a teacher, or some other state functionary. There were courses in chemistry and mathematics with the systematic introduction of material of increasing difficulty. The next generation of mathematicians and experimentalists were trained through this systematic educational scheme. Establishment of the national university also increased the availability of such instruction. These same educational institutions, rather than the Académie then Institut and finally Académie, became the setting for renewed research and recruitment of the next generations of mathematicians and experimental physicists.[11] Further public instruction in the observational and experimental sciences was also available at other state institutions such as the Bureau des Longitudes and Le Musée des Sciences Naturelles.

The establishment of the École Polytechnique, together with the changed purposes for teaching science and the increased numbers of students led to the first discernible structure of professions within the sciences. The new importance of science and the expansion of teaching created possibilities for positions, although cumul intensified.[12] Patronage still determined the early years of a developing ca-

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9 See *The Organization of Science and Technology in France, 1808–1914,* Robert Fox and George Weisz, eds. (New York: Cambridge University Press, 1980), and Geison *Professions*. For a history of the École Polytechnique, see Ambroise Fourcy, *Histoire de l'École Polytechnique* (Paris: Belin, reprint, 1987), and Terry Shinn, *Savoir scientifique et pouvoir social, l'École Polytechnique, 1794–1914*. For the later challenge to the hegemony of the École Polytechnique, see Antoni Malet, "The École Normale and the Education of the Scientific Elite in Nineteenth-Century France," *Asclepio,* 43 (1991): 163–187.

10 However, see Grattan-Guinness, "Grand-Écoles, petits Universités: Some Puzzled Remarks on Higher Educations in Mathematics in France, 1795–1840," *Hist. Univ.* 7 (1988): 197–225.

11 The title and organization of the premier French scientific society supported by the state changed with the political turbulence from 1790 to 1820. The Académie des Sciences in Paris was disbanded in August, 1793 and replaced by the Institut at the end of 1795. The Institut was organized into three classes, the first of which covered the sciences. These included mathematics, physics, chemistry, botany and medicine. At the restoration of the monarchy, the Institut reverted in March 1816 to the Académie des Sciences.

12 Cumul refers to the custom of accumulating positions in state teaching institutions that exacerbated the competition for place within the sciences in early nineteenth-century France.

reer in the sciences. No one was more adept and able at dispensing this patronage than Laplace. Positions and preferment went wherever possible to young men willing to use his ideas and approaches to the solutions of problems in physics and mathematics. Biot's obsequiousness towards Laplace and his work in celestial mechanics would be comical if it were not for the results. It worked. Biot's career path was easier than that of other young colleagues. One of the latter was Augustin Fresnel who offended the Laplacians and never invoked Laplace's intellectual authority in his own work. However, Fresnel had the help of François Arago. Arago's position at the Observatory, obtained originally through the patronage of Laplace, and as editor of *Annales de Chimie* allowed him in turn to dispense patronage. Poisson's early election to the physics section of the Institut was similarly a matter of political patronage. His election was a further demonstration of the power of a small group within the Institut to direct its affairs. The election of promising scientists to sections not especially connected to their research specialty had been used even before the Revolution. For Poisson, with no private income, election to the Institut was crucial.[13]

Patronage and political power within the institutions of science shaped the careers of individuals, the prizes they might, or might not be awarded, and the publication, or lack of publication of prize essays by the Institut. However, these political forces did not prevent independent assessment of intellectual worth. While Joseph Fourier's prize-winning essay on heat was not published by the Institut (or with the restoration the Académie) for over a decade, its contents were well known to mathematicians in Paris. He was also elected to the post of Executive Secretary in 1820, a sure indication of the decline of Laplacian influence. Fourier, being older and a high-level government official, had the ability and connections to stay the course.[14]

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13 While not delving into the politics of Institut elections, there is no evidence that Poisson's election was other than another political victory for Laplace. His election may even have been a payment on a political debt. Pierre Costabel, "Poisson, Siméon-Denis," *Dict. Sci. Bio.* vol., 10, 480–490, 481. Neither Poisson nor anyone else regarded his papers on electrostatics as other than mathematics, or marking a definitive change in the analysis of physical problems. For an alternative interpretation, see R. W. Home, "Poisson's Memoirs on Electricity: Academic Politics and A New Style in Physics," *Brit. J. Hist. Sci.* 16 (1983): 239–259.

14 For the impact of Institut politics on the publication of Fourier's essay of 1811 see, Grattan-Guinness and Jerome Ravetz *Joseph Fourier (1768–1830). A Survey of his Life and Work* (Cambridge MA.: MIT Press, 1972). For the impact of patronage in mathematics in general in this era see, Grattan-Guinness, *Convolutions in French Mathematics, 1800–1840* (Boston: Birkhäuser, 1990), 3 vols., vol. 1, chap. 2. For Biot, see Eugene Frankel, "Career-making in post-revolutionary France: The Case of Jean-Baptiste Biot," *Brit. J. Hist. Sci.* 11 (1978): 36–48, and "Corpuscular Optics vs. the Wave Theory of Light: The Science and Politics of a Revolution in Physics," *Soc. Stud. Sci.* 6 (1976): 141–184. See also, Crosland, *Science under Control: The French Academy of*

The Laplacians wielded great political clout within the scientific community for a crucial period but this power should not be confused with intellectual authority. In the long run, becoming a "Laplacian" did not garner those men secure reputations in French science. Poisson's scientific reputation peaked early in his career and declined thereafter, even as his political power rose. The assessment of his intellectual accomplishments sank to the point that, after his death Poisson, was regarded as someone who used other's ideas to develop his own career.

The Laplacians were unsuccessful in stilling alternative visions of physical processes, and of alternative foundations for the calculus. Laplace could and did influence the choice and the wording of prize-essay questions in the mathematical and physical sections of the Institut and later in the Académie. However, when it came to awarding prizes, Laplace's influence was less monopolistic. Augustin Fresnel's work was crowned even though his methods and ideas were not in accord with those of Laplace, and despite Laplace's skepticism about the validity of his mathematical methods so was Fourier's. The delayed publication of these works became scandals that damaged the Académie and Laplace, not Fresnel or Fourier. From 1790 on, in addition to Académie journals, others existed independent of its influence. Fourier and Fresnel could make their ideas known and establish a reputation beyond Laplace's influence. Indeed this multiple outlet for publications in science made that tight centralized control impossible.

The name might change, and function of the Académie des Sciences might be more restricted than in the old regime, yet entry into it capped a life in science. Membership was the ultimate legitimation of a scientist's research and it was still the most prestigious scientific institution in Paris.[15] The social structure of the academy continued to reflect the changing disciplinary structure of the sciences. While physics, narrowly defined, was already a section, others also appeared in the Institut. This reflected the fragmentation of the experimental and observational sciences already threatening the Académie's institutional monopoly of science in the 1780s. However, the Académie no longer had a corner on the presentation and publication of research. Important issues in chemistry were published elsewhere, and the *Annales de Chimie* and the *Bulletin des Sciences par la Société Philomatique de Paris* offered alternative, and quick publication of important issues in physics.[16] The ponderous pace of the full account of research that would appear from the academic press bequeathed a polished presentation to posterity. Alternative journals were more attractive to address colleagues on important research

Sciences, 1795–1914 (Cambridge: Cambridge University Press, 1992), 44–49. Patronage prevailed in other disciplines, see Dorinda Outram, *Georges Cuvier* (Manchester: Manchester University Press, 1984).

15 The changes in the Académie are detailed in Hahn, *Anatomy*, chap. 9.

16 Crosland, *In the Shadow of Lavoisier: The Annales de Chimie and the Establishment of a New Science* (Oxford: British Society for the History of Science, 1994), chap. 2, discusses the changes in possible publication outlets for research in this era.

issues. The continuation of such journals signaled a paying audience engaged as practitioners in the experimental sciences. However, for most of these journals, economic viability meant pleasing an audience with diverse sets of interests in each issue. Physics needed to be published in its broadest sense. Even the *Annales de Chimie* added physique to its purview in the Napoleonic era.

Experimental Physics

Physics as a term still carried both broad and narrower meanings. The work of Arago and Alexander von Humboldt on geomagnetism kept the broader goals of physics alive, although physics could no longer lay claim to the broad methodological field of observation and experiment. Chemistry and the life sciences marked out their own domains of competence. Physics in the narrower sense still included the experimental exploration of sound and hearing, light and color. While experiment still defined the discipline, the phenomena covered depended largely on individual interpretation, or a journal editor, and the needs of the marketplace.[17] Experiments were expected to be careful, quantitative laboratory experiments whose results were often encapsulated in algebraic form. Speculations about the operation of nature stayed close to the tabulated results of these quantitative experiments, and rarely ventured beyond the phenomenological. Simultaneously, physics and chemistry were seen as drawing closer together through electrochemistry. An instrument developed in experimental physics had implications for some important problems of experimental chemistry. While the goals of their research might diverge practitioners in both disciplines shared a common methodological standard.

All of these developments bring early, nineteenth-century French physics closer to that of the twentieth century. But whether this physics marks the definitive breaking point between natural philosophy and physics in the modern sense still requires closer examination. Relying on conceptual realignment to distinguish between the two is clearly insufficient.[18] The concepts introduced between 1800 and 1850, while important are symptoms of change, not causes. Conceptual change taken in isolation tells us nothing about practices.[19]

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17 For an alternative interpretation of the range of meaning of the term, see Crosland, *Science Under Control*, 34–36. Crosland does not take into account the broader sense of the term inherited in this era from earlier in the eighteenth century.

18 For example, see Pearce Williams, "The Physical Sciences in the First Half of the Nineteenth Century: Problems and Sources," *Hist. Sci.* 1 (1962): 1–15.

19 Robert Silliman, "Fresnel and the Emergence of Physics as a Discipline," *Hist. Stud. Phys. Sci.* 4 (1974): 137-162, with a nod to methodological factors locates physics in the introduction of the wave theory of light.

Clearly the era between 1790 and 1830 in France marked some kind of watershed noted by more than one historian. Using textbooks in experimental physics Cannon claimed that "physics itself was invented by the French around the years 1810–30." René Häuy's *Traité Élémentaire de Physique* of 1803 was definitely not physics while Biot's *Traité de physique expérimentale et mathématique* of 1816, "was beginning to grasp at something like our concept of physics." Yet Cannon neither detailed what physics was, or is, nor the differences between physics of Häuy and Biot, nor those aspects of Biot that make him more "modern" contrasted with what Häuy was doing. In short there is no detailed exploration of what the changes were or the process of change. There is merely a sense that things progressed in a direction that makes the product more familiar to us, although not completely. Cannon named those men in the nineteenth century, Michael Faraday and John Herschel who were not, and those of the mid-nineteenth century, James Clerk Maxwell, who were physicists in our sense of the term without delineating what differentiated the two groups.[20]

John Heilbron also has located the beginnings of modern physics in the same era and country through a detailed history of the practices of experimentalists and the "theoretical work" of Laplace, Biot, and Poisson. Poisson's speculations about electricity and its action were vague. His strengths lay in his mathematics, as "exact description" over "a qualitative model deemed intelligible." Heilbron assumes here an inherent clarity of mathematical over vernacular descriptions of phenomena and ignores the history of mathematics. Vernacular descriptions of physical processes are not inherently muddier than mathematical ones even though qualitative. And, mathematics can be used to obfuscate and hide conceptual muddle while bringing quantitative precision to physics.[21] He accepts the term "physique-mathématique" as physics, not mathematics.[22]

In this era, if mathematical physics was mathematics, we need to understand what precisely was changing in physics to then decide where the origins of the modern discipline might lie. We must judge whether the development of carefully designed and executed quantitative experiments, whose results were analyzed for error and then algebraically joined to the mathematical expression of those results, is sufficient to define physics. If so, in what sense was the physics that these men created "modern."

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20 Susan Faye Cannon, "The Invention of Physics," in Cannon, *Science in Culture: The Early Victorian Period* (New York: Neale Watson, 1978) 111-136, 115.

21 For an alternative view see Elizabeth Garber, "Siméon-Denis Poisson: Mathematics versus Physics in Early Nineteenth-Century France," in *Beyond History of Science,* Garber, ed. 156–176.

22 For mathematical physics as mathematics in early nineteenth-century France, see Grattan-Guinness, "Mathematical Physics in France, 1800–1835," in *Epistemological and Social Problems* Jahnke and Otte eds., 349–370 and *Convolutions*, chap. 7.

Electricity and Magnetism

The quantification of electrostatics began in Charles Augustin Coulomb's experiments of the 1780s. The mathematical exploration of Coulomb's results was the work of Siméon-Denis Poisson. In 1811 in two long memoirs, Poisson developed a mathematical theory from Coulomb's systematically gathered and analyzed results.[23] Poisson was closely associated with Laplace and explicitly used Laplace's methods and extended them in his work. Therefore, we must examine Laplace to understand Poisson and the other "Laplacians" and whether they developed a modern form of "physics."

In the 1780s Laplace turned his attention to experimental physics in order "to extend the realm of geometry."[24] To interpret this and Lagrange's statements in the same decade as despairing of the calculus and seeing it drying up is to miss the point.[25] In the 1780s physical experiments became quantitative and the results of these experiments expressed algebraically. These algebraic expressions were potential raw material for the calculus. The extension of the calculus through solving the problems of mechanics was becoming more difficult. This new source, experimental physics, might possibly open up untold opportunities for mathematics and mathematicians.

Also, in the 1780s, Lavoisier drew Laplace into experiments on specific heats. After their successful completion of these experiments, Laplace returned to the calculus in his work on light, capillarity, and the paths of the planets.[26] Whether because of his experiences as an experimentalist or not, Laplace retained respect for the results of experiment and observation in his later forays into the calculus. In planetary theory he insisted that the mathematical theory must account for the observed deviations of the planets. Mathematicians could not explain them away as had d'Alembert and Clairaut.[27] While Laplace expended energy on very detailed calculations, he never doubted the superiority of the calculus over experimental physics or observation. In spite of the need to accommodate the results of ob-

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23 Poisson, "Sur la distribution de l'électricité à la surface des corps conducteurs," *Mém. Institut* (1811): 1–92, and "Seconde Mémoire sur la distribution de l'électricité à la surface des corps conducteurs," same journal, (1811): 163–274.

24 Letter from Laplace to Lagrange in Lagrange, *Oeuvres de Lagrange* J. A. Serret, ed. (Paris, 1867–1892), vol. 14, 124.

25 Eugene Frankel so interprets Lagrange's remarks, in Frankel, "Biot and the Mathematization of Experimental Physics," *Hist. Stud. Phys. Sci.* 8 (1977): 33–72, 34–35.

26 At the outset Laplace seems to have been ambivalent about the collaboration. See Grattan-Guinness, "Laplace," in *Dict. Sci. Bio.*, Gillispie ed., Supplementary volume, 273–403, 315.

27 For a full, contemporary assessment of Laplace's work in the light of early work on planetary motions see, John Playfair, "Review of Laplace, *Mécanique Célèste*," *Edinburgh Rev.* 11 (1807): 249–284.

servation, which were always subject to error, the mathematician must remain as independent as possible of "every empirical process, and to complete the analysis, so that it shall not be necessary to derive from observations any but indisputable data."[28] In addition Laplace believed that only empirical laws and the calculus were necessary for the exploration of nature.

Laplace's interest in physical phenomena was as a source of problems within the calculus. The physical problems he chose to examine could be directly related to problems in astronomy, or those that could be reduced to the mathematics that stemmed out of his work on celestial mechanics. The pattern of his approach was to assume that the physical phenomena, refraction, capillarity etc., were caused by central forces acting between the particles of matter at insensible distances. Even while admitting that the effect might be macroscopic and there was no way of knowing the force law that was operative, Laplace assumed that the force law acted over sensible distances. Once this was accomplished, all the mathematical techniques he had developed in celestial mechanics were brought to bear on the problem at hand. And the amount of analysis brought to bear was prodigious. Laplace approached these problems as he did the mathematics of celestial mechanics.

Laplace made his mathematical reputation by analyzing the complex interactions of the planets, considering the planets themselves as finite bodies, taking into account the subtle influence of their shape on their motions and the irregularities of their paths. The key to his mathematical success in celestial mechanics was in considering terms in series neglected by others, or integrating functions not integratable before, or by the invention of new mathematical techniques. To consider the planets as finite mathematical bodies, Laplace needed a mechanics for finite, masses. He established this mechanics by first deducing the law of universal gravitation "from observation." Kepler's laws were then used to establish the elliptical paths of the planets and the parabolic paths of comets. Experiments on pendula and astronomical observations verified this mathematical result for the earth from which Laplace argued that every particle of matter on the earth must be such a source of force or the centre of gravity of the earth would shift. Newton's name did not appear anywhere in this discussion.[29] Whether his derivation was valid or not, Laplace took Newton's law of gravitation not as an assumption about the operations of nature but as a statement deduced through mathematics from the results of observation. No speculations about the operations of nature were necessary. The law was operative everywhere, and became his foundation for the mathematical field of *physique-mathématique*.

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28 Laplace, *Traité de Mécanique Célèste*, translated with a commentary as *Celestial Mechanics* by Nathaniel Bowditch, vol. I, Bk. I.

29 However, to get from Kepler's laws to universal gravitation, Laplace needed Newton's third law. See Laplace, *Celestial Mechanics* Vol. 1, 256.

Physical principles did not exist for Laplace as independent sources of order. There were only two such sources, experiment and analysis. Of these two, analysis was clearly the more dependable. In the first book of *Mécanique Célèste* there was no attempt at detailed physical reasoning to establish his general mechanics.[30] Discussion of physical points was at a minimum. This led to less than satisfactory explanations of physical circumstances. A particle moving on a sphere described a great circle because "there is no reason why it should deviate to the right rather than the left of this great circle." Not a word about forces acting on the particle on the sphere. Having set up the most general laws of mechanics the rest of his celestial mechanics was, as far as possible, a deductive system rooted in mathematics with minimal reference to an empirical base. His deduction and then assumption of the operation of gravitation between matter, macroscopically and microscopically, only limited his mathematics by defining a relationship between the form of the function that was the first derivative of the force and the function that represented the force law. Mathematically, Laplace had introduced the potential function. Its physical implications remained unexplored.

Laplace did not use his physical model to guide or limit the development of his mathematics. Mathematical need was the criterion for deciding which terms, variables, or functions were eliminated or reduced to the status of constants or simply dropped. There was no attempt at offering a physical justification for a mathematical necessity.[31] He did not discuss the physical significance of eliminated variables, and their disappearance is mathematically necessary but physically mysterious. In short, all manipulations were mathematically, not physically, convenient.

Because of the overwhelming importance of analysis, Laplace deduced results that were known empirically. His derivation of Snell's law never referred to its empirical foundation nor did he mention that experiments existed that confirmed his analytical result. He did not note that his deduction might provide evidence

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30 Laplace, *Celestial Mechanics* opens with a general discussion of the laws of mechanics. Laplace noted the conservation of *vis viva* and angular momentum and the Principle of Least Action. This last principle did not, in Laplace's opinion, require a metaphysical justification, it "is in fact nothing more than a remarkable result of the preceding differential equations." Of all the principles of mechanics, only the law of inertia and that of force being proportional to the velocity depend on observation. See Laplace *Celestial Mechanics* vol. 1, bk. I, chap. ii & viii.

31 This is particularly obvious in his theory of capillarity which is explicitly based on central forces acting at insensible distances. Laplace set up an integral of a general function. The form of the function remained undefined, although it could be limited if he introduced the physical terms that he used to set up the problem. Also note the criteria he used to change variables etc. See, Laplace, *Celestial Mechanics,* vol., IV, Supplement to bk., X. Jean Dhombres, "La théorie de la capillarité selon Laplace: mathématisation superficielle ou étendue?" *Rev. Hist. Sci.* 43 (1990): 43–77, sees Laplace's work as a "separation of physical inspiration from analytical calculation."

for his basic physical hypothesis.[32] For Laplace, the empirical evidence supplied by experiments on capillarity were confirmations of his mathematics. There were several instances in his discussions of analytically deduced results where he missed the opportunity to comment on them physically, or see them as occasions for experiment. They occur in the middle of an argument and are mathematically uninteresting. The results finally deduced are physically incorrect or uninteresting from an experimental point of view.[33] The physical fruits of all this complicated calculus were very few. The results Laplace reproduced were known empirical results. For all his analysis Laplace was unable to penetrate further into the structure of matter or of the interaction of light and matter than contemporary, vernacular theories.

Laplace did not integrate his physical model with his mathematical analysis. There was no sense of what physical process was represented by the mathematics. He did compare certain analytical results with experiment and the formulae were interpreted mechanically, but not in terms of microscopic or macroscopic forces. The explanations were vernacular and based on the changes of velocities of moving particles.[34] The model and the analysis were decoupled. In complicated situations he tended to add causes, to further complicate the physical description. This was analogous to his work in celestial mechanics. Nor did he argue which of these disturbing causes (friction in the case of capillarity, the attraction of particles of heat to light in the case of refraction) could be put into analytical form, or how, or indicate which terms they were represented by in his analytical relationships.

On balance Laplace does not seem to be doing modern mathematical physics. He was adding a vernacular explanation to an eighteenth-century mathematical solution to a mathematical problem defined in the context of a physical phenomena.[35] Physical phenomena were still serving mathematical purposes. Solutions to the kinds of mathematical problems Laplace built out of the problem of the motions of the planets required the utmost confidence in complicated analysis, along with a technical brilliance in its manipulation to solve increasingly difficult differen-

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32 The simplifications that he introduced to obtain this result are not strictly warranted by the physical situation. Laplace *Celestial Mechanics* vol. 4, bk. X, 453, See also Bowditch's note on p. 469 and his comment on p. 471.

33 See his deduction of the limitation of the internal reflection of light, Laplace *Celestial Mechanics* vol. 4, bk. X, 462. His attention was on the velocity of the particles of light as they passed through several media. He argued that the velocity in the final medium will be the same as if the light passed from the first to the final medium without passing through all the intermediary ones.

34 See his explanation of total internal reflection, Laplace, "Mémoires sur les mouvements de la lumière dans les milieux diaphanes," *Mém. Institut* (1808): 300–342.

35 This in contrast to Robert Fox, "The Rise and Fall of Laplacian Physics," *Hist. Stud. Phy. Sci.* 3 (1971): 89–136, and Roger Hahn, *Laplace as a Newtonian Scientist* (Los Angeles CA.: University of California Press, 1967).

tial equations with new methods or with the unexpected use of older ones. This, together with the tenacity to work through such complexities with mathematical imagination, produced methods that, if not elegant, were from an imagination that delighted in complexity.

Poisson took much of Laplace's approach to problems, his goals of going beyond colleagues' previous solutions through the construction of complex mathematical problems, and the attachment to a particular, mathematically constructed model of matter to annex electrostatics to "geometry." This annexation was the first success in expanding the range of the calculus beyond mechanics. Beginning in Coulomb's experimental results, Poisson literally transferred the methods of Laplace's celestial mechanics to the case of electrostatics. He noted in passing that, in the latter case, there was attraction as well as repulsion, then focussed on attraction. Beginning with Coulomb's results that the attractive force at a point within a closed conductor was zero, Poisson commented that Laplace in his *Mécanique Célèste* had shown that the "attraction of surfaces that were almost spherical to interior points was zero." The physical cases of gravitation and electrostatics were mathematically equivalent. Poisson then considered the mathematical problem of the depth of the electric fluid over a spheroidal and almost spheroidal surface so that the attraction at any interior point was zero. To do this, he had to look for the action of the electric fluid on any point, within, on, or outside of the surface. Poisson then stated that the result of his analysis was that the effect of the electric fluid was proportional to its depth, and that while the problem appeared simple, it was actually tricky and he had found a defect in previous analyses. Poisson considered the attraction of spheroids covered with a thin layer made up of molecules between which central forces act for which,

> the components of attraction or repulsion that a body exerts at a given point, were expressed by the partial differentials of a certain function of the coordinates of this point, namely, the function that represents the sum of the molecules of the body divided by their respective distances to the given point: therefore we designate the sum as V.[36]

Having defined V mathematically, no more was said of its physical origins. Poisson did not connect the results deduced using this definition of V to either Coulomb's law or the results of Coulomb's experiments. While $V = V(x, y, z)$, Poisson did not use the functional relationship between the coordinates, given the force-law he has defined above, to limit the kinds of solutions he sought. In using the potential function Poisson deduced the equivalent of Gauss' law, but did not connect it to the physical result that he seemed to be addressing. Immediately after obtaining Gauss' law, Poisson then investigated the distribution of the electric fluid to satisfy the condition that, in equilibrium, the force in the interior of the sphere

36 Poisson, "Mémoire sur la distribution de l'électricité à la surface des corps conducteurs," *Mém. Institut* (1811): 1–92, 14.

is null. His argument was complex and designed to show mathematically that the force at the surface of the spheroid has no tangential component.

The mathematical thrust of Poisson's work was further emphasized in his second paper on electrostatics. The main point of the paper was to draw mathematicians' attention to his transformation of series that did not appear to converge into others that did and that lead to a solution in finite form. These were important issues for mathematicians of the time and so was the second issue on which he concentrated, avoiding definite integrals.[37]

The physics used in the introduction to the paper came directly from Coulomb's works. And while he deduced important new physical results, Poisson did not recognize them as such. They existed in a stream of analysis that climaxed in the solution of a mathematical not a physical problem. Here, as in his later papers, Poisson began with a specific physical model which was quickly translated into a mathematical expression for a force. All the analytical apparatus of rational mechanics could operate upon this expression and both model and physical problem become irrelevant to the solution. The model of electricity as a fluid that spread over the surface of the spheroid came from Coulomb, as did the idea that the depth of the fluid was proportional to the intensity of the electric force. Poisson deduced the depth of the fluid for many different particular cases of spheroids and spheres acting on each other. He calculated V at some point due to spheres and spheroids at very large, and at very close, distances in terms of the depth of the electric fluid. The important issue was to reduce the expression for V into finite form and there his interest in the function ceased. The electrical aspects of the case did not enter into his solution, as he did not explain how the expressions containing V were connected to the action of electricity. In his work were many clever mathematical techniques without much indication of how all the analysis might be relevant to the physics of electrified bodies.

Under these circumstances it is difficult to see Poisson's work on electrostatics as "pivotal in the development of a new vision of physics."[38] Poisson accomplished what Laplace had hoped to do–extend the range of analysis beyond mechanics and into experimental physics to create a *physique-mathématique*. The unification of physics and mathematics was still in the future.[39] The problem Poisson solved had been set as a prize problem by the mathematical section of the Institut. From the reactions of his colleagues, Poisson had solved a mathematical not a physical

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37 Poisson, "Second mémoire sur la distribution de l'électricité à la surface des corps conducteurs," *Mém. Institut* (1811): 163–274. These same patterns are followed also in his later papers on magnetism.

38 Home "Poisson's Memoir in Electricity," 259. Heilbron, *Electricity,* shares the view that Poisson's work was crucial for physics.

39 Garber, "Siméon-Denis Poisson," treats Poisson's work as mathematics and sets it in the context of early nineteenth-century French mathematics.

problem. The problem was to express a phenomena in analytical form, and then apply the calculus to solving the resulting equation, a partial differential equation of the first order. Poisson did so by fixing on the force between the particles of the fluid. The mathematics of these problems were already explored. Electrostatics was opened to annexation by the calculus.

This is not to belittle Poisson's achievements but to recognize that if Poisson did not discuss the physical significance of his analysis, neither did any of his contemporaries. From the lack of hostile reactions to Poisson's papers on electrostatics, we have to conclude that they were a satisfactory mathematical solution to the problem in the terms acceptable to the French scientific community.[40] He had considerable talent for mathematics, which is evident even in his early papers, but not for physics. He was inventive at solving mathematical problems not completed by others. Laplace's solution of the attraction of spheroids gave Poisson the opportunity to solve it for more complex cases while encompassing a new domain of experimental physics within the calculus. Even as the direction of mathematics changed, Poisson remained convinced that the proper direction of research in mathematics lay in the solution of the partial differential equations emerging from physical problems. The partial differential equations that were the proper focus of mathematicians were those of the same form as the equations of mechanics.[41]

While Poisson turned his attention to a field of experimental physics that had resisted mathematization for decades, André Marie Ampère mathematized a completely new and unexpected phenomenon, the connection discovered by Hans Christian Oersted between current electricity, magnetism, and mechanical force. Ampère was initially a member of the mathematical division of the Institut and had published in the growing controversy over the foundations of the calculus. In 1820, he turned to the experimental investigation of Oersted's results.[42] Ampère was a mathematician blessed with manual dexterity and an interest in metaphysical speculations as well as experiments. He assisted Arago in demonstrating Oersted's experiment to the Académie and within the month reported on his own experiments.[43]

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40 This assessment of Poisson is also shared by Grattan-Guinness, *Convolutions*, and by Louis L. Bucciarelli, "Poisson and the Mechanics of Elastic Surfaces," in *Siméon-Denis Poisson,* Michel Métivier, Pierre Costabel and Pierre Dugac, eds. (Paris: Vrin, 1981), 75–104.

41 See Costabel, "Poisson, Siméon-Denis," *Dict. Sci. Bio.,* Supplementary vol., 482.

42 See, Judith Grabiner, *Origins*, chap., 5. Ampère's early publications in mathematics were in the theory of functions, the integration of partial differential equations, the calculus of variations and on doing without the "infinitely small" in the calculus. On this last problem, see Thierry Guitard, "La querelle des infiniments petits à l'École Polytechnique au xix^{e} siècle," *Hist. Sci.* (1986): 1–61, 5–21.

43 See Ampère, "De l'action mutuelle des deux courans électriques," *Ann. Chim.* 15 (1820): 59–76. His experiments on current-carrying wires were announced in Ampère, "Note

Oersted had demonstrated the reciprocity of the mechanical effect of a current-carrying wire on a magnet and of a magnet on a current-carrying wire. Ampère then speculated that two current-carrying wires might mechanically affect one another, then demonstrated the parameters delimiting their interactions. In the middle of his series of experiments, in which Ampère responded to the work of other experimentalists, including Biot and Savart, and Michael Faraday, he produced his law of force between two current elements, namely,

$$F = r^{-n}\{\sin\alpha \sin\beta \sin\gamma + k \cos\alpha \cos\beta\} ii' \, ds \, ds'$$

where ds and ds' were the current-carrying elements, i and i' were the "strengths" of the respective current, r was the distance between the current elements, α, β were the angles between the current elements and the line joining them, and γ was the angle between the plane of the connecting line and one of the current elements.[44] There was a difference between this force law and algebraic expressions of the results of experiments previously put into mathematical form. In previous cases the expression was an algebraic reflection of the experimental results. Ampère's expression was in differential form and extrapolated, mathematically, beyond the geometry of the laboratory. The mathematician already operated to abstract a generalized law of force from the realities of the research laboratory.[45] To determine n and k, Ampère continued to experiment. In an analogy with gravitation Ampère speculated that $n = 2$. Experiments seemed to confirm this analogy.[46] Experiment also indicated that $k = -1/2$.

His continuing experiments were accompanied by non-mathematical speculations on the possible action of the electric fluid within the wires and across the

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sur un mémoire lu à Académie Royale des Sciences, 4 Décembre 1820," [Sur l'action mutuelle de deux éléments de courans électriques,] *J. Phys.* (1820): 226–230. Details of Ampère's involvement and experiments are in Christine Blondel, *A.-M. Ampère et la création de l'électrodynamique* (Paris: Bibliothèque Nationale, 1982). However, see the exchange between Blondel, "Ampère and the Programming of Research," *Isis,* 76 (1985): 559–561 and Pearce Williams' reply. Whether or not Ampère's work was programmatic or, at a crucial stage, depended on chance does not affect the argument here. See also Alfred Kastler, "Ampère et les lois de l'électrodynamique," *Rev. Hist. Sci.* 30 (1977): 143–157. See also J. R. Hoffmann *André-Marie Ampère* (Oxford: Blackwell, 1995).

44 Ampère, "Mémoire sur la détermination de la formule qui représente l'action mutuelle de deux portions infiniment petits de conducteurs voltaiques," *Ann. Chim.* 20 (1822): 398–421.

45 Ampère was not the first mathematician to perform his own experiments. Étienne Louis Malus did his own experiments on refraction then polarization and developed his own mathematical exploration of the phenomena. Jean Baptiste Biot first published papers on the calculus and a companion volume to the mathematics of Laplace, *Mécanique Célèste* before turning to experimental physics. Biot's first experimental work was in electricity, sound and then in 1804, heat conduction.

46 For a discussion see Kastler, "Ampère et l'électrodynamique," 153–154.

space between the conductors. However Ampère did not connect these hypotheses with the results of his continuing experiments. When he drew his experimental results together to develop a mathematical theory of "electrodynamics," he used none of these ruminations. Ampère's final statement on this stage in his electrodynamic work was presented in the mathematically elegant memoir of 1823.[47] His mathematics was based on a series of experimental results together with the results encapsulated in his initial force law.

Ampère was at pains to separate his approach to the mathematization of electrodynamics from that of Biot and Savart. The latter had their own mathematical forms based on keeping intact the Laplacian approach to the mathematization of experimental physics and retaining the distinction between magnetism and electricity.[48] To obtain results for whole circuits, rather than circuit-elements Ampère needed to use both line and surface integrals. This was the first time that such mathematical techniques were required to bring a domain of experimental physics into mathematical form.[49]

In its final form the reader was presented with two papers, rather than one. The first part of the memoir presented the phenomena and the non-mathematical physical model to explain the phenomena in terms of molecular forces and molecular currents.[50] The second part was a generalized mathematical theory devoid of any physical modelling or processes. The molecular models did not enter into either the setting up of the problem nor in choosing how the mathematical development of the problem might proceed. The two approaches existed fully developed in their own spheres, both clear, both worked out in detail: Metaphysics and experiment are contained in the first half, experimental results expressed algebraically as the starting point for the mathematical second. The differences of Ampère's from previous presentations in this tradition lay not in the simultaneous production and presentation of a series of new experimental results, interpreted physically in non-mathematical terms, together with their integration into an expanding domain of mathematics. His uniqueness lay in his experiments and in the sophistication of his mathematical techniques. The ultimate result does not read as theoretical

47 Ampère, "Mémoire sur la théorie mathématique des phénomènes électrodynamiques uniquement déduite de l'expérience," *Mém. Acad. Sci., Paris* 6 (1823) [1827]: 175–388.

48 See Biot and Félix Savart, "Sur l'aimantation imprimée aux métaux par l'électricité en mouvement," *J. Savants* (1821): 221–235. However, it is difficult to see Ampère's work as Laplacian, as in Christine Blondel, "Vision physique éthérienne, mathématicien laplacien: l'électrodynamique d'Ampère," *Rev. Hist. Sci.* 43 (1990): 123–137. It was in this contested context that Poisson produced his mathematical papers on magnetism. Poisson, "Deux mémoires sur la théorie du magnétisme," *Mém. Acad. Sci., Paris* 5 (1822): 247–338, 488–533.

49 For a discussion of the mathematics of Ampère's work see, Grattan-Guinness, "Lines of Mathematical Thought in the Electrodynamics of Ampère," *Physis* 28 (1991): 115–129.

50 Ampère, "Sur la théorie mathématique des phénomènes électrodynamiques," 175-200.

physics with the integration of theoretical ideas, mathematically expressed and developed to reveal physical significancies. Physics was again used to explore mathematically important integrals and techniques.

J. R. Hoffmann rightly emphasizes the need to not take this final presentation as indicative of how Ampère's work actually proceeded. However, he sees Ampère as having "broader ambitions" of producing a physical theory based on oscillation in the etherial fluid. This might be true, but Ampère's physical imagery remained speculative and non-mathematical, only experimental results were extrapolated to a mathematical form. While his experimental work and the metaphysical gloss are familiar from eighteenth-century predecessors, there is no sense of the development of a physical point of view in the mathematical half of this massive paper.[51]

Historians tend to focus exclusively either on the physical concepts or on the mathematics in Ampère's work. Historians of physics examine the physical ideas in Ampère's experimental papers and the first half of his 1823 paper where he used physical models to explain the mutual interactions of the current-carrying wires.[52] Historians then assume that Ampère must be doing physics also in the second half of that paper. However, only his experimental results are used to set up the initial statement of his mathematical problem and no physical ideas guide his solutions to those problems. His experimentally-deduced force law allowed Ampère to argue that his equations were of the same type as those of mechanics. He could thus apply all of the techniques of that mathematical trade to their solution. However, as the mathematics developed, neither his molecular models nor his experiments impinged on the direction of that development. We are still some way from theoretical physics.

Heat

Both historians of mathematics and physics claim Joseph Fourier's work in the conduction of heat for their disciplines. And, his work was indeed significant for mathematics immediately, theoretical physics decades later.[53] Our concern here is how Fourier's work on heat conduction fitted into the disciplines of physics and/or mathematics in the early decades of nineteenth-century France. Its later absorption

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51 J. R. Hoffmann, "Ampère, Electrodynamics and Experimental Evidence," *Osiris,* 3 (1989): 45-76. For Ampère's ideas on the ether, see Keith Cavena, "Ampère, the Etherians and the Oersted Connection," *Brit. J. Hist. Sci.* 13 (1980): 121–138.

52 See, Theodore M. Brown, "The Electric Current in Early Nineteenth-century French Physics," *Hist. Stud. Phys. Sci.* 1 (1969): 61–104, and L. Pearce Williams, "Ampère's Electrodynamic Molecular Model," *Contemporary Physics* 4 (1962): 113–123.

53 For a discussion of Fourier's work as mathematics in the first half of the nineteenth century see, Garber, "Reading Mathematics, Constructing Physics: Fourier and his Readers, 1822–1850," in *No Truth Except in the Details,* A. J. Kox and D. M. Siegel, eds. (Netherlands: Kluwer Academic, 1995): 31–54.

into other disciplines will be considered elsewhere. Given the standards of practice of French mathematicians in the first two decades of the nineteenth century and the reactions of his colleagues, it makes more historical sense to place Fourier's work in the history of mathematics. To judge how far Fourier was from being a theoretical physicist, we need to consider the controversies surrounding Fourier series when they first appeared and examine changes in the calculus that came to fruition in the 1820s. We must, therefore explore the history of French mathematics in two crucial decades and Fourier's work in development of the first, logically defensible form of the calculus.

Historians of physics have retrospectively claimed Fourier for their discipline from the importance of his mathematics for later developments within physics. They have argued that his influence was crucial for the development of concepts and as a means of expressing all manner of wave phenomena and arbitrary functions that occur in physics and engineering. This later importance does not, however, tell us how Fourier's work on heat or Fourier analysis was regarded and used in the first decades of the nineteenth century. So important did Fourier's methods and the concept of flux become for physicists that they and some historians have inverted his purposes. The physics that was retrospectively seen within Fourier's work by middle and late nineteenth-century physics must have been his intended object of study in the first place.

Fourier followed the standard practice of early nineteenth-century mathematicians by choosing a physical problem to explore a mathematical domain, arbitrary functions. His initial attempt at transforming experimental results on the conduction of heat along a bar of metal into mathematical form was using a mechanical model. He abandoned this approach, as it was only successful in some particular mathematical cases and a mathematical failure in the general one.[54]

Fourier's work also has been seen as creating a new theory of heat within physics, conceptually beyond that of earlier caloric theories.[55] However, as most historians of physics agree, none of the molecular or other processes Fourier used for the conduction of heat ever entered into his mathematical analysis of the problem. As Fourier noted, "if the mathematical laws which the effects of heat follows are carefully examined, it is seen that the certainty of these laws does not rest on any physical hypothesis."[56] Fourier repeated the claim that the principles of

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54 For an analysis of this initial attempt, its eighteenth-century mathematical roots and its shortcomings, see Grattan-Guinness and Ravetz, *Joseph Fourier,* chap. 3, 36–81. See also Amy Dahan, "J. Fourier: L'élaboration de la théorie analytique de la chaleur," *Sci. Techn. Persp.* 1 (1981): 7.1–7.41.

55 Robert Marc Friedman, "The Creation of a New Science: Joseph Fourier's Analytical Theory of Heat," *Hist. Stud. Phys. Sci.* 8 (1977): 73–100.

56 Fourier, "Théorie du mouvement de la chaleur dans les corps solides," *Mém. Acad. Sci., Paris* 4 (1819–1820) [1824]: 185–555, 192. This appears also in Fourier, *Analytical Theory,* 40–41. Fourier, "Théorie," listed here is the first part of Fourier's memoir

his work depended upon, "a very small number of primary facts, the causes of which are not considered by geometers, but which they admit as the result of common observations confirmed by all experiment."[57] Immediately following this disclaimer, Fourier gave a descriptive, molecular explanation of the conduction process, one of radiation from molecule to molecule. But he did not put this physical process into a generalizable mathematical form. Fourier abandoned the approach. However, physical descriptions survived, scattered through the text although they remained unrelated to the mathematical analysis of the problem. Fourier published a separate account of the physical theory of heat conduction and radiation.[58]

On another level, historians of physics see Fourier as developing the important concept of flux and giving it particular meaning in his theory of heat.[59] While flux became important for later developments within physics, Fourier himself neither named it, nor attached any physical significance to it.[60] Fourier established the equation for the motion of heat on the basis of the principle of "the uniform, linear movement of heat." He went to some lengths to demonstrate that the notion we name flux emerged from experimental sources that supported his analysis of the flow of heat into, then out of, a thin slab and the losses across its thickness. His reasons for his detailed discussion were mathematical, "because the neglect of it has been the first obstacle to the establishment of the mathematics." He argued further that if we did not make a complete analysis of the elements of the problem, "we should obtain an inhomogeneous equation, and, *a fortiori,* we should not be able to form the equations which express the movement of heat in more complex cases."[61] Within the context of his mathematics, Fourier was careful to avoid definitions that were not phenomenological or deduced directly from experiment. All this has been said before in the context of connecting Fourier to the later, intellectual

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presented to the Institut in 1811. The second part was published as, Fourier, "Suite du mémoire intitulé: Théorie du mouvement de la chaleur dans les corps solides," same journal, (1821–1822) [1826]: 153–246. By the time these papers appeared their contents were already published as Fourier, *Théorie analytique de la chaleur,* (Paris, 1822), trans. by Alexander Freeman as Fourier, *The Analytical Theory of Heat.* This translation follows the original closely and the translator's comments are carefully separated from the author's original. Such is not the case in the version of this work in Fourier's collected works.

57 Fourier, *Analytical Theory of Heat,* 6.

58 Fourier, "Questions sur la théorie-physique de la chaleur rayonnante," *Ann. chim. phys.* 6 (1817): 259–303.

59 John Herivel, *Joseph Fourier: The Man and the Physicist* (Oxford: Clarendon Press, 1975) chap. 9, emphasizes the importance of this as a contribution to physics. See also Friedman, "Fourier."

60 The first time I have seen the name used was by Philip Kelland.

61 Fourier, *Analytical Theory,* 59.

movement of positivism. However, Fourier was echoing what was a commonality amongst French mathematicians of this era.

Much has also been made of the experiments Fourier performed and their integration into his mathematical analysis. However, his experiments were used to establish the initial equations of motion for heat under particular circumstances, that is, Fourier used experiment in the same ways as his mathematical contemporaries. In fact the experiments Fourier used to establish his equation were a disparate lot and he offered no physical reasons for doing them. However, they did allow him in ordered mathematical fashion to generalize his mathematical equations of motion for heat.[62] These experiments were described briefly, and no results were given in the text. In places where Fourier worked out the analysis fully and the form of the function could be compared with experiment, the latter were mentioned as confirming the results of the analysis. Again the descriptions were brief with no data.[63]

Fourier used experiment in the same way as Poisson, Laplace and other mathematicians of this era. In Fourier's case they were more critical because he built his mathematical case through a series of particular examples. Usually mathematicians developed their mathematical case by trying for the most general possible solution of the partial differential equations, then considering a series of particular solutions. Fourier reversed this usual practice. He was then compelled to argue that his method of going from the particular to the general was indeed mathematically legitimate.[64]

All this was necessary because, while Fourier could demonstrate in particular cases that his trigonometric series solutions for the equation of motion for heat were mathematically defensible, he could not do so in the general case. Fourier used infinite series of trigonometric functions to represent arbitrary functions. His proofs for their convergence behavior and hence legitimacy as continuous functions that were differentiable and integrable were questionable. Since the vibrating string controversy, these particular series had been banished from analysis as functions that did not behave well. Lagrange's seemingly definitive solution of the wave equation managed to avoid their overt use. In insisting on using them, Fourier went beyond the boundaries of accepted mathematical practice.[65]

Fourier also called upon experiments in other ways. Nature still guaranteed

62 The examples used are a row of disparate bodies, generalized into a line, a ring for the two-dimensional equilibrium case, and various shaped bars for the three-dimensional cases.

63 Fourier cited an experiment on a metal ring heated at different points and its temperature taken at other points and claimed that these experiments "fully confirm" his mathematics. Fourier, *Analytical Theory,* chap. 2, p. 90.

64 Fourier, *Analytical Theory*, 85.

65 All of Fourier's work on heat theory was completed before Cauchy began his lectures on the calculus where he replaced the older calculus with new definitions of the derivative,

French mathematicians that solutions existed. Other mathematicians used experimental results as confirmation of their mathematical solutions to particular mathematical cases, without being careful about matching the mathematical to the idealized experimental case. Fourier needed a close match between experiment and mathematical derivation. Nature was the only guarantor of the validity of his mathematical work. He justified his procedure in two different ways, firstly with the general, unremarkable assertion that, "nature is the most fertile source of mathematical discoveries." The second was his concentration on the particular cases worked out after his development of Fourier series, that is, after stating the general, important mathematical consequences of his work for the expression of arbitrary functions. These particular cases were all physically significant. He explicitly defined and defended this method against the usual mathematical approach of going through all mathematically particular cases without regard to their physical significance.[66]

> That not only does Fourier develop the general equations for the propagation of heat, then follows the solutions out but gives these solutions in a form which eases numerical application. He [Fourier] regards this interpretation of the calculus as a degree of perfection that is necessary to obtain in all applications of analysis to the natural sciences.[67]

If this is not done "the truth is not the less hidden in the formulae of analysis than it was in the physical questions themselves." Fourier also made claims for the importance of the flow of heat to the physics of the earth and the economy. His own attention to such problems were as particular examples of his mathematical methods. In the extended abstract that Fourier published in 1816 and 1817 of his prize essay of 1811, he did not dwell on those physical aspects of his theory that were important later. After describing the content of the various sections, he focussed on the mathematical accomplishments of the author.[68]

Fourier's title to his work, *Théorie analytique de la chaleur,* and his claim that rational mechanics was his model, clinch his place in early nineteenth-century mathematics. While denying that the laws of motion for heat were reducible to

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continuity, and the integral that he then used to prove the simple and not so simple results of the calculus that others had taken for granted. For a full discussion of the mathematical difficulties of Fourier's work, see Grattan-Guinness and Ravetz, *Fourier,* and Grattan-Guinness, *Convolutions,* chap. 9, 597–602. Cauchy enters in chapter 10.

66 For particular examples See, Fourier, "Sur la température des habitations et sur le mouvement varié de la chaleur dans les prismes rectangulaires," *Bull. Soc. Philo.* (1818): 1–11, and "Extrait d'une mémoire sur le refroidissement séculaire du globe terrestre," *Ann. Chem. Phys.* 13 (1820): 418–438. More papers by Fourier on the secular cooling of the earth appeared in the 1820s after the publication of his *Analytical Theory.*

67 Fourier, "Extrait de théorie de la chaleur," *Ann. Chim. Phys.* 3 (1817): 350–375.

68 Fourier, "Extrait," and Fourier, "Note sur la chaleur rayonnante," *Ann. Chim. Phys.* 4 (1817): 259–303.

those of mechanics, he may well have had in mind Poisson's criticisms and the latter's mechanical model for the flow of heat rather than rational mechanics that he referred to repeatedly as a model. What he accomplished for heat had been done already for mechanics. Even while the forms of the fundamental equations were quite different both domains of physics were now encompassed by analysis.[69] Rational mechanics appeared in analogy to his work in several places and as an exemplar throughout his preliminary discourse. Although the Laplacian model of matter and the mathematical form for the equations of motion and other fundamental laws of mechanics might not apply specifically, rational mechanics was the model Fourier tried to imitate.

As recent research in the history of mathematics amply demonstrates, the solution of problems defined mathematics as a discipline in the eighteenth and early nineteenth centuries. The solutions of problems taken from mechanics were used as arguments for the mathematical surety of the calculus before mathematicians secured any rigorous foundation for the calculus. Problems grounded in reality that the calculus could solve were guarantees of its mathematical validity. This was the level of the usual response to criticisms of the calculus, especially that of Bishop Berkeley.[70] On this score Fourier echoed his contemporaries to reinforce the validity of the shaky foundations of his own mathematics.

> The profound study of nature is the most fertile source of mathematical discoveries. Not only has this study, in offering a determinate object to investigate, the advantage of excluding vague questions and calculations without issue; it is besides a sure method of forming analysis itself, and of discovering the elements which it concerns us to know, and which natural source we ought always to preserve.[71]

Fourier went on to observe that the analytical equations first introduced into mathematics by Descartes (the calculus), unknown to the ancient geometers, extend to all natural phenomena, and,

> There cannot be a language more universal and more simple, more free from errors and from obscurities, that is to say more worthy to express the invariable relations of natural things.

The reactions of Fourier's contemporaries to his work were to his mathematics, complicated by the disputes already brewing over the proper foundations for the calculus. Physical problems and the solutions to the equations they generated were the battleground for the very soul of the calculus. By the time Fourier's work in

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69 Clifford Truesdell also notes this in *The Tragi-comical History of Thermodynamics, 1822–1854* (New York: Springer-Verlag, 1980), 51–58, 53. He views Fourier as a mathematician.

70 See Grattan-Guinness, *Development,* and *From Calculus to Set Theory*, and Judith Grabiner, *Origins.*

71 Fourier, *Analytical Theory,* 7.

heat theory appeared, a rigorous foundation for the calculus was becoming urgent. His work allowed other mathematicians to draw clearer battle lines. In his initial, incomplete paper of 1807, Fourier challenged standard mathematical practice. He used the separation of variables to solve partial differential equations of the second order, as well as trigonometric series in mathematical situations where functional alternatives were the more normal approach.[72] Before Fourier's paper could appear in print, its mathematical lacunae were pointed out by Poisson. While noting the incorrectness of the physical foundations of Fourier's physics, Poisson detailed the problems with his mathematics.[73] Lagrange saw Fourier's mathematical approach as simply unacceptable. This was hardly surprising. Lagrange had also developed the only defensible formulation of the calculus whose canons of practice Fourier so clearly violated.

Fourier responded to his critics both publicly and through Laplace. The issues centered upon the ultimate legitimacy of Fourier's mathematics in its general form and his competency as a mathematician. Fourier bested his major critic, Poisson, even though Poisson persisted with his own version of a mathematics of heat published years after Fourier's death.[74] He refused to answer Biot in public. Fourier had gained some crucial insight into the mathematical expression of the temperature distribution along a bar from Biot's experiments and Biot's mathematical expression of those results. Fourier did not acknowledge this debt. Ungracious as this was, Biot's expression of his results was stated as the sum of two exponential functions, with the remark that any mathematical theory of the phenomenon would have to take the complexity of his data into account. Biot gave no details of how he obtained this expression. This was the solution to the problem whose original equation of motion was Fourier's quarry. Biot was not, in Fourier's eyes, a mathematician, only an experimentalist supplying grist for his mathematical mill.[75] In his first paper Biot described in the vernacular the equation for the conduction of

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72 For details of Fourier's mathematical methods and how they differed from his colleagues, see Grattan-Guinness and Ravetz, *Fourier.*

73 Poisson, "Mémoire sur la propagation de la chaleur dans les corps solides," *Bull. Soc. Philo.* (1807): 112–116. These arguments are repeated in Poisson, "Sur la distribution de la chaleur dans les corps solides," *J. Phys.* 80 (1815): 434–441. Both were published before Fourier's text. For details of this criticism and Fourier's reaction, see Grattan-Guinness and Ravetz, *Fourier* and Herivel and Costabel, *Joseph Fourier face aux objections contre sa théorie de la chaleur, lettres inédits, 1808–1816* (Paris: Bibliothèque Nationale, 1980).

74 See Grattan-Guinness and Ravetz, *Fourier,* 463–471 for the derivative character of much of Poisson's mathematics in this area.

75 Biot wrote both experimental and mathematical papers on the conduction of heat in 1804. See, Biot, "Mémoire sur la propagation de la chaleur, et sur un moyen simple et exact de mesurer les hautes températures," *J. des Mines* 17 (1804): 203–224, and, Biot, "Sur la loi mathématique de la propagation de la chaleur," *Bull. Soc. Philo.* 3 (1804): 215–216.

heat along, and the radiation of heat from, a bar in thermal equilibrium. The equation itself was not written down or solved until the second, separate mathematical paper.[76]

Despite criticism and delays in the publication of his early work, Fourier extended his discussion of the expression of arbitrary functions in terms of trigonometric series, then developed their expression in integral form, that is, developed Fourier analysis. All of this mathematics lay in the hands of the secretary of the Institut, then of the Académie des Sciences, until the 1820s, and, as Fourier worked his way into the inner circles of power within science. In 1822 the third version of his *Analytical Theory* was published in the same year that he was elected secrétaire perpétuel of the Académie. This publication was his first opportunity to give an extended account of his work, and to defend it publicly.[77]

Given the practices of mathematicians in early nineteenth-century France, we must place Fourier's work within the history of that discipline. Placing Fourier's work on heat in the history of mathematics makes his use of his own experimental work more understandable. This does not detract from the obvious value of the work nor its position of influence in the later development of physics, mathematics, and engineering. In examining reactions to Fourier's heat theory papers, we can see patterns in the discussions that reveal what aspects of that theory were important to his contemporaries. Fourier's mathematics were dissected. Equally important are those issues passed over in silence, that is his experimental work.

The other aspect of early nineteenth-century French work on heat that became crucial for the later development of physics was Sadi Carnot's examination of the heat engine. This was passed over in silence by most of his contemporaries in physics and mathematics for two reasons. The first was political. As a son of Lazare Carnot, his work was without social grounding in the governmental institutions of restoration France. It could be political folly simply to notice it. The other was that he was an engineer and he addressed engineers and the problems they faced in understanding steam engines rather than in producing a mathematical theory of heat.

He followed the tradition of French engineering in reducing his steam engine to an idealized, general form of a heat engine. Historians have seen Carnot's understanding of the cyclic nature of the operation of the heat engine as derived from his father's work in mechanics. However, his understanding of what was

76 Truesdell, *The Tragicomical History*, 51, suggests that Biot was not capable of thinking on this level and that his equation was suggested by Laplace. There are indications that in earlier mathematical problems, Biot was guided in his choice by Lacroix. His solution of a problem in the partial differential equation of sound is pendantic. See Biot, "Sur l'intégration des équations différentielles partielles et sur les surfaces vibrantes," *Mém. Institut* 4 (1802): 21–111. See also Eugene Frankel, "Career Making."

77 The publication of the 1811 prize paper was predictably slow. It oozed from the presses of the Académie in two parts, appearing in 1824 and 1826.

physically happening along each of the four stages of the cycle was his own. And his explanation of these stages was non-mathematical and in terms of caloric, and its conservation in the complete cycle of the engine. The fall of caloric from a higher to a lower temperature made the extraction of mechanical work possible. While Carnot's description of the physical processes involved in completing a cycle of his ideal heat engine was clear, his goals were those of an engineer. His aim was an expression for the efficiency of such an idealized system, and understanding the operating conditions for real steam engines to maximize their performance. Carnot also needed to make plausible his assumption that the operation of his idealized heat engine extended to all types of working substances. All his results on the behavior of the specific heats of gases were deduced from known experiments. The construction of his mathematical derivation for the temperature dependence of the "motive power" of heat was relegated to a long footnote. This derivation came directly from known gas laws and was completely independent of his assumptions about the nature of heat. In the 1830s yet another engineer, Émile Clapyeron constructed a mathematized theory based on Carnot's analysis of the heat-engine cycle, and experiments on the specific heats of gases. Clapyeron specifically based his mathematics only on well established hypotheses and distanced his work from that of Lavoisier and Laplace. Neither work stirred the imagination of other mathematicians or physicists until the late 1840s.[78]

Light and Elasticity

The last historical problem set we will consider is on the changes in ideas about the nature of light that engendered a complex of mathematical theories in elasticity. Some of these mathematical theories became important in the middle decades of the nineteenth century as physicists adopted mathematics as the language of theory.

Until the early nineteenth century the phenomena of light were assumed as empirically understood, although physical explanations of its nature and interaction with matter remained problematical. In the eighteenth century, interpretations of the nature of light remained as contested as those of caloric. The primary assumptions were either that light was a stream of particles, or a disturbance propagated through a substance that pervaded space. These hypotheses left the details of the action of light to subsidiary assumptions, and all manner of tropes, similes, and metaphors.[79] The principal area of difficulty for both sets of physical,

78 Sadi Carnot *Réflexions sur la puissance motrice du feu* (Paris: Vrin reprint of 1826 edition, 1979). Émile Clapyeron, "Mémoire sur la puissance motrice de la chaleur," *J. École Poly.* 16 (1834): 153–190. Truesdell, *Tragicomical* discusses Clapyeron's mathematics. See also Jean Dayantis, "Carnot, Clapyeron et la théorie calorique au dix-neuvième siècle," *Rev. Quest. Sci.* 164 (1993): 105–130.

79 The ranges of such physical speculations have recently been explored by Casper Hakfoort, *Optics in the Age of Euler*, and Geoffrey Cantor, *Optics after Newton.*

vernacular theories lay in the phenomena where light and matter interacted, namely double refraction and to a lesser extent simple refraction. Laplace's political power within the Institut in the first decade of the nineteenth century and his analytical interest in light, as an offshoot of his work in celestial mechanics, guaranteed that the problem areas of of light would become the subjects of prize essays. The subject of the prize essay made public in December 1807 was double refraction.[80]

In 1810 the work of Etienne-Louis Malus was crowned. His prize essay included the detection, experimental establishment, and analysis of a new phenomenon, polarization. This new phenomenon complicated the explanatory picture and guaranteed further prize questions on the same subject. Malus had presented his experiments on the refractive power of opaque crystals to the Institut one month before the prize problem was announced. In working further on Iceland spar, he discovered the polarization of reflected light.[81] As Buchwald has amply demonstrated, Malus' experiments changed experimental optics. Malus worked with sophisticated instrumentation, reported, then analyzed his results carefully.[82]

If we explore his long prize essay, we find that it falls into three sections, each one separate from the others. The first section is mathematical on "Des questions d'optiques qui dépendent de la géométrie." This section consisted of the mathematics of known phenomena dependent on light being propagated in straight lines. When light is reflected or refracted from a surface, the equation for the system of rays emerging from the encounter was

$$\frac{x-x'}{m}=\frac{y-y'}{n}=\frac{z-z'}{o}$$

where m, n, o were "arbitrary functions of x', y', z'." So far no physics. Malus was working towards the general problem of considering,

> a system of right lines emanating from all points of a curved surface, that obey any analytical law whatsoever, this system being regarded as the locus of the intersection of two systems of developed surfaces.[83]

Malus proceeded to treat all the mathematical cases he could and expressed them in general functional form. In the case of refraction, Malus used the notion that at the surface light was turned through an angle by a force perpendicular to that surface. This assumption reduced optics to the mathematics of mechanics. To

80 Events leading up to this prize problem and work done previously by William Hyde Wollaston on double refraction are recounted in Jed Buchwald, *The Rise of the Wave Theory of Light* (Chicago: University of Chicago Press, 1989) chaps., 1, 2.

81 The phenomenon was announced in 1809, Malus, "Sur une propriété de la lumière refléchie par les corps diaphanes," *Bull. Soc. Philo.* 1 (1809): 266-269.

82 Malus, like Coulomb who achieved the same transformation in the study of electricity, was a military engineer. For the details of Malus' experiments, see Buchwald, *Rise,* 31–36.

83 Malus, "Traité d'Optiques," *Mém. Institut, Paris* 2 (1811): 214–302, 221.

Malus it was irrelevant how the force operated on the light to cause it to deviate from its straight-line path. It was only "necessary to calculate its effects."[84] Malus handled double refraction in the same way.[85] The physical explanation of refraction and polarization existed separately and escaped "all quantitative determination." Malus relied on the speculative construction of short-range attractive and repulsive forces acting close to the surface of the refracting body as physical, explanatory devices.[86] He did not put these ideas into analytical form, nor could he in principle subject them to experimental examination.[87]

Experiments on polarization also became the ground on which Biot made his professional mark. Initially Biot worked with Arago on the experimental determination of the indices of refraction of various gases.[88] He claimed to deduce results only trivially different from those of Laplace in his *Mécanique Célèste.* The mathematical part of the paper showed that Biot had developed his own analytical expression for the refracting power of gases and traced through Laplace's work on the refraction of light for formulae corresponding to the conditions of his experiments. However, the analytical derivation was in terms of rays of light and their angular changes of path as they pass through different media. The vernacular description of what occurred as the light passed through the media was in terms of the forces causing these deviations. These forces were not connected to the analytical derivations from his experiments.[89]

Biot continued with a series of careful, quantitative experiments coupled with long analytical deductions from his results. Yet, even while explicitly denying any use of hypotheses, his papers on light are peppered with them.[90] In the mathematical exploration of the results of his experiments on the polarization of light in birefringent crystals, Biot expressed the equation of motion for the changes in the

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84 Buchwald notes that this section contains no physics and is purely analytical, Buchwald, *Rise,* 38.

85 Malus, "Théorie de la double réfraction," in "Traité," 303–508.

86 See Malus, "Sur une propriété des forces répulsives qui agissent sur la lumière," *Mém. Soc. Arcueil* 2 (1809): 143–158.

87 For details of Malus' work, see A. Chappert, *Etienne-Louis Malus (1775–1812) et la théorie corpusculaire de la lumière* (Paris: Vrin, 1977), p. 124.

88 Biot and Arago, "Sur les affinités des corps pour la lumière, et particulièrement sur les forces réfringentes des différents gaz," *Mém. Institut* 7 (1806): 301–387. The analytical part of the paper is "Part II," 363–387.

89 For the cutthroat conditions in the competitive domain of experimental physics and in particular the competition between Arago and Biot, see Buchwald, *Rise,* 79–88. Arago did not follow the new trend toward experimental physics and quantification using complicated instrumentation. Biot chose an aggressive prosecution of this new methodology for experimental optics.

90 For example, see Biot, "Sur un nouveau genre d'oscillation que les molécules de la lumière éprouvent en traversant certains cristaux," *Mém. Institut* 12 (1812) [1816]: 1–371, p. 60.

axis of polarization of the light as

$$\frac{dx^2}{dt^2} = \phi(i - x) - \phi_1(i - x),$$

where ϕ is the force and x the angle of the axis of polarization at time t. This problem was worked out in its most general form only to be truncated into simple harmonic motion to obtain numerical results that could be compared with experiment.

In the introduction to this long paper, Biot claimed that his experiments showed the successions of oscillations of the "luminous molecules." The equation of motion of the planes of polarization were the equations of motion of these particles of light. After assuming this he developed a non-mathematical, physical theory of the changes undergone by the particles of light. This physical theory was devoid of mathematical, although not of logical, reasoning or clarity of concept. While his experiments on the polarisation of light in birefringent crystals were careful and reported at length in the *Mémoires* of the Institut, his theoretical account was less than successful. The experiments recounted the changes in reflected and transmitted light in birefringent crystals for all the colors of the rainbow. These results and the differences in the colors of the reflected and two refracted rays were important in his attempt to draw together the Laplacian theory of matter and light; centre of force molecules acting on luminous particles that set up oscillations in those luminous particles.[91] Biot combined the particulate theory of matter and of light that produced waves. However, this phenomenon was more easily explained by waves than particles. His assumption was that the axis of polarization of the light was gradually changed by the action of the molecular forces as the light particles traveled through the medium.[92]

There were other conceptual problems. He implicitly assumed that the equation of motion of the polarization axis was the same as the equation of motion of the luminous molecules. Biot then investigated this simple harmonic motion for a single molecule. However, his comparisons were between the mathematics of a microscopic structure and experiments of macro-phenomena. No consideration was given as to how this micro-motion led to the macroscopic effect. He assumed they were the same. There were problems as well in the mathematical part of the paper. The wave motions of the luminous molecules were reduced to the simplest of motions without considering how this motion led to an ordinary and extraordinary ray within the crystal. Again we have both a physical theory and a

91 See Biot, "Sur une nouvelle application de la théorie des oscillations de la lumière," *Mém. Institut* (1812) [1816]: 1–38.

92 Biot continued to develop this vernacular, physical theory in Biot, *Recherches expérimentales et mathématiques sur les mouvements des molécules de la lumière* (Paris: 1814), and, "Nouvelles expériences sur le développement des forces polarisantes par la compression dans tous les sens des cristaux," *Ann. Chim. Phys.* 3 (1816): 386–394.

mathematical one existing in their separate spheres of explanation. Significantly, Biot tried to bring the mathematical theory into a form that could be compared legitimately with his experimental results.

Into this battleground between the Laplacians in the Institut strode another outsider, also an engineer, Augustin Fresnel. Fresnel entered into a field already occupied and in terms of the early nineteenth century well covered both mathematically and physically. It would be rough going. He disturbed both mathematicians and experimental physicists by demonstrating the inadequacy of physical emission theories of light with a new phenomenon, diffraction.[93] The majority of the first six papers Fresnel wrote on diffraction remained unpublished during his lifetime. He claimed priority for his work in the *Annales de Chimie et Physique* under the auspices of Arago, who had recently joined its editorial board.[94] Arago had been squeezed out of the developing field of studies on polarized light and became a valuable ally for Fresnel in the Académie.

Fresnel showed considerable sophistication as an experimental physicist. He devised a number of simple yet ingenious experiments to demonstrate numerically as well as qualitatively the fallacies in the "Newtonians"' theories of light and to establish the plausibility of the wave theory.[95] He used the very methods, quantitative experiments that the Laplacians prided themselves as their innovation, to show the inconsistencies in their reasoning. His experiments on diffraction were quantitative and exact and deceptively simple. From them he deduced a series of algebraic relationships that illustrated in detail the wave nature of light. From simple geometry he deduced the fringe spacings of the interference waves, including those of different orders, from which he obtained a value for the wave length of light.[96] He repeated this and used a geometrical argument, based on the idea that points on the wave surface were the center of wave motion, as an explanation for

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93 Buchwald details Fresnel's early life and experiments on light and the circumstances under which they were performed in Buchwald *Rise,* chap. 5.

94 Fresnel, "Mémoire sur la diffraction de la lumière," *Ann. Chim. Phys.* 1 (1816): 239–281. Some of these early memoirs were deposited at the Institut and later the Académie des Sciences, and/or read by Arago at sessions of these institutions. All of his early papers were published in Fresnel, *Oeuvres complètes* H. de Senarmount, E. Verdet and L. Fresnel eds., (Paris: Imprimérie Impériale, 1866–1870), 3 vols. Fresnel's work on light is in the first two volumes.

95 The first occurrence of these arguments is in Fresnel, "Premier mémoire sur le diffraction de la lumière," *Oeuvres* vol., 1, 9–34, 10–15. The best presentation is in his prize essay, Fresnel, "Mémoire sur la diffraction de la lumière," *Mém. Acad. Sci. Paris* (1821–1822) [1826]: 339–487, 341–349 where he builds a case that the emission theory contradicts itself. Fresnel always referred, discretely, to the emission theory as "Newtonian."

96 Fresnel, "Premier mémoire sur la diffraction de la lumière," *Oeuvres* vol., 1, 9–34, 25-34.

reflection.[97] The above arguments were then repeated along with a group of improved measuring techniques.[98]

Fresnel was asked by the Académie committee considering the work that became his prize essay, to add an analytical part to it. They deemed it incomplete.[99] In his amended prize essay the mathematical work that accompanied his experiments included an expression for the wave length of light that did not depend on any particular model for the mode of action of light. The detailed development of his ideas on the nature of light itself was accomplished through his experiments and expressed clearly and argued logically in the vernacular. Fresnel did not actually develop a mathematical theory of the nature of light and its action based on his physical descriptions of its nature. The purpose of his mathematics was not to explore the nature of light or its propagation or interaction with matter but to replicate the expressions he had already deduced by geometry and algebra from his experiments. Fresnel demonstrated graphically how the maxima and minima of intensity represented by the fringes of his experiment occurred through the superposition of waves. From the geometry of his experiment, he deduced the algebraic relationship between the wavelength of the light, the distance of the fringe maximum from the center of the pattern of fringes, and the distance between the screen and the source of the diffraction pattern. The more sophisticated mathematics of the calculus replicated these earlier results.[100]

Analysis and the calculus did not bear much weight with Fresnel. In contrasting the Newtonian and his own theory of light, Fresnel put forward his criteria for choosing between the two. The choice must be made on the basis of the conceptual simplicity of the hypotheses necessary to explain the phenomena, not on the calculus, although the latter was more easily applied to the Newtonian than the wave theory of light. In invoking the economy of nature, Fresnel noted that "nature is not troubled by the difficulties of analysis," and appeared to produce the maximum number of phenomena through the minimum number of causes.[101]

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97 See Fresnel, "Complément au mémoire sur la diffraction," *Oeuvres*, vol. 1, 41-61.

98 Fresnel, "Mémoire sur la diffraction de la lumière," *Ann. Chim. Phys.* 1 (1816): 239–281; reprinted in *Oeuvres* vol. 1, 93–122, and in Fresnel, "Supplément au deuxième mémoire sur la diffraction de la lumière," in *Oeuvres* vol. 1, 131–170.

99 We'll take the charitable view that the committee, consisting of Arago, Biot, Gay-Lussac, Laplace, and Poisson, a majority of Laplacians, was taking as standard practice that both quantitative experiments and analytical development of their results be accomplished in a prize-winning essay. Malus and Biot had the effect of increasing expectations.

100 For the first statement of this, see Fresnel, "Mémoire sur la diffraction de la lumière," *Ann. Chim. Phys.* 1 (1816): 239–281, in *Oeuvres,* vol., 1, 89–122, 93–100. In the prize essay, Fresnel, "Mémoire sur la diffraction de la lumière," *Mém. Acad. Sci. Paris* 5 (1821–1822): 339–475, 361–364.

101 Fresnel, "Mémoire sur la diffraction de la lumière," *Mém. Acad. Sci. Paris* 5 (1821–1822): 339–475, 340.

And yet analysis had its uses. In the mathematical addition to his prize essay Fresnel took over the available mathematics of fluid motion. He also accepted the assumption that the velocity of propagation of the waves was the same for all wavelengths and that the intensity of the wave motion was proportional to the amplitudes of the motions of the particles. In addition to deducing the positions of the maxima and minima for the fringes produced by diffraction at a slit, Fresnel tried to find expressions for the intensity of the bright fringes. There was no attempt at a complete theory of light, that is, to follow the particular motions of the ether to produce the required intensities. Fresnel began with the total intensity of the light at point P. This was defined as the sum of the elementary waves spreading through P. The sum of all the "small motions at P," the actual intensity, was

$$\sqrt{\left(\int dz\cos\left(\pi z^2\frac{(a+b)}{ab\lambda}\right)\right)^2+\left(\int dz\sin\left(\pi z^2\frac{(a+b)}{ab\lambda}\right)\right)^2},$$

where the limits on the integration were zero and infinity. dz was any small distance along the primary wave and z was the distance of P from the source. λ was the wavelength of the light and a and b were the distances of the point on the wave front from the source and from the screen, respectively.[102]

Fresnel presented a confused mathematical argument. The goal of the mathematical investigation was limited to the replication of his experimental results so that he could undertake a direct comparison between the two. Biot and Malus had done the same but from the basis of a more detailed analytical development of the mathematical implications of their experiments. Much of the uniqueness attributed to Fresnel by Robert Silliman is therefore undermined. Both Malus and Biot did careful, numerate experiments, analyzed, and we could argue then developed their ideas mathematically with far more confidence and mathematical skill than Fresnel. However, we no longer accept the physical foundation for the analyses of those experiments. Their efforts have been undervalued. Because we do still accept the grounds for Fresnel's explanation, we overlook many of his shortcomings.[103] Fresnel did not have that kind of control or interest in the mathematical material. Both Buchwald and Nahum Kipnis postulate much more mathematical and physical coherence in Fresnel's work than his papers suggest. Many of the physical concepts made explicit in their analyses are implicit in Fresnel papers. His mathematics is fragmented and needs frequent interpolations on their part.[104] While Fresnel's

102 Fresnel added all the waves that reach P from points that were λ and $1/4\lambda$ and so on from each other. See Fresnel, "Note sur la théorie de la diffraction," *Oeuvres* vol. 1, 171–181, and "Mémoire sur la diffraction de la lumière," *Mém. Acad. Sci. Paris* (1819): 339–487, 383–407.

103 See Silliman, "Fresnel and the Emergence of Physics as a Discipline," *Hist. Stud. Phys. Sci.* 4 (1973): 137–162.

104 Buchwald, *Rise* and Kipnis, *History of the Principle of Interference* (Boston: Birkhäuser, 1991).

physical understanding of interference, the transverse nature of the ether waves and Huygens' principle became more confident, his grasp of the mathematics of wave motion was only ever partial.[105]

By transposing all the mathematics of the early nineteenth century into vector form, Buchwald actually diminishes the mathematical difficulties with which Fresnel grappled in trying to mold wave theory to his needs. Fresnel worked in Cartesian coordinates and algebraic equations, standard analytical equipment for that era. Given the mathematical state of wave theory in the early nineteenth century, it is not surprising that Fresnel's work in it was so fragmented. The problems of waves in elastic continua were problems that promised great rewards within rational mechanics for any mathematician able to bring them within the domain of the calculus. Even before Fresnel's prize essay on interference, Poisson and Sophie Germain were again addressing the conceptually difficult and mathematically challenging problem of the mathematics of continua.[106] Fresnel's work stimulated more mathematical work on elasticity in the 1820s that changed the context of his later work on double refraction. Fresnel did not use any of this recent mathematical work in his own construction of transverse waves in a now solid ether. He justified the necessity for transverse waves from the phenomena of polarization and constructed the ellipsoidal wavefront within birefringent crystal largely through physical arguments, geometrical illustrations and some calculus. While he used the device of theorems, the arguments in the theorems were a combination of physical theory developed logically in non-mathematical language joined in places to geometrical illustrations and algebraic extensions of his ideas. He again argued that his idea of the transverse wave emerged from experiment. If mathematicians' equations of motion of fluids did not agree with this hypothesis, it was because they are based on "mathematical abstraction." While these equations might represent some of the properties of fluids, the mechanical ideas they were based on did not take all the motions of actual elastic fluids into account.[107]

Contemporary assessments of Fresnel's work focussed on his experiments, not on his mathematics. As far as his contemporaries were concerned, his work belonged to the experimentally defined discipline of physics. Fresnel's early experiments stimulated mathematicians' elaborate explorations of wave motions in elastic solids. These explorations in turn were later used to explore physical theories of

105 See Humphrey Lloyd, "Report on the Progress and Present State of Physical Optics," *Rep. British Assoc.* (1834): 295–413, 387. See also Grattan-Guinness, "Review of Buchwald, *The Rise of the Wave Theory of Light*," *Ann. Sci.* (1989): 185.

106 For Poisson and Cauchy on waves see Grattan-Guinness *Convolutions*, vol. 2, chap. 10. For Sophie Germain see Louis L. Bucciarelli and Nancy Dworsky, *Sophie Germain: An Essay in the History of the Theory of Elasticity* (Dordrecht: Reidel, 1980). Some aspects of Poisson's work in the mathematics of elasticity is dealt with in Garber, "Poisson."

107 Fresnel, "Mémoire sur la double réfraction," *Mém. Acad. Sci. Paris,* 7 (1827) [1830]: 45–176, 80.

the ether.[108] The problem of transforming physical images into a mathematically expressed, physical theory of light was not solved easily. Cauchy developed the rational mechanics of the continuum that later was imbued with physical meaning.[109]

By the second decade of the nineteenth century, there were two Parisian approaches to exploiting the mathematical opportunities offered by the deformation of solids under external forces and constraints to produce internal motions. These approaches were symbolized by the work of Lagrange versus that of Laplace. The problem where these two methodologies clashed was in the analysis of Sophie Germain and Denis Poisson on the vibration of elastic plates.[110] The goals of the study of elasticity ranged from understanding engineering problems, as in the case of Coulomb, to the explorations of the calculus afforded mathematicians by this particularly difficult branch of rational mechanics.

Ernst Chladini's experiments on vibrating plates stimulated the Institut to offer, as the prize essay for 1809, the mathematics of vibrating plates and to compare the results with experiments. The question had to be reset twice before it was awarded, with reservations, to Sophie Germain. Her flawed derivation of the equations of motion for the elastic surface was based on Lagrangian mechanics. In his *Mécanique Analytique* Lagrange reduced the physical properties of bodies to geometry and used the principle of virtual velocities and variational calculus to obtain equations expressing extremum conditions. He drew into one analytical net the mathematical problems of both statics and dynamics. He also managed to withdraw the mathematical consideration of both of these physical subjects from the domination of hypotheses to the elegance of algebra. With analogical reasoning, Germain argued that the sum of the curvatures of the plate would play the same role in the theory of plates, as the curvature of the elastic central-line

108 There is no modern book-length study of early nineteenth-century mathematics of elasticity. For Cauchy and Navier see Grattan-Guinness *Convolutions* vol. 2, chap. 15. There is a short overview in J. J. Cross, "Theories of Elasticity," in *Companion Encyclopedia* Grattan-Guinness, ed. vol. 2, 1023–1033. However, modern vector notation is used throughout which makes the development of these mathematical works look deceptively easy. For a difficult source see Todhunter, *A History of the Theory of Elasticity and the Strength of Materials.* The aspects of these theories that relate to theories of light and the ether are also in Whittaker, *A History of the Theories of Aether and Electricity.* The difference between a mathematical and physical theory expressed in mathematical language is missing from Whittaker. He also uses modern notation so much of the historical context and content of all this work is lost.

109 In later chapters we will examine the reactions of physicists to these explorations of the mathematics of elasticity and what they found wanting in those explorations.

110 Amy Dahan-Dalmedico, "La mathématisation des théories de l'élasticité par A-L Cauchy et les débats dans la physique mathématique française (1800-1840)," *Sci. Techn. Persp.* 9 (1984–1985): 1–100. Note especially her scheme for clearly placing the mathematicians within various traditions and the changes in those traditions through the maze of published papers.

did in the theory of rods. She used Euler's work on vibrating rods and extended his reasoning to vibrating plates. Germain argued that the action of the forces on the plate was proportional to the sum of the inverse of the change in the radii of curvature of the plate. Using the principle of virtual work, she obtained an equation for the vibration of the elastic surface. Her basically incorrect equation was reworked by Lagrange into the form Germain adopted and then solved for particular cases.[111]

Denis Poisson was hot on Germain's mathematical trail. He criticized her work on technical and conceptual grounds.[112] Poisson argued that the only foundation for the mathematical analysis of physical problems was the consideration of the forces between the molecules making up bodies.The problem with Germain's analysis was that it was geometrical and lacked any appropriate hypotheses for the mathematical study of physical phenomena.[113] Germain was a stand-in for Lagrange whom Poisson also criticized on the same grounds. He had also recently published his own text in mechanics, a rival to that of Lagrange. He then rederived Germain's revised equation of motion on what he took to be more appropriate foundations.

In his first paper on the equilibrium of elastic surfaces, Poisson looked at an isolated molecule and assumed that deformation of the surface changed the distance between molecules and sought the force that would return the surface to its original form.[114] Poisson also derived the equation of motion for a vibrating plate that required the expansion and then truncation of series. All relationship to either the original problem, or the model of matter used to set up the equations, were lost. His paper was derivative at best and a demonstration of mathematical acrobatics in search of a known goal.[115] Navier later questioned Poisson's actual use of his

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111 Her solution was published as Sophie Germain, *Recherches sur la théorie des surfaces élastiques,* (Paris 1821). For details of Germain's derivation of her equation of motion and solutions, see Bucciarella and Dworsky, *Sophie Germain*. While they do not spare Germain's essay from criticism, they make the importance of being male and having powerful, committed patrons in the mathematical world of early nineteenth-century Paris all too evident.

112 As soon as the professional mathematicians entered into the fray Germain was marginalized. Her isolation from the discipline and inability to gain the necessary technical training meant that her work could not be technically as sophisticated or regarded with anything but condescension.

113 Poisson, "Mémoire sur les surfaces élastiques," *Mém. Institut* (1812) [1814]: 167–226. Bucciarella and Dworsky, *Sophie Germain* argue that the prize essay was initiated by Laplace to further Poisson's career, although they offer only circumstantial evidence. However, Poisson's paper on the prize topic was published by the Institut even as he withdrew from the competition on his election to that institution.

114 Poisson had introduced this model in 1811 in Poisson, *Traité de Mécanique* (Paris: 1811), 1833 edition translated by H. H. Harte as *A Treatise on Mechanics* (London, 1842).

115 Even Todhunter notes this might be a display of analytical skill that did not add to the physical discussion of the problem. See Todhunter, *History of Elasticity* vol. 1, 212.

model and Germain pronounced it "useless if not harmful."

In this paper and in his earlier ones on electricity, Poisson established a pattern of claiming to use forces acting at insensible distances to establish the equations to be solved. The restrictions that central forces might put on the derivation of the equation of motion or its solution were never enforced. Poisson developed the mathematics as generally as possible and the solutions he attempted were at the most general level he could complete. Both were achieved with all manner of technical mathematical devices that might make nonsense of the physical problem that initiated the mathematics. Poisson never distinguished the physically plausible from the mathematically interesting. In the terms within which he worked, the coincidence of mathematical results with previously obtained experimental ones confirmed all the mathematical apparatus brought to bear on the problem.

Poisson used each and every opportunity to bring his mathematical talents to bear in every domain of physics. Simultaneously, he began to defend the calculus against the assaults of the work of Fourier and Cauchy. Much of his work was devoted to arguing against the mathematical continuity between integration and summation in Cauchy's calculus. Poisson insisted on the use of summations in any problem relating to mathematical problems dealing with molecules. This insistence was rhetorical. Poisson could not solve such problems himself without using integrals.[116] We need to see these papers as part of the ongoing debate between the mathematical professionals over the foundations and methods and legitimacy of the results of the calculus. It was not a debate within physics or about the foundations of a physical theory of elasticity. Poisson's concerns were the proper foundations of mathematics. He also sought more legitimate ways of expressing solutions to equations deduced from physical problems. The mathematics should reflect the structure of the physical world.[117]

In the 1820s, a three-cornered fight over the mathematics of elastic solids erupted among Poisson, Navier, and Cauchy. Navier investigated the general equations of motion of elastic solids. Beginning with center of force molecules Navier assumed that the forces between the molecules, when they are drawn apart, was proportional

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Bucciarella and Dworsky, *Sophie Germain*, 75–76, note the mathematical character of this paper.

116 Poisson, "Mémoire sur l'intégration de quelques équations linéaires aux différences partielles, et particulièrement de l'équation générale du mouvement des fluides élastiques," *Mém. Acad. Sci. Paris* 3 (1818) [1819]: 121–176, and "Sur les intégrales définies," *J. École Poly.* 11 (1820): 295–341. For a discussion of Poisson's mathematical concerns see, D. H. Arnold, "The Mécanique Physique of Siméon-Denis Poisson: The Evolution and Isolation in France of his Approach to Physical Theory (1800–1840)," *Arch. Hist. Exact Sci.* 27 (1984): 248–367, vol. 28 (1984–1985): 27–266, 287–307, and Grattan-Guinness *Convolutions*.

117 This argument is in contrast to Arnold, "Poisson," who treats Poisson's molecular model not as a necessary start for his mathematics but as a physical theory.

to the increment in and some function of the initial distance. He developed an expression for the component of the force acting on the displaced molecule, and from this obtained the equations of motion of the molecules in terms of their displacements along the axes (a, b, c). The component of force, X, along the a axis was

$$-X = \eta\left(3\frac{\partial^2 x}{\partial a^2} + \frac{\partial^2 x}{\partial b^2} + \frac{\partial^2 x}{\partial c^2} + 2\frac{\partial^2 y}{\partial a\,\partial b} + 2\frac{\partial^2 z}{\partial a\,\partial c}\right),$$

where η was the integral,

$$\eta = \int_0^\infty \frac{2\pi}{15}\,\rho^4\,F(\rho)\,d\rho$$

and $F(\rho)$ is the force between two points at distance ρ apart. To eliminate $F(\rho)$ was a mathematical exercise in Lagrangian mechanics. Essentially Navier constructed the work done by all the forces that he assumed acted upon a single molecule from all the molecules in the solid. Using the calculus of variations for the equilibrium case, Navier deduced the differential equations above and the boundary conditions at the surface of the body.

As Navier recognized, this was an exercise in mathematics.[118] He had taken the simplest case for the external forces. Cauchy weighed in with the more general case of forces acting at any angle to the surfaces of the solid. Cauchy also considered the solid as a continuum. The paper was not published by Cauchy in full until 1828.[119]

Among the stream of papers on analysis Cauchy published during the 1820s and into the 1830s were several on the mathematics of elastic bodies. In these papers he clearly represented the internal physics of bodies under various forces. While setting up the equations of equilibrium and motion within the elastic solid, the physical imagery of stresses and strains could be vivid and explicit. However, they were not consistent, and the physics of elasticity was not developed from the back and forth between Poisson, Cauchy, and Navier in these mathematical papers of the 1820s. All three derived similar, if not identical, equations from different physical starting points. They all quarreled over the validity of their

118 Navier, "Mémoire sur les lois de l'équilibre et du mouvement des corps élastiques," *Mém. Acad. Sci. Paris* 7 (1821) [1827]: 375–393, an abstract appeared in *Bull. Soc. Philo.* (1823): 177–183.

119 An abstract appeared earlier as Cauchy, "Recherches sur l'équilibre et le mouvement intérieur des corps solides, ou fluides élastiques," *Bull. Soc. Philo.* (1823): 9–13. The full paper was published in three installments, "De la pression ou tension dans un corps solide," "Sur la condensation at la dilation des corps solides," and "Sur les équations qui expriment les conditions d'équilibre ou les lois de mouvement intérieur d'un corps solide," in Cauchy *Exercises de mathématiques* vols. 2 and 3 1827 and 1828 respectively.

various mathematical results.[120] The point was to best one's competitors in being able to solve the partial differential equations of the motions of elastic bodies for cases that one's competitor had failed to solve. Or, to add complexity to a solution in some other case, turning a factor assumed to be a constant into a function. These were exercises in mathematical proficiency not physical insight. Between Poisson and Cauchy there was also the serious mathematical issue of how, or if, summations should replace integrations. They both used integrations to obtain solutions to the particular equations that they could in principle solve. Poisson mathematically deduced a ratio for lateral contraction to longitudinal elongation for a thin bar that was not valid in general.[121] Cauchy also extracted other such seemingly measurable ratios from his mathematics. Experiments that were difficult did not decide the issue of the validity of either mathematical approach.[122]

These strenuous mathematical exercises were given added urgency with the development of Fresnel's theory of light. Mathematical exploration of the propagation of waves through solids and liquids was given renewed impetus. The mathematical theories of elastic solids were elaborated along the lines of Cauchy's approach during the 1830s and 1840s by Gabriel Lamé. The physical muddle that resulted was sorted out later in the century by George Gabriel Stokes and others when mathematical theories of the ether became an urgent issue in the developing discipline of physics in Britain.

French Mathematics and Physics c. 1830: Some Conclusions

To assess the changes in both French mathematics and physics between 1790 and 1830, we need to keep in mind the differences between these two disciplines in terms understood in that era. Even as the calculus was redefined through the work of Cauchy in the 1820s, mathematicians continued to appropriate the expanding domains of experimental physics. The algebraic expressions of experimental results remained the starting points for excursions into the mathematics of light, heat, and elasticity. Cauchy's analysis opened up new avenues of mathematical exploration and expression. The display of mathematical virtuosity through problem solving was still an important route to a reputation and career in French mathematics. Joseph Liouville used Cauchy's analysis to explore generalizations of the mathematics of heat conduction. He promoted the coefficient of conductivity from

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120 Grattan-Guinness, *Convolutions*, chap. 15.

121 The ratio appeared in Poisson, "Mémoire sur l'équilibre et le mouvement des corps élastiques," *Mém. Acad. Sci. Paris* 8 (1828): 357–570, 623–627, 451.

122 For an assessment of Poisson's work in elasticity see, Garber,"Poisson," and L. L. Bucciarella, "Poisson and the Mechanics of Elastic Surfaces," in *Siméon-Denis Poisson et la science de son temps,* Michel Métevier, Pierre Costabel, and René Dugas, eds. (Paris: École Polytechnique, 1981), 75–104.

a constant in Fourier's original work into a generalized function in three dimensions. Electrodynamics served to lead him in other mathematical directions. As his recent biographer has claimed "many of Liouville's most important mathematical ideas continued to be inspired by physics."[123] This statement holds for many mathematicians in nineteenth-century France.

Despite the transformation of the foundations of the calculus in France, mathematics remained a method that reached into other disciplines for its problems to expand its domain. Poisson gave this search the name of *mathematical* physics. The mathematics of the nineteenth century was more sophisticated than that of the pre-1820s era. The mathematical explorations offered by the expanding fields of experimental physics were done by professionals who, in the state institutions of higher education, taught the following generation of mathematicians, and above all, engineers. By 1830, major new physical phenomena uncovered by experiment in the early decades of the nineteenth century were the starting point for mathematics, explored from various points of view. However, the work of such mathematicians did not develop into theoretical physics. The mathematicians sought the most general solutions possible from the equations deduced from physical phenomena. The arbitrary functions and coefficients used in the solutions to partial differential equations were not connected to physical conditions, processes, or the model establishing the initial mathematical problem.

This was true even of the Laplacians who claimed to root their mathematical physics in a particular physical model. In Laplace's case, his physical model was the starting point for his mathematical forays into a broad range of physical phenomena, provided they could be reduced to forces acting at a distance. He could then operate on the problem with all the analytical power of rational mechanics. However, after Laplace gave the initial statement of his model of matter the mathematical problem was expressed in terms of general equations of motion. Solutions to these equations were arbitrary functions. No restrictions were placed on their behavior that derived from the physical input of his initial model of matter. He did not use any criteria to separate the physically plausible from the mathematically possible. Obtaining a result that coincided with known experimental results were noted, but Laplace did not indicate the implications inherent in the mathematics for other physical situations. What was most striking about both Laplace and Poisson was that their physical model never changed even as they reworked and extended their mathematics.

In Poisson's solutions the physical model remained sketchy and only minimally connected to the mathematics at the beginning of the problem. Thereafter physical model and mathematics did not meet. His criteria for choosing the direction of development of his reasoning were solely mathematical. Any consideration of

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123 See Jesper Lützen, *Joseph Liouville (1809–1882): Master of Pure and Applied Mathematics* (New York: Springer-Verlag, 1990), 22.

Poisson's work was complicated by his place in the transformation of mathematics in France during the first two decades of the nineteenth century. Resolutely Poisson held to conservative opinions with respect to continuous and hence differentiable functions, proper series, the representation of arbitrary functions, and integrals. Even as Ampère, but above all others Cauchy, were redefining the integral so that many definite integrals would become finite and hence useful in problem solving, Poisson argued that summations of forces between molecules could not be transformed into definite integrals.[124]

Cauchy's attempts to redefine the integral as the limit of a summation process threatened Poisson's physical model. The model guaranteed the existence of solutions to those problems Poisson regarded as the core of mathematics itself. Therefore, Poisson tried to tie his physical model more closely to mathematical methods in a scheme that even he found unworkable in practice.[125] Indeed Cauchy's calculus was a great threat to Poisson and his reputation which had been built upon the eighteenth-century tradition of transforming what had been asserted to be divergent series into functional solutions. His years of toil improving the mathematics of others to bring a recalcitrant partial differential equations to integration using the restricted series and functions available in the older calculus were being devalued.[126]

In 1807, when Poisson initially criticized Fourier's use of trigonometric series, he was voicing conventional mathematical opinion. However, by 1830 Poisson was an isolated, mathematical conservative. He persisted in reworking the solutions of others to fit his own pattern of mathematics. This only made those solutions more difficult to accomplish. His work could be discounted by the younger generation of mathematicians.[127]

If the Laplacians cannot be seen as doing theoretical physics, neither can their more mathematically adventurous contemporaries. The mathematics they created

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124 However, in his mathematics the function representing that summation of forces bore no relationship to the law of force between the molecules through which Poisson established his equations of motion. Poisson, "Mémoire sur l'équilibre et le mouvement des corps élastiques," 366. See Garber, "Poisson."

125 Poisson published papers on purely mathematical issues throughout his life. The ones of concern here are Poisson, "Sur les intégrales définies," *J. École Poly.* 9 (1813): 215–246. "Sur l'intégration de quelques équations linéaires aux différences partielles, et particulièrement de l'équation génèrale du mouvement des fluides élastiques," *Mém. Acad. Sci. Paris* 3 (1818) [1819]: 121–176 and "Sur la manière d'exprimer les fonctions par des séries de quantités périodiques, et sur l'usage de cette transformation dans la résolution de différents problèmes," *J. École Poly.* 11 (1820): 417–489.

126 Representing a sum by an integral whenever convenient did not appear to bother him in his earlier papers.

127 Reworking Fourier's methods on heat, to reproduce both his equations of motion for heat and his solutions to those equations under various boundary conditions, took Poisson over two decades. Poisson, *Théorie mathématique de la chaleur* (Paris: Bachelier, 1835).

using problems drawn from physics were rich in physical implications that were deciphered by others. However, the physics from which they drew their problems, while still centered on experiment, had changed radically. Emblematic of those changes were the textbooks by René Häuy and Jean-Baptiste Biot examined by Cannon.[128] Both textbooks were popular, yet they addressed different audiences. Häuy described physics as the science of familiar phenomena. The instruments described and the objects of study were deliberately chosen to explain the commonplace. This was a text for the general public, or a text on an elementary level. In Häuy's text the relationships used to express experimental results were in terms of simple geometry. At its center were the descriptions of phenomena, not the discussion of the instrumentation, methods of taking measurements, or their expected accuracy. However, Häuy text was up-to-date with references to current research and researchers and was liberal with descriptions of their work.

Biot's audience were clearly pre-professionals, those planning careers as engineers or physicists for whom discussions of the commonplace would be digressions. The laboratory and quantitative experiments were the focus of Biot's text. In it Biot devoted large amounts of space to instrumentation, and the results of experiments and their analysis. Yet the only mathematics in the text was contained in the algebraic relationships Biot drew up between measured quantities. And these became unduly complicated because Biot insisted on the primacy of the data. No mathematical development of theoretical ideas intruded. What theory existed was expressed non-mathematically, if technically. Biot's modernity lies in his focus upon the technical details of experiments and their results. These were clinically dissected and analyzed. Mathematics was subordinated to the needs of the experimentalist and this was Biot's meaning of "physics experimental and mathematical."[129] Biot was a Laplacian, but on the other side of the disciplinary divide, an experimentalist for whom theory was still expressed in the vernacular. His objections to Fresnel were matters of physics, not mathematics, and expressed in the non-mathematical terms used by physicists. In his papers as a mathematical Laplacian, particular solutions of equations could well match specific, idealized experimental situations. This was an important innovation.

By 1830 the practices of experimentalists had redefined the discipline but not its relationship to mathematics. Experiment was still the heart of the discipline, but it was quantitative experiment conducted with elaborate instrumentation so that the measurements taken and their accuracy could be understood and analyzed. This self-conscious understanding and analysis of methods was a function of the increasing competition for priority in the development of a reshaped field. As

128 René Häuy, *Traité Élémentaire de Physique*, and Jean-Baptiste Biot *Traité de physique expérimentale et mathématique*.

129 Frankel's argument cannot be sustained by either Biot's textbook or his research. See Frankel, "Biot."

important were the constant improvement in those methods. Physical hypotheses had changed their function and were integrated by experimentalists in new ways into the discipline. While hypotheses still emerged from experiment, and along with their implications kept in non-mathematical language, they were subjected to the systematic cross-examination of auxiliary experiments. The numerical results of experiments put into algebraic form might also be extrapolated using geometric arguments. However, there was no development of physical theory from first principles through to predicted experimental results, all expressed in mathematical language where physical imagery and mathematics interacted and experimental results were embedded in the very mathematics of the theory.

One aspect of being an experimentalist had changed. Experimentalists were expected to produce, to first order at least, the mathematical explorations of the results of their laboratory labors. This may have resulted from the fact that many of the most imaginative experimentalists began their careers either as students of mathematics or engineers. Within physics itself mathematics was molded to the narrow needs of the experimentalist. Experimental physics was also becoming a discipline of professional practitioners. In France experimental physics was no longer embedded in the general culture. Nature had retreated to the laboratory and was becoming the exclusive property of men intent on examining the esoteric details of its operations. The goals of those measurements, remeasurements, and detailed understanding of instruments were towards much more narrowly defined goals. The broader notion of physics, still seen in the research of Arago, was no longer the path to an academic career.

None of the mathematicians or experimentalists of France separated their work from the needs of the state or the consideration of solving practical problems. French mathematicians and experimentalists pursued their renewed disciplines as professions with the boundaries of those disciplines that were laid down in the eighteenth century. The transformations occurred within those boundaries and did not allow physicists to transcend them. The creation of theoretical physics occurred within the different cultural contexts of Britain and the German States to which we now turn. The rich complex of mathematical explorations of physics and the legacy of quantitative experiment inherited from the French changed both disciplines in Britain and Germany. Both British and Germans reinterpreted that legacy and through those reinterpretations they broke through some of its limitations.

Chapter V

On the Margins: Experimental Physics and Mathematics in the German States, 1790–1830

Prologue

Paris was not the only site for the practice and development of mathematics and experimental philosophy around the turn of the nineteenth century.[1] Throughout the eighteenth century, in Britain and the German States, experimental philosophers and mathematicians built their own traditions, interacting with, but not overwhelmed by, the research of the French. After 1800 the achievements of French experimentalists and mathematicians intruded into those traditions and began to change them. These intrusions reoriented research problems and the terms of their solution by both experimentalists and mathematicians. At the same time and on a broad scale, British and German societies went through metamorphoses. In the German states, the invasion and occupation of the Rhineland by the French accelerated these changes. Experiences in the Napoleonic wars added to the structural, economic translocations already affecting Britain.

In these countries the changes wrought by war and economic transformation recast the educational systems and the place within those societies of both the study of nature and mathematics. New opportunities and social forms for the practice of experimental philosophy and mathematics emerged within either new or renewed educational institutions. While the details of the patterns of these developments

1 Although this narrative focuses on events and changes in Britain and the German States, other sites in Europe continued their own traditions in the study of nature and interacted with those states. Among the more important in this period are those of Sweden and Denmark. Physics in the United States does not enter this account until physics became a profession and only in the person of Josiah Willard Gibbs. American physics had its own distinct social history. However, the pervasive "baconianism" that Robert Bruce, *The Launching of Modern American Science, 1846–1876* (New York: Alfred Knopf, 1987) Prologue, found so dismal was not so different from the standards of Europe of similar eras.

were unique to each society within the German States and Britain, the general trend and end results were the same for experimental philosophy and mathematics. Experimental philosophers and mathematicians became professionals, mainly within institutions of higher learning. The actual practices of mathematicians and experimental philosophers within these newly professionalized disciplines were quite different from those of earlier generations. Alterations in these disciplines were shaped by changes internal to their particular societies. Socially, these re-formed disciplines became the modern scientific professions of physics and mathematics. The social function of research became the advancement of careers rather than the affirmation of social place within a general literate culture. The social justifications for such research quickly became moral, economic, and political to secure then consolidate these new professions within the emerging industrial societies of the nineteenth century.

Intellectually members of these disciplines also reacted to, and then tried to emulate, the achievements of early nineteenth century French mathematicians and experimentalists. The intellectual reorganization of the practices of both British and German mathematicians and experimentalists manifested these reactions. However, emulation did not reach replication. British and German mathematicians and experimentalists interpreted French mathematics and experimental physics in light of practices already in place. In both societies the French example and native traditions led to reconstitutions of mathematics and physics. These reconstitutions required periods of studentship, followed by reappraisal, then refashioning the complex heritage of methods, problems and standards of solution available from the French after 1800.

The periods of social and cognitive reappraisal set mathematics and physics into new channels that led to the redefinitions of both research problems and their solutions. One outcome of these reorientations was that France was replaced by Britain, and then Germany as the center of scientific life in Europe.

The ultimate effects of these processes take us beyond 1830. In the era, 1790–1830, France remained the center of the scientific life of Europe and Britain and Germany were still on the margins. The period of emulation of the French by the British and the Germans began in the 1820s. The cultural matrices of the educated elites of the German states and Britain led natural philosophers and experimentalists, in their interpretations of the French experimental physics and mathematical physics, to struggle with problems the French no longer worried about. These problems emerged from the interaction of experimental philosophy, natural philosophy, and mathematics. Both mathematicians and experimentalists in these societies were drawn into a series of philosophical dilemmas because members of both these disciplines believed that examinations of the appearances of nature could reveal its underlying operations.

In the German States and Britain, one of the more important issues that was debated was the relationship between empirical and deductive knowledge. Other more specific metaphysical topics exercised experimental physicists and philosophers. As in earlier metaphysical disputes, general guiding principles upon which theories might be established could be a node of consensus. The actual working out of those speculations about nature led in their details to a myriad of different theories. General principles had to be supplemented by other hypotheses which directed theories in wildly different directions. This was especially true in an era when analogy and illustrative example still filled the place that mathematical exposition served later.

The diverse social and cultural settings of the German States and Britain led to quite different interpretations of the French heritage they all sought to emulate. The particular institutional forms that emerged to house British and German mathematicians and experimentalists who reshaped this French heritage were also quite different. This was partly due to the forms already available and the differing opportunities that opened up within these societies between 1790 and 1830. Therefore we will examine them separately. However, after 1830 this more open access began to close. Entering into the disciplines of mathematics and experimental philosophy began to be mediated by new formal educational requirements. Formal education became a prerequisite to joining the research community. These educational requirements also intruded into merely understanding the content of mathematics and experimental philosophy. Intermediaries between the research communities and the broader, educated public became necessary. The general audience was disengaged from the practice of the investigation of nature and became more passive observers of the feats of scientific professionals.

Thus the era from 1790 to 1830 was pregnant with possibilities of which only a few were brought to term and safely delivered.

Physics and Mathematics in the German States, 1790–1830

To study experimental philosophy in the German States in the late eighteenth and early nineteenth centuries, we have to take seriously aspects of that work that seem most alien to the standards of the twentieth century. There was no one institutional setting for physics. The private laboratory and literary salon as well as universities and academies of science were centers for the discussion and development of physical ideas. Other factors separating the physics of this era from our own include a passionate discussion of metaphysics by experimental philosophers. As important, and discussed at great length, were the proper philosophical foundations for a genuine experimental knowledge of nature that was not purely empirical and the legitimacy of mathematics within the practice of experimental philosophy. In some of these debates, experimental natural philosophy became a branch of philosophy. And much of what was written as "theory" seems speculative, even non-scientific. However, the issues discussed in such unlikely social and intellectual settings haunted physics in Germany long after 1830, as later changes transformed the discipline into its modern social forms.

Physics in this era was still experimental philosophy and a part of the shared culture of intellectuals. In German-speaking areas of Europe, natural philosophy and philosophy could not be separated. This was partly because of the belief that the study of experimental phenomena could uncover the actual workings and structures of nature. The findings of experimental philosophers and interpretations of the meanings of their results contained within them metaphysical and philosophical implications no philosopher could ignore. Debates within the sciences were of vital interest to philosophers. Yet within this vital, contentious culture there was no one metaphysics, experimental or explanatory approach available to experimental physicists.[2]

Historians trace this fragmentation ultimately to the fragmented character of the German-speaking world. Yet there existed a semblance of social cohesion in the group interested in the relationship between philosophy, metaphysics, and experimental philosophy. The community consisted of the faculty at state universities, of private secondary or higher educational institutions, state civil servants, members of the professions of law or medicine, members of the state academies of science or letters, and finally private scholars. This corps saw itself and self-consciously acted as a distinct group in the varied German societies. Members of this community were self-recruited from the full range of social ranks from aristocracy to burgers to skilled artisans.[3] They no longer acted as members of the social rank into which they were born. As a group they exhibited no attachment to a particular place or state. Careers came before social or political identity. To enhance their individual careers, they moved from one state to another, and from the service of one state to that of another.

Throughout the disruptions of the Napoleonic wars, small gatherings of such like-minded individuals would discuss, sometimes passionately, the ties of experimental philosophy to metaphysics. Historians have traced the common metaphysical ground shared by philosophers and experimentalists through the eighteenth

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2 The fragmented intellectual and social character of this era has been detailed by David Knight, "German Science in the Romantic Period, 1781-1831," in *The Emergence,* Crosland, ed. 161–178, and Barry S. Gower, "Speculation in Physics: The History and Practice of Naturphilosophie," *Studies Hist. Phil. Sci.* 3 (1973): 301-356. For recent studies on Romanticism and science, see *Romanticism and the Sciences,* Cunningham and Jardine, eds. (Cambridge: Cambridge University Press, 1990) and *Romanticism and Science in Europe (1790–1840),* Stefano Possi and Mauritz Bossi, eds. (Dordrecht: Kluwer Academic, 1994). For a useful survey of the literature, see Trevor Levere, " Romanticism, Natural Philosophy, and the Sciences: A Review and Bibliographical Essay," *Persp. Sci.* 4 (1996): 463–488.

3 For a full discussion on these points, see C. E. McClelland, *State, Society and University in Germany, 1700–1914* (New York: Cambridge University Press, 1980).

century. The most influential philosopher in Germany before Immanuel Kant on some of these issues was Christian Wolff. However, Wolff's writings seem to describe the usual division of labor at the University, rather than offer a structural explanation of, for example, the relationship between mathematics and experimental philosophy. Kant reemphasized some of the accepted ties between experimental philosophy and metaphysics, and made others problematic. He also changed the debate by focussing upon analytical categories rather than descriptive terminology. Simultaneously, he established a pattern of thinking about the experimental sciences and their relationship to philosophy. Experimental philosophy would uncover the real workings of nature, that is, explore essences. For Kant, understanding that inner structure was both a matter of intuition and deduction as well as empirical exploration.[4]

While Kant distinguished between experimental philosophy and metaphysics, the distinction remained obscure to his contemporaries.[5] Kant did sketch a place for mathematics in the understanding of nature. This was a less than complete discussion and was illustrative rather than analytical. His model for a complete theory was Newton's theory of gravitation which he accepted as both empirically grounded and deductively developed through mathematics. From his philosophy of mathematics, Kant developed the notion that the truths of geometry come from the method of constructing its proofs. Universal gravitation was "mixed." It was both derived from a priori laws and deduced from data. He did not explore this further. His philosophy needed interpretation to establish a legitimate place for mathematics within experimental philosophy. This was urgent, because in eighteenth-century German universities, mathematics and experimental physics were quite distinct disciplines. Also, Kant did not explain how Newton had merged these separate discipline or how they might be related on some general, philosophical level. He merely stated through his example of Newton's work that they were necessary for the full understanding of nature. Others would investigate this problem later. He also sketched a philosophy of mathematics itself.[6] Kant was also a champion of new standards of scholarship. Research and scholarship was a search for truth. However, while Kant might challenge the state's interference in the search for

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4 Frederick Gregory, "Kant's Influence on Natural Scientists in the German Romantic Period," in *New Trends in the History of Science,* R. P. W. Visser, H. J. M. Bos and C. Palm, eds. Proceedings of a Conference at the University of Utrecht, 1986 (Amsterdam: Rodopi, 1989), 53-66. See also Michael Heidelberger, "Some Patterns of Change in the Baconian Sciences in Early Nineteenth-Century Germany," in *Epistemological and Social Problems,* Jahnke and Otte, eds. 3-18.

5 See Gregory, "Kant's Influence," 58, and Gower, "Speculations in Physics," 310-320.

6 See Michael Friedman, "Kant on Concepts and Intuition in the Mathematical Science," *Synthese,* 84 (1990): 213–257, and Friedman, *Kant and the Exact Sciences,* (Cambridge MA.: Harvard University Press, 1992) and *Kant's Philosophy of Mathematics,* Carl J. Posy, ed. (Dordrecht: Kluwer Academic, 1992).

truth, he also accepted an explicitly utilitarian function for the university.

By 1800 the philosophical, metaphysical, and physical bequest of Kant had become a mixed blessing. Kant's acceptance of utility as a goal of academic life was an idea in serious jeopardy in the realm of the *Gelehrte.* He was also a Newtonian. Most German intellectuals associated Newton with French atomism and mechanism and these ideas were very much out of German intellectual fashion. Further, in the early 1800s, mathematics did not seem very useful in solving the unexpected and fascinating puzzles presented by recent experimental findings, especially those associated with electrochemistry. In this case, other aspects of Kant's metaphysics still helped. While Kant contended we only observe matter, underlying the surface phenomena was a reality of forces that brought about change. The forces were polar, and equilibrium was a dynamic balance between competing forces. Despite their disclaimers, the notion of polar forces and dynamic equilibrium became an important explanatory form in German natural philosophy and later in theoretical physics.

Alternative philosophies of nature, native and hence anti-French, were far more attractive. The most important of these alternatives was Naturphilosophie that, in the context of research, seemed to lead its adherents into new areas of investigation unthinkable in mechanistic terms.[7] Proponents of a German philosophy of nature, including Naturphilosophie, distanced themselves from Kant in their rhetoric, yet used many of the principles guiding his philosophy of the sciences. One of these was the unity of nature. Another principle from Kant that endured were in the terms used in explanations of phenomena. The dynamic, biploar forces of Kant appear in Naturphilosophie and its rivals in this era. However, the focus of the explanations changed.

After 1800 what needed explaining was not matter but the processes of change themselves. In a different context, German chemists criticized and rejected the "atomic" and "mechanical" theory of Lavoisier because it did not address the fundamental problems of chemistry, affinity and cohesion; the problems associated with chemical activity itself. For experimental philosophers, processes established a new matrix of research problems. Forces and other common elements ranked above structure as in such an explanatory framework for those processes. However, principles did not determine the detailed content of such explanatory schemes or theories. Hence versions of Naturphilosophie multiplied.[8] This was especially true

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7 See S. R. Morgan, "Schelling and the Origins of his Naturphilosophie," in *Romanticism and the Sciences,* Cunningham and Jardine, 25, Gregory,"Kant, Schelling and the Administration of Science in the Romantic Era," *Osiris,* 5 (1989): 17-35. See Knight, "German Science," on the reasons for the rejection of Kant by Schelling, the leading philosopher in the Naturphilosophie group.

8 For an overview of the varied versions of, and ways in which, Naturphilosophie was used as explanation in physics, see Keith Caneva, *Mayer,* chap. 3. See also Gregory, "Romantic Kantianism and the End of the Newtonian Dream in Chemistry," *Arch. Int.*

because in Germany as elsewhere in the early nineteenth century, ideas developed into theories through literary devices and illustrative examples. Eighteenth-century standards of argument still prevailed in the philosophy of nature.

In the philosophically charged atmosphere surrounding experimental philosophy, particular phenomena were invested with great significance. Those of electrochemistry were taken as especially meaningful, whether or not the experimentalist identified himself with Naturphilosophie or some other explanatory scheme. While experimentalists agreed that such phenomena seem to draw experimental physics, in the narrow sense, and chemistry closer together, the significance of this renewed connection remained elusive. Hans Christian Oersted, for example, saw physics and chemistry as intertwined in theory, method, and language.[9] Through experiments he was planning, Oersted looked for a relationship between heat, light, and "electrical conflict."[10] Ludwig Wilhelm Gilbert's renaming of the *Annalen der Physik* to *Annalen der Physik und Chemie* encapsulated the sense of the closeness of experimental physics and chemistry and the significance of that closeness. Yet he only sketched the significance he saw in that relationship as did the next editor of the *Annalen*, Johann Christoff Poggendorff.[11]

Historians differ over the importance of Naturphilosophie for the development of experimental physics in Germany in the nineteenth century.[12] Interpretations depend on how closely historians expect individuals to hold to the details of a chosen version of Naturphilosophie. While Oersted's work follows a general pattern that might put him in the Naturphilosophie camp when we examine the philosophical ideas he explicitly discussed those notions are Kantian.[13] Johann Wolfgang von

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Hist. Sci. 34 (1984): 108–123.

9 See letter from Oersted to Weiss, 12 May, 1829, Oersted, *Correspondance avec divers savants,* M. C. Harding, ed. and trans. (Copenhagen: H. Aschenhoug and Co., 1920), 280–289, 285.

10 Oersted, *Correspondance,* and Walter Kaiser, *Theorien der Electrodynamik im Neunzehnten Jahrhundert* (Hildesheim: Gerstenberg, 1981), 22.

11 See Poggendorff, "Vorwort," *Ann. Phy.* 1 (1824): vii.

12 See See Gower, "Speculation in Physics," and Gregory, "Influence," for general discussions of the influence of Naturphilosophie on physics. H. A. M. Snelders, "Romanticism and Naturphilosophie and the inorganic natural Sciences, 1797–1840: An Introductory Survey," *Stud. Romant.* 9 (1970): 193–215, argues that Naturphilosophie "deeply" influenced scientific life in Germany. See also Snelders, "Point Atomism in nineteenth-century Germany," *Janus,* 58 (1971): 194–200. See also Walter D. Wetzels, "Johann Wilhelm Ritter: Romantic Physics in Germany," in *Romanticism,* Cunningham and Jardine, eds. 199–212.

13 Gower, "Speculation in Physics," puts Oersted in Schelling's camp. Kaiser, *Electrodynamik,* does not. H. A. M. Snelders, "Oersted's Discovery of Electromagnetism," in *Romanticism*, Cunningham and Jardine, eds., 228–240 argues that in his research Oersted believed in the unity of nature, yet also accepted Kant's critical theory. See also Oersted, *Correspondance,* for his discussions of philosophical issues.

Goethe also opposed both Naturphilosophie and Newtonianism. Yet his work followed patterns of problems and of solutions that fit into the Naturphilosophie approach to the study of nature.[14]

In these specific examples and those of many others, the philosophical issues central to physical theories and general speculations about the operations of nature were shared with other experimental philosophers of differing philosophical persuasions. This shared, general understanding of the important problems to be solved, and the terms in which the solutions to those problems should be couched, continued throughout the nineteenth century in German physics. Explanations of the operation of nature in terms of bipolar forces continued through several decades and generations of physicists. Specific adherence to Naturphilosophie might later be denied, but investigation of change and the explanation of change in terms of forces remained. Also shared was the belief that the investigation of the appearances of nature, the phenomena, would uncover the real structures and processes of nature. Experimental philosophy was not merely descriptive but penetrated the actual workings of nature.

Whatever divisions there were within the German physics community, physics as a discipline was still defined broadly, in the eighteenth-century sense of the term. It covered all the experimental sciences and included the important areas of mineralogy, meteorology, and climatology. This range of concerns was reflected in the pages of the first journal addressed to German experimental philosophers, *Journal der Physik.* In 1790 the manifesto of its editor, F. A. C. Gren proclaimed that the journal was addressed to a narrow audience, not those who "merely lecture as a pastime," or, "write diverting essays." However, this was not a research journal. It was one of news in experimental philosophy. He reprinted material from foreign and domestic journals and used its pages to argue for phlogiston and other issues in chemistry.[15] The next editor, L. W. Gilbert inherited Gren's chair in chemistry and physics, and his journal. Gilbert's own research was in the domain of what he called "physical chemistry" or "chemical physics." While he sketched Lavoisier's chemical ideas, he also used explanatory components taken

14 For Goethe's work on light and opposition to Naturphilosophie see Keld Nielsen, "Another Kind of Light: The Work of T. J. Seebeck and his Collaboration with Goethe," *Hist. Stud. Phys. Sci.* 20 (1989): 107–178, 21 (1991): 317–397. See also H. D. Irmscher, "Goethe und Herder im Wechselspiel von Attraction und Repulsion," *Goethe Jahrbuch,* 106 (1989): 22–52.

15 Gren, "Vorrede," *J. der Phys.* 1 (1790): 137. See also Dieter B. Hermann, *Die Entstehung der Astronomischen Fachschriften in Deutschland, 1798-1821* (Berlin: Archenhold-Sternwarte, 1972), 137. Thomas H. Broman, "J. C. Reil and the "Journalization" of Physiology," in *The Literary Structure of Scientific Argument,* Dear, ed. 13–42, establishes the same pattern of publication, news and reprinted materials, in the early years of the *Archiv für die Physiologie,* although in other ways it represented a break with earlier medical journals.

from atomism, together with the language and imagery of dynamics and forces. He was also highly critical of the theories of both Schelling and Hegel as speculative. He continued Gren's policy of reprinting articles. However, the articles were explicitly limited to articles from journals of physics, not mathematics. It was through this journal that a great deal of the new, French, quantitative experimental physics became known in Germany.[16]

Some historians have taken the reprinting of articles from foreign journals as evidence of the low quality of the sciences in the German states in this era. However, Gren and Gilbert's journal was not backed by state funds, as were those of academies of science. We must see the journal as performing a different function for a different audience than that of the research journal for a profession. The content of this journal followed the same pattern as other eighteenth-century journals for a general, elite audience. In this case, the audience sought was not quite so broad and the content of the publication narrower than usual, yet the eighteenth-century form continued. Gilbert and Gren's venture was private. To be successful they had to appeal to a broad audience, but did not have enough active contributors for this new medium of publication–the journal.[17] In the early 1800s the presentation of ideas in texts that served student needs was more rewarding, financially and professionally for the German professoriate.

Johann Karl Fischer's history of physics also reflected the broad and inclusive meaning of the term in the *Journal.* Fischer distinguished physics, broadly and narrowly, and decried the current fashion of trying "to derive natural phenomena and their laws as mere conclusions *a priori*." For Fischer, physics was a discipline grounded in experiment and the goals of its practitioners were to understand the processes of nature. His account of physics ranged over experiments and observations in physics and chemistry, observational astronomy etc. This wide range and experimental bias is also present in J. D. Reuss' organization of physics in his *Repertorium.*[18]

Physics as reflected in these pages, Gren's, and then Gilbert's journal, and elsewhere was an empirical science, not necessarily quantitative, but done with care and precision. Theory was presented in non-mathematical terms understood by a broad general educated audience and theorizing meant launching oneself into metaphysics. Vague ideas and experiments whose goals were obscure were present in the literature. Yet carefully delineated, qualitative experiments and descriptions

16 Hans Schimank, "Ludwig Wilhelm Gilbert und die Anfange der 'Annalen der Physik'," *Sudhoff's Archive,* 47 (1963): 360–372.

17 Journals in natural philosophy published in Britain and France in the same era as commercial ventures shared the same mix of reportage, replication and articles.

18 Johann Karl Fischer, *Geschichte der Physik,* and J. D. Reuss, *Repertorium Commentationum et Societatibus Litterariis Editarum* (New York: Burt Franklin reprint of the Göttingen edition of 1805) 6 vols., vol. 4 Physics.

with clear explanations of the meanings of the results also existed in the same journals. As in the work of Oersted and Ohm, and the later work of Michael Faraday, the absence of quantification did not necessarily preclude clarity either of experimental purpose and accomplishment, or of physical interpretation. Clarity was more than quantity. And numbers can only convey meaning if they measure something that is predetermined on other grounds as meaningful.

In such a discipline, seen as related to chemistry with interpretations of phenomena tied closely to metaphysics and philosophy, mathematics held only a marginal place. At its most useful it served the purpose, for experimentalists doing quantitative experiments, of reducing their data and expressing those results in algebraic form, if they took the analysis of their work that far. Most experimentalists were suspicious of the use of mathematics within physics. For some it was impossible to believe that any knowledge of nature could be gained through mathematics. This was the opinion of Seebeck and a loudly voiced, majority opinion. Analysis (the calculus), it was argued, was devoid of the imagery necessary to develop ideas about the real structure of the external world and its processes. In the case of electromagnetism,

> The mathematician conceives the phenomena of electromagnetism, in the first instance only as diverse modifications of motions; if he succeeds in setting up a fundamental equation into which all the factors that influence the kind and magnitude of the motion enter as *elements*, whose particular values, exactly determinable by the formula itself, exactly specify the motion itself, then he has incontestably satisfied the demands placed on the so-called mathematical physics.[19]

In mathematical versions of electromagnetism, attractive and repulsive forces appear merely as positive or negative quantities. A mathematical investigation could proceed no further. The source of such effects was of no concern to mathematics, as the latter gives only a quantitative account of the phenomena. A physical explanation, however, demanded such an exploration into the sources of the positives and negatives. Only physical theories were "capable of capturing the essence of the phenomena." This criticism was mild compared with those launched by adherents to a metaphysics in which mathematics was taken to be a purely deductive form, a product of the mind, that in principle could not relate to phenomena of the real world.[20]

Not all experimentalists with such philosophical commitments were necessarily hostile to mathematics. In Oersted's opinion, mathematical abstractions were

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19 Quoted from C. H. Pfaff, a chemist, in Kenneth Caneva, "From Galvanism to Electrodynamics. The Transformation of German Physics and its Social Context," *Hist. Stud. Phys. Sci.* 9 (1978): 63–159, 86.

20 See Caneva, "Galvanism to Electrodynamics," for some of these reactions to French mathematical physics.

different from physical quantities and hence distinct from them. The disciplines that formed around these different entities were complementary. On philosophic grounds, Jakob Friederich Fries rejected Goethe's color theory because there was no mathematical development to complete it. Fries represented an important, minority, opinion.[21] Pfaff also placed himself in this minority camp when he added that, at the same time, physical explanations must also be mathematical. While he did not explain exactly what he meant, there were experimentalists prepared to concede that both a physical explanation of phenomena and a mathematical description of those phenomena might be equally valid. Yet these two kinds of examinations of nature remained in separate disciplines. Johann Tobias Mayer went so far as to acknowledge that when branches of physics became subject to mathematical treatment, they were handed over to mathematicians to become part of mathematics. Mathematical physics of the French variety was the domain not of physicists but of mathematicians. He regarded the work of Euler and Lagrange on sound and Fourier on heat as mathematics, not physics.[22] Opinion on the relationship of the experimental investigation of nature and mathematics ranged over the map, and was expressed only in general terms.

While the relationship between the empirical results of experiment and the analysis of those results by mathematicians had to be addressed in the philosophically charged atmosphere of early nineteenth-century German science, no experimentalist did it very systematically. With the triumphs of French mathematical and experimental physics, a more systematic handling of this issue was in order. In addition to Fries, Ernest Gottfried Fischer tried to develop a generalized, systematic study of this relationship. For Ernest Fischer, knowledge of nature came from both investigations into the nature of forces and their effects on bodies. Working from Kant's ideas of the necessity of deductive thought in the construction of knowledge, he argued that a philosophy of nature was only possible when expressed in the most general fashion, and that was impossible without mathematics. Physics provided the observations, the foundation upon which mathematicians could build. Fischer used Kant's idea that most general knowledge of bodies lay in their motions, and that necessarily introduced mathematics. Yet mathematics only entered into the picture to attain a complete synthesis once all knowledge of the phenomena was

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21 Heidelberger, "Some Patterns of Change," in *Epistemological and Social Problems,* Jahnke and Otte eds., 3–18, argues that in this era no mathematics was possible in physics. For Seebeck and Fries see, Nielsen, "Other Kind of Light. Part I," 162-163 and "Part II," 341-343. For Fries on mathematics see, F. Gregory, "Neo-Kantian Foundations for Geometry in the German Romantic Period," *Hist. Math.* 10 (1983): 184–201, and, "Die Kritik von J. F. Fries an Schelling's Naturphilosophie," *Sudhoff's Archive,* 67 (1983): 145–157. See also Fries, *Abteilung 3. Schriften zur angewandte Philosophie II Naturphilosophie und Naturwissenschaft* (Darmstadt: Scientia Verlag Aalen, 1979 reprint) 5 vols., Gert König and Lutz Geldsetzer introd., vol. 1.

22 See also Jungnickel and McCormmach, *Intellectual Mastery of Nature,* vol.1, 44–45.

completed.[23] Mathematics explored the quantitative, physics the qualitative aspects of nature.[24] Fischer did not change the mathematics-physics boundary. What was new in his discussion of experiments was the emphasis on exactness of measurement, corrections for possible errors, and the theory of instruments. Mathematics was a adjunct to experiment. As disciplines, mathematics and physics remained distinct. In general, the difference between analysis and experimental physics was that the latter led to an understanding of nature which was not possible with mathematics because it was purely deductive.

By the 1820s some of the experimental physicists trying to argue for a place for mathematics in the study of nature were already emulating the French.[25] In the same decade there were also German mathematicians already following the French and entering the new mathematics through problems presented to mathematicians from the results of quantitative, experimental physics. In the early nineteenth-century German universities taught enough mathematics for the needs of the landed, the higher echelons of the civil service, technical state functionaries and the medical profession. By the 1820s mathematics as a university study was being promoted through the new *Wissenschaftideologie* and reflected a rapidly changing institution for, and philosophy of, education.

University Reform and Career Opportunities

In the third decade of the nineteenth century, the practices of physicists and mathematicians changed again, driven by institutional opportunities and cognitive realignments. The cumulative impacts of the first generation of appointments made to reform the Prussian universities become manifest in the 1820s. This reforming impulse brought with it a new philosophical foundation for the study of mathematics. This new philosophy would indirectly affect the practice of physics and mathematics in later decades. At the same time experimentalists and mathematicians faced the issue of the relationship between mathematics and physics more systematically. In addition to this philosophical topic was the more pragmatic one of developing research problems and solutions that brought recognition. The obvious model was that of French mathematics and quantitative, experimental physics. Firstly, mathematicians needed to argue that mathematics was a study that on philosophical grounds belonged legitimately within the university.

The philosophical debates to justify the study of mathematics within the newly constituted universities and attempts to domesticate French methods did not lead

23 Ernst G. Fischer, *Lehrbuch der mechanischen Naturlehre* 2 vols., (Berlin: G. C. Nauck, 1826-27), vol. 1 part 2, 3.

24 For other examples see Fritz Krafft, "Der Weg von den Physiken zur Physik an den deutschen Universitäten," *Ber. Wissen.* 1 (1978): 123–167.

25 In terms of doing accurate, quantitative Experiments, there was one huge obstacle, money. See Jungnickel and McCormmach, *Intellectual Mastery,* vol. 1, chap. 3.

to any single set of practices in mathematics or physics. Discussions continued between individuals concerned with fundamental philosophical issues surrounding the source of their knowledge. These seemed as urgent as the debates over the results that French methods might generate. The decade of the 1820s as never before was one in which opportunities opened up for the practice of research in the university context as never before. But this was not without some cost. In emulating French research, practitioners gradually excluded the general, educated elite from entering into the research process. In experimental physics, the specialized equipment, training in its use, and techniques necessary for its manipulation, began to separate the general public from the experimentalists. Howls of protest and moans of regret about the direction of such "modern" research were part of the chatter of the 1820s. The work of the laboratory became remote from the world of the educated elite. Experimentalists and the educated public soon could only meet in the context of the debate over philosophical issues generated by the results of work within the laboratory.

The intellectual arguments outlined in the previous section took place against the political and military turmoil of the last years of Napoleon's domination of continental Europe. Until that matter was Settled, the reforms already begun of the Prussian university system could not be followed through and take hold. Nor could the philosophy guiding those reforms inform debates over scholarship, especially in the sciences. However, the reforms in practice of both experimentalists and mathematicians were developed through the solutions to specific problems, not through the contemplation of the principles of problem solving or experimental practice. We must therefore address the function of both the social reform of the German university and the rhetoric of *Wissenschaftideologie* in guiding the disciplinary changes in mathematics and physics.

Historians have long argued over the impetus behind the reform of the German university system and the impact, or lack of it, of the rhetoric of the educational philosophy in which those reforms were expressed. Even if we accept that rhetoric at face value, it is not at all clear that university reform was a simple translation of this new ideology–of the university as moral training through research as pure knowledge–into institutional form.[26]

Actual reforms were carried out within the context of political practicability and fiscal possibility. The most philosophically minded minister in charge of education,

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26 This is of course the traditional historiography of the German university reform. F. Paulsen, *The German Universities and University Study,* Frank Thilly, trans. (New York: Charles Scribner, 1906) is the most obvious example of this genre. For a recent example see Elinor S. Shaffer, "Romantic Philosophy and the Organization of Disciplines: the Founding of the University of Berlin," in *Romanticism,* Cunningham and Jardine, eds. 37–54. One can add to this numerous histories of specific universities across Germany published in the late nineteenth and early twentieth centuries.

Wilhelm von Humboldt, lasted less than one year in his civil-service post.[27] His successors, who actually forged the institutional reforms within the universities were compromisers, bureaucratic realists, and ambitious for careers within the Prussian civil service. Their ambitions were also hedged by the limitations of their budgets and of available personnel.[28]

The rhetoric surrounding the philological seminar spread, along with the method of the seminar, as the teaching mode in other university disciplines. However, these did not help to define research problems and methods specific to those other disciplines such as mathematics and physics. Acceptance of the rhetoric of description from philology and the use of research as the teaching model meant that mathematics could shed the stigma of "bread study." The choice of problems discussed in seminars and the standards of solution passed to students came from other sources. However, it followed that only those with specialized training could claim a place in that discipline and that training was only available at the university, inculcated in the context of the new teaching mode of the seminar. When this model became the norm in both mathematics and physics, access to even experimental physics was narrowed and proscribed for many.[29]

However, no one has denied the opportunities for the pursuit of careers in the sciences that opened up in these revitalized state institutions of higher education. Yet we cannot see the career opportunities as determining, in and of themselves, either the success, or the directions of development, of research in either the disciplines of mathematics or of physics. Social change did not necessarily determine cognitive realignments.[30]

Interpretations of cognitive changes in the sciences, specifically physics that are solely based on sociological criteria, cannot explain why the disciplines in the natural sciences were cognitively transformed in the nineteenth century.[31] The problems shared by Marxist scholars, ethnomethodologists, sociologists of knowledge, and cultural contextualists is that no explicit, general connections can be made between social structure, ideology, or cultural context and the details of the

27 Paul R. Sweet, *Wilhelm von Humboldt: A Biography* 2 vols., (Columbus OH.: Ohio State University Press, 1980), gives ample evidence that Humboldt was only happy as a private citizen. He shed public offices as soon as possible.

28 For details of these and other limitations, see McClelland, *State, Society and University in Germany,* 122-132.

29 An example of this was Seebeck's attempts to obtain a university post and the reasons for his difficulties in doing so. See Nielson, "A Different Kind of Light, Part II." Ohm suffered similar difficulties.

30 We shall examine France in the nineteenth century in chapter IX. See also Laudan, "Ideas and Organization," for a case of active organization without intellectual redirection.

31 For a discussion of the problems of social constructivism, see Stephen Cole, *Making Science: Between Nature and Society* (Cambridge MA.: Harvard University Press, 1992).

results of physical arguments.[32] The theory that the university formed the vanguard of bourgeois hegemony with the privat docent as its proletariat must explain why the particular form of the intellectual changes that occurred in the sciences can be linked to the bourgeoisie and no other class.[33] It has been argued that the universities gave unprecedented career opportunities for the sons of the bourgeoisie. Ideology and social change did the work of economic interest.[34] Yet it is not apparent that through much of the nineteenth century, the German bourgeoisie held the universities in high regard. Many of the men discussed in this and subsequent chapters were the sons of gymnasia teachers, the clergy, civil servants, or minor aristocracy. In another highly regarded sociological approach, Stichweh's exploration of the development of the modern scientific disciplines is descriptive rather than interpretive. None of the details of the actual contents of the research problems that implicitly define his notion of physics enter into his analysis.[35]

Recent sociological examinations of the nineteenth-century German university systems emphasize the role of the state in extending its power over the previously autonomous corporations of the universities.[36] The problem with this interpretation is that it is not at all clear why the Prussian state would support reform within the universities when its goal was control. Yet the state was largely successful in this enterprise. Or was it? The expansion of the role of the state in the reform suggests that the state bureaucracy reclaimed its elite position in Prussian society by capturing the universities through the creation and wielding of *Wissenschaftideologie.*[37]

32 Elizabeth Garber and Fred Weinstein, "History of Science as Social History," in *Advances in Psychoanalytic Sociology,* Rabow and Platt, eds. (Malabar FL.: Krieger Pub., 1987), 279–298.

33 Alexander Busch, *Geschichte des Privatdocenten* (Göttingen: Abhandlungen zur Soziologie, 5 (1959)) for this Marxist interpretation. This and other interpretations of the development of the universities in the German states are outlined in McClelland, *State, Society and the University, chap. 1.*

34 One has to question just how many young men from the bourgeoisie, or any other class, could be absorbed by the university system. The rigors of the course of study, then the shear physical stamina to attain the status of Ordinarius even in the middle of the nineteenth century must have defeated most aspirants. There are indications that the education ministry of the Prussian state discouraged university attendance when careers that required a university education were, in their opinion, overcrowded. McClelland, *State, Society, University,* chap. 3.

35 Rudolph Stichweh, *Zur Entstehung des modernen Systems wissenschaftlicher Disziplinen: Physik in Deutschland, 1740–1890* (Frankfurt am Main: Suhrkamp, 1984.)

36 R. Steven Turner, *The Prussian University and the Research Imperative, 1806-1848,* unpublished PhD dissertation, 1973.

37 By *Wissenschaftideologie* is meant here "the active pursuit of integrated, meaningful and pure knowledge" as "the highest calling of man," McClelland, *State, Society, University,* p. 24. The social conservatism of *Wissenschaftideologie* is discussed in Turner, "The Prussian Professorate and the Research Imperative, 1790-1840," in *Epistemological and*

In such an interpretation political compromise was necessary. The monopoly of entry to the university through the Gymnasia, and *Wissenschaftideologie* reinforced the old social barriers between the cultivated classes and the laboring majority of the population.[38]

None of these interpretations explore how institutional reforms led to the specific disciplinary changes that can be mapped through the first half of the nineteenth century.

There is some indication that the rhetoric of this ideology emerged from the state of affairs already existing at Göttingen and Halle and other universities. The rhetoric was partially descriptive as well as prescriptive.[39] Rather than look to the explicit statement of this ideology in the early nineteenth century, the origins of the redefinitions of mathematics and physics must be found in the changes in philology begun decades before in Göttingen and Halle and given a philosophical gloss later.[40]

Jacobi's mathematics seminar at Königsberg was modeled on August Boeckh's philological seminar in Berlin. This explains the emphasis on teaching through research. However, this does not give us any criteria to understand why certain research problems were picked out, and used as model problems for students to solve. The model of philology also does not indicate how German mathematicians understood when those problems were solved. Finally, the philology seminar model does not demonstrate how that model led to a new definition of the discipline and profession of mathematics.[41] The question remains as to how *Wissenschaftideologie*

Social Problems, Jahnke and Otte, eds. 109-122, although he does not link it with the aristocracy.

38 McClelland *State, Society, University*. Kees Gispen supports this view in his explanation of the place of engineers in German society in the nineteenth century. Kees Gispen, *New Profession, Old Order: Engineers in German Society, 1815-1914* (Cambridge: Cambridge University Press, 1989) chap. 1. One can challenge his ideas on the commitments of all academics later in the century to the notions of social hierarchy he claims they accepted. Hermann von Helmholtz's son Richard became an engineer, a career he followed with the encouragement of his father. Helmholtz also understood the economic need for research institutions for industry, part of the justification for the establishment of the Physikalisch-Technische-Reichanstalt. See David Cahan, *An Institute for an Empire: The Physikalisch-Technische-Reichanstalt, 1871–1918* (Cambridge: Cambridge University Press, 1989).

39 McClelland *State, Society, University*, p. 112, who finds Wilhelm von Humboldt's rhetoric a nostalgic recollection of his years of freedom as a student at Halle then Göttingen. See also Robert S. Leventhal, "The Emergence of Philological Discourse in the German States, 1790–1810," *Isis,* 77 (1986): 243–260.

40 See William Clarke, "On the Dialectical Origins of the Research Seminar," *Hist. Sci.* 27 (1989): 111–139, offers a more complex description of the origins of the nineteenth-century seminar over a long time period.

41 See R. Steven Turner, "The Growth of Professorial Research in Prussia, 1818 to 1848–

changed actual disciplines.[42]

The problem with all these interpretations is that none of them integrate the actual paths of intellectual change that occurred across the scholarly spectrum within the German higher educational systems, either in ideas, methods or language. It is not at all clear that acceptance of *Wissenschaftideologie* necessarily carried with it the demand to redefine the foundations of a discipline, its methods, problems or the criteria defining solutions to those problems. Research as a moral calling did not define what was researched, how that research was done, nor did it define when a solution to a problem was reached. These decisions were made by mathematicians and experimentalists choosing to follow the model of the French. One can argue that the "research imperative" with its disdain for "bread-study" turned the attention of mathematicians from the solutions of problems to the more esoteric issues of the nature of number, space, and function. But these developments in mathematics did not come to fruition until long after *Wissenschaftideologie* had lost its rhetorical force. In the first decades of change in the German States, mathematicians solved problems, many of them from physics, and through them uncovered new avenues of research in mathematics. This approach, inherited directly from French mathematicians, was followed by Jacobi, Lejeune Dirichlet, and Bernard Riemann, all individuals important for the initial successes of mid-nineteenth-century German mathematics. The emphasis of some German mathematicians on the foundations of the calculus was available in the work of Lagrange, Cauchy, Fourier and others. Foundational issues were also part of the inheritance from France.[43]

The historiography of mathematics is dogged by the rhetoric of *Wissenschaftideologie.* The first histories of nineteenth-century mathematics were those of mathematicians and are purely intellectual narratives.[44] Written by mathematicians brought up within the rhetoric of German mathematics, the image of the disci-

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Causes and Context," *Hist. Stud. Phy. Sci.* 3 (1971): 137–182, 148–149.

42 Functional theories of the scientist do not bring us any closer to a solution of this dilemma. See, Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study* (Englewood Cliffs NJ: Prentice-Hall, 1971). The concept of role simply leads to a narrative of events, not an interpretation of those events.

43 The pursuit of foundational issues by German mathematicians was not unique as we shall see when we discuss Britain. What foundational issues were found to be important within these two sets of practitioners was different.

44 See *Die Mathematischen Wissenschaften,* Felix Klein ed. (Leipzig: Teubner, 1914). Volumes included in the series covered the teaching of mathematics in Germany, its philosophy and the nature of mathematics as a science. The history volumes, *Vorlesungen über Geschichte der Mathematik,* M. Cantor, ed. (Leipzig: Barth, 1894-1907) 4 vols., was a collection of essays by mathematicians on various aspects of the history of mathematics. This older historiography of mathematics is discussed in Dirk Struik, "The Historiography of Mathematics from Proclus to Cantor," *NTM* 17 (1980): 1-22.

pline was the model of "Wissenschaft" pure, intellectual, universal.[45] Yet Felix Klein had another program for mathematics which he propagated and expanded upon through the authors of the series he edited. Mathematics was indispensable for and actually subsumed the other sciences, most notably physics.[46] There was a power struggle within the mathematics profession in the late nineteenth century that had its roots and manifestations in the ways in which both the philosophy and the history of mathematics were portrayed, as well as in the problems and methods of mathematics itself. Recent historiography of mathematics developed by professional historians includes the intellectual history of the discipline with a consciousness of its social and cultural context.[47]

Yet the changes that redefined what problems mathematicians worked on as research, and the training offered by the research seminar, could only effect changes in the profession if the trainees from the seminars could be placed. This in turn depended on gaining enough social support to continue training the next generation. This was not assured, and it took time. The new mathematics spread slowly. Indeed the two processes, social and intellectual occurred simultaneously. During the 1820s mathematicians elaborated a philosophy of mathematics that claimed purity from their redefinition of mathematics.[48] In their research German mathematicians annexed the problems and methods of the French before creating their unique research traditions in mathematics based on their understanding of "pure" in mathematical terms.

Experimental physicists were less burdened than mathematicians with the need to justify their work as "pure." However, it took longer for physicists to change their discipline structurally. They too took their models from France and through learning, then practicing these methods refashioned their discipline. In the development of both disciplines we see the same patterns of emulation, then reworking of French methods and problems into new channels.

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45 On this see H. N. Jahnke, *Mathematik und Bildung in der Humboldtschen Reform* (Göttingen: Vandenhoek und Ruprecht, 1990.)

46 On this see, Lewis Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany* (Philadelphia: American Philosophical Society, 1983) and David E. Rowe, "Klein, Hilbert and the Göttingen Mathematical Tradition," *Osiris* 5 (1989): 186-213.

47 On the problems and excitement of this approach, see Herbert Mehrtens, "The Social History of Mathematics," in *Social History of Nineteenth-Century Mathematics,* Mehrtens, Bos, and Schneider, eds. 257-280. This collection also includes a bibliography on the subject. On the early nineteenth century, see Mehrtens, "German Scientific Renaissance in Mathematics," in *Social and Epistemological Problems*, Jahnke and Otte, eds.

48 Gert Schubring has argued that mathematicians used *Wissenschaftideologie* to create the modern German mathematical profession. See Gert Schubring, "The Conception of Pure Mathematics as an Instrument in the Professionalization of Mathematics," in *Social History,* Mehrtens Bos and Schneider, eds. 111–134.

Changes in Physics in the 1820s

During the decade of the 1820s physicists, especially those of the first generation trained in the reformed Prussian universities, turned away from Naturphilosophie and towards the French for models of investigating nature. This did not change the sense that physics was an empirical study, or, removed the problem that German physicists saw in the relationship between such empirical knowledge and the deductive knowledge of mathematics.[49]

Perhaps the first German experimental physicist whose research revealed the direct influence of the French was Georg Ohm. However, the subject of his research, galvanic electricity, was not one in which the French had shown any interest. Using galvanic electricity, the French explored the relationship between electric currents, and between electricity and magnetism. Ohm examined the properties of current electricity itself. He was also explicitly influenced by Fourier and based his own research patterns on Fourier's work.[50] His 1827 mathematical paper on galvanism was preceded by a series of experimental papers on many aspects of the behavior of the galvanic circuit and its components. Through these experiments Ohm developed laws of conduction that took into account all of the components of the circuit, and included measurements on the conductivity of the different metals in the circuit. In this context Ohm developed the notion of "equivalent length" as a measure of the resistance of a component under study compared to a standard wire.[51] None of these experimental papers included any mathematics other than

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49 This interpretation clearly diverges from that of Jungnickel and McCormmach *Intellectual Mastery*, vol. 1, who accept as physics much of what was mathematics in the early nineteenth century. While they recognize the importance of French mathematical physics, they do not explore what that discipline was, or, how the Germans understood that discipline. They also do not define what they mean by the discipline whose history they are narrating, that is, theoretical physics. On this last point, see also Cahan, "Pride and Prejudice in the History of Physics: The German Speaking World, 1740–1945," *Hist. Stud. Phys. Sci.* 19 (1988): 173–191, and Pearce Williams, "Review of *Intellectual Mastery of Nature*," *Hist. Math.* 15 (1988): 389–392.

50 There are numerous references to Fourier in Ohm's mathematical paper on galvanic electricity. See Kenneth Caneva, " Ohm, Georg Simon," *Dict. Sci. Bio.* vol. 10, 186-194, 188.

51 See John L. McKnight, "Laboratory Notebooks of G. S. Ohm: A Case Study in Experimental Method," *Amer. J. Phys.* 35 (1967): 110-114, 111-112. The first paper in which this comparative measure of resistance appears is Ohm, "Vorläufige Anzeige des Gesetzes, nach welchem Metalle die Contact-Electricität leiten," *J. für Chem. Phy.* 44 (1825): 110-118, reprinted in *Ann. Phy.* 4 (1825): 79-88, and Ohm, *Gesammelte Abhandlungen*, E. Lommel ed., (Leipzig: Barth, 1892), 1-8. Ohm described experiments comparing the conductivity of several metals and ordering them with respect to their conductivity in Ohm, "Über Leitungsfähigkeit der Metalle für Elektricität," *J. Chem. Phy.* 44 (1825): 245-247, reprinted Ohm, *Abh.*, 9-10. He argued with the work of Becquerel and Barlow on conductivity in Ohm, "Über Electricitätsleiter," same journal and volume, 370-373,

the statement of the algebraic laws deduced from his data relating "loss of force" to various "equivalent lengths of the conductors" in his circuit and one simple differentiation and integration to reach a more general form of his empirical law.[52]

The development of the mathematical implications of his law were published separately. In the first half of this paper, Ohm used a geometrical analogy to illustrate the meaning of "electroscopic force" and "tension" for the overall circuit excluding the electrochemical cell, and reconstructed his experimental law through this analogy.[53] Physically Ohm was trying to develop a language for the potential in the galvanic case, much as Priestley and Cavendish had in the electrostatic one.[54] Because Ohm used accepted explanatory terms, most importantly force, and did not clearly differentiate between force and potential he complicated his physical explanations.

Ohm then investigated the circuit by considering the "flow" of electricity at individual points of the circuit, excluding the electrochemical cell. His language in this section directly reflected Fourier's in his theory of heat, for example his observations on the "diffusion" of electricity. Taking the annulus as an appropriate analogy for a circuit, Ohm constructed a partial differential equation for the diffusion of electricity in a closed annulus, which is precisely the form given by Fourier for the diffusion of heat in the same geometrically shaped thermal conductor.[55]

$$\gamma \frac{du}{dt} = k\frac{d^2u}{dx^2} - \frac{bc}{w}u.$$

reprinted Ohm, *Abh.* 11-13.

52 The mathematical manipulation appeared in Ohm, "Vorläufige." The linear form of his law appeared in Ohm, "Bestimmung des Gesetzes, nach welchem Metalle die Contactelektricität leiten, nebst einem Entwurfe zu einer Theorie des Voltaischen Apparate und des Schweiggerischen Mutliplicators," *J. Chem. Phy.* 46 (1826): 137-166, reprinted Ohm, *Abh.*, 14-36, 25.

53 Ohm, *Die galvanische Kette, mathematisch bearbeitet* (Berlin, 1827), reprinted, Ohm, *Abh.*, 61-186, translated as Ohm, "The Galvanic Circuit, Investigated Mathematically," in *Scientific Memoirs, selected from the Transactions of Foreign Academies and Learned Societies and from Foreign Journals,* Richard Taylor, ed. vol. 2 (New York: Johnson Reprint of 1841 edition, 1966), 401-506, that closely follows the German edition. The geometrical analogy appears on pps., 405-416.

54 It is best to keep in mind that in the 1820s, while one can connect electrostatic and galvanic phenomena, they were kept separate, even seen as two kinds of electricity. See Thomas Archibald, "Tension and Potential, Ohm to Kirchhoff," *Centaurus,* 31 (1988): 141-163 on the development of the concept. Gustav Robert Kirchhoff, "Über eine Ableitung der Ohm'sche Gesetze, welche sich an die Theorie der Elektrostatik anschliesst," *Ann. Phy.* 76 (1849): 506–513, translated as, Kirchhoff, "Ohm's law and Electrostatics,"*Phil. Mag.* 37 (1850): 463-468, drew Ohm's work and electrostatics together.

55 Ohm, "Galvanic Circuit," *Scientific Memoirs,* 451. This equation appeared in Fourier *Analytical Theory of Heat,* 88. The differentials are partial differentials. See also Bernard L. Pourprix, "La mathématisation des phénomènes galvaniques par G. S. Ohm (1815-1817)," *Rev. Hist. Sci.* 42 (1989): 139-154, on the mathematical aspects of Ohm's work.

In setting up his equation Ohm applied Fourier's arguments in the thermal cases directly to the galvanic circuit.[56] Leading up to the above derivation was a long discussion on whether such inhomogeneous partial differential equations were proper in mathematics. Ohm traced this mathematical quarrel back to Fourier and Poisson on the one hand and Laplace on the other. He argued against Laplace because, in transforming physical phenomena into differential form, Ohm assumed that the effects of forces between infinitely small bodies could only extend a certain distance. Laplace did not limit his microscopic forces to microscopic distances. However, Ohm's actual mathematics depended neither on microscopic forces between particles, nor on the assumption that matter is made up of particles. The derivation was appropriated from Fourier.[57]

There was no explanation in Ohm's construction of his partial differential equation of physical processes. Nor did Ohm attach any physical significance to the constants in the functions of integration. The special case Ohm explored, where $b = 0$, was the steady state case, although this was not stated as such by Ohm.[58]

The final expression that Ohm obtained for u, the "electroscopic force" was,

$$u = \frac{a}{2\ell}x + a\sum_{i=1}^{i=\infty}\left\{\frac{1}{i\pi}\sin\frac{i\pi(\ell + x)}{\ell}e^{-k'\pi^2 i^2 t/\ell^2}\right\}$$

To reach this solution Ohm used the functional methods of Laplace and Poisson as much as the mathematically more radical ones of Fourier. There were actually two exponential terms, the functional equivalent of the Fourier series, which were reduced to one by taking a mathematical condition so that only the negative exponential was left. Ohm asserted that, as $t \to \infty$, the expression would become linear with only the first term remaining. In this linear, rump equation, x was the direction in which the electricity flowed, and a was left undefined physically.

Ohm was not merely influenced by Fourier but annexed pages of Fourier to solve the problem of reducing the galvanic circuit to mathematics. He then solved the mathematical equation following the methods of Fourier and other French mathematicians. In hewing to the French model, Ohm examined a mathematically special case to replicate the empirical results of his experiments. This would

56 Heidelberger, "Change in the Baconian Sciences," argues that in accepting Fourier Ohm abandoned ontology. This meant that Ohm went against the prevailing notion in physics that knowledge of nature could not be gained using mathematics. He was, therefore, judged to be doing bad physics. Ohm suffered outraged criticism from Georg Friedrich Pohl, a Hegelian, and his experiments were also criticized by Gustav Theodor Fechner. However, the evidence is that in the 1820s a commitment to physical explanation did not preclude investigating the mathematical implications of empirically deduced relations given the opinions current amongst mathematicians and physicists.

57 This was noted also by Caneva, "Ohm,"190, although his language is more muted than my own.

58 This was also noted by Caneva, "Ohm," 191.

validate his mathematics. He did not consider whether the special, mathematical case mirrored the conditions of his experimental one. An omission seen also amongst French mathematicians.[59] Ohm was working closest to French mathematical physics. While he did not create the mathematics, he explored it in some depth. And, this was hardly surprising, Ohm was a mathematician and a teacher of mathematics at the Koln gymnasium.[60] Ohm's subsequent mathematical papers on the same subject were further manipulations of the mathematical equations of his 1827 paper. They were also published separately from his physical, vernacular explanation of the difference between force and tension.[61]

In his practices Ohm was replicating the French. He presented the complete solution to a problem, experimental in physics, and the mathematical implications, in mathematics. And in the mathematical papers Ohm discussed only mathematically significant cases. In his answers to criticisms of his empirical law, Ohm's defense of his work was confined to his experiments and the physical models of his rivals.[62] This pattern of considering the mathematics of physical phenomena as mathematics reappeared in his subsequent work on sound and light. Again Ohm took all kinds of mathematically developed special cases and connected them to empirically known results without any specific assumptions about light or sound other than their wave nature. They were exercises in the mathematics of waves moving through homogeneous substances.[63]

The later, physical model and physical interpretative weight placed on this 1827 paper did not appear in the original. To label Ohm as a physicist and his mathematical work as physics was to miss the point of what he was actually trying to accomplish. At the beginning of his 1827 paper, Ohm echoed Fourier in his belief that his investigation would "secure incontrovertibly to mathematics the

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59 Yet physical information lies within the mathematics. The rate at which the exponential term approaches zero depends on $k'\pi^2 i^2 t/\ell^2$ where ℓ is the length of path, k' is the conductivity, and $i = 1, 2$, etc. Ohm did not investigate this physically or mathematically.

60 Jungnickel and McCormmach, *Intellectual Mastery* vol. 1, see Ohm's purposes as physical because Ohm did not test for the convergence of the series he was using. His model, Fourier, did not either. By contemporary standards both were doing mathematics.

61 Ohm, "Nachträge zu seiner mathematischen Bearbeitung der galvanischen Kette," *Archive für die gesammte Naturlehre* 14 (1828): 475–493. The physical explanation appeared in Ohm, "Nachweisung eines Überganges von dem Gesetze der Elektricitätsverbreitung zu dem Spannung," same journal 17 (1829): 1–25.

62 See Ohm, "Zur Theorie der galvanischen Kette," *J. Chem. Phy.* 67 (1833): 341–354, reprinted in Ohm, *Abh.*, 560–572.

63 See Ohm, "Über die Definition des Tons, nebst daran geknüpfter Theorie der Sirene und ähnlicher tonbildender Vorrichtungen," *Ann. Phy.* 59 (1843): 513–565, reprinted Ohm, *Abh.*, 587–633, and Ohm, "Erklärungen in einaxigen Krystallplatten zwischen geradlinig polarischtem Lichte wahrnehmbaren Interferenz-Erscheinungen in mathematischer Form mitgetheilt," *Abh. der Math.-Phy. Cl. König. Bayerische Akad. Sci.* 7 (1853): 43–149, 267–370, reprinted Ohm, *Abh.*, 665–855.

possession of a new field of physics, from which it had hitherto remained almost totally excluded."[64] Ohm's experimental results were physically important but his goal was not to produce a physical interpretation of those results in mathematical form.[65]

Neither can we straighforwardly interpret Franz Neumann's research as physics. In the same decade as Ohm, Neumann emulated the French in producing experimental results and separate, mathematical explorations arising from his experiments. Neumann's early research followed a pattern he exploited all his life. In all his work, experimental and mathematical, he had predecessors on which he modeled his own work. The focus of much of his early experimental research was the exploration of the physical properties of crystals, initially extending the work of Gustav Rose. In the mathematical analysis of the properties of crystals, Neumann used Joseph Fourier for heat, Augustin Fresnel, Navier, Cauchy, and Poisson for light. In his work in electromagnetism he followed Ampère.

As in all mathematical physics, Neumann tried to push the mathematical analysis beyond the solutions of his predecessors into more general mathematical territory. However, much of this mathematical work was limited by physical considerations. In his mathematical work on heat conduction in crystals, he went beyond Fourier's work on homogeneous solids, where thermal conductivity was a constant, into solids in which conductivity became a function of the symmetry of the crystal. In his work on light, the elasticity of the ether also became a function of the symmetry of the crystal, not a constant of its motion as in homogeneous solids. In electromagnetism, Neumann included more geometrical cases than those explored already in the experimental results of Heinrich Friederich Emil Lenz and Faraday. In this last example Neumann was able to develop a general mathematical approach from which all the physical occurrences of electromagnetic induction could, in principle, be deduced. In the case of his work on crystals there was no general mathematical expression or function from which he could deduce the physical cases that led to the experimental results which were the starting point of his mathematical work.

Neumann's position was difficult. He had examples of completed solutions to physical problems, both the experimental and the mathematical part, but he did not share with his French predecessors the same level of formal training in mathematics. He did understand the mathematical game enough to know that in mathematical physics points were scored through the display of technical prowess. The mathematical problem usually emerged as a partial differential equation to be

64 Ohm, "The Galvanic Circuit," 404.

65 Initial reactions to Ohm's law were to his experiments. In 1831 Fechner carried out an extensive series of experiments to test Ohm's experimental results that were in turn criticized by Ohm. Fechner, *Maassbestimmungen über die galvanische Kette* (Leipzig: F. A. Brockhaus, 1831). Fechner was followed over the century by many others. For the fate of Ohm's Law see Caneva, "From Galvanism to Electrodynamics."

solved either for the first time, or in a more general form than before. Thus if the density, in the case of elasticity, had already been considered as a constant, the next technical step was to assume the density $\rho = \rho(x, y, z)$. After reconstructing the equations of motion for such a material, the solution might be pushed further by particular cases by assuming a given functional form for ρ, or, for the general case. The only limitation on the mathematical exercise was that the mathematician had to begin in known experimental results and replicate other known results. This was usually done by imposing restrictions on the mathematical solution obtained, without actually noting whether the restrictions were physically plausible.

To complement his mathematical work, Neumann extended previous experimental work into new domains. He began with heat conduction in crystals. Neumann also added the new German passion for precision. He applied the example set by Bessel and his analysis of astronomical measurement to all his experiments on crystals.[66] Neumann's experimental research began as an extension of the quantitative methods of French physics into mineralogy. His purely geometrical examination of crystal symmetry became his dissertation in which he developed new methods of stereographic projection.[67]

His subsequent experimental work on crystals was published separately and kept distinct from his mathematical work on crystals. In doing both he was following his model in the exploration of heat, Joseph Fourier. Fourier was an early and crucial influence upon Neumann and his research methods. Neumann copied from Fourier's theory of heat at least enough to remember his results, if not his derivations. He also copied Fourier's justification for his theory of heat,

> The differential equations of the propagation of heat express the most general condition and reduce the physical questions to problems of pure analysis and this is the proper object of theory.[68]

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66 See Kathryn M. Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics* (Ithaca NY: Cornell University Press, 1991), chap. 2 for a discussion of Bessel's analysis of the second-pendulum, and Neumann's use of Bessel's approach in the analysis of experimental data.

67 The experimental methods were developed in Franz Ernst Neumann, *Beitrage zur Krystallonomie* (Berlin: 1823). Neumann published one paper on crystal symmetry before his dissertation, Neumann, "Über das Crystallsystem des Axinits," *Ann. Phy.* 4 (1825): 63-76. Neumann's dissertation was published as, "De tactionibus atque intersectionibus circulorum et in plano et in sphaera sitorum, sphaerarum atque conorum ex eodem vertice pergentium commentatio geometrica,"*Isis,* (1826): cols., 349-369, 468-489, see Franz Ernst Neumann, *Gesammelte Werke,* M. Krafft, E. R. Neumann, H. Steinmatz and A. Wangerin, eds. 3 vols. (Leipzig: B. G. Teubner, 1906-1928), vol. 1. In these papers Neumann displayed his ability to visualize relationships in space that is so apparent in his work in optics and electromagnetic induction.

68 Olesko, *Physics as a Calling*, 123. Olesko is the latest to note this. See Woldemann Voigt, "Gedächtnissrede," in Neumann, *Gesammelte Werke* vol. 1, 3–19. The quotation is from Fourier, *Analytical Theory*, 6.

This remained Neumann's goal. Mathematical physics was distinct from physics itself which was quantitative experiment. Mathematical physics was, and remained for Neumann, part of mathematics. His research commitment was to solving problems completely, both the experimental and the mathematical part.

As a potential new faculty member within the reformed Prussian University system, Neumann asked that his teaching assignment match his research commitments. He wanted a post where he might teach the mathematics that would complement his experimental research, namely, mathematical physics. He therefore requested a position where he could teach those aspects of physics which had "received a higher mathematical development or those which are capable of being so treated."[69] He also set about teaching experimental physics, which he accomplished quickly. To reach the goal of teaching mathematical physics took longer. He also spent the next twenty years expanding his research into a complete, experimental and mathematical understanding of crystals.

His later experimental work on the specific heats of minerals was new and distinguished by the ways in which Neumann treated his data, not in his stance towards "mathematical" developments. Here he used Fourier's expression for the loss of heat from the surface of a sphere to correct an expression in the reduction of data from the method of mixtures. Neumann was using analysis, not a theory, to examine data. What was new was his ability to appropriate Fourier's analysis into a novel experimental situation. Fourier allowed him to improve, in ways parallel to Bessel's work in observational astronomy, his understanding of his data. This was not the experimental confirmation of deductions from a generalized mathematical solution for an equation expressing physical processes. His models were geometrical and algebraic. As an experimentalist, Neumann's considerable innovations were directed to improving physics as experiment and grafting mathematics onto that. In these early papers there was no new image of mathematical physics.

We have in the 1820s two important specific examples of German physicists emulating the French in the pursuit of experimental and mathematical physics. Their mathematical sophistication was decidedly below that of their model. Given the quality of the training available in mathematics for them as students, it was remarkable that Ohm and Neumann accomplished so much in this decade. However, available mathematical training for would-be mathematical physicists in Germany was about to change.

Changes in Mathematics in the 1820s

During the 1820s mathematicians developed a new philosophical justification for the practice of their discipline that reflected the values of the current neo-humanist

69 Luise Neumann, *Franz Neumann Erinnerungs blätter von seiner Tochter* (Leipzig: F. S. B. Mohr, 1904), 226. Also quoted in Olesko, *Physics as a Calling*, 129.

philosophy of the Prussian university system.[70] With this new philosophical justification, mathematicians could claim the ultimate value of mathematics–training the mind. The mathematician most successful in articulating this philosophy was August Crelle. He also embodied that new philosophy in the title and contents of a new journal, *Journal für Reine und Angewandte Mathematik.*[71] To develop his philosophy of mathematics, Crelle reached back to the ideas of Immanuel Kant. Among other aspects of Kant's philosophy he adopted was the separation of mathematics into "pure" and "applied." "Pure" mathematics was the mathematics of quantity, pure number–pure because it was the product of human intellect alone.[72] Geometry was not such a pure product of the human intellect, being both dependent on reason and experience and hence "applied." Crelle made this philosophy manifest in the contents of his journal. Within its covers were papers on both the algebraic and geometrical parts of mathematics.

Crelle and others carried the banner of this new philosophy and content of German mathematics into the institutional forms made available through the reformed university systems. Mathematicians quickly remade their discipline into an academic profession that became the pattern sought by organizing members in other scientific disciplines.

These values were successful within the university but led to less happy outcomes for other institutions. One such struggle was over the curriculum for the proposed Berlin Polytechnic.[73] The curriculum both defined the social place of the institution and its graduates.[74] For Crelle, mathematics was the foundation of all technical education. The crucial issue here was what Crelle would have included in "mathematics" both pure and applied. Mathematics also embraced "the mathe-

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70 We will only consider those aspects of German mathematics that affected physics in this era.

71 See Wolfgang Eccarius, "Der Techniker und Mathematiker August Leopold Crelle (1780-1855) und sein Beitrag zur Förderung und Entwicklung der Mathematik im Deutschland des 19 Jahrhunderts," *NTM,* 12 (1975): 38-49, and, "Zur Gründungsgeschichte des Journals für reine und angewandte Mathematik," *NTM,* 14 (1977): 8-28.

72 See Gert Schubring, *Die Entstehung des Mathematiklehrerberufs im 19 Jahrhunderts* (Basel: Beltz Verlag, 1983), and Jahnke and Otte, "Origins of the Program of the 'Arithmetization of Mathematics'," and Gert Schubring, "The Conception of Pure Mathematics," in *Social History*, Mehrtens, Bos and Schneider, eds. 21-49 and 111-134 respectively.

73 Many hands and opinions tried to shape this institution. Alexander von Humboldt tried to add chemistry to its curriculum and make the institution a second École Polytechnique.

74 Kees Gispen, *New Profession, Old Order,* chap. 1, 15-43 sketches the problems of engineers in German society against the general social and cultural background and the role of education in the defining of engineers in German society in the early nineteenth century.

matical parts of physics."[75] Mathematics was to train the mind and number, space, and force were its subjects.[76]

This was neohumanism at its most forceful but not necessarily at its most successful. When it was finally established, the Berlin Polytechnic did not have a curriculum based on Crelle's ideas.[77] However, Crelle was successful, along with Jacobi, in establishing this definition of mathematics in universities and of remolding mathematics there. Jacobi claimed the high ground for mathematics because of its esoteric nature. In a letter to Alexander von Humboldt, Jacobi voiced the opinion that the most "lofty of sciences were the most impractical." He considered his work in astronomy (the subject under discussion was Neptune) as in the proper sense mathematics. In this work he had never considered its application to actual astronomical problems.[78] Jacobi delighted in the consternation he caused at the 1841 meeting of the British Association when expressing similar opinions.[79]

Crelle's definition of the content of mathematics also emulated the content of French mathematics. Mathematical physics and the mathematical parts of physics remained mathematics. Although the problem with the French, in Crelle's opinion, was that they emphasized application too much.

Crelle's assessment of what the discipline of mathematics covered, number, space and force, was commonly adopted amongst mathematicians and younger academics. Jacobi and the other young faculty would create this new academic profession and discipline of mathematics and outstrip the French. Not that there emerged from these efforts one exclusive set of practices that defined mathematics. However, by the last third of the nineteenth century, the approach to mathematics through the solution of physical problems was relegated to the level of a secondary

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75 Schubring, "Mathematics and Teacher Training: Plans for a Polytechnic in Berlin," *Hist. Stud. Phys. Sci.* 12 (1981): 161-194, 174.

76 Crelle defended the "purity" of mechanics in Crelle, *Encyklopädie deutsche Darstellung der Theorie der Zahlen* (Berlin: 1845), vol. 1, iii-iv. See also Schubring, "Mathematics and Teacher Training," 178.

77 Gispen, *New Profession, Old Order,* notes that the Polytechnic and its curriculum reflected the now lowly place given "bread study." The mathematics in the curriculum was low-level and the Berlin Polytechnic became the training ground for engineers. The Polytechnic was overseen by the Trade Ministry rather than the Kultus Ministerium which had important political outcomes as well as effects on the curriculum.

78 K.–R. Biermann, "Über die Förderung deutscher Mathematiker durch Alexander von Humboldt," in *Alexander von Humboldt: Gedenkschrift zur 100 Wiederkehr seines Todestages* (Berlin: Akademie Verlag, 1959) 83-160, p. 88. See also Biermann,"Der Briefwechsel zwischen Alexander von Humboldt und G. J. Jacobi über die Entdeckung des Neptun," *NTM,* 6 (1969): 61–67.

79 See *Briefwechsel zwischen C. G. J. Jacobi und M. H. Jacobi,* W. Ahrens, ed. (Leipzig: B. G. Teubner, 1907) 22.

art in the hierarchy of German mathematics.[80]

Physicists and physics were not untouched by the philosophical issues surrounding the necessary use of some mathematics in the newly quantified experimental physics. While historians have detected a turn away from Naturphilosophie in the 1820s, this did not include an automatic realignment of physics along the lines of the modern discipline. Nor did this theoretical turn necessarily point the way to a physics in which experiment and mathematics were integrated into a recognizable form of theoretical physics As German physicists turned to the French as a model for doing physics, given the discussion and philosophical context of all their science, the issue of the relationship between physics and mathematics was problematic. If physics was still the search for essences and physical imagery necessary for theory, what had purely deductive knowledge, mathematics, to do with physics?

Although a philosopher, and neither a mathematician nor a physicist, Jakob Fries justified to both Georg Wilhelm Muncke and Gauss the use of mathematics in experimental philosophy. Fries systematically developed Kant's ideas on mathematics and its relation to natural philosophy to reestablish Kantian categories and a place for Newtonian gravitational theory in the sciences. For Fries, mathematics had two parts "pure" mathematics, which can only give us knowledge of laws in their general form, and "applied" mathematics in which the particular characteristics of the case emerge. Mathematics was necessary for the complete understanding of "external" nature through mechanics, the mathematics of the corporeal world.[81]

In the 1830s Muncke became one of the editors of *Physikalisches Wörterbuch* and wrote the entry on physics. Muncke described physics in Kantian terms; it was empirical, yet not merely a descriptive (historical) study of nature because it was also explanatory. For the latter, hypotheses were necessary. Mathematical physics began where experimental and theoretical physics ended because it started with the quantities uncovered by experimental physics. Because it was a deductive science mathematical physics was also "pure."[82]

Muncke's statements were more illustrative assertions and analogies made between physics and astronomy and the history of mathematical physics than a coherent argument for how these elements might be pulled together into a single discipline. He had no way of actually connecting the two disciplines except to

80 In the 1820s we detect similar divisions of the intellectual territory between mathematics and physics in the new philosophical context in the short-lived *Zeitschrift für Physik und Mathematik* edited by Andreas von Baumgartner and Andreas von Ettingshausen.

81 Gurt König, "Einleitung zur Abteilung," to Jakob Friedrich Fries, *Sämtliche Schriften* vol. 13, Schriften zur Angewandten Philosophie II Naturphilosophie und Naturwissenschaften (Darmstadt: Scientia Verlag reprint, 1979), 7-15, 12-14.

82 Muncke, "Physik," in *Physikalisches Wörterbuch* 11 vols (Leipzig: Schweikert, 1825–1845) vol. 7 (1833): 493–573, 510–511. His effort to distinguish theoretical and experimental physics appear on pages 503–504.

claim that "as long as we neglect to use mathematical methods" physics was incomplete. He could only point to the example of the French and their role in developing mathematics and to state simply that, for the further study of phenomena, the calculus and geometry were as important as experiment.

Many statements reinforcing this view of mathematics and physics occurred in the entry on "Mathematics" by Heinrich Wilhelm Brandes. Pure mathematics was about quantity and had no bearing upon the empirical realm. He did not include geometry in pure mathematics because it referred to experience, only quantity qualified for inclusion. While mathematics was independent of experience, it did develop the consequences of hypotheses in the sciences through the rules of arithmetic and geometry. In its development there were no contradictions, unless from human frailty. From the rules of arithmetic, those of algebra and the calculus followed "in a natural sense." Reducing natural phenomena to mathematical form had much to recommend it, as mathematics could easily examine the correctness of hypotheses. Brandes gave some general, loose justifications for a relationship between physics and mathematics but no systematic understanding of how this might be accomplished in practice. Also Brandes did not seem to understand the difficulties of examining the correctness of hypotheses with mathematics.[83]

Other definitions of physics existed that did not generate such tensions. Poggendorff published a manifesto, as he began his long career as editor of the *Annalen der Physik und Chemie*. Only purely scientific matter was to appear. The drift of physics towards chemistry was overwhelming but his volumes would include work in meteorology and physical geography. Pure mathematics lay outside its coverage. Mathematics was included only in so far as it "made experiments more precise or where a series of data can be brought together into an essential relationship through a theory created from the principles of mechanics." Poggendorff assumed, along with his colleagues, that mechanics was a branch of mathematics. Overuse of formulae would be avoided and mathematics would find a place in his journal only "when it reflects the true interests of physicists."[84]

In his first decade as editor, Poggendorff reprinted many foreign articles in experimental physics, but afterwards domestic research papers predominated. It was not until the 1840s that mathematical physics entered the journal as a steady stream in papers on the wave theory of light, and electricity. Many of them were also translations from foreign journals.[85] In the 1840s most of the papers were experi-

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83 Brandes, "Mathematik," in *Physikalisches Wörterbuch*, vol. 6 part 2, 1473-1485.

84 Poggendorff, "Vorwort," *Ann. Phy.* 1 (1824): vii.

85 See Jungnickel and McCormmach, *Intellectual Mastery,* vol. 1, chap. 5 for some of the details of the types of articles published during the 1840s. Their comments on the content of this, and other journals in chapter 2 are based on the assumption that "mathematical physics" was physics in the 1820s. The overlap in coverage between Poggendorff's and Crelle's journal in their account is left unexplained.

mental, some were on instrumentation and Poggendorff still published articles in chemistry, meteorology, and climatology.

Not that algebra, even expressions using the calculus did not appear in *Annalen* papers. However, the role these expressions played and their relationship to physical concepts, and the exploration of the implications of these concepts for the behavior of phenomena, require some examination. Poggendorff published an account of Ampère's electrodynamics without any mathematics at all. This is hardly surprising because the physics of Ampère's work was self-contained. On the other hand, Laplace's mathematical theory of capillarity appeared in full. But in this version the mathematical part by Laplace was separated from the physical explanation of Biot. In 1831 Poggendorff published a translation of Airy's work on the theory of light with a footnote that this well known mathematician had made a particular study of the subject. The subject was clearly mathematical but an important one for physicists and he referred to other papers on light included in the same volume. Among those papers was Fresnel's mathematical paper on double refraction. The kind of mathematics that reflected the true interests of physicists was broadening. Something was changing but the consequences and direction of those changes were still unclear.

Mathematical discussions of physical phenomena were becoming of more interest to the real needs of physicists. Yet mathematical physics was still identified as a branch of mathematics, and when published, lay in juxtaposition to the experimental papers on the journal's pages. If we assume that Poggendorff's journal mirrored the state of research in physics during these decades, theoretical physics did not appear in the *Annalen* in any recognizably modern form until the middle of the 1840s. Prior to the 1840s, articles in mathematical physics were usually translations. Clearly both Poggendorff and his readers assumed mathematical physics was, at the very least important for physics, but not as yet physics. Nothing had changed to redefine the boundaries between mathematics and physics.

The process from a separate experimental physics and mathematical discipline of mathematical physics to theoretical physics was difficult, piecemeal and sometimes elliptical rather than linear. The triumph of theoretical physics in the last third of the nineteenth century has made the disciplinary distinctions prevailing in the first part of the century invisible. Retrospective interpretations of the contents of, say Ohm's or Neumann's work similarly mask the transformations these men helped to bring about within their disciplines. Recent accounts of nineteenth-century theoretical physics see the discipline as a cluster of approaches to theory without distinguishing their origins within German physics or outside that discipline's assumed boundaries. Every paper that refers to what has become accepted as physical has been treated as such. Thus, we have a narrative of what physicists did, but not what theoretical physics was, or how it came into existence. In these accounts theory in physics becomes a smudgy mess of different practices with no particular reason for their existence except for their usefulness at the time, and the personal preferences of the historical actors. It also leads to some complex problems of accounting for certain forms of physics such as Neumann's. And it does not explain why mathematical physics continued to be considered different from theoretical physics and treated as such. It also ignores how and why mathematicians could still publish in areas of "physics" and loudly claim the domain of mathematical physics as their own.

What this historical approach does require is that we accept any mathematical expression of a physical problem and its mathematical development as physics. This belies the contention that theoretical physics was created in the reformed universities of Germany in the nineteenth century. Such mathematical solutions to physical problems were available from the time of Newton. We need to pay attention to the descriptive language of these two intellectual disciplines. No one in Germany used the terms mathematical and theoretical physics interchangeably, or simultaneously. The first was an inheritance from the French, the second a creation of the nineteenth century.

Changes occurred in both the professions and disciplines of mathematics and physics that were interconnected and form a continuum of interests through time and across changing disciplinary boundaries. Both disciplines later laid claim to the mathematical development of physical problems. German mathematicians continued to use emerging areas of physics as sources for research problems in mathematics. At the same time academics who became physicists or who held appointments as physicists began to publish mathematical investigations of the same physical phenomena. Initially the investigations within either discipline were mathematics. In the 1840s these mathematical investigations began to diverge as the aims of physicists became more closely defined and included the conscious exploitation of hypotheses. Mathematicians continued to develop their version of mathematical physics using the mathematics of physics problems, solved in more general ways. Simultaneously, physicists were creating a new discipline, theoretical physics. Physicists annexed parts of that domain, mathematical physics, that had belonged exclusively to mathematicians, then reconfigured it to match their changing disciplinary needs through the consideration of a few crucial problems in electrodynamics, light, and heat.

Chapter VI

On the Margins: Experimental Philosophy and Mathematics in Britain, 1790–1830

Experimental philosophers in Britain developed their own forms of theoretical physics during the same period as the Germans. In broad outlines, the processes through which these transformations occurred were the same. Socially experimental philosophy became a profession rather than an avocation; passage into the research community narrowed from self-education to formal, certified educational levels within the universities of Britain. Access to entry into the research communities was consequently constrained by these formal, educational gateways. Training became the modern apprenticeship of graduated courses, problems sets, and textbooks along with laboratory courses. Access narrowed to the social institutions of science that had appeared as open and serving many cultural, economic, and social purposes in the late eighteenth century. Their memberships and purposes became limited to the professional, research oriented physicist. The institutions that had been intellectually universal and geographically local became narrowly specialized and geographically national.

Research itself was transformed in its practices, purposes, subject matter, and the outlets available for its dissemination. The coverage in scientific journals changed from being broad to increasingly specialized. The style of their contents changed from the loquacious to the more terse, footnoted prose of the scientific paper. Texts were replaced by journals as the major outlet for research. Yet, the core of physics remained experiment. While speculation was always allowable, if hedged with cautions as to its use, the development of such speculations into theories, through loose argument by analogy, illustration and metaphor, were jettisoned for theory expressed in the languages of mathematics. The myriad forms of theories were replaced by fewer alternatives, subject to the test of quantitative experiment. In this, physicists gained the ability to predict the outcomes of experiments. French mathematical physics and quantitative experimental physics were the instruments for much of this process. The outcome was not a copy of French science. As in the

German case, native traditions mutated this newly acquired heritage into unique forms.

All of these processes began in the thirty years that are the subject of this chapter. They were not completed until the last third of the nineteenth century.

One of the social costs of these changes was the isolation of physicists from the general public and the need for mediators between research practitioners and that public. In the nineteenth century these mediators were often professionals who understood the different audiences that they needed to address, research peers, student-apprentices, students destined for other careers, an informed public and the more general, interested public. These various listeners were looking for different kinds of understanding through physics and each required different languages of explanation, depth of coverage, and the kind of argument necessary to draw that audience into the subject matter.

Many of these changes were completed after 1830. Yet by 1830 the possibilities so apparent in the early nineteenth century for the practice of experimental philosophy had narrowed in form, access, and content. While the final outcomes of these transformations can be described in general outline in language close to the description of developments in the German states, the passage to professionalization and theoretical physics was unique, much more diffuse, and driven by economic as well as ideological and political forces. The net results were quite different in detail from those of Germany. Within the three decades described here, only a few of the social and intellectual forms explored became lasting possibilities. The universities of Oxford and Cambridge retained, for the most part, their function of a general education for the elite. By the middle third of the nineteenth century, with the economic development of southern Scotland, Scottish universities offered an alternative more open to the educational needs of the industrial economy. At the end of our period, symbolically, if not practically, London University was beginning to do much the same. Higher education was opening up for more men further down the social scale. The education offered was for a society, economy, and political order of a different kind from that assumed and still operative in the educational philosophy of Oxbridge.

Social Institutions

The particular dates chosen for this section emphasize this era's essential social and intellectual continuity of the study of nature with the eighteenth century. However, the social and intellectual forms of the eighteenth century came under increasing strain during the 1820s and then dissolved to re-form during the 1830s and 1840s.

In the decades around 1800 experimental philosophy was still an avocation for most of its practitioners who formed a loosely connected network. Experimental philosophy still defined a set of practices rather than a specific series of research problems or a theoretical stance. Physics still covered, for example, every aspect of electricity from static electricity to the physiology of electric fishes. The study of sound and optics included the anatomy of the ear and eye and the perception, as well as the nature of, sound and light and their propagation through space. The study of nature was an increasingly popular aspect of a general culture shared by many varied groups in society. Institutions to inform, demonstrate, and even develop the study of nature multiplied during this era. They catered to all groups and classes under a broad range of economic, cultural, and ideological hats.

Many of the institutions that supported the study of nature were volunteerist and provincial. Their titles, purposes and functioning often included much more than experimental philosophy. Their success depended upon meeting the aspirations and interests of a broad, local, audience.[1] While these institutions were local, their members were not isolated or scientifically unsophisticated.[2] The societies might function for social or cultural self-improvement, as a center for sharing information, research results amongst local practitioners, friends of science and local "worthies" of science.[3] Some institution offered the enterprising an opportunity to transmute

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1 For the diversity represented in such organizations, see *Metropolis and Province: Science in British Culture, 1780–1850,* Ian Inkster and Jack Morrell, eds. (London: Hutchinson, 1983). See also Ian Inkster, "Cultural Enterprise: Science, Steam, Intellectual and Social Class in Rochdale, *circa* 1833–1900," *Soc. Stud. Sci.* 18 (1988): 291–330; Jack Morrell, "Early Yorkshire Geological and Polytechnic Society," *Ann. Sci.* 45 (1988): 153–167, and "Bradford Science, 1800–1850," *Brit. J. Hist. Sci.* 18 (1985): 1–23; J. N. Hays, "Science in the City: The London Institution 1819–1840," *British J. Hist. Sci.* 7 (1974): 146–162, and "London Institution," *Ann. Sci.* 39 (1982): 229–274. The localism and attempts to preserve it in Edinburgh as natural philosophy became science, and national rather than local, are detailed in Steve Shapin, "The Audience of Science in Eighteenth-Century Edinburgh." D. S. L. Cardwell *Organization of Science in England in the Nineteenth Century* second edition (1973), makes little mention of these institutions while focussing on mechanics institutes and the redbrick universities. The narrative of the mechanics institutes is well known, its meaning still contested. See Roy Heyden, "The Glasgow Mechanics Institution," *Phil. J.* 10 (1973): 107–120; Steve Shapin and Barry Barnes, "Science, Nature and Control: Interpreting Mechanics Institutes," *Soc. Stud. Sci.* 7 (1977): 31–74; Ian Inkster, "Science and Mechanics Institutes, 1820–1850: The Case of Sheffield," *Ann. Sci.* 32 (1975): 451–474, and, "The Social Context of the Educational Movement: A Revisionist Approach to English Mechanic's Institutes, 1820–1850," *Oxford Rev. Educ.* 2 (1976): 277–307. See also Gordon W. Roderick and Michael D. Stephens, *Scientific and Technical Education in Nineteenth-Century England* (New York: Barnes and Noble, 1972) chaps., 8 and 9.

2 For a sense of the networks these institutions formed see, Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford: Clarendon Press, 1981), chap. 2 and Appendix II, 544.

3 Nathan Reingold's classification of the supporters and practitioners of science, while developed for the American context, seems apt for this period in early nineteenth-century Britain.

their association with such an institution into a career in science.[4]

There was no set pattern for the management or day-to-day operations of these volunteerist organizations. Trustees set policy that was carried through by a paid, or unpaid administrator. Many shared a financial hand-to-mouth existence, as trustees tried to match institutional programs with perceived local social and cultural demands. Much of the financial distress resulted from the desires of trustees for a concrete form for their interests and aspirations.[5] If they were lucky, such institutions met on the premises of earlier cultural institutions.

Appeals to a large audience were necessary for economic survival, unless the institution had access to local philanthropists. The audiences and hence the libraries, field excursions, lectures, and lecture series of these societies varied as much as their purposes. The ideologies that drove programs varied from the radical to the conservative. Nature might serve all these purposes simultaneously.[6] However, there appeared to be no correlation between class and the ideological purposes that science was seen to represent.[7]

The utility of science was almost universally accepted, even if not acted upon in the institutions' programs. Where utility was a part of the institutions' programs, the economic purposes of these foundations were revealed in the ties assumed by the entrepreneurs, professional, and skilled workers between an understanding of

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4 Arnold Thackray, *John Dalton: A Critical Assessment of his Life and Science* (Cambridge MA: Harvard University Press, 1972) chaps. 4 and 5, details the ways John Dalton fashioned his career at the Manchester Literary and Philosophical Society. Dalton was not the first to do this through the Manchester institution, see Frank Greenaway, *John Dalton and the Atom* (Ithaca NY.: Cornell University Press, 1966), 91–95 on Thomas Henry. There were also Humphry Davy and Michael Faraday at the Royal Institution. For John Phillips at the Yorkshire Philosophical Society see Morrell and Thackray, *Gentlemen of Science* and Martin Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985.) On Charles Lyell's use of London institutions in his early career see, Jack Morrell, "London Institutions and Lyell's Career, 1820–41," *Brit. J. Hist. Sci.* 9 (1976): 132–146.

5 The Royal Institution and many Mechanics institutes were caught in this situation. For the links between architectural form and social values see Sophie Forgan, "The Architecture of Science and the Idea of a University," *Studies Hist. Phil. Sci.* 20 (1989): 405–443.

6 For a case study of such political purposes see, Steve Shapin, " 'Nibbling at the Teats of Science': Edinburgh and the Diffusion of Science in the 1830s," and Michael Neve, "Science and Commercial Utility: Bristol, 1820–1860," in *Metropolis and Province,* Inkster and Morrell, eds. 151–178 and 179–204, respectively. See also Shapin, "Mechanics Institutes," and Thackray, *Science in Manchester.* The ideological purposes of the Bristol Pneumatic Institution should also be noted here.

7 See Dorinda Outram, "Science and Political Ideology, 1790–1848," in *Companion to the History of Modern Science* R. C. Olby, G. Cantor, J. R. R. Christie, and C. Hodge, eds. (New York: Routledge, 1990), 1008–1023.

nature and improvements in manufacturing and personal economic status.[8]

The Royal Society of London and the Royal Institution fit into this pattern, although their source of patronage and membership were national rather than local. Active members and attending audiences were local. In the case of the Royal Society, its scientific purpose represented only one of its functions in the early nineteenth century. Membership was a mark of social status guaranteed by the Society's ties to the politically powerful and the socially prominent.[9] Scientifically prominent members were elected to the society after their scientific worth was guaranteed from other, authoritative sources; unless the science was accompanied by the social attribute of money, or noble birth. The Royal Society was a gathering of the learned to reinforce their social place at the metropolitan center rather than as leaders in research. The conduct of their meetings reinforces this impression.[10]

Initially, the Royal Institution was intended as the complement of the Royal Society. Sir Joseph Banks was on its first board of directors. In diffusing useful knowledge, the Royal Institution put into social form the contentions of natural philosophers that what they did was indeed useful. The Royal Society remained with the more gentlemanly function of pursuing natural philosophy. In his lectures as professor of natural philosophy at the Royal Institution in 1801, Thomas Young tried to "diffuse useful knowledge." His attempt was a disaster. In his lectures on mechanics, Young crammed in the principles of mechanics, the laws of motions, forces, levers, and collisions, followed by a discussion of architecture, carpentry, machinery, clocks and the raising and moving of large weights.[11] His lectures assumed his audience shared a level of knowledge and seriousness of purpose that matched his own. Given the lectures and materials available elsewhere, Young's expectations were unrealistic. The subject matter of his lectures hardly lent themselves to the visual displays that Humphry Davy used in the same institution to much the same audience. Young seemed to drive away the economically essential audience even as Davy attracted it. He acknowledged his defeat by resigning in 1803.

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8 For such goals in the foundation of some societies see, Arnold Thackray, *Science in Manchester* and Robert E. Schofield, *The Lunar Society*.

9 Physicians were a significant percentage of its membership. It was still important for ambitious physicians to be seen as learned advisers as well as mere healers, and as having social contact with the wealthy and powerful. See Harold Cook, "The New Philosophy and Medicine in Seventeenth-Century England," in *Reappraisals of the Scientific Revolution,* David Lindberg and Robert S. Westman, eds. (Cambridge: Cambridge University Press, 1990) 397–436. For the Royal Society's social function and political ties, see Marie Boas Hall, *All Scientists Now: The Royal Society in the Nineteenth Century* (Cambridge: Cambridge University Press, 1984), chap. 1.

10 The diversity, intensity, productivity and independence of the research done by John Dalton in Manchester, Humphry Davy, Michael Faraday, and Thomas Young at the Royal Institution argues for this social function of the Royal Society.

11 Thomas Young, *A Course in Natural Philosophy* (London: Joseph Johnson, 1807) 2 vols.

The institution was shaped by Davy and his career and was reshaped to attract the fashionable London audience.[12] By 1807 the syllabus of the Royal Institution included lectures on moral as well as natural philosophy, drawing, engraving, music, and poetry. Count Rumford's vision of an institution for the diffusion of useful knowledge was gone. Yet the Royal Institution flourished even as its appeal for funds on a national level floundered. It became one of the most successful local cultural institutions of the early nineteenth century.

The multiplication of these institutions and their popularity in the early decades of the nineteenth century meant that a career in experimental philosophy was not consumed by constant, countrywide, itinerant lecturing or teaching. A base in London or some institution in a large city could support a career. Dalton did some lecturing, usually in the summer in the Lake District for the tourist trade.[13] Such professionals also brought tensions into the institutions that housed them. Their needs sometimes clashed with the survival of the institution. Research brought recognition but not income. The lecture series such professionals had to deliver were for large audiences and directed to entertainment as well as enlightenment. It was rare that they could repeat Davy's success and combine the two. Michael Faraday separated the two functions, instituting the Friday night lecture series at the Royal Institution.

For a fee, these institutions offered the public opportunities for an education in natural philosophy unavailable except for the privileged few who attended university. How many besides Faraday transformed such opportunities into careers is still obscure.[14] As obscure is the role that these institutions played in educating the working class in those scientific principles that were commonly seen as the foundation for the arts they practiced. According to this philosophy of progress, knowledge of such sciences should improve their economic future. It was also a means of cultural and moral uplift. However, in the institutions controlled by the working class, indications are that, in the interest of survival, systematic lecture series in the sciences were sacrificed for less demanding and more entertaining fare. Only those institutions catering to the upper and middle classes could afford to offer lecture series in natural philosophy. And these were only delivered in large cities where a significant percentage of the population could afford the expense. Lectures in experimental philosophy delivered over several weeks were more likely

12 On the uses that Humphry Davy made of the Royal Institution in shaping his career see, Jan Golinski, "Davy and the 'Lever of Experiment,' " in *Experimental Inquiries,* Homer LeGrand, ed., 99–136.

13 For Dalton and his lecturing see Thackray, *Dalton,* and Greenaway, *Dalton and the Atom,* chap. 5. For London see J. N. Hays, "The London Lecturing Empire, 1800–1850," in *Metropolis and Province,* Inkster and Morrell, eds. 91–119.

14 Jan Golinski has traced chemists active in London in the early nineteenth century, their education and their professions before they specialized in chemistry. See Jan Golinski, *Science as Public Culture*, chap. 8.

to be offered at universities. Whether we like it or not, the universities remained the locus for education in physics, and hence directed at a privileged minority in British society.

Natural Philosophy and the Universities

The university was a more promising ground for a systematic introduction to experimental philosophy. The aspects of experimental philosophy taught depended on the interests of the faculty of the university. Most students, if they were to enter a profession, were training to become clergy, lawyers, or physicians. Of these only medicine required a systematic training in any experimental science.[15] Experimental philosophy was directed to the cultural, not the professional training of students. Liberal doses of natural theology and moral philosophy dotted the texts for and lecture notes derived from these courses. Chemistry was the first field to break this mold, as chemical skills were in demand in the growing industrial cities of the north and Scotland. It was not coincidental that in 1819 Thomas Thomson established the first chemistry laboratory course for students. In the 1820s, the government made more effort to regulate the medical professions. Private lecture series and courses multiplied in training hospitals directed to future physicians and pharmacists. These courses in purpose, structure, and function form a decisive break with the prevailing teaching in experimental philosophy. Their purpose was to produce competent practitioners, not the liberally educated.[16]

In general, experimental philosophy courses were untouched by this changed social purpose for experimental skills. University courses in experimental philosophy remained as general surveys, meant to display the characteristics of nature through factual information and demonstration experiments.[17] Judging by the content of texts, even in Scotland where such surveys were bolstered with claims for the utility of experimental philosophy, there were few connections made between course material and technology. Lectures were liberally scattered with references to the creator and natural theology lurks in the background, if not the foreground of all of them.[18] There were no systems of courses of graduated difficulty leading

15 Evidence for the most systematic education in chemistry available in Britain was at Edinburgh University, the locus of the best medical training.

16 See *Metropolis and Province,* Inkster and Morrell, eds.

17 The empirical foundation of the sciences assumed in Britain in this period has been emphasized more than once. See Michael Shortland, "A Mind for the Facts, some Antimonies of Scientific Culture in early nineteenth century Britain," *Arch. Int. Hist. Sci.* 36 (1986): 294–324.

18 However, authors of texts in natural theology were already feeling the pressures of experimental philosophy and natural history. They had to adjust their arguments to accommodate new phenomena and current interpretations of a mechanical world. On William Paley see, Neil Gillispie, "Divine Design and the Industrial Revolution: William Paley's

the student from novice to professional competence. Nor was there any need for certification as an end to such an educational enterprise.

There was one great difference between the lectures given by university faculty in England and the lecturers such as Young, Davy, and later Faraday. These three men constructed their careers through their lecture courses and drew their audiences into their research. Their audiences were at the very creation of the knowledge, not just exposed to a demonstration of nature's readily repeated characteristics. In those audiences were their research peers as well as the general public.[19] Such professional lecturers were freer to question the prevailing, broadly Newtonian, explanations of the phenomena revealed in their lectures. They could offer very successful alternatives to long-standing research issues in experimental philosophy and chemistry.

The same imperative to introduce students to research results did not motivate most university faculty. They were primarily expected to transmit to their students the rudiments for entry into gentlemanly culture. A faculty member who developed a reputation as a good teacher would enhance his income. Assuming that he did research, he was not under the same pressure to present his research results to his audience. Some faculty did present their research within their lectures to students, the most obvious being Joseph Black at Edinburgh. The pressures to do so could not match the imperatives of survival in the newly created scientific institutions such as the Manchester Literary and Philosophical Society and the Royal Institution. At a university, research and teaching need not be integrated. In some settings, the problem of losing status existed if the university lecturer was seen as pushing his ideas upon the public, as a projector pushed his schemes for making money.[20]

The function of the faculty, to pass to the next generation the accepted cultural foundations for a gentlemanly existence, actually worked against the intrusion of research results into lectures. At Cambridge, as the Tripos became more important, students were reluctant to attend courses that did not "pay." Unless the material in the course was related to the subject of likely examination questions, classrooms were mostly empty.

University texts for students offered a blander, more acceptable form of experimental philosophy for their students than the texts of independent lecturers. These texts share some common features. Courses on experimental philosophy were surveys dominated by experimental demonstrations. The broadest terms were laid out by John Playfair as, "the knowledge of the general laws obeyed by the phenomena of nature, whether in the intellectual or the material world." Playfair's description

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abortive Reform of Natural Theology," *Isis,* (1990): 214–229. Gillispie points out that Paley's audience included the urban middle class as well as Cambridge students.

19 See Jan Golinski, *Science as Public Culture*, chap. 7.

20 See Jan Golinski, *Science as Public Culture,* chap. 2 and William Cullen's problems with "speculation" at Edinburgh University.

betrayed the presence of Common Sense and the educational philosophies of Scottish universities. Other texts do not go so far as to include psychology in the net of natural philosophy.[21]

University lecturers shared the same Newtonian, non-mathematical and non-technical explanations for the phenomena that were the heart of their lectures.[22] In this vernacular Newtonianism, as at Oxford, the "mathematical approach is apparently deliberately avoided."[23] The subject matter of the lectures joined natural theology to natural philosophy that elevated the mind and, with demonstration experiments, proved God's power.[24] Utility was popular, although proclaimed rather than systematically explored.[25]

The only mathematics in these unrelenting factual narratives punctuated with descriptions of experiments and physical explanations were algebra and geometry. Many experiments were qualitative, very few quantitative as in Biot's text of the same era.[26] The sample experiments were chosen to demonstrate as simply as possible an explanation of the operation of mills, machines, etc.

This utilitarianism reaches into other sections of the lectures as well. Playfair's section on physical astronomy was descriptive. His discussion of the regularities

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21 For Cambridge see the textbooks discussed in chapter II. In the Scottish universities the same pattern prevailed. See, John Playfair, *Outline of Natural Philosophy* (Edinburgh: A. Constable, 1812–1814), 2 vols., as 1. The volumes are clearly from notes for lectures. They are too cryptic for delivery to students. See also John Robison, *Elements of Mechanical Philosophy, being the Substance of a Course of Lectures on that Science* (Edinburgh: Constable and Co., 1804). The broad sweep of the meaning of the term "physics" in this era is in Michael Shortland, "On the Connexion of the Physical Sciences: Classification and Organization in Early Nineteenth-Century Science," *Hist. Scientiarium* 41 (1991): 17–36.

22 For a discussion of mechanical philosophy and its inclusive character, see Crosbie Smith, " 'Mechanical Philosophy' and the Emergence of Physics in Britain: 1800–1850," *Ann. Sci.* 33 (1976): 3–29, 6–14. Smith locates the beginnings of this tradition in Robison's lectures and sees the same patterns in William Meikleham's lectures in natural philosophy at Glasgow University from William Thomson's notebooks on those lectures.

23 Gerald L. E. Turner, "Experimental Science in Early Nineteenth-Century Oxford," in *Hist. Univ.* 8 (1989): 117–135, 123.

24 The ends of science, as expressed by natural philosophers themselves in this era, are discussed by G. A. Foote, "Science and its Function in Early Nineteenth-Century England," *Osiris* 11 (1954): 438–454.

25 Predictably, Davy saw the ends of science as Truth. The relationship between early nineteenth-century science in Britain and the ideas and values of Romanticism are tenuous at best. Very few natural philosophers expressed even Davy's early commitments. Utility was far more compelling within the changing economic environment of northern England and Scotland.

26 A need was however seen for such a text because Biot was translated. See Jean-Baptiste Biot, *Traité de physique expérimentale et mathématique* 4 vols., (Paris: Dateville, 1816), translated by John Farrar.

of the planets was in terms of Kepler's laws put into geometrical form. Fluxions were mentioned in his definition of velocity and acceleration, then dropped. He noted Laplace's *Mécanique Céleste* but did not explore it. On the basis of the rest of the course students would have a hard time making any sense of Laplace. Algebra and geometry were used to express empirical laws, such as those of optics. From these empirical laws, other, equally empirical results were deduced, some of which had already been demonstrated. Beyond a statement of Newton's laws of motion and gravitation, the *Principia* did not enter into experimental philosophy. There were no problem sets.

Initially in the lectures, the definition of experimental philosophy was broad. The actual topics covered were narrowly defined by mechanics with some optical phenomena thrown in. The patterns discussed here for lectures were replicated in all universities including the later ones of Dionysius Lardner at London.

In short the lectures read as protracted encyclopedia articles, many of which were indeed written by the same lecturers. They were both lectures and articles intended for the same audience to serve similar cultural purposes. John Robison's article on "Physics" in the *Encyclopedia Brittanica* remained unchanged from the third through the seventh editions. In his article on mechanics Robison separated mechanical philosophy from mathematics. D'Alembert and Lagrange were,

> merely employing the reader in algebraic operations, each of which he perfectly understands in its quality of an algebraic or arithmetical operation, and where he may have the fullest conviction of the justness of his procedure. Well all this may be (and, in the hands of an expert algebraicist, it generally is,) without any notions, distinct or indistinct, of the things, or the processes that are represented by the symbols made use of.[27]

Study of the natural world and the manipulations of mathematical entities were distinct activities. Similarly, astronomy was based on accurate observations, necessary for "philosophical inference." Mathematics did not enter into Robison's discussion.[28]

John Playfair made much the same distinctions. However, Playfair set the sciences in a hierarchy with mathematics in first place. Its "progress" has been "one principal instrument applied by the moderns to the advancement of natural knowledge." The other instrument of progress was experiment and the method of induction. Bacon's philosophy and experiment received far more attention than did mathematics. Even in this systematic account of the sciences mathematics entered only by deducing results logically from principles established "by experi-

27 John Robison, "Dynamics," *A System of Mechanical Philosophy,* with notes by David Brewster (Edinburgh: John Murray, 1822) 4 vols., vol. 1, 157–158. These are Robison's articles for the *Encyclopedia Brittanica.*

28 Robison, *System,* vol. 3.

ence." The example used was Galileo's relationship between the distance fallen by a body and the square of the time taken for the descent. There was a decided gap between Playfair's claims for the importance of mathematics and his demonstrated use of it in both experimental philosophy and astronomy.[29] Playfair discussed the development of mathematics only so far as Descartes and the invention of logarithms, ending on a Scottish note. There was no mention of fluxions or the calculus with which Playfair was very familiar. He divided experimental philosophy into divisions, mechanics, astronomy, optics, then the imponderables, heat, electricity and magnetism. His narrative was organized to illustrate the central function of induction in the "progress" of the sciences.

John Leslie's article on the eighteenth century made even stronger claims for mathematics in the development of natural philosophy but had no better illustration of this claim than Playfair's. Leslie stated that in the eighteenth century mathematics, when introduced into physics and the practical arts, had brought great results. He classified all of the sciences into two great classes, the "pure or speculative" and the "applied or practical." The latter included optics, electricity, magnetism and theories of heat and their application in the mechanical arts. "Pure physics" was now limited to magnetism and electricity, neither of which contained much "geometry." Since Leslie only stated the basic principles behind each of his sciences, how their application worked on the mechanical arts was no clearer than how mathematics was applied in the other sciences. These essays also give us an idea of the history of natural philosophy from the Edinburgh point of view in the early nineteenth century. It was unrelentingly empirical, inductive, and experimental. Mathematics entered only after experiment, to confirm, with its generality, the results of those experiments.[30]

Published from Edinburgh, the *Encyclopedia Brittanica* displayed, wherever possible, the concerns of Scottish academics for philosophical consistency. The *Encyclopedia Metropolitana* had quite different goals and a different set of authors. The editors and contributors were either educated or taught at the University of Cambridge which heavily influenced the classification and content of the entries on the sciences. The philosophical categories of Samuel Taylor Coleridge dictated the organization of the text. In their turn his categories reflected the concerns of contemporary German academics, for whom the pure sciences were those of the

29 John Playfair, *A General View of the Mathematical and Physical Sciences since the Revival of Letters in Europe* in Stewart, Mackintosh, Playfair and Leslie, *Dissertations on the History of Metaphysical and Ethical and of Mathematical and Physical Sciences* (Edinburgh: Adam and Charles Black, 1824–1835), (Edinburgh: 1824), 2 vols., as one, 433–572. Richard Yeo, "Reading Encyclopedias: Science and the Organization of Knowledge in Dictionaries of Arts and Sciences, 1730–1850," *Isis,* 82 (1991): 24–49 discusses the function of these discourses.

30 John Leslie, *A General View of the Progress of Mathematical and Physical Science, chiefly during the Eighteenth Century* in Stewart et al, *Dissertations.*

mind, separated from the "mixed sciences" such as mechanics, optics, and astronomy. In Coleridge's hierarchy there was also a third level, the applied sciences that depended on changes in bodies. These sciences included electricity, magnetism, and chemistry. These applied sciences did not rest on any general, purely intellectual source for the knowledge they generated and hence were on a decidedly lower intellectual plane. This philosophical hierarchy was reinforced by the order of publication of the volumes, starting from the top down. Articles only referred to material already published displaying the intellectual dependency in Coleridge's scheme.[31]

The content of the individual articles reflected the content and organization of current teaching at Cambridge. The mathematics included geometry, algebra, and fluxional calculus. Mechanics was included in the volumes on mixed mathematics, yet treated as a subfield of mathematics.[32] John Herschel's article on physical astronomy was also in the volume on the "mixed" sciences. Neither of the articles on mechanics and physical astronomy could be read with any understanding without intimate knowledge of fluxions or the French calculus respectively. Audiences for this level of treatment was small and the enterprise failed. The volumes also reflected some of the crucial changes occurring in sciences in the 1820s. Articles were narrowly focused and written by specialists. Herschel on astronomy detailed the new standards of measurement. He also showed how to use the mathematical results of celestial mechanics and turn them to solving problems of observational astronomy, both uses of mathematics that were recently imported from Germany. By the time the *Encyclopedia Metropolitana* began publication, the older, long accepted intellectual geography of the sciences was under considerable strain, as was their social organization. Simultaneously, in the late 1820s agitation began for an education to meet the expectations of the middle class and a generation that needed technical training for the new economy.

Intellectual Organization of Research, 1800–1820

Material directed to colleagues within a research context still displayed the eighteenth century classification of the sciences. Science still held the eighteenth-century meaning of knowledge in a general sense. Philosophy could mean moral philosophy, that is practical philosophy, or the study of nature, natural philosophy and physics. In dictionaries of technical terms physics was not defined formally as the experimental study of nature. However, the various entries on the study

31 For a detailed discussion see Yeo, " Reading Encyclopedias."

32 For example, in the section on the equilibrium of elastic lamina the equations were solved in terms of equations of condition without any indication of the physical condition the equilibrium represented. Although the author noted that the integration of the equations was only possible when the oscillations of the lamina were small. Peter Barlow, "Mechanics," *Encyclopedia Metropolitana* vol. III, 1–140.

of the powers of nature, properties of natural bodies, and their interactions, the objects of study of physics, restricted its methods to those of experiment. And, experiment was defined as the trials made to uncover these attributes of nature. Natural philosophy encompassed ideas and theories about those powers, properties and interactions and was coextensive with experimental philosophy. After a statement of Newton's laws of motion, Newtonian philosophy was discussed at some length non-technically as a gloss on the books of the *Principia*. Charles Hutton included mechanics, however, as a "mixed mathematical science." Mixed mathematics itself was the effort to,

> reason mathematically upon physical subjects, such just definitions cannot be given as in geometry: we must therefore be content with descriptions; which will be of the same use as definitions, provided we be consistent with ourselves, and always mean the same by those terms we have once explained.[33]

Geometry belonged to abstract or pure mathematics, the "science of quantity." Pure mathematics was speculative but also had the advantage of leading more surely to truth than experiment. To end a dispute in pure mathematics, all one needed to do was to show that an opponent had not stuck to his definitions, or had argued incorrectly.

From 1790 to 1820 research in experimental philosophy followed these same patterns. The encyclopedia literature only occasionally reflected the new principles introduced into natural philosophy. These new principles were not introduced by university professors and dons, the contributors to those articles. They came mainly from outsiders, constructing new ways of doing natural philosophy both socially and cognitively.

While John Dalton worked within a Newtonian tradition, his concept of the atom was only tenuously connected to Newton's commitments. In Dalton's work the ultimate physical and chemical particles of matter became one. His theory, developed within the net of questions of meteorology, poached upon ground that chemists had regarded as theirs. The sometimes fierce opposition from chemists to Dalton's ideas was only partly due to the incompleteness of Dalton's evidence and the arbitrary nature with which Dalton was forced to construct his molecular formulae.[34] Specialist boundaries were breached and the newly established claims of chemists to empiricism were called into question.[35]

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33 Hutton, "Mixed Mathematics," in *Mathematical and Philosophical Dictionary containing An Explanation of Terms and an Account of the several Subjects under the Heads Mathematics, Astronomy, and Philosophy, both Natural and Experimental,* C. Hutton, ed. (New York: Georg Ohms Verlag, reprint of 1796 edition, 1973), 2 vols.

34 For a recent study of this opposition see, L. A. Whitt, "Atoms or Affinities? The Ambivalent Reception of Daltonian Theory," *Studies Hist. Phil. Sci.* 21 (1990): 57–89.

35 For Dalton's theory, see Greenaway, *John Dalton and Atomism,* and for a critical appraisal,

Dalton's opponents used hypotheses as freely as he, yet chemists were not able to acknowledge a place for "speculation" for many decades. Experimental philosophers already had recognized the necessity for hypotheses.[36] Natural philosophy was both a method of exploring nature and, properly separated and reported, a series of speculative ideas about nature. The same consciousness of both method and hypotheses are in the lectures of Robison and other university lecturers on mechanical philosophy. Disputes between experimental philosophers were not over the use of hypotheses as such but whether particular hypotheses met current criteria for legitimacy.

In the early decades of the nineteenth century, Dalton was forced to defend his ideas on many fronts. So was Thomas Young. Claiming also to be a Newtonian, Young fractured that heritage beyond his contemporaries' recognition of it.[37] Young's lectures and research on light conformed to inherited patterns of natural philosophy, but with a new emphasis on certain aspects of experimental philosophy emerging from France. His work in optics emerged from his medical interest in vision, and that followed his earlier work in acoustics.[38] The latter encompassed both the phenomena of sound and their explanation, the functioning of the ear as the organ of hearing, and the sense of hearing itself. Optics, therefore, included the phenomena of light and their explanation, as well as consideration of the eye and vision.

Young's contemporaries made no clear distinction between the phenomena of sound and hearing, light and vision. The means of detection and observation of light and sound were direct and depended upon the acuity of the observer's hearing and sight. Even in an age in which musical ability was an important social skill, there were disputes about the phenomena of sound, that is, what could be heard. The all-encompassing aspects of these studies, which mirrored the breadth of natural philosophy itself, led to many misunderstandings of Young's intentions and the meanings that underlay his analogy between sound and light as wave phenomena.

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see Thackray, *John Dalton.* For the arguments surrounding his theory and chemists' claims of empiricism, see Allen J. Rocke, *Chemical Atomism in the Nineteenth Century from Dalton to Cannizzaro* (Columbus OH.: Ohio State University Press, 1984).

36 For the reality behind the rhetoric of the chemists, see Rocke, "Methodology and its Rhetoric in Nineteenth-Century Chemistry: Induction versus Hypothesis," in *Beyond History of Science,* Garber ed., 137–155. Geologists were also less able to see any legitimate function for hypotheses during this era. See Rachel Laudan, "Ideas and Organization in British Geology."

37 As much research has recently demonstrated, Newton's work was sufficiently complex as to admit of a broad range of theoretical opinions being attached to his name. For example, see Thackray, *Atoms and Powers,* and *Conceptions of Ether,* Cantor and Hodge, eds.

38 Young's work in vision begins with Thomas Young, "Outlines of Experiments and Enquiries respecting Sound and Light," *Phil. Trans. R. Soc. London,* (1800): 106–150, and "Mechanism of the Eye," same journal (1801): 23–88. He retained this interest in his lectures at the Royal Institution. See Young, *Lectures on Natural Philosophy.*

One phenomenon over which there was much dispute was that of beats in sound. Some experimentalists claimed that they existed, others that they did not.[39] Young's wave theory of light was dismissed on grounds that no longer apply in the study of sound and light. Young differentiated sound and light from hearing and vision, but did not explicitly express this to his contemporaries. Many of them, besides Henry Brougham, had difficulty understanding Young's experiments. These men included John Robison and Robert Woodhouse who needed repeated exposure to Young's work to finally extract from it Young's principle of interference.[40]

At the same time Young had to invent a language in which to express physical ideas that he was uncovering piecemeal. The idea of wavelength is absent, although Young used the ambiguous term "breadth" of an undulation without further explanation. Similarly, amplitude was absent, although Young wrote of the height and depth of a wave.[41] Young inherited the term frequency from acoustics but did not relate this to any of the other characteristics of his waves.[42] He also did not use the kind of geometrical diagrams developed by Fresnel that eased the task of his audience with visual representation. Young's work was within experimental philosophy and his explanations were verbal.

And finally, Young's hypothesis that light was a wave motion was at odds with contemporary ideas about light. He also embedded that hypothesis within his narrative account of his experiments, not as a climatic statement at the end of the series. He drew his new concept of light out of analogies between well-known phenomena in acoustics with known results in experiments on light. Explaining Newton's rings, Young noted that rings of the same color occurred at distances from the center of the pattern where the distance between the two glass plates were in an arithmetic progression, that is, at d, $2d$, $3d$, and so on. This was the same relationship that occurred with the production of the same note in "organ pipes which are different multiples of the same length." If light was a continuous impulse of ether, "it may be conceived to act on the plates as a blast of air does on organ pipes, and to produce vibrations regulated in frequency by the length of the lines that are terminated by the two refracting surfaces."[43] Young also contrasted

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39 Nahum Kipnis, *Principle of Interference,* chaps. II, and III, notes this in his account of the criticisms of Young's early work on acoustics and vision.

40 See Kipnis, *Principle of Interference*, for Robison, 56 and Woodhouse, 147–148.

41 The notion of wavelength did not exist mathematically, although the idea of amplitude was defined mathematically.

42 Young was further hampered by an inability to communicate his own work and ideas, especially to a general audience. See George Peacock, *Life of Thomas Young* (London: John Murray, 1855), 135 and Alexander Wood, *Thomas Young, Natural Philosopher, 1773–1829* (London: Cambridge University Press, 1954), 137.

43 Young, "Outline of Experiments." The phenomena are, however, different. The colors from thin plates are the result of refraction, then interference. The organ pipe phenomena are from standing waves.

Huygens' and Newton's theories of light and the difficulties with the latter in explaining refraction.

The acceptance of Young's suggestion that light was, like sound, an undulation, in the ether rather than in the air depended upon his audience accepting his interpretation of Newton, a series of his own experiments, and the reinterpretation of other still controversial phenomena. He used the principle of superposition to explain beats as well as interference, yet the annihilation of sounds from different sources contradicted experience.[44] Young was exposed on various grounds. In his replies Young leaned towards the undulatory theory because of the phenomena of colors, and it was here that he focussed his own research, presenting his experiments, and the new phenomenon of interference, in his two Bakerian Lectures.[45]

To emphasize his Newtonian roots in his first Bakerian lecture, Young presented his theory in the form of Propositions and Scholia. The only demonstrations offered were analogies to the behavior of fluids and sound. In his second Bakerian lecture, and in his lectures at the Royal Institution, he intermixed experiments and theories. Young therefore lost the dramatic climax of usual accounts of empirical research where piling up empirical evidence appeared to force the researcher by induction into a particular theoretical position. As his ideas and experiments developed and he reacted to criticism, Young also changed details of his explanation of the phenomena.[46]

Most of his readers and audience did not dispute the quality of his experiments. Many of those experiments included measurements, thicknesses of glass plates, the distance of the diffracting object from the eye and the screen. He also included other factors. In his algebraic relationships were sines, cosines and tangents of measured angles.[47] In other cases his data were presented raw to the reader with no further explanations. Neither Young's use of measurement, nor of geometry and algebra to deduce a general relationship from his data, were points of comment in the barrage

44 Technically the most serious criticisms were from John Robison, "Temperament of the Scale of Music," *Supplement to Encyclopedia Brittanica* 3rd., ed. This was a reply to Young's earlier work on sound. The more damaging critique from Brougham was yet to come.

45 Young, "The Bakerian Lecture [1801]. On the Theory of Light and Colors," *Phil. Trans. R. Soc. London* (1802): 12–48, and, "The Bakerian Lecture [1803]. Experiments and Calculations relative to Physical Optics," same journal, (1804): 1–16. Between these two lectures Young had presented a brief description of his two-slit experiment in his lecture series at the Royal Institution.

46 His papers give a more than usually intimate account of the evolution of his ideas, complete with imprecisions, muddles, and backtracking in the research process. This interpretation is in contrast to that of Kipnis, *Interference of Light* who sees Young's work as a linear progression moving towards a modern, generalized theory of wave motion.

47 See Young, *Lectures,* figure 442 for his two-slit experiment; "Bakerian Lecture [1803]," 171 and "[1801]," 160.

of criticism that ensued. Yet his work hardly constituted a mathematical theory of light, or even of interference. In his lectures and papers on natural philosophy what mathematics he did develop was presented separately from his physical work, experiment and hypotheses.[48] Despite Young's radical stance in denying Newton's theory of light and in his presentation of his own ideas, he did not challenge contemporary demarcations between physics as experiment and hypothesis, and mathematics, generated from the results of physical experiments.[49]

Young's presentation of his ideas on light were complicated by his multiplication of hypotheses about the ether and its relationship with matter, and effect of this interaction on light. In 1801 he explained the production of fringes inside and outside the shadow of a small object as depending upon the refraction of light in the ether atmospheres surrounding the particles of matter. By 1803 he explained interference by the difference in the length of path traveled by two portions of light reflected from two parts of the body. Young admitted that ether atmospheres were unnecessary.[50]

Between his two Bakerian lectures Young delivered a long series of lectures on natural philosophy at the Royal Institution, the last of which were on light. Like Davy, Young drew his audience into his research, to enhance the credibility of his ideas, and answer his critics which, by this time included Henry Brougham. Brougham had performed experiments on light himself and had his own strongly held views on its nature. In his opinion, Young's experiments were not new and the phenomena well known, irrelevant, or irreproducible, and his interpretations of all his experiments were incorrect. Nothing Young had done merited the name philosophical. Young's work challenged the methodology of Brougham's one domain of direct research experience. Since method defined the field of experimental philosophy, Young's challenge was fundamental. Brougham was defending a philosophical tradition, and the carefully constructed rhetoric of experimental philosophy where empirical evidence led to, but was not intermingled with, hypotheses.[51]

Young's answer to Brougham came in his lectures at the Royal Institution. He

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48 Kipnis argues that Young "must" have had a mathematical theory of the interference of light although there is no evidence for it. He even argues that Young also understood the modern concept of wavelength and its relationship to the frequency of a wave motion.

49 See Young, *Lectures,* vol. 2. In this volume is a catalogue of some 2,000 published items, many of which are followed by Young's remarks on their contents along with those of other authors. Many items are from continental mathematicians.

50 See G. Cantor, "The Changing Role of Young's Ether," *Brit. J. Hist. Sci.* 5 (1970): 44–62, for a discussion of Young's ether.

51 Henry Brougham, "Bakerian Lecture on Light and Colors," *Edinburgh Rev.* 1 (1803): 450–456. Brougham was writing for the same kind of audience that attended Young's lectures. See also G. Cantor, "Henry Brougham and the Scottish Methodological Tradition," *Stud. Hist. Philos. Sci.* 2 (1971): 69–89. I would put more weight on the significance of this challenge because of the central place of method in defining experimental philosophy.

presented an overview of optics, from physical optics to vision and the anatomy of the eye. Young repeated much of his previous work, including his criticism of Newton's theory of light, but the tone had changed. The emphasis was on denying the validity of the particle theory of light, rather than compiling evidence for the undulatory alternative. Beginning with the assumption of light as an undulation in an elastic Medium, Young stated that light must display the phenomena of superposition, as do sound and water waves. Here Young needed to define superposition, then describe clearly its visual effects. He described the phenomenon by asserting that waves travelling along different paths could destroy or enhance each other at certain points. This led to the production of dark fringes when the difference in path length was some multiple of an odd number of half "undulations." The bright fringes were formed from path differences of whole undulations.[52] From his experiments, Young estimated the wavelengths of the various colors. He discussed the effect of varying slit-width and of removing the barrier between the slits. After a survey of the production of colors from thin plates and soap bubbles, together with interpretations in terms of diffraction, interference, refraction and reflection, colored bodies, and their lack of fringes. The meteorological production of color rounded out his survey. Clearly Young was the master of the subject. Brougham was reduced to ad hominum attacks.

Young's lectures on optics, like Davy's on chemistry, were important in the presentation of his research to the public within whose culture his results would be judged. However, rather than focussing simply on his research Young plunged into a protracted survey of the whole of optics, describing many phenomena, instruments, and experiments. This avalanche of empiricism buried the significance of his own research. Young violated the general principles of instruction that Davy clearly exploited, especially those particular to the cultural context of his audience. Cruickshank's satirical cartoons of Davy's performance at the Royal Institution catch one important feature missing in Young's lectures, the need to entertain as well as inform.[53] Young had that opportunity. While experiments on light were not as dramatic as those of Davy's chemistry, they had a beauty and even a romantic appeal he seemed unable to exploit.

On several grounds Young needed the support of this group. His ideas evolved rapidly and his idiosyncratic presentation of hypotheses intermingled with experiments worked against acceptance of those hypotheses. Within the group of specialists wedded to particular practices that Young transgressed, his experiments were admired, his ideas ignored. Young, therefore failed, in this unique cultural enterprise. Davy prevailed and established his research as knowledge in front of the audience at the Royal Institution before his research colleagues accepted his exper-

52 Young, *Lectures,* vol. 1, 464, for the definition of superposition, 464–464, for his description of fringes.

53 See Golinski, *Science as Public Culture,* for science as theater.

iments and results as valid. Despite some early disasters, Davy built a successful, very public career in science. He drew into both chemistry and experimental philosophy ideas alien to the prevalent Newtonian tradition. For natural philosophy, these innovations lay in his focus upon the agents of change, or power, and his concept of the unity of nature.[54] Davy's dramatic experiments in electrochemistry gave currency to his emphasis on power and on his chemical theory of galvanic electricity. While Davy's speciality was chemistry, his research had an impact on ideas on the nature of matter and helped to reorient experimental philosophers to the agents of physical change.

Such lectures and the production of research in this era pinpoints that education and practice in experimental philosophy was very dependent upon the individual. While an education in experimental philosophy could on the individual level be systematic, there were no institutional forms for such training.[55] The product of such self-education was a diversity of interpretation and variety of approaches to the study of nature that has to be understood individual by individual. The labels with which practicing natural philosophers covered such discrepant ideas imply that the labels point to subtexts rather than the scientific questions at hand. Newton's name still added respectability to ideas and his work was still a rich source for hypotheses, most of which Newton would not have recognized as his own.[56] Possibilities in terms of hypotheses multiplied partly because there were no tight disciplinary matrices into which individual experimental philosophers had to fit. Only a series of loose confederations existed, made up of others of similar philosophical persuasion.

About the only consensus available was the common understanding that natural philosophy was first and foremost experiment. Opposition sprang up whenever the primacy of experiment was undermined as in the case of Young, Davy, and Dalton. This trio of outsiders to the gentlemanly vocation of natural philosophy made natural philosophy a means of earning a living and forced new ways of using experiment onto their colleagues. They gave a new emphasis to hypotheses as a necessary aspect of natural philosophy. We can hardly see these three men as constituting any coherent group, other than in their social climbing and abilities to use opportunities to secure careers in science. While we can argue that Dalton's work lay in the broad tradition of Newtonianism, he did not share the all encompassing mechanism of many other natural philosophers. Young's rejection of Newton's

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54 See Golinski, *Science as Public Culture,* chap. 7, and "Humphry Davy and 'the Lever of Experiment'," in *Experimental Inquiries,* Legrand ed., 99–136.

55 As an illustration the example of Michael Faraday points to the possibilities of such an education. That his training was largely through the Royal Institution lectures and reading points to social disorganization.

56 The subtexts could be ideological or social–simply using a name to enhance a fledgling career.

work was on entirely different grounds than those of Davy, but as radical within experimental philosophy as Davy's rejection was in chemistry. One could hardly connect Young to Davy's romantic vision of both the cognitive and the cultural aims of the study of nature.[57] The intellectual spread among these three men might be broader than among their academic brethren but the differences, in their visions of the cultural place and uses of the study of nature were a mirror of the breadth of possibilities for that study in the early nineteenth century.

However, the wide ranging possibilities of such plurality enhanced by these early entrepreneurs began to narrow during the 1820s, as the standards of French experimental physics and of mathematics began to change the practices of British natural philosophers. Economic changes were also forcing the study of nature into new, narrower paths as attempts to regulate education in, and the practices of, experimental philosophy and mathematics came to a head in the 1830s.[58]

Mathematics in Britain, 1790–1820

Having sketched the social, institutional and intellectual boundaries of natural philosophy in this era, we now have to consider the social and intellectual domains of mathematics and mathematicians. Until the recent work of a handful of historians, mathematicians in Britain in the eighteenth and nineteenth centuries were depicted as isolated from their continental peers and trapped in the cramped intellectual quarters of Newtonian fluxional calculus. Mathematicians were neither isolated nor cramped.[59] However, during the eighteenth and early nineteenth centuries, mathematics served very different cultural purposes in Britain than those on the continent. As the foundation of a "liberal education" at Cambridge, mathematics served a social and cultural function unknown in continental Europe. The type and level of mathematics taught was directed towards developing standards of logical consistency rather than technical competence. Not coincidentally, part of the Cambridge mathematics curriculum consisted of liberal doses of Newton's *Principia*. Mechanics was mathematics in Britain as well as on the continent. Mathematics was also a part of the general culture of the middle and upper classes

57 Recent biographers see Davy's commitment to certain key ideals of romanticism one of the few aspects of his life that gave it any coherence. David Knight, *Humphry Davy: Science and Power* (Oxford: Blackwell, 1992). See also Trevor H. Levere, "Humphry Davy, "The Sons of Genius" and the Idea of Glory," in *Science and the "Sons of Genius": Studies on Humphry Davy*, Sophie Forgan, ed. (London: Science Reviews, 1980), 33–58, and Christopher Lawrence, "The Power and the Glory: Humphry Davy and Romanticism," in *Romanticism and the Sciences,* Cunningham and Jardine, eds., 213–227.

58 The nature of this study does not allow me to explore the ways in which the working class constructed their own versions of experimental philosophy for their own purposes.

59 For example, see the correspondence between Colin MacLaurin and Clairaut in Greenberg, *The Shape of the Earth*, 412–425.

with mathematical problems appearing in general magazines. These problems were not simplistic and were devised by professional mathematicians. A particularly successful solution of such a problem could lead to a career in mathematics.[60]

The only institution in which mathematicians could replicate themselves was Cambridge University. However, there were many other institutions that afforded careers for mathematicians, other universities and military schools being some of the alternatives. A university post, other than at Cambridge, meant teaching more than mathematics, as we can see in the lives of both John Leslie and his successor at Edinburgh, John Playfair. Given Playfair's interest and competence in French mathematics, and his reputation as a teacher of mathematics, the lack of any mathematics in his lectures on natural philosophy reinforces the idea of their separation throughout this era. For those men teaching mathematics at either universities or military colleges such as Sandhurst, Woolwich, or Portsmouth, research and teaching remained separate. Mathematics, even in military colleges, was grounded in the useful, gunnery and navigation, and usually consisted of courses in geometry and algebra.

As Guicciardini has shown, fluxional calculus was not in its dotage, although it suffered under severe structural limitations. However, by 1800 mathematicians in Britain were beginning to turn to the alternative continental calculus. By 1810 these men were using French sources and exploring beyond them into mathematical territory that they opened up for themselves. They had also begun to introduce the French calculus to a broader audience by publishing in the *Philosophical Transactions*. And in an attempt to introduce students to these methods, they began to publish textbooks and translate French texts into English. For a public beyond this, there were the articles on the calculus in Hutton's dictionary.[61]

Before the stunning annexations of heat, light, galvanism, and electromagnetism by French mathematicians, British mathematicians had taken over the mathematics that referred to familiar problems through their use of fluxions, namely mechanics and celestial mechanics. Playfair's lament on the state of mathematics in Britain in his review of Laplace's *Système du Monde* and *Mécanique Célèste* was overdrawn.[62] Playfair claimed that continental calculus was a closed book to British

60 Niccolo Guicciardini, *The Development of Newtonian Calculus in Britain, 1700–1800* (Cambridge: Cambridge University Press, 1989) details the development of fluxional calculus. He discusses the level of competence of mathematicians in Britain in the later decades of the century in chap. 7, and the place of mathematics in general culture in chap. 8.

61 Hutton, *Mathematical and Philosophical Dictionary.* For comments on Hutton's dictionary see Grattan-Guinness, "French *calcul* and English Fluxions around 1800: some comparisons and Contrasts," *Jahrbuch Überblicke Mathematik* (1986): 167–178, 171–172. See also Grattan-Guinness, "Before Bowditch: Henry Harte's Translation of Laplace's *Mécanique Célèste*," *NTM*, 24 (1987): 53–55 on early translations.

62 Playfair, "Review of Laplace's, *Mécanique Célèste*," *Edinburgh Rev.* 11 (1807): 249–

mathematicians. They neither knew the principles nor the methods that continental mathematicians could take for granted in their readers. Playfair blamed the reward system in science, the value placed on utility, and the hostility of the Royal Society of London towards mathematics. The reference to the Royal Society was code for Sir Joseph Banks its president. By 1800 his hostility to mathematics and mathematicians was legendary. A near rebellion in the ranks of the Society's fellows in the 1780s, many of whom were mathematicians, began with the removal of Charles Hutton as foreign secretary of the society. The discontent was against what was seen as Banks' arbitrary rule and hostility to mathematics and the mathematicians within the society. Order and Banks were restored.[63]

James Ivory and William Wallace, both employed as teachers at military colleges, made significant steps towards understanding continental calculus. In the 1790s Wallace abandoned fluxions for the French calculus and was anticipated by Legendre in some methods in perturbation theory.[64] In 1819 Wallace succeeded Leslie as professor of mathematics at the University of Edinburgh.[65] Together with Charles Hutton and Peter Barlow, Wallace introduced the French calculus to British mathematicians. In the first decade of the nineteenth century, other mathematicians, such as Ivory, abandoned fluxions altogether. By 1814 Toplis had translated Laplace and Ivory was following some points in the *Mécanique Célèste* on the attraction of ellipsoids using Euler's notation for partial differentials and developing aspects of Legendre's mathematics.[66]

Long before the agitation of the Cambridge "Analysts" there was a network of mathematicians, many outside of Cambridge, working to bring attention to the new continental calculus.[67] Analysis in the sense used by the Cambridge group was broad and meant the use of algebraic rather than geometric methods to solve mathematical problems. It also included the use of algebraic definitions of functions,

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284, and "Review of Laplace's *System of the World,*" same journal, 15 (1808): 396–417. Playfair was not alone in this lament. See, John Toplis, "On the Decline of Mathematical Studies, and the Sciences dependent upon Them," *Phil. Mag.* 20 (1805): 25–31.

63 For an account of this disturbance, see Russell McCormmach, "Henry Cavendish and the proper Method of Rectifying Abuses," in *Beyond History of Science,* Garber ed. 35–50, 37–38. See also David P. Miller, "Into the Valley of Darkness."

64 Wallace translated Legendre as well as Lagrange into English in the *Mathematical Repository.*

65 For Wallace's role in the mathematical importations from the continent see M. Pantiki, "William Wallace and the Introduction of Continental Calculus to Britain: A Letter to George Peacock," *Hist. Math.* 14 (1987): 119–132.

66 See James Ivory, "On Attraction of Homogeneous Ellipsoids," *Phil. Trans. R. Soc. London,* 99 (1809): 345–372, and same journal 102 (1812): 1–45.

67 For a reassessment of the role of the Cambridge group see Phillip Enros, "The Analytical Society, 1812–1813: Precursor to the Renewal of Cambridge Mathematics," *Hist. Math.* 16 (1983): 24–47.

and the acceptance of the new French calculus and their notation. The brash young members of the Analytical Society emphasized the use of notation, but that was not the heart of the matter. In ways parallel to Lavoisier's new chemical notation, French mathematical notation carried with it different notions of differentiation, integration, and the nature of functions, as well as their representation.

Much of the rhetoric of the Cambridge group was exaggerated and self-serving. While Robert Woodhouse might have been the first to introduce French mathematical notation in his textbook, he was not as isolated as the Analysts pictured him. Although he used differential notation in his textbook, Woodhouse's attempt made little impression on his colleagues and students at Cambridge.[68] Woodhouse's failure was probably because French methods did not "pay" in terms of the Cambridge examination system that was already driving the curriculum.[69] Similarly the undergraduates of the Analytical Society were ineffective until 1817 when they began to infiltrate the Senate House Examination system.[70] By 1800 the lowest denomination of a "pass" degree was well established and might meet the criteria for a liberal training of the mind. What constituted the upper limit, the best, was open ended. This became even more crucial as honors in mathematics, doing well in the Senate House examinations, could lead to a position at one of the Cambridge Colleges. By 1800 the examination was so competitive that the curriculum was designed to meet the expectations of the examiners.[71] Teaching might be in the hands of the Colleges, but the dons had to cover the topics that would arise in the examinations. These subjects included Euclid, algebra, conic sections, trigonometry, fluxions, fluents, Book I of Newton's *Principia*, the mathematics of astronomy, mechanics, and hydrostatics. The transformation of mathematics in Britain accelerated with the capture of the examination system at Cambridge.

Examination questions and textbooks reveal the meaning of mathematics at Cam-

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68 Robert Woodhouse, *The Principles of Analytical Calculation* (Cambridge: University Press, 1803). See J. M. Dubbey, "The Introduction of the Differential Notation into Great Britain," *Ann. Sci.* 19 (1963): 37–48. Much of Dubbey's argument on the role of the Cambridge Analysts has been superseded.

69 See Enros, "Cambridge University and the Adoption of Analytics in the Early Nineteenth-Century England," in *Social History of Mathematics,* Mehrtens, Bos and Schneider, eds. 135–148.

70 Grattan-Guinness, "Mathematical Research and Instruction in Ireland: 1782–1840," in *Science in Ireland, 1800-1930: Tradition and Reform,* Nudds, et al eds., strongly suggests that French mathematical methods were integrated into Trinity College Dublin more quickly than into the Cambridge curriculum.

71 For the development of the Senate House examinations into the Tripos, see W. W. R. Ball, *A History of the Study of Mathematics at Cambridge* (Cambridge: Cambridge University Press, 1889), and *The Origin and History of the Mathematical Tripos* (Cambridge: E. Johnson, 1880). The change in the function of the examination is mapped by John Gascoigne, "Mathematics and Meritocracy: The Emergence of the Cambridge Mathematical Tripos," *Soc. Stud. Sci.* 14 (1984): 547–584.

bridge. Whether written in the fluxional or continental tradition of mathematics, most of the textbooks were in "mixed mathematics." At Cambridge "mixed mathematics" were mixed in that the principles in which the mathematical problems began were observational or experimental. Physical problems were used to open up mathematical discussion and solution of problems, not to teach physics.[72] Other courses and textbooks examined the physical principles of Newtonian mechanics on which the mathematical superstructure was later erected. No mathematics beyond simple algebra or geometry graced the pages of those texts on natural philosophy. Mathematical problems and solutions were posed and solved in a separate sequence of texts and courses. In Wood's and Vince's text the mathematical matter was in volume 1, on algebra, and volume 4, on trigonometry. The volumes on astronomy and mechanics were descriptive and non-mathematical. Newton's laws of motion were presented as empirical, observational, and approached using Atwood's experiments.[73] Texts on astronomy also presented the material in two different forms, the observational with any calculations derived from the tradition of observational astronomy. Mathematical astronomy was the subject of other textbooks.[74]

The physical principles in the textbooks on natural philosophy were those of Newton's mechanics, with a short discussion of the physical principles of optics. The educational foundation of the Cambridge curriculum was indeed narrow but followed the agreed upon disciplinary geography and separated natural philosophy from mathematics. While the form of the mathematics might differ profoundly from that on the continent, the source for mathematics was held in common, the problems of mechanics whose solutions lay in mathematics.

As if to reinforce this disciplinary division, the research publications of Cambridge faculty also mirror these divisions. Woodhouse published papers in mathematics. After his appointment as Plumian Professor of Astronomy and Experimental Philosophy he published descriptive accounts of the instruments at Cambridge

72 An easily available example of this is in Ball, *Origin,* in the examination questions for 1801, 30–33, and in *Cambridge Problems: Being a Collection of the Printed Questions proposed to the Candidates for the Degree of Bachelor of Arts at the General Examination, 1801–1810* (Cambridge: J. Deighton, 1810).

73 James Wood and Samuel Vince, *The Principles of Mathematics and Natural Philosophy* (Cambridge: J. Burges, 1795–1799) 4 vols.

74 Woodhouse *An Elementary Treatise on Astronomy* (Cambridge: J. Smith, 1812) was largely descriptive. The equation of time and method of computing eclipses derived from the tradition of observational astronomers. Woodhouse explained that unlike geometry astronomy did not spring from simple principles followed by logically deductible results. Everything was connected and accuracy achieved through successive approximations. Woodhouse, *Treatise on Astronomy, Theoretical and Practical* (Cambridge: J. Smith, 1821) separated the physical from the mathematical treatment of the subject. He included the mathematical techniques of Laplace but no discussion of the physical implications of all these manipulations.

university observatory.[75] Though of a lesser caliber Vince's publications also fall into eighteenth-century categories. These include descriptions of unusual meteorological phenomena, criticism of a recent paper on gravitation, with a separate set of mathematical papers.

In the 1820s this gentlemanly, comfortable eighteenth-century existence was already under close scrutiny. Change was being demanded, and not just from within the university from a small coterie of brash undergraduates. The economic and social changes that were beginning to reorient both the teaching and the research practices in the sciences in the 1820s would stir changes within Cambridge, slowly and against powerful inertia, but changes nevertheless.

Natural Philosophy and Mathematics in the 1820s

During the 1820s the general public began to lose its role in scientific institutions and as actors in the production and dissemination of research that had been so important for the first professional scientists in the early years of the nineteenth century. This, paradoxically, was because of increasing opportunities to engage in research, and to obtain a more systematic education in natural philosophy.

At the same time that the number of opportunities to engage in research opened up, positions in the older university were filled with younger men committed to research as well as teaching. Nationally, the number of men practicing within certain subfields of experimental philosophy expanded. Increasingly, the numbers engaged in closely allied research problems reached a critical mass that could support a society dedicated to the narrowly defined needs of this research community. The membership, provincial in its intellectual interests, became geographically national, inverting the intent and the geographical reach of the older, culturally broad, geographically provincial societies.[76] These specialist societies launched their own journals, and restricted membership in the societies to individuals actively engaged in the research the membership regarded as legitimate. The journals, meetings, papers, discussions, and the management of the society focussed upon the needs of this group. The tensions inherent within the earlier, philosophical societies that sought cultural support by appealing to local educated groups were

75 For example, Woodhouse, *A Treatise on Isoperimetrical Problems and the Calculus of Variations* (New York: Chelsea Pub. Co. reprint, 1964), "On the Independence of the Analytical and Geometrical Methods of Investigation," *Phil. Trans. R. Soc. London,* (1802): 85–125, and "Methods of Investigation of the Integration of Certain Expressions with which Problems in Physical Astronomy are connected," same journal, (1802): 85–125, and, same journal (1804); 219–278. After his appointment, Woodhouse, "Account of the Transit Instrument made by Mr. Dolland and lately put up at Cambridge University," *Phil. Trans. R. Soc. London,* (1825): 418–428, (1826): 15–36, and "On the Derangement of Certain Instruments by the Effects of Temperature," same journal, (1827): 144–158.

76 For example, the Geological Society was already organized in this fashion in 1807, the Royal Astronomical Society in 1820.

no longer present. They were replaced by intellectual and social tensions of a different kind.[77]

Since most of the men engaged in research were gentlemen, or at least had access to private funds, such societies effectively marginalized, or excluded the participation of the lower classes in the development and legitimation of research. Many of the specialist societies met in London and only in the social season, November to June.[78] In this they followed the Royal Society of London, that bastion of gentlemanly science that the founders of the specialist societies broke away from. The founding members of these breakaway groups complained of the tyranny of Sir Joseph Banks. The Royal Society itself was a mockery of a scientific society that claimed national representation when it served the needs of a small group of London social and political climbers and ignored the real needs of research.[79] However, when the opportunity arose, members of these breakaway groups all accepted election to the Royal Society. For the individual membership in the Royal Society represented the kind of social acceptance and entry into political spheres that membership in no other scientific or cultural group could vouchsafe. This was true, whether the individual in question deplored the society's connection to "old corruption" or not.[80] The Royal Society thus enjoyed the fruits of the eighteenth-century patronage system, such as still existed by the 1820s. Those members who had earlier opted for the Astronomical or the Geological society later enjoyed and exploited these connections for their own research purposes.[81]

Specialists tried to co-opt supervision of some of the Royal Society's functions as their own. Astronomical specialists deplored the quality of the Royal Society's work overseeing the Board of Longitude and the content of the *Nautical Almanac*. The Astronomical Society membership was particularly vociferous as they counted

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77 See Rudwick, *The Great Devonian Controversy,* for an account of these new tensions, complicated by the search for the correct social tone that would not undermine the gentlemanly pretensions of its practitioners.

78 The social divisions that rent the Geological Society in a later decade are detailed in Rudwick, *The Great Devonian Controversy.* The care with which the British Association orchestrated its social and cultural place in British society throughout the 1830s is detailed in Morrell and Thackray, *Gentlemen of Science.*

79 For the struggles of the geologists see, Rudwick, "The Foundation of the Geological Society of London: Its Scheme for Cooperative Research and its Struggle for Independence," *Brit. J. Hist. Sci.* 1 (1962): 325–355.

80 For example, see the humiliations that Michael Faraday was prepared to endure to become elected to the Royal Society in 1824, June Z. Fullmer and Melvyn C. Usselman, "Faraday's Election to the Royal Society: A Reputation in Jeopardy," *Bull. Hist. Chem.* 11 (1991): 17–28.

81 For example see John Cawood, "The Magnetic Crusade: Science and Politics in Early Victorian Britain," *Isis,* 70 (1979): 493–518.

these activities as part of their specialist purview.[82] The Royal Society yielded to reform slowly. In the 1820s Humphry Davy was able to institute improvements in running the actual meetings of the Royal Society, the quality of the papers delivered and published by the society, and began to run the library as a more scholarly institution. He even opened up the social intermingling of fellows, that is those elected for their social and political connections and the scientific fellows. However, he managed to alienate politically powerful groups within the Society, including those that had helped to elect him. He resigned the presidency in 1827. His reforms did not address the systemic problems of the society, its relationships with the government and its function as a social ladder for London physicians that was so offensive to the growing group of scientific professionals. In 1828 any structural reform was precluded with the election of the Duke of Sussex as president. Jostling for power within the society and hence for control over its future continued until the 1840s.[83]

By the time that the Royal Society was controlled by professional scientists its function as the major site for the display of science and its intellectual development had been usurped by the British Association for the Advancement of Science. However, membership in the Royal Society never lost its place as the crowning social achievement in a career. The overlapping membership of the British Association and the Royal Society helped the former to consolidate its place in Victorian society and helped to divert government largesse to the the Association and the Society.[84]

Specialist societies, dedicated to the needs of the research community, also locked out the general public from participation in the process of the creation, display, and legitimation of research that were the structural support of the careers of Dalton, Davy, and Young. The audience that was so important for science to acquire the correct cultural tone in the early nineteenth century were increasingly only allowed into the conversaziones or the Friday evening general lectures of the Royal Institution and other scientific societies. Social and intellectual mediators became necessary between those engaged in the sciences and the general public.

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82 See William J. Ashworth, "The Calculating Eye: Baily, Herschel, Babbage and the Business of Astronomy," *Brit. J. Hist. Sci.* 27 (1994): 409–441.

83 For a narrative of these events, see Marie Boas Hall, *All Scientists Now.* For a sociological analysis, see Roy Macleod, "Whigs and Savants: Reflections on the Reform Movement in the Royal Society, 1830–1848," in *Metropolis and Province*, Inkster and Morrell, eds. 55–90. On Davy's tenure as president, see David P. Miller, "Between Hostile Camps: Sir Humphry Davy's Presidency of the Royal Society, 1820–1827," *Brit. J. Hist. Sci.* 16 (1983): 1–47.

84 For the patronage of the sciences in this era, see, Cawood, "The Magnetic Crusade," and J. B. Morrell, "The Patronage of Mid-Victorian Science in the University of Edinburgh," in *The Patronage of Science in the Nineteenth Century,* G. L' E. Turner, ed. (Leiden: Noordhoff, 1976), 53–93.

Not insignificantly many of those intermediaries between the world of science and of general culture were women. They were still essential for the enterprise, even when pushed to its margins.[85]

During the same decade criticism, educational, social, and political, began to be directed against Oxford and Cambridge. Part of this critical barrage claimed that their curricula were antiquated. It was no longer enough to train the mind the man must be trained in a particular branch of the arts or the sciences. The professional was defined. Simultaneously, this pressure to reform was felt acutely by the Scottish Universities. Their curriculum, management, and parochial focus was under the scrutiny of a central government ready to economize at any opportunity. While the reforms reached into parts of the fiscal management of these universities, neither the curriculum nor the distinct function that the universities saw that they filled within Scottish society seemed to have been affected.[86] The opening of University College, London introduced other educational alternatives that claimed to offer a more modern curriculum. In large cities philosophical societies also began to offer such courses of study in the 1820s. These courses tended to be systematically delivered on narrowly defined subjects and replaced the broadly conceived surveys in natural philosophy. The rhetorical appeals to moral and intellectual improvement no longer appealed to an audience bent on specific knowledge for economic purposes rather than for entertainment or cultural and social uplift. They were also taught by men recognized for their more narrowly focussed technical abilities and accomplishments.[87] Such courses attracted smaller numbers of students, that is men bent on using the course work in their economic, not their cultural, lives. Appeals to aesthetic ideals and cultural uplift were largely replaced by strict utility as justifications for studying nature. The latter was usually emphasized by mechanical philosophy that pointed in general to the connections of mechanics to mechanisms. Truth was replaced by narrow economic aims.[88]

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85 The preeminent examples are Jane Marcet in chemistry and Mary Somerville in astronomy. See Somerville, *Mechanism of the Heavens* (London: J. Murray, 1831), a translation and commentary on Laplace's *Mécanique Céleste*, and *Preliminary Dissertation on the Mechanism of the Heavens* (London: 1832). See Elizabeth Patterson, *Mary Somerville and the Cultivation of the Sciences, 1815–1840* (The Hague: Nijhoff, 1983). Faraday became his own mediator, establishing Friday evening lectures at the Royal Institution.

86 See J. B. Morrell, "Science and Scottish University Reform: Edinburgh in 1826," *Brit. J. Hist. Sci.* 6 (1972): 39–56.

87 This can be seen in the courses offered by Michael Faraday at the Royal Institution in the 1820s. For changes in lecturing content and styles in London during the 1820s see, J. N. Hays, "The London Lecturing Empire," in *Metropolis and Province*, Inkster and Morrell, eds. 91–119.

88 See George A. Foote, "Mechanism, Materialism, and Science in England, 1800–1850," *Ann. Sci.* 8 (1953): 152–161, and "Science and Its Function," *Osiris*, 11 (1954): 438–454.

None of these criticisms, agitations and changes in the ways in which natural philosophy was taught appeared to change the intellectual boundaries of experimental philosophy as a research discipline. With the new phenomena of light and electromagnetism, British experimentalists became more sensitive to French research. Yet experimentalists in Britain took note only of French experimental results, not mathematical physics. David Brewster was actively involved in the early research into polarization and fought a losing battle against the hypothesis of undulations. As such, he reacted to the experimental work of Fresnel and even of Thomas Young but not the mathematics of Cauchy or any other mathematician.

In Brewster and Faraday we can see the interaction of British experimental philosophers with French experimental physics. Neither succumbed to precise quantitative experiments or the seduction of French ideas about light in Brewster's case, nor electricity, magnetism and their interaction in that of Faraday. Faraday's experiments in electromagnetism were precise and developed in a sequential series that allowed him to explore the specific consequences of his own ideas on electricity and magnetism in great detail. The clarity of the expression of his ideas defies the notion that mathematics was a necessary development for the intelligibility of theories in physics. Later Faraday regretted his ignorance of mathematics, yet in the 1820s he did not feel compelled to address Ampère's mathematical electromagnetism. He confined himself to answering the physical ideas expressed in Ampère's experimental papers.[89]

In the 1820s the mathematical side of the disciplinary divide was kept intact in George Green's work on electricity and magnetism. Scientific life in Nottingham in the early nineteenth century centered upon the Bromley House subscription library. There Green had access to Laplace in Toplis' translation, Charles Hutton's *Course in Mathematics* and other resources in the new French mathematics. He also had the encouragement of Sir Edward Ffrench Bromhead, one of the original founders of the Analytical Society at Cambridge.[90] While Green's work in electricity and magnetism was classified as "mixed" mathematics, it was mathematics. Green explored the mathematical properties of the "potential function" and the

89 The interaction of experiment and hypothesis in the development of Faraday's thought is explored in David Gooding *Experiment and the Making of Meaning* (Dordrecht: Kluwer Academic, 1990). Faraday's reactions to Ampère are detailed in L. Pearce Williams, *Michael Faraday* (New York: Basic, 1965), and "Faraday and Ampère: A Critical Dialogue," in *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday, 1791–1867,* David Gooding and F. A. J. L. James, eds. (London: Macmillan, 1985), 83–104.

90 Sources on Green are few. See H. Gwynedd Green, "George Green," in *Studies and Essays in the History of Science and Learning, offered in Homage to George Sarton* (New York: Schumann, 1947), 552–593. For the richness of the local Nottingham scientific culture in the late eighteenth and early nineteenth centuries, see Ian Inkster, "Scientific Culture and Education in Nottingham, 1800–1843," *Transactions of the Thoroton Society of Nottinghamshire,* 82 (1978): 45–50.

systematic reduction of partial differential equations of the second order to those of the first. His starting point for this mathematical exploration was Poisson's mathematical essays on electrostatics and magnetism, as well as known empirical laws on the same subjects. Along with following the French mathematical example in general, Green echoed Fourier's sentiments on the uses of the physical sciences for mathematics,

> The application of analysis to the physical sciences have the double advantage of manifesting the extraordinary powers of this wonderful instrument of thought, and at the same time of serving to increase them.[91]

Green made explicit the connection with Fourier's work on heat in the next sentence, although he traced his mathematical subject matter back to Laplace and his methods were derived from Lagrange.[92]

Green's goal was to generalize Poisson's mathematical work. Poisson offered no systematic theory of equations of the type

$$a = \int \rho \frac{d\sigma}{r} - \int X dx + Y dy + Z dz.$$

In this case ρ was the density of electricity on the surface of a conducting sphere of radius r, and $d\sigma$ was an element on the surface. The components of the electrical force from an external charge at the surface were X, Y, Z. The first integral ranged over the whole surface, and the expression $X dx + Y dy + Z dz$ was an exact differential. Poisson had performed the integrations in particular cases and the solutions "must be looked upon as an effect of chance rather than of any regular and scientific character."[93] To attack this problem Green wrote $V = \int \rho d\sigma / r$ where V satisfied,

$$0 = \frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2}.$$

Green systematized the mathematics implicit in Poisson's work and rederived some of the latter's results far more directly, noting at the same time that many were also available in Laplace. He developed all the mathematical examples for which he could reach explicit solutions. He also noted in the case of the Leiden jar that the sum total of electricity on all surfaces was zero. He remarked that such a result, while surprising, "would not be difficult to verify" by experiment. Clearly he was concerned with the mathematical, not the experimental literature. Green

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91 George Green, *An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism* (Nottingham: 1828) reprinted in Green, *Mathematical Papers,* N. M. Ferrers, ed. (New York: Chelsea Pub. Co. reprint of 1871 edition, 1970), 1–117, p. 7.

92 For the impact of Fourier on Green and others, see Garber, "Reading Mathematics, Constructing Physics," in *No Truth Except in the Details,* Kox and Siegel, eds.

93 Green, *Essay,* 16.

then proceeded to reach many of Poisson's results in magnetism using the same elegant mathematical approach.[94]

In the 1820s the boundaries between mathematics and physics remained intact. While mathematical methods were transformed by the importation of French mathematics they only reinforced traditional boundary lines between experimental philosophy and mathematics. The starting point for mathematics remained the same. Yet some of the critical changes in the development of theoretical physics in Britain were effected by the graduates and faculty at the university of Cambridge to which we must, from now on, give increasing amounts of attention.

Cambridge was changing, being forced in new directions by its teachers, its students, and the world beyond the Fens. These changes were symbolized by the establishment of the Cambridge Philosophical Society in 1819, and the improved pulse of research at Cambridge and the kind of research done by its faculty. On the student's side was the Apostle's Club in the 1820s. Affecting both students and faculty was the formalization of Cambridge examination practices with the establishment of the Mathematical Tripos in 1824.[95]

Even as their educational philosophy denied this as their goal, the establishment of the Tripos guaranteed that Cambridge would graduate professional mathematicians. With the Tripos, mathematical proficiency defined intellectual excellence. Since honors in the Tripos led to college and university appointments, college tutors, fellows, and university professors became professional mathematicians. With the introduction of analysis into the university examination system, research in mathematics developed in several different directions.[96]

The selection system for faculty and teaching, now driven by the formalized system for honors in mathematics, negated the official educational philosophy of Cambridge. By 1830, despite Whewell's efforts, the Tripos no longer served as a means for a liberal education but as the technical training ground for mathematicians. If the Analytical Society was a symptom of this change, the Apostle's Club was its confirmation. The initial purpose of the Apostles Club was to give to a select few undergraduates interested in literature much that was intellectually absent

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94 Green's theorem, the transformation of a surface into a volume integral, is discussed in J. J. Cross, "Integral Theorems in Cambridge mathematical Physics," in *Wranglers and Physicists: Studies in Cambridge Physics in the Nineteenth Century,* Peter Harman, ed. (Manchester: Manchester University Press), 112–148, 130–132.

95 Another sign of change was the small, yet enthusiastic group of students and faculty attending John Henslow's lectures, field trips, and laboratory exercises in natural history. See Adrian Desmond and James Moore *Darwin: Tortured Evolutionist* (London: Michael Joseph, 1992), and Browne, *Darwin.*

96 See Grattan-Guinness, "Mathematics and Mathematical Physics from Cambridge, 1815–1840: A Survey of Achievement," in *Wranglers and Physicists,* Harman, ed. 84–111, 95–101.

in Cambridge.[97] The narrowness of the curriculum and the consequent isolation of such students were recorded in the bitterness with which the non-mathematically talented or inclined students recalled their years at Cambridge.[98]

What the Apostles Club did for some undergraduates, the Philosophical Society did for faculty seriously engaged in research. It gave them a forum for the mutual exploration of common interests not available elsewhere at Cambridge.[99] Many of the faculty became specialists in their research William Whewell's research in the 1820s on mineralogy was a case in point. The only aspect of the Philosophical Society that marked it off from a modern scientific society was the broad range of research topics discussed in its meetings and its *Transactions.*[100] While the aim of the society was specifically directed to natural philosophy and natural history, mathematics appeared to be the unifying interest of the members of the society. A strictly inductive approach was adhered to in reports on experiments or observations, and the extension of mathematics to new domains of research, mineralogy and geology in the 1829s was evident in Sedgewick's geometrical geology and Whewell's mineralogy. Sedgewick was the "mathematical geologist."[101] Whewell's papers in mineralogy fall into two groups, descriptive, and mathematical.[102]

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97 See Peter Allen, *The Cambridge Apostles: The Early Years* (Cambridge: Cambridge University Press, 1978).

98 See A Don [Leslie Stephen] *Sketches from Cambridge* (London: Macmillan, 1865), 32–47. Frances M. Brookfield, *The Cambridge Apostles* (London: Pittman and Sons, 1906), chap. 1. Arthur Gray, *Cambridge University, an Empirical History* (New York: Houghton Mifflin, 1927), p. 276, on Tennyson's view of his tutor, William Whewell, "Billy Whistle." Thackray got revenge in *The Book of Snobs* where Crump, Master of St. Boniface was based on William Whewell, as was Dr. Sargent in *Lowe the Widower*. John Clive, *Macaulay: The Shaping of the Historian* (New York: Knopf, 1973), p. 21 recounts the disappointment of both Macaulay and his family in the 1830s at his Cambridge record. Macaulay saw himself as an academic failure because he was not a mathematician. See also the agonies of Darwin trying to understand mathematics and struggles to achieve a pass degree. See Desmond and Moore *Darwin*, and Browne *Darwin*. On students see Sheldon Rothblatt, "The Student Sub-Culture and the Examination System in early Nineteenth-Century Oxbridge," in *The University in Society,* Stone, ed. (Princeton NJ: Princeton University Press, 1973) 2 vols., vol. 1, 247–303.

99 For the establishment and the initial aims of the Cambridge Philosophical Society, see A. Rupert Hall, *The Cambridge Philosophical Society: A History, 1819–1969* (Cambridge: Philosophical Society, 1969).

100 The same process occurred at Oxford in the same period. See Sheldon Rothblatt, *The Revolution of the Dons* (New York: Basic Books, 1968) and Arthur Engel, "Emerging Concepts of the Academic Profession at Oxford, 1800–1854," in *The University in Society,* Stone, ed. vol. 1, 305–351.

101 See Crosbie Smith, "Geologists and Mathematicians: The Rise of Physical Geology," in *Wranglers and Physicists*, Harman ed., 49–83, 52.

102 William Whewell, "Report on Recent Progress in Mineralogy," *Rep. British Assoc.*

Students who did attend the reactivated lectures by university professors were treated to a different form of knowledge than earlier generations. The lectures were more narrowly defined and closely linked to the research interest of the faculty.[103]

During this decade the generation that began the transformation of the Cambridge examination system scattered geographically and intellectually. As their careers developed, their attitudes to mathematics and its function in the other sciences and in the Cambridge curriculum spread across a broad spectrum of opinion. These changes were driven by their experiences beyond Cambridge, even for those like Herschel whose earliest publications were in the mathematics so recently imported from France.[104]

John Herschel lost direction after he left Cambridge until he entered the family trade of observational astronomy. By 1820 he already had learned the craft of grinding mirrors and the techniques of astronomical observation. Simultaneously he reported to the Cambridge Philosophical Society on his own experiments on double refraction and polarization.[105] These were not his first forays into experimentation. He was already exploring chemistry as well as the optical properties of various substances. Herschel did not invite mathematics into experimental philosophy, yet he appreciated the new concern with quantification and error in experiment. He also began to establish, then develop an epistemology along with ideas concerning the place of mathematics within experimental philosophy. The disciplines of physics and mathematics were distinct and experiment the surer method of exploring nature. While hypotheses were a legitimate aspect of experimental

(1831–32): 322–365, is a convenient place to see how Whewell separates the hypothetical and physical from the geometrical and mathematical.

103 This is best seen in the geology lectures by Adam Sedgewick, as well as those of E. D. Clark, followed by the lectures of Henslow and Whewell.

104 John Herschel and Charles Babbage published on functional equations and operational methods of the calculus. Herschel's first papers appeared in *Memoirs of the Analytical Society* (1813) and continued into the 1820s in the *Philosophical Transactions of the Royal Society of London* and the *Transactions of the Cambridge Philosophical Society*. They continued to appear even as his research began to focus on chemistry and observational astronomy. See Gunther Buttman, *The Shadow of the Telescope: A Biography of John Herschel,* Bernard Pagel, trans. (New York: Scribner's Sons, 1970), and *Aspects of the Life and Thought of Sir J. F. Herschel,* S. S. Schweber ed. (New York: Arno Press, 1981), 2 vols. Babbage's first paper on functional analysis appeared in 1815. See J. M. Dubbey, *The Mathematical Work of Charles Babbage* (New York: Cambridge University Press, 1978). His assessment of Babbage's achievement must be modified in light of contemporary French work in the same field. See Grattan-Guinness, "Babbage's Mathematics in its Time," *Brit. J. Hist. Sci.* 12 (1979): 82.

105 See, Buttman *Shadow.* His work on double refraction was published as Herschel, "Double Refraction as a Deviation from Newton's Scale," *Trans. Cambridge Phil. Soc.* 1 (1822): 21–42, and that on polarization as, Herschel, "Polarization," *Trans. Cambridge Phil. Soc.* (1823): 1–52.

philosophy, his epistemology was based on his own experience in observational astronomy, chemical and optical experiments.[106] There was room in Herschel's philosophy of the experimental sciences for hypotheses, however, the place for mathematics was less clear.[107]

In some of his papers on Light, Herschel discussed the physical meaning of terms that enter his equations without discussing any physical hypothesis about the absolute nature of light. Some properties simply emerged from a series of experiments with no theoretical context to tie them together. Here he was careful to avoid physical hypotheses and extracted most of his descriptions of the properties of light from experiments, further explicated in geometric or algebraic form. For Herschel, the experiment provided the laws upon which "mixed" mathematics was based.[108] He also pointed out that, for all their labor, mathematicians might achieve results that were analytically correct but which were shown by further experiments to be irrelevant to the behavior of nature. Mathematicians might also wander off into analytical paths that experiments revealed as irrelevant because of problems in the initial observations on which the mathematics was based. His specific example was taken from double refraction, where the physical implications extracted from analysis were rendered nonsensical by Fresnel's recent experimental work. The two realms existed separately and interacted only at those places where such measurements could be made. In addition nature was primary, mathematics was a secondary art.

In physical astronomy such clearcut distinctions between observation and mathematics were less easily made. Mathematics was necessary for reducing the data and dealing with various anomalies of the motion of the planets, sun and moon. Herschel made the physical results of such mathematical manipulations clear in the example of constructing the orbit of a comet from observations. His solutions to the perturbations of planets from elliptical orbits were accomplished using characteristics that could be easily traced in the reduction of the data. He dealt with astronomy in the tradition of observers not mathematicians. His standards for his readership were high, as he expected them to follow him through the new calculus.

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106 See Herschel, *Admiralty Handbook of Scientific Enquiry* (London: Dawson reprint, 1974). His ideas also appeared in his various encyclopedia articles that are examined below.

107 Systematic discussion of Herschel's ideas on the place of hypotheses in "science" appear in Richard Yeo, "Reviewing Herschel's *Discourse*," *Studies Hist. Phil. Sci.* 20 (1989): 541–542, "Reading Encyclopaedias," and in *Defining Science: William Whewell, Natural Knowledge, and Public Debate in early Victorian Britain* (Cambridge: Cambridge University Press, 1993), 92–99.

108 See Herschel, *Treatise on Physical Astronomy, Light, and Sound* (London: Richard Griffin and Co., n. d.). These were reprints, with the original pagination, of articles from the *Encyclopedia Metropolitana* of, "Physical Astronomy," vol. 3, 647–729, 647; "Light," vol. 4, 314–586; "Sound," vol. 4, 747–824, 747.

In his own research he had used Bessel's methods and could extract just the right amount of mathematics to make visible the physical reasons for its use. Yet in his general discussion of the relationship between the experimental and the mathematical sciences nothing had changed. "Pure" mathematics was a higher form of attainment than "mixed" mathematics. The former depended only on the "intuitive perception of abstract truth" and hence led to absolutely correct conclusions. Mixed mathematics, although a lesser art, was definitely mathematics, as he demonstrated in his section on mechanics. Clairaut was the first to derive "the correct and general laws which regulate the equilibrium of a fluid mass acted on by any force, and to point out their connexion with the equations of condition which render a function an exact differential."[109] He began in physics and ended in mathematics. The mathematics of mechanics culminated in the works of Laplace and Lagrange. Lagrange's *Mécanique Analytique* "is a compendium of the general formulae and analytical artifices necessary for the treatment of every problem which can be proposed in the equation of motion of matter." Mathematics, not physics.[110]

As Herschel's research turned to observational astronomy, mathematics became a tool for experimentalists and observers. Anything more was superfluous and for the most part irrelevant, an opinion shared and expressed more vehemently and less eloquently by George Biddell Airy. Airy had not taken part in the initial debates over the new analytical methods. He was one of the first students to go through the examination system at Cambridge after these methods were in place.[111] Very quickly Airy developed his own ideas about mathematics that particularly suited his own talents and from which he developed a singular career. In his autobiography Airy claimed to dislike "mere theoretical problems." This was written after a lifetime of work making mathematics practical at Greenwich Observatory "at any cost of labor." He viewed skeptically any mathematical result that were not based in the physical entities of space, time, and matter. While expressing what seems like a traditional understanding of the relationship between mathematics and physics Airy narrowed this view. He could not see any value in mathematics not used in solving the problems posed by nature. Airy's earliest texts and papers bear out these values, made more explicit and general later. His *Mathematical Tracts* were purely utilitarian treatments of those parts of Newton's *Principia* required for the Cambridge examinations, translating physical problems into analytical form and

109 On the term "mixed mathematics" and its changing meanings, see Gary I. Brown, "The Evolution of the Term "Mixed Mathematics"," *J. Hist. Ideas* (1991): 81–102. Brown points out that during the nineteenth century the term was dropped and replaced by applied mathematics but does not discuss any reasons for this change.

110 Herschel, "Mathematics," *Edinburgh Encyclopedia,* David Brewster, ed. (Edinburgh: W. Blackwood, 1830) vol. 13, pt. 1, 359–383. On Clairaut see p. 381. See also p. 382 where Herschel noted that Clairaut was a mathematician not an astronomer. For Lagrange see p. 383.

111 He was Senior Wrangler in 1823.

developing only that much mathematics necessary to solve those problems. He also took the student through the problems as if they were observational astronomers.[112]

Airy's mathematical tract on the undulatory theory of light was not so obviously a training manual. Airy differentiated between the "geometrical" part of the theory that depended only on assuming that light was a transverse wave that traveled at different velocities in different media and the "mechanical" part of the theory that depended upon hypotheses, "far from certain" on the internal behavior of the ether. In developing mathematical methods, he used only those that were necessary to solve specific problems. The general solution to the wave equation was discarded for various particular ones that were related to actually observed phenomena. Physical interpretations of mathematical results were explicit and unnecessary hypotheses were avoided. Mathematics was focussed here on the needs of physics.

Airy left Cambridge for Greenwich in 1835 and was replaced by James Challis as Plumian Professor. Challis therefore took over the training of students as astronomical observers. He interpreted his responsibilities as having to teach astronomy as natural philosophy. His astronomy lectures dealt with the instrumentation and observational techniques of astronomy; diagrams and drawings were used as illustrations. After the construction of the university observatory, Challis drew students into the observation process and the reduction of data and developed a fine teaching and research facility, a result that surprised the French.[113] Similarly, in his demonstration lectures on natural philosophy, mathematics only entered as the expression of the laws extracted from his experiments. However, in his research Challis developed his own mathematical theories of physical phenomena on a grand scale, closer to French mathematical physics than theoretical physics.[114]

George Peacock, one of the original members of the Analytical Society, was at the other end of the mathematical spectrum. Peacock stayed at Cambridge long enough to become embroiled in the debates over the content of the Cambridge curriculum. He defended algebra that was as mathematics legitimate as

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112 George Biddell Airy, *Mathematical Tracts on Physical Astronomy, The Figure of the Earth, and the Calculus of Variations designed for the Use of Students in the University* (Cambridge: Deighton, 1826).

113 See Harvey Becher, "Voluntary Science in Nineteenth-Century Cambridge to 1850," *Brit. J. Hist. Sci.* 19 (1986): 57–87, 68–70.

114 James Challis, *Notes on the Principles of Pure and Applied Calculation: and the Application of Mathematical Principles to Theories of Physical Force* (Cambridge: Deighton, Bell and Co., 1869). Challis reduced gravity, heat and other forces of nature to mechanical pressure in a fluid ether. While analytically defensible, James Clerk Maxwell described Challis' work as self-consistent mathematically, but physically indefensible. Challis' fluids could not behave as ordinary fluids. See Maxwell,"Challis' 'Mathematical Principles of Physics'," *Nature,* 8 (1873): 279–280, reprinted in *Maxwell on Molecules and Gases,* Garber, Brush and Everitt, eds. (Cambridge MA.: MIT Press, 1986), 126–132.

geometry for training the mind. Geometry had this primary function in the Cambridge liberal education philosophy and teaching at the university. Continuing the eighteenth-century tradition geometry was a mathematics rooted in the physical world. Philosophically geometry was well grounded and based on axioms and definitions and connected through theorems to known results. It could also be taught that way as well and justified as training for the mind. Algebra could claim no such philosophical or pedagogical structure. This criticism threatened the very methods of the analysis so recently introduced into Cambridge. Peacock countered these arguments by putting algebra on the same foundations and creating for it a logical structure. He thus met head-on Whewell's growing opposition to analysis in the curriculum.[115] This exploration of the foundations of algebra did not lead Peacock to Cauchy's calculus but to the development of symbolic algebra–algebra as logic. For Peacock, arithmetic was a restricted form of algebra. Algebra was a generalized form of arithmetic but non-commutative algebra, for example, was impossible.[116]

During the 1830s and 1840s Whewell appeared to win the battle over the curriculum. However, the examinations system worked against his proclaimed goal of using mathematics as the foundation of a liberal education. The competition for place in the Tripos ensured that the examination, rather than the curriculum, mattered among ambitious students. This in turn guaranteed that Cambridge trained the narrowly educated, yet technically accomplished, mathematician Whewell de-

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115 Whewell's opinions on analysis and its place in the curriculum changed over time and in the context in which he was placed and the subject discussed. As his career at Cambridge developed his defense of geometry against the inroads of French analysis grew. For the arguments over the Cambridge curriculum and the meaning of a liberal education see, Martha McMackin Garland, *Cambridge before Darwin: The Ideal of a Liberal Education, 1800–1860* (Cambridge: Cambridge University Press, 1980). M. V. Wilkes has explored the roles of Peacock, and Herschel in shaping the Cambridge curriculum in the middle of the nineteenth century in Wilkes, "Herschel, Peacock, Babbage and the Development of the Cambridge Curriculum," *Notes Rec. Roy. Soc. London* 44 (1990): 205–219, that of Whewell in Harvey Becher, "William Whewell and Cambridge Mathematics," *Hist. Stud. Phys. Sci.* 11 (1980): 1–48.

116 Peacock, *Treatise on Algebra* (Cambridge: Deighton, 1830). For a survey of the historiography and the development of symbolic algebra, see Menachem Fisch, " 'The Emergency which has arrived': the Problematic History of Nineteenth-Century British Algebra–a Programmatic Outline," *Brit. J. Hist. Sci.* 27 (1994): 247–276. For the development of British explorations of "foundational" issues in mathematics see, Elaine Koppelman, "The Calculus of Operations and Rise of Abstract Algebra," *Arch. Hist. Exact Sci.* 8 (1971): 155–242, Joan Richards, "The Art and Science of British Algebra: A Study in the Perception of Mathematical Truth," *Hist. Math.* 7 (1980): 343–365, Helen Pycior, "George Peacock and the British Origins of Symbolical Algebra," same journal 8 (1981): 23–45, "Early Criticism of the Symbolic Approach to Algebra," same journal 9 (1982): 392–412, Joan Richards, "Rigor and Clarity: Foundations of Mathematics in France and England, 1800–1840," *Sci. Context* 4 (1991): 297–319.

plored. This was the French model of the mathematician he began to criticize in the 1820s. While the Analytical Society began at the tail end of the process of introducing the new French mathematics into Britain, the Analysts were largely successful in introducing it into the Cambridge examination system. Initially, Cambridge mathematicians took the forms of the calculus of Lagrange, Laplace, and Poisson, while ignoring the debates that had divided these mathematicians earlier in the nineteenth century. They also largely ignored or criticized the physical models that informed the mathematics of Laplace and Poisson. The Cambridge Tripos carried on the tradition of French mathematical physics, using the mathematics encapsulated within Lacroix's texts and now largely abandoned by the French. Thus it was unnoticed by the French. Their French models also gave them the physical subjects upon which the Cambridge mathematicians built. The mathematics they developed initially was generated by mechanics and celestial mechanics, which formed the basis of examination questions and research problems.

After 1830, later forms of the calculus developed by Cauchy and Fourier became a source of problems and of methods for English mathematicians none of whom explored the incompatibilities between their earlier sources and these later French mathematicians. In the late 1830s, the sources for their mathematics had spread into light, electricity, and magnetism.

What this education and research did not guarantee was a turn towards natural philosophy or theoretical physics. At Cambridge as in France, physical problems continued to be the inspiration for forays into mathematics. Theoretical physics did not appear at Cambridge until after 1850. In the 1820s we already have the development of extremes with respect to mathematics, with Herschel and Airy, proclaiming it as a tool, and Peacock defending what became a uniquely British form of pure mathematics. We also have both Airy and Herschel trying to connect mathematics and its manipulation to physical understanding of their observational results. However, both were suspicious of the use of hypotheses within what they regarded as the purely empirical and inductive sciences.

Yet out of this mix of battles over the curriculum and uncertainty over the place of hypotheses in the experimental sciences, theoretical physics emerged in some of its incarnations during the late 1840s and into the 1850s in the work of George Gabriel Stokes, William Thomson and James Clerk Maxwell.

Part III

Transformations, 1830–1870

Mathematics constitutes the language through which alone
we can adequately express the great facts of the natural world.*

*Ada, Lady Lovelace, "Sketch of the Analytical Engine invented by Charles Babbage," in *Taylor's Scientific Memoirs*, (1843): 696.

Chapter VII

From Natural Philosophy and 'Mixed Mathematics' to Theoretical and Experimental Physics: Britain, 1830–1870

Most accounts of physics in the mid-nineteenth century focus on the conceptual transformations of the field. The date when conservation of energy and field theory became embedded in the research life of the discipline determines periodization.[1] These accounts are closely connected to the sense that the history of physics is written in the lives of the singular individuals that first clarified and stated these concepts.[2] Historians recently have focussed on communities of fellow practitioners and their interactions with their fellows rather than a few individuals in their isolated grandeur as thinkers.[3] The legacy of intellectual leaders is often seen as less clearcut and more given to diverse interpretation than in earlier depictions. Above all, they are seen more often than not as a product of their time and place. Physics has become a product of many hands.[4]

Nowhere is this more certain than in Britain in the middle of the nineteenth century for all the "inductive" and "mathematical" sciences. In a conscious effort to assert a place within British society and at the same time define their cultural function, practitioners of nineteenth-century science developed new social and intellectual forms and introduced new definitions and descriptions of their work. In

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1 This was made explicit by Thomas Kuhn, *Revolutions*, and L. Pearce Williams, "The Physical Sciences in the First Half of the Nineteenth Century: Problems and Sources," *Hist. Sci.* 1 (1962): 1–15. For Williams the history of nineteenth-century physics after the discovery of the conservation of energy was a footnote to what had gone before.

2 Williams," The Historiography of Victorian Science," *Vict. Stud.* 9 (1966): 1977–204.

3 The clearest documented example of this thus far is Darwin's network of colleagues and acquaintances from which he gleaned support, information, and opinions. On a much smaller scale this can be seen in the mutual support system of Maxwell, Thomson, and Tait.

4 The historiography, difficulties, and advantages of biographies in science are discussed in *Telling Lives in Science: Essays on Scientific Biography,* Michael Shortland and Richard Yeo, eds. (Cambridge: Cambridge University Press, 1996).

creating these new social forms, nineteenth-century British scientists responded to the demands of society and took the opportunities of those demands to develop careers. They also used their new institutions as well as older ones to forge a particular image for their disciplines which located science and its practitioners firmly within the socially respectable, ideologically safe classes in British society. Similarly the transformations of their practices were neither random nor arbitrary. These new practices described the changes occurring in the sciences and encapsulated their practitioners ideal vision of their work, their place, and function in society and culture. These were given visible form in the establishment of the British Association for the Advancement of Science. Taking advantage of the economic developments and the call for educational reform in mid-century, scientists forged academic and consulting careers for themselves in government and industry. In doing so they increased the distance between their practices and the general public for whom they continually claimed their work was so central.

The emergence of physics as a distinct field within the sciences took place and reflected the changing practices, functions, and cultural place of the sciences in nineteenth-century Britain. The transformation of mixed mathematics and experimental and natural philosophy into applied mathematics and theoretical and experimental physics exemplified the professionalization of science. These processes also demonstrated the fragmentation of natural philosophy into specialties, its development into an autonomous culture, and its increasing isolation from the general public, even as it became more important for the industrial economy. These displacements also led to transformations in teaching and research practices within natural philosophy and mathematics as they were molded into physics. The site of many of these changes was Cambridge University. Most of the men who created theoretical physics were trained to excel in the Mathematical Tripos. While written against general disciplinary and social changes, the transformation of this mathematical training into a research career in physics is highlighted through the work of a few individuals, George Gabriel Stokes, William Thomson and James Clerk Maxwell. Of these, only Maxwell examined the process they had put into motion, yet they all acted as though they were aware that the goals of their work set them apart from the mathematical majority at Cambridge. They behaved as if they already belonged to an international discipline with members scattered across Europe and the United States.

Keywords

During the early decades of this period, a number of keywords were introduced into the general discourse of science or changed their meanings significantly. Doctrine became theory. Theory connoted a logically developed sequence of consequences from a set of defined ideas. Metaphor and analogy alone no longer served to connect experimental results to hypotheses about nature. Hypothesis replaced speculation to describe conjectures about nature's structure and functioning. Speculation now carried the connotation of recklessness and intellectual infelicity, if not a general coarseness of thinking. Renaming brought respectability and reflected the understanding, both formal and informal, that suppositions about nature were necessary and central to the practice of science. Hypotheses were at the same time of narrower signification than speculation. The latter encompassed the whole theorizing process, analogs, and metaphors, onto which phenomena were loosely attached. Hypotheses signified the foundational ideas upon which a conjectural, yet logical structure was built and into which observations and experimental results were embedded. Finally the study of nature was no longer philosophy but science. This indicated the autonomy that practitioners demanded and established a claim for the results of their explorations of nature which earlier philosophers were wary of. The meaning of this term was also narrowed and clarified by the methods that these practitioners professed as theirs alone to reveal a truth not vouchsafed to other methods or practitioners.

In a revealing invention that built on this new meaning for the term science, William Whewell encompassed the new practices and social function of his colleagues in the term scientist. Whewell recognized that philosophy had lost its unity and limited science to the study of the natural, but not the moral world. He also understood that, as he wrote, science itself was fragmenting. The tendency of science was fragmentation, "the mathematician turns from the chemist," and chemist from the mathematician, and, if left to themselves, the mathematicians divide into the "pure" and the "mixed." Later he differentiated and named the physicist in an English imitation of the French term *physicien.* Experimental philosopher was a term of the past. While he understood the symptoms of the changes in practice around him, named and classified them, Whewell also investigated them historically and philosophically. These investigations were colored by his understanding of the research on the tides he was simultaneously engaged in.[5] For our purposes, the most important aspect of Whewell's study of science lies in his understanding of the necessity for hypotheses within the "inductive" sciences that he formally established as philosophically legitimate.[6]

Whewell also detected changes in the actual methods used to study nature and the influence of the French mathematics and experiment.[7] He noted that the prac-

5 For his remarks on science, scientist, and physicist, see William Whewell, "Review of Mary Somerville, *Connexion of the Sciences*," *Quarterly Rev.* 51 (1834): 58–68, 59–60. See also S. Ross, "Scientist: The Story of a Word," *Ann. Sci.* 18 (1962): 65–85.

6 See Richard Yeo, *Defining Science*. Of his contemporaries John Herschel admired his philosophical work yet disagreed with him, as did Airy and Brewster.

7 Geoffrey Cantor has investigated the juxtaposition of natural philosopher versus scientist on more philosophical grounds in Cantor, "The Reception of the Wave Theory of Light in Britain: A Case Study illustrating the Rôle of Methodology in Scientific Debate," *Hist. Stud. Phys. Sci.* 6 (1975): 109–132. However, Cantor assumes that all papers on wave

tice of experimentalists was now self-consciously quantitative. The exactitude so evident in observational astronomy was only just being introduced into experimental physics.[8] The growing importance of precision and measurement was signaled also in the establishment of a Statistics Section of the British Association in 1833.[9]

The end result of performing quantitative experiments was the narrowing of access to experimental and observational research. Instrumentation and apparatus became ever more expensive, required institutional resources, the resources of the very rich, or the government. The manipulation of instruments and expensive apparatus required systematic training in their use and an understanding of the meanings encoded in the quantities measured. Training included an understanding of how instrument and theory met, hence what was measurable and the limitations of the measurement methodology. As the lower limit on resources to enter the experimental sciences escalated, so did the lower limits on experimental skill. Fewer men could expect to enter the research community. Both the practices of research and training to enter the research community were reorganized.

Simultaneously the changes in institutional structure made the transformation in research manifest and then standard within the experimental sciences. In the vanguard of the display of this new form for the experimental sciences was an equally new organization, the British Association for the Advancement of Science.

The Crucial Turn: the 1830s

The British Association for the Advancement of Science gave scientists a national institutional unity that was previously lacking. Some historians have seen the rich diversity of pre-Association scientific institutions as dividing the scientific community into intellectual, methodological, religious, and ideologically warring factions. Others have tied the lack of a unified social structure for science to pre-

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theory lie within physics without looking at the role of physical imagery in some papers where it is marginal at best.

8 British observational astronomers then physicists went beyond the quantitative French experimentalists in their investigations of the limitations of these new methods. For the development of these methods, see Zeno G. Swijtink, "The Objectification of Observation: Measurement and Statistical Method in the Nineteenth Century," in *The Probabilistic Revolution,* Lorenz Krüger, Lorraine J. Daston and Michael Heidelberger, eds. (Cambridge MA.: MIT Press, 1990), 2 vols., vol. 1, 261-285, Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Sci. Context* 2 (1988): 115–145. See also John Cawood, "The Magnetic Crusade." Cawood narrates the advent of accurate geomagnetic measurement as well as a major lobbying effort to gain funds from the government for this worldwide, long-term effort.

9 This was not the only function of the Section. See Lawrence Goldman, "The Origins of British 'Social Science': Political Economy, Natural Science, and Statistics, 1830–1835," *Hist. J.* 26 (1983): 587–616.

vailing social values of individualism, voluntarism, and laissez-faire economics.[10] No argument addresses the reasons for scientists to actually form a new voluntarist organization. Nor do they explain why the British Association was a success right from its first meeting at York. We need to accept that criticisms and attempts at reform of the Royal Society, the cries of distress at the lack of government support for science, and other discontents expressed in the late 1820s and early 1830s were more than demands for funding, or cultural and personal ambition, although the latter were certainly ingredients in the mix. While the British Association ostensibly was based on the *Gesellschaft Deutsche Naturforscher und Ärtze,* its purposes were somewhat different. A national forum seemed essential to address the needs of and give collective identity to practitioners of the sciences. From the perspective of Britain, France and the German States already possessed such organizations as well as the attention of their governments. Funding of research also could be taken for granted. By 1830 science in France and in the German universities was a profession. The added advantage of the German professoriate was that their universities were government institutions. They were Civil Servants. No such attention or assurances of even minimal funding existed in Britain. At those early meetings, scientists could point to both French and German institutions that gave scientists a communal identity and a social place that British scientists had to forge for themselves. In Britain in the 1820s, experimental philosophers were already fast becoming scientists and their numbers were multiplying. In this era the increasing volume of research around clearly defined problems conveys a sense of disciplinary identity that transcended locality. This was already expressed socially in the establishment of some specialist societies.

The British Association from its first meeting met the need for a national organization for practitioners of science. The sections devoted to specialist reports recognized the ongoing intellectual changes within the sciences and gave them social expression. The general meetings of the whole body reinforced a sense of identity in a new endeavor within British society. In its first decade, the British Association largely defined that identity.

The public and the government had to be educated in the utility of research and the necessity for government funding. British scientists were also under other political restrictions. The Great Reform Bill was generating pressures for change along with social tensions and political suspicion.[11] In science these tensions and

10 The differences are emphasized in *Parliament of Science,* Roy Macleod and Peter Collins, eds.(Norwood: Science Reviews, 1981) and glossed over in Morrell and Thackray, *Gentlemen of Science.* For Individualism, see J. B. Morrell, "Individualism and the Structure of British Science in 1830," *Hist. Stud. Phys. Sci.* 3 (1971): 183–204.

11 For a recent discussion on the historiography of the Reform Act, see John A. Phillips and Charles Wetherell, "The Great Reform Act and the Political Modernization of England," *Amer. Hist. Rev.* 100 (1995): 411–436. The authors argue that despite recent scholarship 1832 marked a watershed in the political life of England.

suspicions were exacerbated by the battles within the Royal Society and the Decline of Science debates. Political damage to the founding of a national organization became very possible unless handled carefully because of the Royal Society's connections with the government and the criticisms of those connections. As well as fostering external skepticism and suspicion, the debates forced divisions within the ranks of the scientists making the practical problem of drawing them together that much more difficult.[12]

William Whewell wanted to retain the monopoly of Cambridge University over mathematics, the science regarded as a key element of the change from natural philosophy to science. Any changes in the status quo, including governmental funding, threatened this primacy.[13] He quickly moved to help frame the British Association when convinced that the movement would be out of his grasp if he did not join. John Herschel was not against government funding, although he considered himself above the political fray because of his wealth. This public persona gave him immense credibility as a spokesman for science. While Charles Babbage, Herschel, and Whewell could join David Brewster in arguing for the utility of science, that utility was tied to the "higher" aims of national government. Herschel and Whewell did not see utility in terms of commerce, industry or the lives of ordinary citizens, closer to Brewster's concerns.

Personal disputes and ambitions could become other reasons to wreck the fledgling organization unless kept within reason. If the British Association was to act as a national organization, scientists with reputations to match had to be at its center. If government was to be convinced that science was important to the nation, scientists with access to the government were necessary to lead that same organization. This new national institution had to be suitably framed and tamed to pose no social, cultural, or ideological threat in such a tense political atmosphere.[14]

Internally the purposes of the Association meshed with those of emerging professionals. Reports and papers delivered in the sections were geared to the research community of the various disciplines. Cultivators of science were welcomed, their numbers helped to demonstrate and add to the claims for government attention. They had no say in the shaping of the institution. The scientific business of the Association was in the hands of specialists who arranged the meetings, chose the programs, and vetted submitted papers. They also chose who addressed the general public in the open sessions, and hence represented science to the laity. In the 1830s the clerisy could define and control this image and even the doing of science within the Association. However, there were already signs that such control

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12 These conflicts and the solutions to them are detailed in Morrell and Thackray, *Gentlemen of Science,* chap. 2.

13 However, Whewell was not above accepting government monies later in the 1830s.

14 The British Association, its social and cultural framing and the maintenance of this stance are detailed in Morrell and Thackray, *Gentlemen of Science,* chap. 3.

was impossible beyond the annual meetings and even within some sections of the Association as well.

The very size and public display of the annual meeting did not meet with universal acceptance. The theatrical aspects of the meetings, the sightseeing expeditions, the social spectacle all guaranteed that the message that the study of nature was being done in new ways was not lost on the public.[15] Yet this new enterprise still functioned within the confines of particular cultural norms. Science remained a gentlemanly pursuit while proclaiming that it was culturally, economically, intellectually, and morally necessary to the nation. In cutting its annual swath through British society, members of the British Association separated themselves visibly from the values expressed in the Royal Society with the latter's ties to outmoded political values and forms. The Association managed to do this without visible signs of political entanglements.

Within a decade the Association was indispensable for the practitioners of the sciences. The Association met many needs of the growing professions within science whether or not the rank and file of the sciences shared the political ideology and cultural pretensions of its leaders. Careers were launched with papers at the sections. New work presented in its meetings was important for physicists and mathematicians who formed no national, disciplinary societies of their own. It united individuals through the research they shared at the annual meetings. If this was not enough, there was a steady stream of commissioned review articles on various branches of the sciences. In the 1830s the subject matter, content, and viewpoints expressed within these reports helped to define what science was.

During the 1830s the papers of Section A helped to define the practices of mathematicians and physicists. The reports in physics and mathematics were narrowly focussed on the tides, electricity and magnetism, conical refraction, algebra, etc. Their authors were all centrally placed in the research community of those subjects. They did not necessarily alter every aspect of those disciplines. The influence of the French was heavy in terms of the problems chosen as subjects of reports. The French also provided the results and methodologies in attacking research problems whose solutions were the subject of annual progress reports. Exploitation of French practices only reinforced the traditional geography and boundaries between the experimental science of physics and mathematics. In detail the reports often reflect the practices and prejudices of the individual making the reports rather than present a coherent sense of either experimental physics or mathematics. They also served other purposes. Both Whewell's and Peacock's reports were intellectual salvos in the ongoing battle over the Cambridge curriculum.

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15 See A. D. Orange, "Idols of the theater: The British Association and its Early Critics," *Ann. Sci.* 32 (1975): 277–294. For the growing unease of some religious leaders over the developing cultural power of science, see Frank M. Turner, "The Victorian Conflict between Science and Religion: A Professional Dimension," *Isis* 69 (1978): 356–376.

Reports to Section A share some common characteristics. Mathematics based on the algebraic relationships deduced from quantitative experiments provided a descriptive development. It did not provide an explication of physical processes. Hypotheses were accepted as an aspect of the search for understanding the processes of nature and rightly belonged within physics. Theoretical considerations were provided separately usually in a non-mathematical, physical addition to the report. Reports in mathematics also set known, experimental results within a net of mathematics. Occasionally the mathematician might predict a new physical result, but not give a physical explanation for its occurrence.

Physics was still experiment. Both Baden Powell's report on radiant heat and Brewster's report on optics were concerned wholly with observations, experimental work and their interpretation. No mathematical deductions or algebraic generalizations grace either paper. Powell included tables of data but deduced nothing from them. The same was true of S. Hunter Christie's report on terrestrial magnetism. The only difference lay in Christie's inclusion of Gauss' suggestions on methods of observation.[16]

On the other side of the disciplinary boundary was George Peacock's report on analysis. Peacock was at pains to establish algebra as equally deductive and rigorous as geometry. He placed French mathematics within the context of his own interest in symbolic algebra. He emphasized that modern analysis bore the same relationship to physics as did geometry to the real world. In principle, analysis was a legitimate "mixed" form of mathematics. Peacock pointed out that while "speculative" mathematics began in principles and definitions that were absolute, the mathematics of the physical sciences were based on contingent definitions and principles that were constantly the subject of experimental research and re-examination. These principles were "the basis of those interpretations which are perpetually required to connect our mathematics with the corresponding physical conclusions." Peacock gave some space in his report to defending Fourier analysis, and showed how Fourier's methods could be applied to discontinuous functions. He even provided a geometrical illustration of how Fourier's solutions could represent such a function, although Peacock also noted the unsatisfactory nature of some of Fourier's proofs.[17]

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16 David Brewster, "Report on Recent Progress in Optics," *Rep. British Assoc.* (1832): 308–322; Baden Powell, "Report on Our Present Knowledge of the Science of Radiant Heat," *Rep. British Assoc.* (1833): 259–301; S. Hunter Christie, "State of Our Knowledge Respecting the Magnetism of the Earth," *Rep. British Assoc.* (1834): 105–130. See also James David Forbes, "Report on recent Progress and present State of Meteorology," *Rep. British Assoc.* 1 (1831): 196–258.

17 George Peacock, "Report on the Recent Progress and Present State of Certain Branches of Analysis," *Rep. British Assoc.* (1833): 185–352, his discussion of Fourier, 248–259. For Peacock's mathematics, see Koppelman, "The Calculus of Operation," *Arch. Hist. Exact Sci.* 8 (1972): 155–242; Richards, "The Art and Science of British Algebra,"; Helene

The critical issue of the relationship between mathematics and observation arose in the reports on experimental physics, which also had been the subject of sustained mathematical development. Humphry Lloyd gave voice to a new relation between experiment and mathematics. Vague physical speculations were no longer enough when mathematicians compared experimental results to those of their mathematics. However attractive an hypothesis, "it is only when it admits of mathematical expression, and when its mathematical consequences can be numerically compared with established facts, that its truth can be fully and finally ascertained." This was important for Lloyd to state because the point of his report was not to investigate "mathematical optics" in detail but to establish the validity of the hypothesis that light was a transverse wave in the ether. He presented the mathematics of William Rowan Hamilton, Augustin Cauchy, James MacCullagh and Augustin Fresnel as mathematical theories.[18] Lloyd pointed out Cauchy's work as, "an interesting department of analysis" but not strictly a physical theory. This was because in Cauchy,

> the phenomena of light are not connected *directly* with any given physical hypothesis; but are shown to be comprehended in the results of the general theory, in virtue of *certain assumed relations* among the constant which that theory involves.[19]

Cauchy chose the coefficients in his equations for the wave fronts in a crystal so that they were compatible with Fresnel's experimental results. The actual behavior of light in a crystal remained a mystery.

Physically detailed theories were not part of mathematics. Experiment only tested the foundations of the mathematics, the hypotheses on which they were based. Physical explication remained in non-mathematical language and belonged to physics proper. In Lloyd's case, he gave Cauchy's mathematics physical meaning (wave fronts) but realized that arbitrary constants added nothing to the physical explanation of the behavior of light. What he did not tell his listeners was how he deduced the physical meaning he had found in Cauchy's analysis.

In a similar fashion Whewell used geometry to treat mineralogy as a branch of mathematics. The physical foundations for mineralogy lay in the optical, and other measured, properties of minerals.[20] James Challis shared the common view that

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Pycior, "Internalism, Externalism and Beyond: Nineteenth-Century British Algebra," *Hist. Math.* 11 (1984): 424–441, and Menachem Fisch, " 'The Emergency which has arrived' ." For a discussion on the impact of Fourier on mathematics in Britain, see Garber, "Reading Mathematics, constructing Physics," in *No Truth Except in the Details,* Kox and Siegel, eds. 31–54.

18 Hamilton's work on optics, as Lloyd knew, stood apart from any specific hypothesis of the nature of light.

19 Lloyd, "Report on the Progress and present State of Physical Optics," *Rep. British Assoc.* (1834): 295–413, 391.

20 Whewell, "Report on the recent Progress of and present State of Mineralogy,"*Rep. British*

problems of physics were the source of mathematical exercises. These types of studies, "the highest department of the physical sciences, may be properly denominated *Mathematical Physics.*" The source for his mathematics and the physical model on which they were based lay in Laplace and Poisson. The problem of universal gravitation was solved only on a macroscopic level, the evidence of its validity was mathematical. Similarly, evidence for the theory of the attraction of particles on the molecular level on which theories of capillarity were based was also mathematical. Challis regarded the results of experiments that matched mathematical solutions as confirming those mathematical solutions. He also noted that such mathematical theories were full of constants to be determined by experiments.[21]

In his report on capillarity, Challis confronted mathematical deductions with the results of ongoing experiments. Yet the details of the molecular-force model and the phenomenological results that it led to were still expressed in the vernacular. There was no explanation of how resultant molecular forces could be expressed algebraically, although their effects were vouched for by experiment.[22] Challis took over a particular form of French mathematical physics and was more successful than either Poisson or Laplace at explaining the actual forces between molecules. Whether Challis practiced mathematical physics or "physical mathematics," it was not theoretical physics. It is difficult to see whether the point of his report was the development of mathematics using solutions to physical problems or merely their confirmation through experiment. He saw the mantle he assumed as coming ultimately from Newton. And Challis' commitment to this foundation of his version of Newtonianism and analysis was lifelong.[23] His research on fluids was based on his conviction that the internal structure of matter depended upon attractive and repulsive forces that were only expressible mathematically, and that the mathematics for this expression was in the work of Laplace and Poisson.

Any analysis of Whewell's reports to the British Association has to deal with the complication of his ongoing battle to define the curriculum at Cambridge. In his report on the mathematical theories of electricity and heat, Whewell disparaged Fourier's mathematics. He did not have too many good things to state about Poisson's either, although he only reported on Poisson's mathematical theory of electricity and magnetism.[24] His actual examination of Poisson's work was cursory

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Assoc. (1832): 322–365

21 James Challis, "Report on the Analytical Theory of Hydrostatics and Hydrodynamics," *Rep. British Assoc.* 3 (1833): 131–151.

22 James Challis, "Report on the Theory of Capillary Action," *Rep. British Assoc.* (1834): 253–294.

23 See Challis, *Remarks on the Cambridge Mathematical Studies and their Relation to Modern Physical Science* (Cambridge: Deighton Bell and Co., 1875). An alternative title might be "Back to Newton."

24 Whewell apparently was ignorant of Green's work.

as Whewell restricted his remarks to those cases where Poisson compared his analytical results directly with experiment. Whewell chose to quibble with the physical model from which Poisson extracted his mathematics. Yet Whewell took Poisson's comparisons at face value. He assumed that the analytical conditions reflected those of the experiments. When he dealt with the differences between mathematical and experimental results, inaccuracy belonged to the observations. Yet he concluded that mathematics and experiment "coincide as near as could be expected."

Whewell understood Poisson's mathematics as its form was that of Laplace's celestial mechanics and grounded in problems familiar from the Cambridge curriculum. He was not so charmed by Laplace's mechanism for the conduction of heat in a body. Nor for that matter did he like Fourier's. He judged Fourier's physical reasoning incorrect. However Whewell had to accept Fourier's equation for the conduction of heat. Hammering away at Fourier, Whewell insisted that he could not dispense with molecular reasoning in his account of the cooling of the earth. He echoed Poisson's criticisms as well. Fourier's solution for the equilibrium conduction of heat in a lamina,

$$\frac{\partial^2 T}{\partial x^2} + \frac{\partial^2 T}{\partial y^2} = 0,$$

such as $T = e^{-mx} \cos my$, were only particular solutions. For a complete solution to this equation boundary conditions for the temperature, T, must be introduced in the form of some "prescribed law." This led him to the consideration of discontinuous functions, a "curious and perplexing part of analysis."

He focused on one issue, an example of the equilibrium radiation of heat from a sphere. He had to consider the work of French mathematicians but regretted that the mathematics of Fourier, Laplace, Poisson and Libri,

> has not been in all respects favorable to the progress of the subject as a branch of experimental and inductive science. The great beauty and curiosity of meaning of the mathematical investigations which offered themselves to our analytical discoverers, have led them to wander in that deep and charmed labyrinth much longer and farther than the demands of physical science required.[25]

Whewell assumed that mathematics contained physical significance and he focussed on unearthing those significances by directly comparing mathematical deductions and experimental results. However, he accepted the methods of mathematicians as sufficient to define the interactions with experiment. He assumed that mathematical theories were necessary but that the focus of the French was mathematics, not the investigation of the physical properties of heat, electricity,

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25 Whewell, "Report on the Recent Progress and Present Condition of the Mathematical Theories of Electricity, Magnetism, and Heat," *Rep. British Assoc.* (1835): 1–34, 29.

or magnetism. However, he did not indicate here how to proceed to reorient this kind of study. He could only complain that the French left the majority of mathematicians behind them. Some simpler form of mathematics might suffice, that of Newton perhaps, although Whewell conceded this would mean some sacrifice of rigor and generality.

During the 1830s Whewell's was not the only report on heat given before the British Association. In 1837 Phillip Kelland wrote a text on the subject partially encompassing Fourier's work. In both text and report, Kelland tried to develop a physical model for heat. He rejected caloric theory because it could not explain radiation and turned to a vernacular version of Poisson's molecular model. When he developed his mathematical theory of heat, he used Fourier. The text remained in two distinct parts. This was not a copy of French mathematics since Kelland developed special cases that led to real physical circumstances. These circumstances were reflected in experiments whose results could be directly compared to the mathematics. He focussed on this aspect of his work in his British Association report. He was hard put to do this given the relationship of physical model to mathematics in his own work and the absence of physical process in Fourier's mathematics. He worked out specific examples that might be tried experimentally. The four mathematical theories did not allow him to do this. Kelland noted that mathematicians, Poisson in particular, had "not presented their results in a form sufficiently tangible to direct or suggest the application of experiment to them." Experiments in and of themselves could not decide among the various mathematical interpretations. Available experiments also were not consistent enough to lead to any one empirical law of conduction. Kelland went on to suggest some experiments that might do that and the difficulties they presented to the experimenter.[26]

Kelland's work on heat was characterized later as mathematically ingenious but physically flawed. Supposedly Kelland confused heat flow and temperature and wrote of "temperature flow." In his research Kelland sought a physical expression for the mathematical generalization of Fourier's relation that heat flow was proportional to temperature difference. Kelland changed this linear relationship into the more general one where heat flow was dependent on some function of the temperature.[27] Kelland was a mathematician who was trying to find an empirical foundation for the mathematical work of Fourier. He then tried to to validate this mathematics as his French models did by comparing experiment and mathematics. However, the experimental results at his disposal were inadequate. He was a mathematician and generalized the conditions of Fourier's work mathematically

26 Kelland, *Theory of Heat* (Cambridge: J. J. Deighton, 1837) and Kelland, "On the Present State of our Theoretical and Experimental Knowledge of the Laws of Conduction of Heat," *Rep. British Assoc.* (1841): 1–25, 25.

27 George Chrystal and Peter Guthrie Tait, "The Reverend Professor Phillip Kelland," *Proc. R. Soc. Edinburgh* 10 (1879): 319–321.

in ways analogous to the mathematically more successful work of Liouville.

In his 1837 text Kelland did more than misunderstand Fourier. He examined all the available mathematical theories of heat for their physical content. He extracted that content from the particular solutions of the equations of motion of heat presented by each of his mathematicians. He then tried to compare the results of this analysis to known experimental results. This skill was not appreciated in the British Association, nor in Cambridge.

Kelland's was by far the most sophisticated use of mathematics within experimental physics in these early years of the British Association. It matched that of Airy's in astronomy. In addition, Kelland did not seem to wish to limit mathematics strictly to the immediate needs of physics, as Airy did in astronomy. Airy complained that Laplace had "banished empiricism from astronomy" in his comment's on Laplace's work on the perturbations of the orbit of Jupiter. Similarly he considered that in Gauss' work on secular variations, "the ingenuity of transformation [of variables] etc., deserves notice, but the theory of perturbations has gained nothing."[28] Airy considered both the *Mécanique Célèste* and Gauss' work in astronomy as mathematics. His concern was the use to which such mathematics could be turned for the astronomer. And he judged most mathematical efforts harshly.

All these reports reflect some common assumptions and expectations but diverge in details. The French were the model for experimental physics and the mathematics that arose from that physics.[29] Yet in the majority of cases the goal was to understand the actual workings of nature. These explanations belonged to physics and were not inherent in the mathematics. At least, the hypotheses from which the mathematics was developed belonged to physics. These, along with the results of careful quantitative experiments, were the starting point of mathematical "theories." Because each report was essentially a review article, the issue of what precisely the mathematical development of physical hypotheses added to the inductive enterprise was usually left moot–Until we come to Airy and Kelland. Airy seriously doubted the necessity for all the analytical baggage attached to the particulars that could be used by the observational astronomer in search of ever more accurate results. For Kelland, experiment was not definitive in deciding which set

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28 Airy, "Report on Recent Progress in Astronomy in the present Century," *Rep. British Assoc.* 1 (1831): 125–188, p. 172. Airy's extreme utilitarian streak appalled William Rowan Hamilton who reported that Airy stated "the Liverpool and Manchester Railway, he said, playfully perhaps, but, I think sincerely, he considered as the highest achievement of man." Hamilton's hesitation may well mean Airy also had a sense of humor. See Robert Percival Graves, *Life of William Rowan Hamilton* (New York: Arno Press reprint of 1882 edition, 1973), 3. vols., vol., 1, 444.

29 We should, however, state that with Peacock's work in algebra the British developed their own sense of "pure" mathematics distinct from those of either France or Germany. See, Joan Richards, "Rigor and Clarity."

of mathematically expressed hypotheses reflected the actual workings of nature.

These reports represented the first systematic attempt to take stock of French research. No one offered an alternative to the French approach except in the most general of terms. All of the reports touched on the difficulties that full acceptance of French methodologies and research goals posed for native traditions of practice and the purposes for research in experimental physics.

The development of some alternatives were apparent in embryonic form. Emulation was not the path of the majority. Both Lloyd and Kelland interpreted the mathematics in specific cases in physical terms and compared results with experiments. Yet these comparisons neither confirmed the mathematics nor physical hypotheses. They opted for quantitative experiment and mathematics, but which mathematics? Even here there was no consensus. Whewell would deny the need for any modern French mathematics just as surely as the other commentators chose them. But the form of analysis they chose ranged from Lagrange to Laplace and Poisson to Fourier.

Quite how this rich mixture of French mathematical and experimental physics was transformed into theoretical physics of various kinds requires that we turn to the universities and concentrate on Cambridge, the battle over what mathematics was taught there, and the contingencies of making academic careers in Britain in physics during the middle third of the nineteenth century

Cambridge University, the *Cambridge Mathematical Journal*, and Theoretical Physics

In the middle third of the nineteenth century all British universities, including Cambridge, came under pressure to update their curricula and broaden the social spectrum of its students. In reaction Cambridge reinforced the narrow foundations of its curriculum and only grudgingly acknowledged the existence of the natural sciences in its examination system in the 1850s. Cambridge seemed to retreat from the new continental mathematics.

In mathematics and physics, other universities responded to the French leadership by reforming the content of their courses and systematizing their teaching of them. Natural philosophy remained a subject taught separately from mathematical physics. However, the content of the natural philosophy courses shifted. Term, or even year-long courses on particular fields in physics, were available to students. Faculty made concerted efforts to include courses on heat, light, and other fields of research in physics while much of the material was encompassed within a mechanical explanatory net.[30] Such focussed courses were empty of the mandatory ties to natural theology of the first decades of the century. The universities of Edinburgh

30 See David Wilson, "The Educational Matrix: Physics Education at Early Victorian Cambridge, Edinburgh and Glasgow Universities," in *Wranglers and Physicists,* Harman, ed. 12–45, for the courses at Glasgow and Edinburgh. For James David Forbes' work in this

and Glasgow further adapted the teaching of mathematics and natural philosophy to the needs of engineers and other future professionals.

New French mathematics and experimental physics had a more immediate impact in Dublin in terms of changes in teaching and in research.[31] The most important research to emerge during the early decades of this influence was that of William Rowan Hamilton. His research in optics and dynamics were in the French mathematical tradition. His optics began as an investigation into the mathematical properties of systems of rays and the surfaces light formed in passing through optical systems. He then turned to the work of other mathematicians on double refraction and the surfaces formed by the ordinary and extraordinary rays in biaxial crystals. Although he preferred the wave theory, Hamilton understood that his mathematical results were independent of any assumptions about the nature of light. His work was,

> not to discover new phenomena, nor to improve the construction of optical instruments, but with the help of the differential or fluxional calculus to remold the geometry of light, by establishing one uniform method for the solution of all problems in that science, deduced from the contemplation of one central, or characteristic relation.[32]

Hamilton's goal was to reduce optics to analysis as Lagrange had reduced mechanics.[33] While only "a secondary result," this deductive method had lead to "some unexpected conclusions." Out of his consideration of the mathematics of Fresnel's work on biaxial crystals Hamilton deduced that the surface of the wave front within the crystal,

> 1st has *four cusps* (at the ends of the optic axes) at each of which the tangent planes are (not, as he [Fresnel] thought, two but) infinite in number; and 2nd, *four circles* of *plane contact*, along each of which the ray is touched, in the whole extant of the circle, by a plane (parallel to

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direction see also, John Campbell Shairp, Peter Guthrie Tait and A. Adams-Reilly, *Life and Letters of James David Forbes* (London: Macmillan, 1873). Forbes also introduced written examinations into Edinburgh.

31 See Grattan-Guinness, "Mathematical Research and Instruction in Ireland, 1782–1840," in *Science in Ireland 1800–1930,* J. R. Nudds et. al., eds. and the account of Hamilton's education at Trinity College, Dublin in Thomas L. Hankins, *Sir William Rowan Hamilton* (Baltimore : The Johns Hopkins University Press, 1980), 22–23.

32 Hamilton to Samuel Taylor Coleridge, October 1832, in Graves, *Hamilton*, vol. 1, 592. Graves notes that this letter was actually never sent. See also Hankins, *Hamilton*, 61–62. Hankins gives a detailed discussion of Hamilton's work on optics, chaps. 4 and 5, and conical refraction in chap. 6. Hamilton identified the characteristic function with the principle of least action, or "least time."

33 Hamilton made this comparison explicit in Hamilton, "An Account of a Theory of a System of Rays," *Trans. R. Irish Acad.* 15 (1824) [1828]: 69–174; 16 (1830): 4–62; 16 (1831): 85–92; 17 (1837): 1–144. In this paper Hamilton was careful to refer to both fluxions and the calculus, although he used continental methods.

> one of the circular sections of the surface of elasticity);[34]

Hamilton deduced "from these geometrical properties, a single incident and unpolarized ray would undergo, not double but *conical* refraction."

This result emerged from the geometrical properties of the rays themselves. It was not a result of any physical assumptions or arguments. Humphry Lloyd interpreted these results in experimental terms and confirmed Hamilton's mathematics. As Lloyd noted in his British Association report conical refraction, because it was independent of any physical assumptions about the nature of light, could not decide any issue about its nature.[35] There were no analytical tricks to disguise whether the mathematical and experimental case might or might not be identical. To bring analytical results and experiment together, Hamilton interpreted the results of analysis in geometrical terms. Hamilton repeated this in his work on quarternions.[36]

Hamilton extended his approach to dynamics. Here also was a physical foundation for this mathematics. He began with a mechanical system made up of a set of attractive and repulsive points. He wanted to introduce mathematical economy into the study of such systems by reducing the motion of such points to the search for, and differentiation of, a single function that satisfied two partial differential equations of the first order and second degree. Hamilton characterized the potential for his method in mathematical not physical terms. While the method had been used in dynamics and optics,

> the peculiar combination which it involves, of the principles of variations with those of partial differentials, for the determination and use of an important class of integrals, may constitute, when it shall be matured by the future labors of mathematicians, a separate branch of analysis.[37]

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34 Hamilton to Herschel, December 18, 1832, Graves, *Hamilton*, vol. 1, 627. See also James G. O'Hara, "The Prediction and Discovery of Conical Refraction by William Rowan Hamilton and Humphry Lloyd," *Proc. R. Irish Acad.* 82A (1982): 231–257. Emphasis in the original.

35 Lloyd, "On the Phenomena presented by Light in its Passage along the Axes of Biaxial Crystals," *Phil. Mag.* 2 (1833): 112–120, 207–210, 116–117, his report to the British Association, Lloyd, "Conical Refraction," *Rep. British Assoc.* (1833): 370, reprinted in Lloyd, *Miscellaneous Papers Connected with the Physical Sciences* (London: Longmans Green, 1877), 1–18. Hamilton's report to the British Association on his optical work appeared as Hamilton, "On Some results of the View of a Characteristic Function in Optics," *Rep. British Assoc.* (1833): 360–370.

36 Hankins in *Hamilton*, sees his real strengths in analysis which, in the formal sense of the papers Hamilton produced and his importance within mathematics, is true. However, what is striking in his account is Hamilton's ability to translate analytical results into geometrical imagery where the reader can visualize the result.

37 Hamilton, "On a General Method in Dynamics," *Phil. Trans. R. Soc. London,* pt. II (1834): 247–308, reprinted in Hamilton, *The Mathematical Papers of Sir William Rowan Hamilton,* A. W. Conway and J. L. Synge, eds. (Cambridge: Cambridge University Press, 1931) 3 vols., vol. 2, Dynamics, 103–211, 105. Hamilton had previously reported on his

However, the first mathematical results of Hamilton's work was not a new branch of analysis but a new method for the integration of partial differential equations. These were also published as a series of lectures on dynamics.[38]

Hamilton drew no new physical conclusions about the motions of point centers of force and while Hamilton put Boscovich's ideas into mathematical form the physical implications of any of his results were seemingly of no interest to him. In general he accepted the idea of light as a wave motion and the idea of immaterial matter. He did not explore either to develop physical theories of the behavior of light or of matter. As with French mathematicians specific physical issues set Hamilton exploring new mathematical possibilities.[39]

While Hamilton was the most prominent, James MacCullagh also took a mathematical approach to the theory of light. In his papers on double refraction MacCullagh noted that the phenomena were just so many isolated facts. He supplied the connective tissue of mathematics by explaining known experimental laws "hypothetically, by introducing a differential coefficient of the third order into the equations of vibratory motion"–a mathematical fix, but with no imagery to catch a physical process.[40] MacCullagh required that his mathematical relationship lead to known experimental laws but his system was still deductive. While his image of the ether was of a particulate medium, MacCullagh did not use its properties to derive his equation of motion. He began with an examination of the geometrical properties of ellipsoids and concluded from the results of Fresnel's work that, since the wave front in a biaxial crystal was an ellipsoid, the particles of the ether could only move in certain directions with respect to the wave fronts within the crystal. From these deductions, he derived Biot's Law and argued by analogy that his results agreed with Brewster's law as well. Important for his future work were MacCullagh's deductions of the mathematical form for the elastic force and the

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work in dynamics and its roots in optics in Hamilton, "On the Application to Dynamics of a general mathematical method previously applied to Optics," *Rep. British Assoc.* (1834): 513–518.

38 See Hankins, *Hamilton*, 196–197.

39 On Boscovichean atomism in Hamilton, see Robert Kargon, "William Rowan Hamilton and the Revival of Boscovichean Atomism," *J. Hist. Ideas* 26 (1965): 137–140, and, "William Rowan Hamilton, Michael Faraday and Boscovichean Atomism," *Amer. J. Phys.* 32 (1964): 792–795.

40 James MacCullagh, "On the Laws of Double Refraction of Quartz," *Proc. R. Irish Acad.* 1 (1836–40): 385–386. Reprinted in MacCullagh, *The Collected Works of James MacCullagh,* John H. Jellett and Samuel Houghton, eds. (Dublin: Hodges, Figgis and Co., 1880), 63–74, 63. MacCullagh published a series of papers on double refraction and reflection, and refraction throughout the 1830s and 1840s. See MacCullagh, "On the Double Refraction of Light in a crystallized Medium, according to the Principles of Fresnel," *Trans. R. Irish Acad.* (1830–32): 79–84; "On the Properties of Surfaces of the second Order," *Trans. R. Irish Acad.* (1836–40): 89–90 and "On the Dynamical Theory of crystalline Reflection and Refraction," *Trans. R. Irish Acad.* 21 (1848): 17–50.

elasticities as functions of the principal axes of the ellipsoid. Physical properties and conclusions arose from his mathematical manipulations, as he observed his deduction of Brewster's law emerged after "some complicated substitutions in the primary equations."[41]

As in the mathematical physics of the French, MacCullagh began in a specific physical problem. He was more careful that his deductions really were in line with experiment, or rather, with the experiments he considered as crucial, first Brewster's then Fresnel's work in refraction and polarization. It was the geometry of Fresnel's theory that led MacCullagh to his own notion of "the equivalence of vibrations." In addition to cleaving close to Fresnel's work, he assumed that since the ether was a mechanical body that all mathematical theories must be compatible with known mechanical principles and rejected Cauchy's work. He did not consider that the geometry of polarized light which he translated into physical terminology constituted a physical theory. The hypotheses he accepted out of Fresnel's work and deduced from his own mathematical development of it "are nothing more than fortunate conjectures." The problem was that the inner structure of the ether was a mystery. Its interaction with light as a transverse wave and the particles of matter were "utterly unknown." The mathematics of the ether was, therefore, subject to changes indicated by experiment. MacCullagh willingly altered the physical properties of his ether since experiment dictated but never presented a coherent set of notions about its physical character. He was fascinated by the relationships between ellipsoids and their tangent planes and stretching these geometrical properties to include light in crystals. Mathematical manipulations were the center of his attention. He continued to change, then generalize his mathematics, and in 1841 MacCullagh was approaching the problem of refraction and reflection in biaxial crystals using the potential function.

Physical imagery had different places in the mathematics of Hamilton and MacCullagh. Hamilton's work was replete with metaphysical necessities from which he drew his mathematics. Physical theory did not emerge from either his mathematics or his metaphysics. For MacCullagh, flexible physical imagery allowed him to correlate the results of his geometry of wave fronts to the small number of available experimental results. We cannot argue that either Hamilton or MacCullagh sought a systematic physical theory of light. Systematics lay within mathematics.

The Irish were not isolated and their work unknown. The British Association met in Dublin in 1835, and on the last day of the meeting, Hamilton was knighted. Hamilton corresponded on scientific, literary, and philosophical matters with Herschel, Whewell, Coleridge and many others in Britain. His work, Lloyd's, and MacCullagh's are epitome's of the responses across Britain to French experimen-

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41 MacCullagh, "A Short Account of Some Recent Investigations concerning the Laws of Reflection and Refraction at the Surface of Crystals," *Rep. British Assoc.* (1835): 7–8. Reprinted in MacCullagh, *Collected Papers*, 55–57, 56.

tal and mathematical research. British mathematicians seemed blind to the quarrels that separated the French into different mathematical camps. Hamilton did not see that embracing a form of metaphysics favored by Poisson and Laplace forced him to abandon the geometric elegance of Lagrange's mathematics. MacCullagh used whatever mathematical results and methods suited his immediate needs. One mathematician that the Irish did not seem to know or to use was Fourier.

The same could no longer be said of Cambridge mathematicians. By the late 1830s, despite the campaign of Whewell to purify the curriculum and rid the university of the French threat, Fourier and the mathematical promise of his methods had infiltrated Cambridge. However, any account of Cambridge mathematics in this era must include Whewell's struggle for control of the curriculum and the ways in which he ultimately lost that struggle.

Whewell and Peacock were both on the side of "reform," although they differed over just what aspects of change they were prepared to support. Peacock, along with other teachers at Cambridge, wanted to retain the French mathematics introduced in the early 1820s in the examination system and in the official curriculum. Whewell would have none of this and he had a good pedagogical point. A Cambridge education trained the mind and the next generation of clergy for the Church of England. A few highly-trained mathematicians schooled in the esoteric arts of analysis were not an advertisement for the claims he and other reformers were making about the Cambridge curriculum. Peacock's work on the foundations of algebra undermined Whewell's contention of the philosophical barrenness of analysis. In their battle over the curriculum at Cambridge, Peacock and Whewell needed to reach two audiences. The first consisted of mathematicians, a minority community within Cambridge but with allies outside Cambridge. They also had to reach the tutors and other faculty within Cambridge, and this is where the philosophical voice was so important. In this struggle Peacock won over the mathematicians. Together with the long term effects of the examination system at Cambridge, this annulled Whewell's short term gains over the curriculum.

Whewell's behavior in the struggles over the curriculum mirrored his actions within the British Association. After his initial offense at Brewster's slurs on Cambridge intellectual life, Whewell wholeheartedly supported the new organization. He also worked to ensure that control of this new venture remained in safe social and political hands. The Association never strayed into radical territory. It was reliable in the sense that the power relationships of the status quo were not upset. At Cambridge Whewell also sought to keep the power relationships intact.[42] This was

42 There is ample evidence that national politics and the internal upheavals in Cambridge were closely linked. For the involvement of both see Joseph Romilly, *Romilly's Cambridge Diary*, annotated and introduced by J. T. Bury (Cambridge: Cambridge University Press, 1967). Romilly notes those occasions at which politics did not enter the conversations during 1831 and 1832. For the impact of political reform on Cambridge, see Garland, *Cambridge Before Darwin*, chap. 2.

precisely what Whewell set out to accomplish within Cambridge. Power relationships between College and Universities would remain the same. The educational function of the university would not change. This commitment to conservation crystallized with the development of Whewell's career which was nurtured by those same power relationships. And to reiterate, as Master of Trinity he had to concern himself with the education of the whole body of students not merely a handful of wranglers. During the 1830s and 1840s, career, experience in teaching and research, and developing philosophical interests mixed with ambitions were channelled into his growing hostility against French mathematics.

By 1840 Whewell had lost the war within Cambridge and growing pressure from Westminster was to force reform on Cambridge in the early 1850s. Whewell's was a holding action. The subjects Whewell did not want introduced into the Natural Science Tripos appeared the year after he died.[43]

As Menachem Fisch has pointed out, Whewell approached the issues of reform in practical ways as well as philosophical ones. He wrote textbooks. It is through those textbooks and developing philosophical ideas that Whewell argued against the new mathematics, not simply on pedagogical grounds but as mathematics. However, his attempt to provide a philosophically defensible alternative to continental analysis was incomplete.[44] These textbooks were only part of his motivation and only one aspect of his published assault. In his Bridgewater Treatise, Whewell argued that those who were truly great inductive scientists were drawn to God through their scientific work, "the very imperfection of the light in which he works his way, suggests to him that there must be a source of clearer illumination at a distance from him." Among those in this group were Robert Boyle, Nicholas Copernicus, Galileo Galilei, and Johannes Kepler. In the group of lesser scientists who gained no religious sense through their science he included d'Alembert, Clairaut, Euler, Laplace, and even Lagrange. This rogue's gallery of mathematicians, whom Whewell knew to be mathematicians not inductive scientists, were the very men whose work he was arguing should be left out of the Cambridge curriculum. Their work formed the foundation for the mathematics that would displace Newton's formulation of mechanics, fluxions, and geometry as the educational mainstay of the university.[45]

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43 See Lewis Campbell and William Garnett, *The Life of James Clerk Maxwell* (New York: Johnson reprint of 1882 edition, 1969), 325, chap. 12.

44 See, Fisch, " The Emergency that has arrived'," 266–276.

45 Whewell, *Astronomy and General Physics considered with Reference to Natural Theology,* Third Bridgewater Treatise, 1833 (London: W. Pickering, 1834). This treatise went through many editions during his lifetime. The title alone indicated that Whewell was categorizing astronomy and physics as observational and experimental sciences, not mathematical ones. See also Richard Yeo, "William Whewell, Natural Theology, and Philosophy of Science in Mid-Nineteenth Century Britain," *Ann. Sci.* 36 (1979): 493–516.

Whewell's texts on mechanics were consistent in one goal, to draw students into the study of mathematics through the consideration of physical problems. Through examples Whewell opened up mathematical methods on a successively abstract level.[46] He introduced new physical principles as he went along to reach new levels of mathematical abstraction. What changed was both his opinion and use of the available physical principles bequeathed him by mathematicians and the actual mathematics he wanted his students to be exposed to. Therefore, he continually altered the content of the succession of textbooks that he wrote for Cambridge students. Simultaneously, he published papers against the calculus introduced after Lagrange and Laplace, particularly that of Fourier, and against continental mathematics in general. He also argued against the physical principles upon which these continental mathematicians had built their mathematics.[47] As Todhunter noted of his introductory text, the principles of statics could be more easily learned from "simple experimental lectures."[48] Whewell was not a physicist. He approached mechanics as a source for mathematics, and, increasingly from his developing philosophy of the sciences. The laws of mechanics must be both logically and empirically defensible, because they were the foundation of mixed mathematics.[49]

The mathematics these texts were designed to teach veered from the radical new analysis in 1819 to the conservative geometry and algebra by 1836. Even the later textbooks were accompanied by higher level mathematical ones that included

46 Some of Whewell's own research in mathematics have pedagogical goals, see Whewell, "Rotary Motion of Bodies," *Trans. Cambridge Phil. Soc.* 2 (1827): 11–20.

47 For Whewell's remarks on Fourier, see Whewell,"Report on the Progress and Present Condition of Electricity, Magnetism," 24–28. His work on the foundations of mechanics is in Whewell, "On the Principles of Dynamics as stated by French Writers," *Edinburgh J. Sci.* 8 (1827–28): 27–39, and, "On the Nature of the Truth of the Laws of Motion," *Trans. Cambridge Phil. Soc.* 5 (1834): 149–172. Whewell does not always differentiate the various meanings of the term force used by the authors under discussion. See also Whewell, *On the Free Motion of Points and on Universal Gravitation including the Principal Propositions of Books I and II of the Principia. First Part of a Treatise on Dynamics* (Cambridge: Deighton, 1833) for a later version of his mechanics.

48 Todhunter, *William Whewell* (New York: Johnson reprint of 1876 edition, 1970) p. 25. He also discusses the problems of Whewell's understanding of the physics.

49 The question of the empirical status of the various forms of mechanics in Newton, Leibniz, Euler and later French authors such as Laplace and Poisson can also be traced through the various editions of his texts. See especially Whewell, *An Elementary Treatise on Mechanics designed for the Use of Students in the University* (Cambridge; Deighton, 1819), second edition 1824, further editions appeared in 1828, 1833, 1836 and 1841. The last edition appeared in 1847. The physical content of each of these editions is detailed in Todhunter, *Whewell,* chap. 2. Whewell fiddled with the contents of his texts in other ways. In 1833 he separated Statics from the fourth edition of his mechanics as Whewell, *Analytical Statics* (Cambridge: Deighton, 1833) as a supplement to the fourth edition of his elementary text on mechanics.

integral and differential calculus. Yet Whewell never completed a smooth transition from physical example to increasingly difficult mathematics that culminated in the calculus. Because of his growing hostility to analysis, at best he managed only to graft the calculus intuitively on to specific problems.[50]

Whewell consistently narrowed the mathematics addressed to the majority of Cambridge students. He could thus refuse a place for the study of the new mathematical domains of light, electricity and magnetism. These subjects represented the locus of the new analysis. However, he could not control the choice of examiners for the university examinations. Nor could he stop college tutors or university professors pursuing French mathematics. Because the university examinations seemed beyond his grasp, the actions of Whewell and other conservatives to stave off the French mathematical menace only aggravated the schizoid situation at Cambridge–a situation that students understood by the 1820s.[51] The college curriculum was so inadequate that any student striving for honors needed a private tutor. There was an official and an unofficial teaching stream at Cambridge. The training required for students to excel in university examinations demanded "very extensive and singularly accurate knowledge, in a wide range of mathematical subjects, combined with perspicuity of thought and language in answering the questions proposed in the examinations." The teaching they received from their college was inadequate "to instruct and discipline the student so as to enable him to attain to that degree of excellence in these points to which he is capable of attaining."[52]

Two systems existed at Cambridge, the official curriculum, which if followed would lead to a pass degree. Distinction of any kind required students and tutors to acknowledge this and supply the necessary training. The teaching members of this underground system were integrated socially into university society. Their teaching function was clandestine and subversive. Private tutors coached students in the aspects of French analysis demanded by the last half of the Tripos examination. William Hopkins was preeminent among these tutors, training more senior wranglers than anyone else of his generation. Important here is the training he gave to three key Cambridge students, James Clerk Maxwell, George Gabriel Stokes and William Thomson. His lectures were grounded in Lagrangian mechanics and mathematics, elegant and an example of French mathematical physics. Rather out of date in terms of its mathematics by the 1850s but required for the Tripos.

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50 Harvey Becher, "William Whewell and Cambridge Mathematics," argues for the mathematical goals of Whewell's texts. However, the unity he sees in the physics and mathematics is in question.

51 Peter Allen, *The Cambridge Apostles,* chap. 1.

52 William Hopkins, "Remarks on the Mathematical Teaching of the University of Cambridge," Trinity College Library. These are notes for Hopkins presentation before the 1851 Parliamentary Commission. As a private tutor Hopkins needed to understand the problems of the system. His livelihood depended upon it and discerning how to bridge the gap.

Physics still generated mathematics.[53]

The same pattern can be seen in Hopkins research in geology. His geology was in line with the prevailing Cambridge method, of making mathematics from empirical evidence. Following the French and Cambridge goals, his geological mathematics was descriptive, not prescriptive of nature.[54] Geological hypotheses shaped the initial equations for his mathematical exploration of the dynamics of the earth's crust. His solutions to those equations were, like Fourier's on heat, whose mathematics he used, purely mathematical. Geological conclusions emerged from those mathematical conclusions. This perhaps is why his actual geological ideas seem so obscure.[55] Following the French model Hopkins performed experiments during the 1850s to support the validity of his mathematics. This was the approach he passed on to his students.

In the 1830s mathematicians at Cambridge developed a great interest in Fourier's work and the possibilities that it offered them as mathematicians. This mathematical culture extended beyond Cambridge to mathematicians in London, Edinburgh, and Dublin. Many were also aware of the work of German mathematicians. Interest in the philosophy of mathematics was also not confined to Whewell and Peacock. A mathematical culture nurtured in Cambridge included graduates, students, college tutors and university professors interested in the new analysis matured in the 1830s. Its ultimate expression became the *Cambridge Mathematical Journal*.

The journal was designed to meet the needs of "reading men" as well as tutors and graduates. The main focus of its first editor Duncan Gregory was the mathematical problems whose solutions were not encompassed by the mathematical methods of mechanics. The mathematics of heat and light introduced the problem of discontinuous functions and their representations, a problem intimately connected to the theory of definite integrals.[56] Over the next decade, the journal was a forum for French analysis. It evolved into an exclusively research publication for work of mathematicians such as Hamilton, George Boole, and Augustus de Morgan.[57]

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53 Hopkins never published a text. These remarks are derived from the lecture notes of Maxwell, Thomson and Stokes in the Cambridge University Library. It is interesting to note that all three preserved these notes.

54 Hopkins, "Researches in Physical Geology," *Trans. Cambridge Phil. Soc.* 6 (1835) [1838]: 1–84. Crosbie Smith, "William Hopkins and the Shaping of Dynamical Geology, 1830–1860," *Brit. J. Hist. Sci.* 22 (1989): 27–52, sees Hopkins work as geology, while not understanding that during the 1830s and 1840s Hopkins' goal was literally to make mathematics from the empirical foundations of geology. In this case the mathematics was the geometry of geology.

55 See Stephen G. Brush, "Nineteenth-Century Debates about the Inside of the Earth, Solid, Liquid or Gas," *Ann. Sci.* 36 (1979): 225–254.

56 Duncan Gregory, "Preface," *Cambridge Math. J.* 1 (1837–1839): 1–2.

57 In 1850 the journal became *Quarterly Journal of Mathematics*. In 1845 it had already

In its early years students were able to brush up on the physical foundations of Tripos questions through discussions of physical principles of mechanics and astronomy. In the first five volumes there were a string of articles on new ways of solving problems, useful methods, and reminders of important points sure to come up on examination questions. There were also articles amounting to short courses for students on aspects of the calculus and others reworking Tripos topics in ways that were clearer than those usually encountered in textbooks and lectures. The articles directed to students were usually short, and most of them were on strictly mathematical subjects. They ranged from subjects encountered in the first year to more esoteric methods, such as the solution to linear equations of finite and mixed differences to Jacobi's methods of solving partial differential equations. In fact the journal contained a great deal on the solutions to partial differential equations, analytical geometry, and so on.

Yet even in these volumes many authors were outside of Cambridge.[58] Many of the papers were on specific problems, many of which began in physics. In the early 1840s there was also a great deal of discussion of mathematical theories of light. The physical theory of Fresnel was discussed as well as his mathematical work. Archibald Smith noted that Fresnel usually used "mixed geometry" which was the best method of establishing theorems even though clumsy and tedious. He planned to establish the same theorems in the more general form of algebraic geometry. In a later paper Smith considered Fresnel's work on crystals, made reference to Hamilton's work, and included his mathematical deduction of conical refraction from the properties of the surfaces of the wave fronts. Physics led to mathematics, then returned to phenomena directly from the mathematics.[59]

Papers also addressed mathematical issues directly from French mathematical physics. The subjects incorporated the work of Joseph Liouville and Gabriel Lamé. And more and more articles represented research, done by mathematicians and written for an increasingly professional audience. Within five years the number of articles addressed to students fell, although William Thomson as editor from 1845 to 1850, hoped to revive them. As if to emphasize the journal's commitment to the new calculus, Gregory wrote the first article on Fourier analysis. It was an attempt to rewrite Fourier's results in functional form because Fourier's proofs were unsatisfactory. The improvement of Fourier's proofs was the subject of Thomson's first paper in 1841.[60] Gregory returned to Fourier in his attempts to develop a differential

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expanded into the *Cambridge and Dublin Mathematical Journal* signalling that its audience was mathematicians. The needs of students were becoming less and less the concern of the editors.

58 These include Arthur Cayley, George Boole and others.

59 Archibald Smith, "The Wave Theory of Light," *Cambridge J. Math.* 1 (1839): 3–10, and, Smith, "Notes on the Undulatory Theory of Light," 84–95.

60 Gregory, "Notes on Fourier's Heat," *Cambridge J. Math.* 1 (1837–1839): 104–107.

operator calculus. This was an extension of Peacock's approach to algebra which separated the "symbols of operation from those of quantity." Gregory pushed this method beyond Fourier whom he described as having "had some unwillingness to give himself up to it entirely." In this first volume, the particular concerns of British mathematicians were already shaping how French mathematics was explored and exploited.[61]

These particular concerns loomed larger and larger on the journal's pages as its editor, Thomson, lamented to Stokes. The majority of papers with supposedly physical titles on light, heat, electricity, and occasionally magnetism turn out to be on mathematical points that arise within those subjects. Even as Thomson took over as editor in 1845, the balance of the contents of the journal did not alter significantly.[62] Increasingly the pages reflected the research interests of the leading mathematicians of Britain, especially Cayley, Boole, and Hamilton.

William Thomson

The papers William Thomson published as a student at Cambridge and as editor of the *Cambridge Journal of Mathematics* fit into contemporary mathematical practices. Given the standards of the time, they cannot be considered as physics. When Thomson moved to Cambridge as a student in 1841, he was already well versed in French mathematics. His papers on Fourier fit the French mathematical model. In his first papers Thomson discussed sine series separately from cosine series, a subject neglected by Fourier. In the course of his discussion, Thomson defended Fourier and pointed out mathematical errors in Kelland's *Theory of Heat.* The paper had a purely mathematical point.[63] The connections Thomson saw between the motion of heat and electrostatics lay in their shared mathematical forms. He thus extended to heat the analogies that others had previously drawn between electrostatics and gravitation. This is accepted by all historians. He did not see this as a path to a physical analogy. Thomson used those same mathematical forms to

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William Thomson, "On Fourier's Expansion of Functions in trigonometric Series," same journal 2 (1939–1841): 258–262. Thomson's second paper was also an exercise in Fourier analysis. Thomson, "Notes on a Passage in Fourier's Heat," same journal 3 (1841): 25–27.

61 Gregory, "On the Solutions to partial Differential Equations," *Cambridge J. Math.* 1 (1837–1839): 123–131. See also Gregory, "On the Integration of simultaneous differential Equations," *Cambridge J. Math.* 1 (1837–39): 173–181, and "On the real Nature of Symbolical Algebra,"*Trans. R. Soc. Edinburgh* (1838) [1840]: 208–216. There were many articles in the following decade on differential operators. Grattan-Guinness refers to it as a "fad." Grattan–Guinness, "Mathematics and Mathematical Physics from Cambridge, 1815–40," in *Wranglers and Physicists,* Harman, ed. 84.

62 The early editors of the journal were Gregory then Robert Leslie Ellis, William Thomson to 1850, and Thomson and William Ferrers to 1855 and then Ferrers.

63 Thomson, "On Fourier's Expansion of Functions in Trigonometric Series."

mediate solutions to some mathematically tricky problems in electrostatics, heat, and gravitation. By inverting the process, Thomson converted theorems in the mathematics of the attraction of ellipsoids into mathematical statements about the flow of heat. He then replaced a series of sources of heat, electric charge, or gravitational attraction by an "isothermal surface." He also noted that by replacing the particular constants in his basic equations, the problems of heat, electrostatics, and gravitation were reduced to the solution of the same mathematical equation–hardly the statement of a physicist for whom the differences between these forces of nature were constitutive of a study of them.[64]

Thomson's examination of isothermal surfaces followed that of the French mathematicians J. M. C. Duhamel and Liouville. "Isothermal" referred to a surface with certain mathematical not physical properties. He finished this series of papers by using the mathematics of heat to develop ideas about particular kinds of orthogonal surfaces. He defined a point using curvilinear coordinates and then traced what kinds of surfaces were generated by the equations of motion for heat.[65] Aspects of these papers are confusing to a reader unfamiliar with this method of referring to and constructing mathematics. Thomson moved back and forth between the mathematics identified by the physical problems from which they emerged. The physical names labeled a type of mathematics that did not refer to anything else. To demonstrate the rightness of his mathematical approach, Thomson confirmed a proposition in Gauss' work on attraction by replacing material points by his surface. He then examined the properties of these functions and surfaces. He finished in standard mathematical fashion with an experimental result. The absence of charge within a hollow conductor was a result that "is confirmed" by mathematics.[66] Physics generated mathematics. Thomson was moving towards a generalized mathematical method of treating these disparate physical cases through one mathematical approach opened up by Fourier.

Historians usually cast Thomson as a physicist, building a new physics based in the mathematics of "geometric," that is, macroscopic entities. This approach is traced back to the unique philosophical framework of Common Sense philosophy

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64 Thomson, "On the uniform Motion of Heat in Homogeneous Solid Bodies and its Connection with the Mathematical Theory of Electricity," *Cambridge J. Math.* 3 (1841–1843): 71–84. Gravitation is introduced on p. 83. Helmholtz, "Sir William Thomson's *Mathematical and Physical Papers*," *Nature* 32 (1885): 25–27 also noted the mathematical character of Thomson's early work. See also Cross, "Integral Theorems," p. 35.

65 Thomson, "On the Equilibrium of the Motion of Heat referred to Curvilinear Coordinates," *Cambridge J. Math.* 4 (1843–1845): 33–42, and "On the Lines of Curvature of Surfaces of the second Order," same journal 4 (1843): 279–286.

66 Thomson, "Demonstration of a Fundamental Proposition in the Mechanical Theory of Electricity," *Cambridge J. Math.* 4 (1843–1845): 223–226. He was referring to Gauss, "Allgemeine Lehrsätze," in which Gauss stated that gravitation, electrostatics and magnetism are "special cases of the particular mathematical solutions being sought." p. 241.

infiltrating the natural philosophy courses at all Scottish universities that set them apart from their English counterparts. Most historians of physics do not worry about what constituted mathematics in the middle of the nineteenth century, nor the common elements shared by Scottish and English scientists through that mathematics. If we do consider mathematics, then the disciplinary boundaries derived from twentieth century practice dissolve and reform into a different geography. The inheritance of historians of physics included categories created in the middle of the nineteenth century partly by the rewriting of the history of mechanics by, among others, William Thomson. The early disciplinary geography and the changes in boundaries and relationships between physics and mathematics became invisible.[67]

At Glasgow University, even if mathematical and experimental natural philosophy were taught by the same professor, they were kept distinct. The first was mathematics, the second the phenomena and vernacular explanations of those phenomena. Experimental results were the starting points for the mathematics developed in the mathematical physics course. The evidence of textbooks indicates that the courses lived alongside, yet largely unconnected, to each other.[68] Meikleham's course in natural philosophy was phenomenologically grounded with no mathematical development of the principles drawn from experiment.

Thomson was, therefore, familiar with the frame of reference of Cambridge teaching. He was also trained in mathematics by using physical problems to generate mathematical conclusions and even new branches of analysis. He quickly became a Cambridge mathematician in the radical camp of mathematical research at the university. Continuing his work on the mathematics of electrostatics, Thomson argued that both Coulomb's and Faraday's results were true. What was needed was a mathematics that brought them together. In at least three places he pointed out that his was a mathematical theory, "independent of physical hypotheses."

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67 Crosbie Smith and Norton Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989), have done precisely this. They also treat Thomson as a physicist not as someone who helped to create this new discipline. While Norton Wise has argued that Thomson was led to energy conservation through mathematics he treats all the methods that Thomson used as aspects of physics, thus reading twentieth-century givens back into the 1840s. See Wise, "William Thomson's mathematical Route to Energy Conservation: A Case Study in the Role of Mathematics in Concept Formation," *Hist. Stud. Phys. Sci.* 10 (1979): 49–83, and Harold Sharlin, "William Thomson's Dynamical Theory: An Insight into a Scientist's Thinking," *Ann. Sci.* 32 (1975): 133–147.

68 This is reinforced in Thomson's case as his father was professor of mathematics at Glasgow and wrote textbooks for his own courses covering some of the new analysis. See Smith and Wise, *Energy and Empire,* chap. 1, where they claim that the elder Thomson in his texts minimized abstraction. This is an indication that he followed the mathematical path of the French closely in outline, physical problems leading to mathematical excursions, if not in the details of techniques.

> Fourier's laws for the motion of heat constitute a mathematical theory, properly so-called; and when we find the corresponding laws to be true for phenomena presented by electrical bodies we may make them the foundation of the mathematical theory of electricity: and this may be done if we consider them merely as actual truths, without adopting any physical hypothesis.[69]

He accomplished his mathematical goal using a combination of the mathematical analogies he had already drawn from Green and Gauss in his papers on heat and electrostatics.

This paper was written in Paris during the pivotal period of 1845. Thomson spent eight to ten hours a day in Regnault's laboratory, then rushed over to Liouville's for mathematical company. The dual nature of his existence, and the separation of physics and mathematics were symbolized by the geography of his life. Thomson began to break down this separation in the next five years, after his appointment as professor of natural philosophy at Glasgow university. At Glasgow he had to teach students destined to become engineers, not liberally educated gentlemen. He also had to show faculty, administration, and town that he was not merely a clever Cambridge mathematician.[70] He turned to experiment, developed a taste for it, and enlisted the voluntary labor of his students.[71]

In mathematical physics there was no place for hypotheses or models that would interfere with the mathematical generality of solutions. After completion of the mathematics, particular solutions were compared with experiment. Very rarely did mathematics lead to prediction. Experiment and mathematics interacted only at beginning and the end of the mathematical process. To develop any kind of theoretical physics Thomson had to transcend these limitations. He had to accept that hypotheses could be developed into theories that subordinated mathematics to the needs of physical imagery. Then, mathematical physics and experimental physics provided him with a dilemma that could not be resolved through mathematics alone.

In the months that Thomson spent in Regnault's laboratory, he was introduced to Clapeyron's mathematical investigation of Carnot's physical theory of heat. He extended and generalized Clapeyron's mathematics, and directly from this mathematics drew physical implications about the measurement of temperature.

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69 Thomson, "On the Mathematical Theory of Electricity in Equilibrium," *Cambridge Dublin Math. J.* 1 (1845): 75–95, 86. This paper was a translation, with additions of the paper Thomson had published in Liouville's journal earlier. See Thomson, "Note sur les lois élémentaires d'électricité statique," *Liouville J.* 10 (1845): 209–221.

70 Thomson initially taught experimental physics and mathematical physics as separate courses.

71 This did not constitute systematic laboratory training for experimental physics or engineering students. Students were thrown directly into his research in both physics and engineering.

From this he created the absolute scale of temperature.[72] This was reinforced by his brother James' equally physical deduction from mathematics—that the freezing point of a substance decreased with an increase in pressure. William Thomson devised and performed the experiments that confirmed the mathematics.[73]

However, James Prescott Joule's experiments indicated the conversion of heat into work and work into heat. These experiments posed a direct challenge to the principle of the conservation of heat upon which Thomson and Clapyeron based their mathematics.[74] Already doubting Carnot's physical theory of heat, Thomson nevertheless published his mathematical generalization of Clapyeron's work.[75]

Thomson then refereed William J. C. Macquorne Rankine's paper on the mechanical theory of heat. In Rankine's work, the idea of heat as work was given explicit mathematical expression as the *vis viva* of the motion of the particles of bodies. Rankine then assumed that the forces between the particles of bodies were some function of the density of the body. With this assumption, Rankine deduced Joule's relationship between heat and work, and other known gas laws. Rankine limited his mathematics to physically plausible cases, ignoring the mathematically generalized ones. Yet he deduced his physical results from his mathematics without

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72 Heat and its measurement, along with temperature, were important in marine engineering.

73 William Thomson, "On an Absolute Scale of Temperature founded upon Carnot's Theory of the Motive Power of Heat, and Calculated from the Results of Regnault's Experiments on the Pressure and Latent Heat of Steam," *Phil. Mag.* 33 (1848): 313–317. James Thomson, "Theoretical Considerations on the Effects of Pressure in Lowering the Freezing Point of Water," *Trans. R. Soc. Edinburgh* 16 (1849): 575–580, *Cambrdge Dublin Math. J.* 5 (1850): 248–255. The experiments were reported in William Thomson, "On the Effect of Pressure on the Freezing Point of Water, experimentally demonstrated," *Proc. R. Soc. Edinburgh* 2 (1850): 267–271, *Phil. Mag.* 37 (1850): 123–127. Neither the absolute scale of temperature, nor the lowering of the freezing point, depended on any assumptions about the nature of heat itself. It can be deduced directly from the form of the equations, giving the symbols their initial physical meanings. See Clifford Truesdell, *The Tragicomical History of Thermodynamics.*

74 In 1847 Joule read an account to the British Association of improved experiments that he was convinced demonstrated the conversion of mechanical work into heat and suggested the reverse transformation should also take place. See James Prescott Joule, "On the Existence of an equivalent Relation between Heat and Ordinary Forms of Mechanical Power," *Rep. British Assoc.* (1847): 55, in full in *Phil. Mag.* 31 (1847): 173. For accounts of Joule's experiments and the origins of his work in heat, see John Steffens, *James Prescott Joule* (New York: Science History Pub., 1979), D. S. L. Cardwell, *James Joule: A Biography* (Manchester: Manchester University Press, 1989), and William Cropper, "James Joule's Work in Electrochemistry and the Emergence of the First law of Thermodynamics," *Studies Hist. Phil. Sci.* 19 (1988): 1–15.

75 Thomson, "An Account of Carnot's Theory of the Motive Power of Heat with Numerical Results deduced from Regnault's Experiments on Steam," *Proc. R. Soc. Edinburgh* 24 (1849): 198–204 and in full in *Trans. R. Soc. Edinburgh* 16 (1849): 541–574.

reference to his molecular hypothesis.[76] Rankine went further in generalizing his physical notions, giving them mathematical expression by introducing the concept of energy.[77] Thomson was convinced even before this later step, that Rankine had demonstrated a mathematical alternative to caloric theory that reproduced known experimental results. More significantly, Rankine had changed the relationship between physical imagery and mathematics. He only pursued the mathematics necessary for the production of physically meaningful results, that is, results that could be compared directly with experiment.

Whatever philosophical struggles Thomson had over accepting the new ideas on heat, he reworked his earlier paper on caloric theory. He altered only those results that required mathematical change in light of Joule's principle which he now accepted as the foundation for the mathematical theory of heat. In this series of papers Thomson expressed his physical understanding of what the changes in mathematics meant for the behavior of heat. However, his physical remarks were in terms of the general principles not in terms of any one mechanical model of heat.[78] As he published this series of papers in which physical interpretation was integrated into mathematical development, Thomson was embroiled in a dispute with Rudolph Clausius on the cause of the decrease in temperature of a vapor rushing through an orifice. Clausius opted for a thermal, Thomson a mechanical explanation.[79]

Thomson realized that this was an "un-reversible" phenomenon. Irreversibility was physically as well as mathematically real.[80] From this date for Thomson, neg-

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76 William J. C. Macquorne Rankine, "On the Hypothesis of Molecular Vortices, or Centrifugal Theory of Elasticity, and its Connexion with the Theory of Heat," and "On the Mechanical Action of Heat especially in Gases and Vapors," *Trans. R. Soc. Edinburgh* 20 (1850–1851) [1853]: 87–120, 147–190, 565–589, and *Phil. Mag.* 7 (1854): 1–21, 111–122, 172–185, 239–254.

77 For Rankine's thermodynamics and its significance see, Keith Hutchinson, "Der Ursprung der Entropiefunktion bei Rankine und Clausius," *Ann. Sci.* 30 (1973): 341–364; "W. J. M. Rankine and the Rise of Thermodynamics," *Brit. J. Hist. Sci.* 26 (1981): 1–26. Hutchinson notes the mathematical character of Rankine's work yet judges it only in terms of its physical conceptual clarity.

78 Thomson, "On the Dynamical Theory of Heat with Numerical Results deduced from Mr Joule's "Equivalent of a Thermal Unit," and M. Regnault's "Observations on Steam"," *Trans. R. Soc. Edinburgh* 20 (1851) [1853]: 261–288.

79 Clausius, "Über der bewegende Kraft der Wärme und die Gesetze die sich daraus für die Wärmetheorie selbst ableiten lassen," *Ann. Phy.* 79 (1850): 368–397, 500–524, translated into *Phil. Mag.* 2 (1851): 1–21, 102–119, 16–17. Thomson's challenge was in Thomson, "On a Remarkable Property of Steam connected with the Theory of the Steam Engine," *Phil. Mag.* 37 (1850): 386–389. The argument continued in later issues in this and the following year of both the *Philosophical Magazine* and *Annalen der Physik.*

80 The suddenness of this idea and the importance Thomson attached to it is revealed by its insertion in the string of papers on modifying his mathematical development of Carnot's

ative time values were meaningless. Mathematical expressions for a system at zero time now carried cosmic as well as physical significance.[81] Thomson stated his new found physical understanding of irreversibility and changed his interpretation of his own earlier work on Fourier, "when heat is diffused by conduction there is a *dissipation* of mechanical energy, and full *restoration* of it to its primitive condition is impossible." From this proposition Thomson deduced the mechanical work extractable from an unequally heated body by equalizing the temperature of that body. This was the mechanical equivalent of the heat "put out of existence." Although he never published a mathematical derivation of this result, Thomson understood that in every cycle of a heat engine, some energy becomes "unavailable" for work and was cast out as heat. This loss was inevitable and irreversible. Thomson followed this statement with an integral expression for the final temperature in the cycle of a heat engine coupled with his conclusion of the heat death of the universe. For detailed mathematical arguments for the cosmical consequences of irreversibility Thomson relied on Fourier and the cooling of the earth as the concrete physical example.[82]

Within a short period of time Thomson's research had changed radically. His experiences with heat convinced him of the necessity for theories of physical processes. Within those mathematical theories the mathematics was limited by requirements of the mechanical model, under the guidance of the principle of conservation of energy. Like many of his contemporaries, Thomson explored the mathematical implications of the principle of conservation of energy, but in his case there was now a real integration of mechanical principles, with mathematics confined to physically plausible outcomes along with the integration of experimental work to exemplify this tight connection.[83]

Thomson became bold in predicting the physical meanings encoded in mathematical language that went beyond known experimental results. His early daring is best exemplified by his work on the Atlantic Telegraph and his explanation of signal

theory. See Thomson, "On the Universal Tendency in Nature to the Dissipation of Mechanical Energy," *Phil. Mag.* 4 (1852): 304–306, and *Proc. R. Soc. Edinburgh* 3 (1852) [1857]: 139–142.

81 In the papers he published on Fourier analysis in the 1840s Thomson dealt with negative time as a mathematical quantity. It had no physical significance. See Thomson, "Note on Some Points in the Theory of Heat," *Cambridge J. Math.* 4 (1843–1845): 67–72.

82 See Joe Burchfield, *Lord Kelvin and the Age of the Earth* (New York: Science History Publications, 1975). See also Smith and Wise, *Energy and Empire*, chaps. 4, 5, 16.

83 See Thomson's papers on thermoelectricity. He enlisted his students in the experimental half of the labor. Thomson first mentions thermo-electric phenomena in Thomson, "On the Dynamical Theory of Heat," (1851): 15–16. Other papers followed, see, Thomson, "Account of Researches in thermo-electricity," *Proc. R. Soc. London* 7 (1854–1855): 49–58; "On the Dynamical Theory of Heat. Part VI Thermoelectric Currents," *Trans. R. Soc. Edinburgh* (1854): 123–172, *Phil. Mag.* 11 (1856: 214–225, 281–297, 379–388, 433–446.

attenuation. His physical grasp of the problem was encompassed in that explanation, and in his recommendations for countermeasures.[84] His physical theories became dependent on ever more intricate mechanical molecular models. They were specific, mechanical systems that on the micro-level were miniature systems that obeyed the same mechanical laws as macro-phenomena. Directly from the mechanical properties and behavior of these molecules, Thomson shaped the functioning and operation of the macroscopic physical system. The most graphic of his later physical approach to phenomena were in his models of the ether. Mechanical models became Thomson's way of visualizing the operations of nature. If he could not make a mechanical model of a theory, he could not understand it. His mechanical models were models whose motion could be grasped visually as well as expressed mathematically. Whether these models were compatible with one another to make a coherent picture was a lesser problem. Thomson invented them to solve one physical problem at a time.[85]

Thomson's approach to the solutions of physical problems was encapsulated in his textbook on mechanics authored with Peter Guthrie Tait. This text was not just a reworking of mechanics using energy conservation as its conceptual foundation. It redefined the subject pedagogically. Force, not a concept either Thomson or Tait used in their research, physically tied the fields of mechanics together. This replaced the analysis that mathematicians used to hold mechanics together. Mathematicians moved from statics to dynamics through virtual displacement.[86] Here the text began in kinematics. Through the balance of forces, statics became a special case of dynamics. Dynamics supplied the explanations for the motions of bodies described kinetically. Thomson and Tait devoted a lot of space to statics

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84 For details on Thomson's work on the Atlantic telegraph, see Smith and Wise, *Energy and Empire*, chap. 19, 661–684. Usually seen as evidence of the marriage of research and engineering the experiments done by Thomson and his students on the electrical and chemical properties of the copper in the cables are another indication of his commitment to the experimental investigation of nature.

85 See Kelvin (Thomson), *Baltimore Lectures and Modern Theoretical Physics: Historical and Philosophical Perspectives,* Robert Kargon and Peter Achinstein, eds. (Cambridge MA.: MIT Press, 1987). This is a reprint of the notes of the lectures as they were given, rather than the longer version published later, along with historical and philosophical essays. The models that appear in almost every lecture are made up of various combinations of spring systems or vortices. See, 9–10, 48–52, 77–81, 82–94, 108–111, 120–124, 125–128, 135–144, 145–150, 152–157. The focus of the lectures was on the difficulties besetting the mechanical, molecular theories of dispersion, refraction, and fluorescence. For the power and limitation of Thomson's models see Smith and Wise, *Energy and Empire,* chaps. 12, 13. For Thomson on vortex atoms see Robert Silliman, "William Thomson: Smoke Rings and Nineteenth-Century Atomism," *Isis,* 54 (1963): 461–474. For mechanical atomic models and their limitations see, Garber, "Molecular Science in Late Nineteenth-Century Britain," *Hist. Stud. Phys. Sci.* 9 (1978): 265–297.

86 The other approach was to separate the two subjects, motion and statics altogether.

and elasticity, both important subjects for their engineering students. Elasticity was also becoming important in Thomson's own research into the ether and its interactions with matter.

Thomson and Tait also rewrote the history of mechanics.[87] Theorems were taken from Euler, Green, Gauss, Legendre, and others in mathematical isolation. The only criteria applied was the theorem's relevance to the solution of the physical problem at hand. The theorems were stated mathematically, then explained physically and applied to solve specific mechanical problems. Many problems sprang from Tripos questions. Yet they were exercises in statics, kinematic, and dynamics *not* mathematics. Physical explanations of the results of the mathematics followed during and at the end of the mathematical solutions. Mechanics became a subject of physics, not a launch pad into mathematics. The authors developed the mathematics they needed to solve physical problems. Physics remained the focus of their attention.[88] And they conceptually refocussed mechanics by interpreting many results in terms of potential and kinetic energy.[89]

Thomson and Tait had annexed mechanics for physics and it became a source for colleagues as well as students. Their approach meant that they could reinterpret the mathematics of Euler, Green, Gauss, Legendre, and others in terms of the physics implicit in their mathematics. Therefore, "Euler discovered that the kinetic energy acquired from rest by a rigid body in virtue of a impulse fulfills a maximum-minimum condition."[90] By attributing to Euler a concept only recently developed, the foundation of their physics was given a respectable ancestry although it falsified Euler's understanding of the problem. They also contended that Lagrange extended this to a connected system of bodies struck with any impulse.[91] They forced the past of mechanics, which had been part of mathematics, into their version of what it must have really been physically. They made the past of mechanics over into physics.[92] All of this is understandable given the audacious

87 Smith and Wise, *Energy and Empire*, chap. 11, recount the writing of the text and its historical importance in terms of the new conceptual foundation of energy conservation. However, they also accept much of Thomson and Tait's interpretation of the work of their predecessors without comment or investigating the reasons for their interpretations.

88 This explains, in part, the popularity of the text and its rapid translation into German.

89 The clearest examples of this are in their treatment of Green's potential. See Thomson and Tait, *Principles,* vol. 2, article 482, 28–29. This physical interpretation is in marked contrast to Thomson's mathematical uses of the potential in the 1840s. See Thomas Archibald, "Physics as a Constraint on Mathematical Research,"

90 Thomson and Tait, *Principles,* vol. 1, article 311, 285.

91 Thomson and Tait, *Principles*, part 2, article 37.

92 Their reworking of the history of mechanics and its reshaping into the concept of energy was not always successful. Horace Lamb and George Darwin made additions to later editions and noted that Thomson and Tait's "attempt to deduce the principle of virtual velocities from the equation of energy alone can hardly be regarded as satisfactory."

character of their text, and their need to establish credentials that would give to them a respectable pedigree. They were successful and erased the mathematical context of the development of mechanics.

While Thomson and Tait accepted the use of hypotheses within natural philosophy, they never addressed this issue formally, although they discussed the grounds for believing in the hypotheses they did use. That ground was experimental evidence. Mathematical theories of planetary motion were well grounded, those of geometric optics were carried "far beyond the limits of experiment." The hypothesis that heat was a form of energy came from experiment although many formulae were still "obscure and uninterpretable," as the mechanics of the motions of the particles of matter were unknown. Only mathematical analysis existed in those physical fields of the lowest tier of this hierarchy of surety, electricity, magnetism, heat and light.

The contingencies of building careers in the new professions opened up by the sciences led Thomson and Tait into investigations of nature and away from their common starting point in Cambridge mathematics. Yet that transition was never quite complete. For Thomson, mechanical models were means of solving individual problems. The consistency with which he used mathematical methods was absent from his mechanical models. The details of the latter depend entirely on the specific problem at hand and could change radically even when dealing with the same physical body, such as the ether. Models were heuristic and necessary for Thomson to grasp the mathematics and its physical meaning. Consistent with this were the ways in which Thomson taught. Experimental and mathematical physics were separate courses. He limited use of his mechanics text to the mathematical physics course for honors students for the MA.[93] Comprehensive physical theories expressed in the language of mathematics did not emerge from his work, although his research contained streams of ingenious solutions to particular problems based on the mathematical analysis of mechanical models. Tait was even less interested in physical, rather than mathematical, consistency. His quarrel with Josiah Willard Gibbs in the 1880s over vectors revolved around Gibbs' desecration of the mathematical integrity of quarternions. The issue of the usefulness of vectors for physics did not enter into his argument.

Thomson and Tait published a model on how to teach this new discipline; others had already done this in their teaching. Although he left no textual monument with which to bedevil historians, the most important of these men was George Gabriel Stokes. Stokes was quite clear on where mathematical argument was appropriate

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Thomson and Tait, *Principles*, vol. 1, 266, footnote.

93 See David B. Wilson, *Kelvin and Stokes: A Comparative Study in Victorian Physics* (Bristol: Adam Hilger, 1987), chap. 3. For the division of his thought into the mathematical then the experimental see Thomson, "Elasticity," *Encyclopedia Brittanica,* ninth edition, vol. 3, 1–112, the experiments are described, 1–84, mathematical theory, 84–112.

and where physical hypotheses began and the extent of their legitimacy. And it is with Stokes, we see the first conscious and consistent separation of mathematical issues on the one hand and the needs of physical theory on the other.

George Gabriel Stokes

William Thomson was not alone in recognizing and transcending the limitations of Cambridge mathematics in the exploration of nature. George Gabriel Stokes had begun earlier, yet his work followed a different line of development from Thomson's. While committed to a mechanical view of nature, Stokes was a good deal more discerning in its use. Like Thomson, Stokes was trained in the Cambridge mathematical tradition and was able to extend that mathematics through the consideration of physical problems. However, from the beginning of his career Stokes's research papers were of three types. He worked within Cambridge mathematical tradition by using physical problems to extend other mathematicians' work. His first papers on hydrodynamics were improvements upon the theorems of Cauchy and Poisson and the mathematics of Laplace. What he was after were better solutions to certain partial differential equations.[94]

Secondly, Stokes clarified and examined key issues about the physical hypotheses being actively pursued by his contemporaries. At the time he graduated as first Wrangler and Smith's Prizeman, Stokes was performing experiments and speculating about the nature of the ether. His early physical papers on the ether contain a minimum of mathematics. Stokes claimed results without going through the analytical details and speculated on whether the ether was at rest, or was dragged along with the earth. His focus was on the physical implications of Fresnel's and later authors' mathematical work. Stokes concluded that the laws of reflection and refraction were unaffected by any motion of the ether and there were no experimental tests available to choose between the two hypotheses.[95] Published separately from his mathematical work Stokes dealt here with a subject entirely within natural philosophy. The interaction of the ether and matter was speculative and hypothetical.[96]

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94 See Stokes, "Steady Motion of Incompressible Fluids," *Trans. Cambridge Phil. Soc.* 7 (1842): 439–454, 465; "Some Cases of Fluid Motion," same journal 8 (1849): 105–137, 409–414, abstract in *Phil. Mag.* 31 (1847): 136–137, and, "On the Theories of the Internal Friction of Fluids in Motion, and of the Equation and Motion of Elastic Solids," *Trans. Cambridge Phil. Soc.* 8 (1849): 287–319, abstract in *Phil. Mag.* 29 (1846): 60–62. Stokes' mathematical work is discussed in Cross, "Integral Theorems," 136–137, 144–145.

95 Stokes, "On the Aberration of Light," *Phil. Mag.* 27 (1845): 9–15, and, "On Fresnel's Theory of the Aberration of Light," same journal (1846): 76–81.

96 Stokes, "On the Constitution of the Luminiferous Ether, viewed with reference to the Phenomenon of the Aberration of Light," *Phil. Mag.* 29 (1846): 6–10.

Thirdly, Stokes separated those of his mathematical papers deliberately written to explore the physical implications of the mathematical analysis that emerged from a branch of physics. Physics led to mathematics and then back to physics. In these papers physical hypotheses were the means to establish basic equations. These hypotheses were kept deliberately on a general level, relating for example, to the general behavior of fluids, rather than descending to the particulars of the internal structure of fluids to generate the equations of motion. He began in the equations of motion for a homogeneous, compressible fluid and the equation of continuity. These were restricted by noting that small oscillations meant that he could omit terms denoting the compressible character of the fluid. Sometimes specific physical problems led him to extend theories of fluids. In the case of his work on pendula, it was the failure of mathematical theories to meet the results of Sabine's carefully performed experiments. These experiments led Stokes to believe that internal friction operated in fluids and the equations of motion of fluids needed to be generalized to take this into account.

Stokes restricted the general equations of motion to cases that modeled particular physical circumstances whose solution would allow him later to compare his numerical results directly with a series of experiments.[97] Mathematicians might have delighted in seeking the mathematical consequences of heterogeneous fluid flow but the case of a tangential stress being developed in the fluid mirrored the experimental cases Stokes wanted to address. This mathematical case was therefore the one he focussed upon. Also the course of his mathematics was further restricted to expressions from which he extracted physical consequences. The point of this paper was to extract physical information and experimental consequences. No molecular models, no explanation of how friction within a fluid might arise. Stokes described what happened and gave mathematical expression to the fluid's behavior.

He began with the general equations of motion of a fluid with internal friction,

$$\frac{dp}{dx} = \rho\left(x - \frac{\partial u}{\partial t} - u\frac{\partial u}{\partial x} - v\frac{\partial u}{\partial y} - w\frac{\partial u}{\partial z}\right) + \mu\left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2}\right)$$
$$+ \frac{\mu}{3}\frac{d}{dx}\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right),$$

with similar equations for dp/dy and dp/dz, where u, v, w were the components of the velocity of the fluid along the x, y, z axes, p the pressure and t the time, ρ the density of the fluid and μ "a certain constant dependent on the nature of the fluid." Stokes then confined himself to a series of special cases of these equations that were dictated by physical conditions. The motions of the fluid were small so that terms in the squares of the velocities could be neglected and the density could

97 The experiments he referred to were those performed by Bessel in the 1820s and by Sabine and others on the seconds pendulum.

be treated as a constant. He also took the force X, Y, Z as a component of gravity. The monster equation above was reduced to,

$$\frac{dp}{dx} = \mu\left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2}\right) - \rho\frac{\partial u}{\partial t},$$

and the equation of continuity, $\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0$. Working systematically through the mathematics, Stokes eliminated cases by imposing physical conditions until he could consider pendulums that were cylinders and spheres performing small oscillations in spaces restricted by other cylinders and spheres. These conditions meant that the number of arbitrary constants introduced into his solutions was restricted to one. Stokes called this the "index of friction" of the fluid which could be determined by experiment. Taking a qualitative demonstration of its existence, he developed a theory based on the general mathematized properties of fluids, expanded to include this new phenomena, then reduced the mathematics to a state of direct comparison with several different experiments.[98]

In his report to the British Association on hydrodynamics, Stokes interpreted the results of all mathematicians physically. He only dealt at length with those results that had physical content. He mentioned Ostrogradsky's paper in passing, although it was on the motion of a fluid in a cylindrical basin. However, "the interest of the memoir, however, depends almost exclusively on the mathematical processes employed, for the result is very complicated, and has not been discussed by the author."[99] In another case Stokes suggested that a mathematical investigation was characterized as "one of great complexity and very little interest," that is, of physical interest.[100] Here Stokes also gives explicit physical meaning to the terms in the mathematics of fluids.[101]

Quite explicitly Stokes separated his physical and mathematical understanding of the same piece of work.[102] In his discussion of Fourier series, he was at pains to show the mathematical advantages of Fourier analysis over functional solutions to the same partial differential equations. Here the point was mathematical, to extend

98 Stokes, "On the Effect of the Internal friction of Fluids on the Motion of Pendulums," *Trans. Cambridge Phil. Soc.* (1851): 8–106.

99 Stokes, "Report of Recent Researches in Hydrodynamics," *Rep. British Assoc.* Part I (1846): 1–20, reprinted in Stokes, *Mathematical and Physical Papers* (Cambridge: Cambridge University Press, 1880) vol. 1, 157–187, 162.

100 Stokes, "Report," 168.

101 See Stokes, "Report," 183–184 where he explains physically what terms St. Venant used to describe the motion of fluids where the pressure was not equal in all directions, and the physical results of these suppositions.

102 In the case of fluorescence Stokes could not complete the transition from experiment to mathematics and physics. The physical foundation of his mathematics were insufficient to analyze even his own experiments.

Fourier series to cases beyond their usual range. Temperatures become functions of coordinates, and the results of these mathematical explorations were not referred back to measurable temperatures or other physical conditions. He left the results in terms of functions and arbitrary constants. The results illustrated mathematical methods, but did not elucidate any physics.[103]

Stokes was quite conscious of his separation and treatment of mathematics from physics. He was also clearheaded about his use of physics to generate mathematics. He put it to Cayley that

> Thomson and I are at present writing to each other about potentials. I think that potentials may throw light on the interpretation of $f(x + \sqrt{-1}y)$. How horrible you would think it to prove, even in one's own mind, a proposition in pure mathematics by means of physics.[104]

Whether Cayley was horrified or not, his report on dynamics of 1857 served to show the distance between mathematicians and physicists in the middle decades of the nineteenth century. Cayley's report traced "the investigations of geometers in relation to the subject of analytical dynamics." He recounted the successive development of mathematical methods. In conclusion Cayley reminded his audience that the differential equations of dynamics "are only one of the classes of differential equation which have occupied geometers." He then noted the work of Jacobi and Pfaff in the theory of the solution of partial differential equations. Mathematicians could still claim mechanics.[105]

Stokes' ability to differentiate mathematical nicety from physical meanings was put to the test when he was appointed as Lucasian professor of mathematics at Cambridge. In this capacity he took over the lecture demonstrations on hydrostatics and optics from Challis. As one student later put it, before Stokes,

> we had to get up natural philosophy by a painful exercise of the imagination on diagrams and descriptions, and the abstractions formulated by mathematicians to make calculations possible which presented Nature as a lifeless statue.[106]

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103 Stokes, "Critical Values of the Sums of Periodic Series," *Trans. Cambridge Phil. Soc.* 8 (1849): 533–583, abstract in *Phil. Mag.* 33 (1848): 309–311.

104 Stokes to Cayley, 29th Oct., 1849 in David B. Wilson, *The Correspondence between Sir George Gabriel Stokes and Sir William Thomson, Baron Kelvin of Largs* (Cambridge: Cambridge University Press, 1990), 2 vols. vol. 1, 81 footnote. This runs contrary to the usual interpretation of Stokes' work which is seen as dominated by physics. See David B. Wilson, *Kelvin and Stokes,* and E. M. Parkinson, "George Gabriel Stokes, 1819–1903," *Dict. Sci. Bio.*, vol. 13, 76–79.

105 Arthur Cayley, "Report on the Recent Progress of Theoretical Dynamics," *Rep. British Assoc.* (1857): 1–42.

106 G. D. Liveing, in *Memoir and Scientific Correspondence of the Late George Gabriel Stokes, 1819–1902,* Joseph Larmor, ed. 2 vols. (Cambridge: Cambridge University

Stokes "at once set the study on a new footing" with his experiments. He demonstrated conical refraction, a result that was well known and liable to be on the Tripos.

Stokes was well aware of his audience. Professional audiences were now becoming large enough that papers on the different aspects of one topic, mathematical, experimental and hypothetical were placed in different publications. He used these publication alternatives to address specific issues. By the middle of the 1840s with the *Cambridge Mathematical Journal,* mathematicians had an outlet that catered to their interests.[107] Physical speculations were of no interest to this group unless they led to new mathematical puzzles. Stokes explained the physical content of mathematical theories in articles published in *Philosophical Magazine* that catered to experimental physicists. Stokes' physical interpretations appeared here without losing his audience in a sea of impenetrable analysis. Stokes' papers, therefore, had clearly delineated purposes that he followed through in their structure. Those directed towards mathematical investigations and those of physical interpretation and investigation were quite distinct.

The clarity and shift of priorities were a break with mathematical physics at Cambridge where any physics emerged from the particulars of a generally developed mathematics. In Stokes' work mathematical physics became a branch of physics as well as one of mathematics. In the mathematical physics addressed to physicists, Stokes focussed only on the amount of mathematics necessary to make the physical points clear. In addressing mathematicians, the physical meanings of the mathematics was either absent or a minor point of the paper.

Stokes' and Thomson's interests and talents complemented one another. In their early correspondence Stokes and Thomson explored the mathematics and physics of their separately favored subjects, fluids for Stokes, electrostatics for Thomson. In all cases Stokes was the more perceptive mathematician.[108] He also helped Thomson to define his physical cases from which the mathematics developed more precisely. In trying to understand Gregory's recent work on differential and integral calculus, Thomson used examples from physical cases. Physics generated and made mathematics intelligible. While Stokes did not follow Thomson into experiments in electricity he could, through mathematics, advise Thomson on both his physics and mathematics. Stokes chose not to speculate physically in the same way as Thomson or even Maxwell. However, both of the latter referred to him and

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Press, 1907), vol. 1, 91–97, 96. Liveing points out the costs to Stokes of all this work. Stokes had to set up the demonstration experiments on his own, at great expense in time and trouble.

107 The Cambridge Philosophical Society together with the Royal Societies of London and Edinburgh as before published papers over a broad range of topics to their members.

108 See Stokes' comments on Thomson's paper on orthogonal surfaces. See Stokes to Thomson, 10th April, 1847, in David B. Wilson, *Correspondence* vol. 1, 23–25, 25.

deferred to his judgment on both physical and mathematical issues.[109]

Stokes was important to both Maxwell and Thomson on two counts. The first was his ability to clarify issues of physics, versus those of mathematics. The second was his ability to present research where he differentiated the goal of exploring the mathematical implications of a physical situation from exploring just how the mathematics might be tamed to meet physically plausible conditions and hence expose the physical content within the mathematics. Stokes remained an important source of mathematical support and a judicious critic of physical speculations for both men.

Stokes' career and his reputation was never on a par with those of Maxwell and Thomson. He was neither an entrepreneur, nor did he have a private income. As one of the secretaries of the Royal Society, he expended enormous amounts of energy on the work of others. The standards with which he shaped his research did not fit well into a discipline and profession that began to judge a career in terms of conceptual innovation. Clarifying what within French mathematical physics was important for the physical understanding of nature, versus that which opened up new areas of analysis, seemed less important to later generations who could take those criteria for granted. In the actual development of theoretical physics in Britain, such an ability was crucial in investing mathematical expressions with physical meaning and knowing when the mathematics was not speaking to the physical problem at hand. It is no wonder then that both William Thomson and James Clerk Maxwell held Stokes in such high regard, both as a source for mathematical insight and as a critic of their physical speculations. The relationship with Maxwell was so close that, at Maxwell's death, Stokes became the executor of his intellectual estate.

James Clerk Maxwell

Maxwell was closer to Stokes in methodology and consciousness of his own usage of hypotheses, their delights and dangers, than any other colleague. He was also more systematic than Stokes in his exploration of his own uses of hypotheses and in the more general question of when and where they were necessary. By the time Maxwell graduated from Cambridge in 1854 he had a range of types of physical explanation to draw on from the work of Thomson, Stokes, and others. He also inherited their experimental work and the integration of those experiments with their theoretical interests. This included Thomson's mathematical work on electricity, his integration of experiment with heat theory in thermoelectricity, and his examples of nature as mechanism. From Stokes, Maxwell could draw on his experiments on friction in gases and on the motion of pendula together with his

109 This was captured by Tait in "George Gabriel Stokes," *Trans. Cambridge Phil. Soc.* 18 (1904): 303–304 and in *Scientific Correspondence of Stokes*, Larmor ed., vol. 1. It was echoed in Thomson's remarks on the same occasion, 277.

hydrodynamics, as well as his work in light and on the ether that was an extension of his work in fluids.

Maxwell shared with Stokes and others an interest in the relationship between images of nature and reality, and the particular example of Stokes' clear criticism of mechanical models of the ether. Much has been made of the roots of Maxwell's philosophy in the unique aspects of his Scottish education. However, in looking at the group of natural philosophers and physicists in Scotland and England as well as Ireland, the same philosophical issues and range of responses to the problems posed by the relationship between images of nature and its reality are found across cultural and social boundaries. The assumption of the uniqueness of the Scottish philosophical heritage of Thomson and Maxwell cannot explain the baroque nature of Thomson's later detailed molecular models of the ether, magnetism, and electricity. Within these accounts Stokes' distance from these same models also remains somewhat mysterious given his educational background. Stokes' research matched the "geometrical" descriptions given to the kind of natural philosophy that supposedly emerged from Scottish natural philosophy. Maxwell's use then discarding of the same kind of mechanical models as Thomson's speaks for a continuing search for heuristic methods to explore nature, rather than a lifelong commitment to any philosophical program.

The argument over the special place of geometry within this Scottish philosophical tradition also fails, if we take into account the research interests as well as the textbooks of many professors of natural philosophy and mathematics in Scotland.[110] What these physicists had in common was intensive mathematical training at Cambridge. What they did with that training depended on the contingencies of their careers. Stokes remained at Cambridge. The research problems of interest there and the requirements of his chair led him to consider some physical problems rather than others. The opportunities of Glasgow and study in Paris changed the direction of Thomson's work. Maxwell was able to exploit several different approaches to the exploration of nature. Unlike the other two, he had an independent income that distanced him for some years from the immediate needs and the social context of a profession. It allowed him to pursue theoretical physics along several different fronts simultaneously.

What also joined these and other physicists of the mid-nineteenth century was the conviction that they were searching out the true structure and functioning of nature. Maxwell simultaneously explored the philosophical grounds on which to develop theories of nature while constructing them in mathematical form and interpreting them physically. Early in his career Maxwell explored the physical

110 On this heritage, see Richard Olson, "Scottish Philosophy and Mathematics, 1750–1830," *J. Hist. Ideas* 32 (1971): 29–44, and *Scottish Philosophy and British Physics* (Princeton NJ.: Princeton University Press, 1975). See also Peter Harman, "Edinburgh Philosophy and Cambridge Physics: the Natural Philosophy of James Clerk Maxwell," in *Wranglers and Physicists,* Harman, ed., 202–224.

importance of mathematical expressions in his discussion of Faraday's electrotonic state.[111] This search and its results were of greater consequence for the development of theoretical physics than the parallel descriptive, mathematical efforts of his French predecessors. Interest in what we characterize as the philosophical aspect of theories about the physical operation of nature were a integral part of the development of theoretical physics in Britain. Philosophical issues therefore surface within otherwise technical papers in the work of Maxwell as he consciously structured new kinds of explanations of physical phenomena. Although we might find the appearance of such discussions out of place, they did not seem to disturb his contemporaries.

Much of Maxwell's discussion revolved around the relationship between models of nature and reality that Stokes had touched upon in less formal ways, and of the role of mathematics in constituting those images. Maxwell explored how explanations of nature ought to be structured while simultaneously structuring explanations of specific phenomena and processes. He shared a general commitment to a mechanical world view with Thomson and Stokes. Yet he dropped the use of mechanical models in the mode of Thomson for the more defensible foundation of the Langrangian and Hamiltonian formulations of mechanics. If we argue that his detailed vortex model of the ether must be taken seriously as a Maxwellian model of reality, we have to ignore his detailed discussions on analogy and his later mechanical and electrical reformulations of his electromagnetic theory.[112] To claim Maxwell's adherence to specific models of the ether, historians and philosophers must ignore parallel developments of Maxwell's theory of gases, from specific molecular models to increasing abstraction, from mechanical specifics to statistical mechanics. In this last stage Maxwell used the Hamiltonian formulation of mechanics and hence his gases obeyed only the most general laws of mechanics.[113]

111 Maxwell, "Faraday's Lines of Force," *Trans. Cambridge Phil. Soc.* 10 (1856): 27–83, reprinted in Maxwell, *Scientific Papers* vol. 1, 155–229, 188–189, 209.

112 Maxwell's commitment to the specific mechanism of vortices has been reaffirmed in Daniel M. Siegel, *Innovation in Maxwell's Electromagnetic Theory: Molecular Vortices, Displacement Current and Light* (Cambridge: Cambridge University Press, 1991), and, "Mechanical Image and Reality in Maxwell's Electromagnetic Theory," in *Wranglers and Physicists,* Harman, ed. 180–201, and "Thomson, Maxwell and the Universal Ether in Victorian Physics," in *Conceptions of Ether,* Cantor and Hodge, eds. 239–268. Maxwell's reformulations of his electromagnetic theory are discussed in C. W. F. Everitt, *James Clerk Maxwell Physicist and Natural Philosopher* (New York: Charles Scribner's Sons, 1975), 80–111.

113 Other historians and philosophers also maintain Maxwell's skepticism towards models and specific mechanisms. See Peter Harman, "Edinburgh Philosophy and Cambridge Physics," in *Wranglers and Physicists,* Harman, ed. 202–224, and "Maxwell and Modes of Consistent Representation," *Arch. Hist. Exact Sci.* 6 (1970): 171–213. Others have argued that aesthetic principles guided at least Maxwell's theory of the electromagnetic field. See, Alfred M. Bork, "Maxwell's Displacement Current and Symmetry," *Amer. J.*

The dichotomies in recent Maxwell historiography arise from seeing him as either a displaced philosopher, or a working physicist. As a physicist, lifetime philosophical consistency and rigor is a secondary consideration.[114] Yet if we exclude either aspect of his life and work that run consistently through both, we can make no coherent sense of him.[115] Maxwell's philosophical concerns were constitutive of the new enterprise in which he was engaged, the construction and defense of what we now call theoretical physics.[116]

Maxwell's discussion of the relation of physical imagery to reality began in his discussion of analogy.[117] He claimed that analogies only signified mathematical likenesses not physical ones. His discussion of specific mechanical models was a method of constructing a mathematical likeness of nature, not a replica of it. Maxwell was not drawn into the pursuit of mathematics but held more closely to the goal of the exploration of nature. He was able to distinguish those aspects of his mathematics that led to physically plausible results and those that were mathematically interesting. He also published them separately. Maxwell quickly learned that neither experiment, nor mathematics alone were adequate paths to the construction of physical theory. In 1849 while a student at Edinburgh with James David Forbes, he began an experimental investigation into the perception of color. He continued this work for nearly two decades, throughout the period of his early work on electricity, magnetism, and gases. He also performed experiments to confirm the predictions of his theories on electromagnetism and gases so that the direct juxtaposition of experiment and mathematics continued throughout his life. Maxwell also saw the limitations of both in that,

> experiment furnishes us with the values of our arbitrary constants, but only suggests the form of the functions. Afterwards, when the form is not only recognized but understood scientifically, we find that it rests on precisely the same foundations as Euclid does, that is, it is simply the contradiction of an absurdity.[118]

Phy. 31 (1963): 854–859, and Joan Bromberg, "Maxwell's Displacement Current and his Theory of Light," *Arch. Hist. Exact Sci.* 4 (1967): 218–234.

114 See Daniel Siegel, "The Origins of Maxwell's Displacement Current," *Hist. Stud. Phys. Sci.* 17 (1986): 19–146.

115 M. Norton Wise, "The Maxwell Literature and British Dynamical Theory," *Hist. Stud. Phys. Sci.* 13 (1982): 175–205, noted this impasse.

116 For the importance of philosophical issues throughout his life see, Lewis Campbell and William Garnett, *The Life of James Clerk Maxwell* (New York: Johnson Reprint of 1882 edition, 1969), passim.

117 For Maxwell on physical analogies, see Joseph Turner, "Maxwell on the Method of Physical Analogy," *Brit. J. Hist. Sci.* 6 (1955): 226–238, and Robert Kargon, "Model and Analogy in Victorian Science: Maxwell's Critique of the French Physicists," *J. Hist. Ideas* 30 (1969): 423–436.

118 Campbell and Garnett, *Life*, p. 261.

Mathematics could express relations between things, and indeed relationships of the same form could describe separate physical processes. Recognizing these same forms when going from a known to an unknown process could be helpful. The relationships established by mathematics did not distinguish the two processes physically.[119] The analogy between fluids, heat, and electricity was mathematical, "a similarity between relations, not a similarity between things related."[120]

Maxwell's work on analogies was based in William Thomson's mathematical analogies between electrostatics, heat, and gravitation. Maxwell used the idea of mathematical analogy in his attempt to give mathematical form to Faraday's ideas on lines of force which he accepted as empirically grounded. To mathematize them, Maxwell drew the physical analogy between lines of force and the stream-lines of an incompressible fluid. This analogy allowed Maxwell to adopt all the mathematical apparatus of hydrodynamics. By doing so he replicated known laws in electrostatics, magnetism, and current electricity. The vernacular physical representation was matched by the mathematical expression of hydrodynamics and known laws of magnetism, electrostatics, and current electricity. Maxwell then went beyond the replication of known results. He distinguished between magnetic induction and magnetic force, between a flux and a force, by looking at the flow of his fluid through a resistive medium and the changes in its flow as it crossed a boundary into a medium of different porosity. The flow of the fluid was continuous but there was a pressure difference across the boundary. The direction of flow of the fluid in the second medium was not necessarily that of maximum pressure drop. Physically translating the symbols into their electric and magnetic counterparts, Maxwell noted the distinction between current density and electric intensity, magnetic induction and magnetic force.

This first sortie into electricity and magnetism was an exercise in Stokes' approach to mathematical physics. In a second such exercise Maxwell tackled the Adams prize question of 1856–the stability of Saturn's Rings. The essay was laid out in proper mathematical style as a series of propositions pursued in logical order by reducing the steps in the investigation to mathematical form. However, this was an exercise in physical astronomy rather than celestial mechanics. The mathematics was kept under control. Every proposition had a physical point to it and Maxwell accompanied each mathematical result with a physical interpretation. He accompanied the mathematical condition that delimited stability for a solid ring by a physical explanation. He based his conclusion that a solid ring was unlikely on observations and accepted laws of planetary astronomy. This was mathematical

119 Maxwell, "Analogies. Are there Real Analogies in Nature?" in Campbell and Garnett, *Life*, 235–244, 243.

120 Maxwell *Elementary Treatise on Electricity* William Garnett ed. (Oxford: Clarendon, 1881), section 64. For a more extended discussion see, Maxwell, "Faraday's Lines of Force," in *Scientific Papers,* vol. 1, 155–229, 156–158.

physics from a physical point of view. To demonstrate the plausibility of his rings of small satellites, Maxwell transformed his mathematical model into mechanical reality. He had models constructed to demonstrate the motions of these satellites as a disturbance travelled as a wave around a circle of the particles that made up a ring.[121]

By 1860 Maxwell had moved beyond Stokes on two fronts. He was constructing two theories, one of gases, the other of magnetic phenomena based upon the particular physical behavior of specific mechanical models, pursued mathematically and interpreted physically. The first was on the interaction of the molecules of gases, the other his vortex model of the action of magnets and of electromagnetism. The first focussed on matter, the other was a material representation of physical change. Both of these mechanical models led to predictions that Maxwell followed up himself. Taken from Clausius, his kinetic model of a gas made up of billiard ball molecules randomly colliding with one another led to predictions about the thermal behavior of the transport properties of gases. The viscosity of such a gas was independent of its pressure, a very unexpected result, and varied as the square root of its absolute temperature.[122] While his experiments seemed to confirm his model for a gas, Maxwell took them as only confirming the rightness of his mathematical conclusions. They did not tell him anything of the validity of his molecular model.[123]

The steps from his vortex model of electromagnetic interactions to the laboratory were less direct than those from kinetic theory to the measurement of the transport properties of gases.[124] When Maxwell first began to study electricity and magnetism, the range of phenomena to be covered by any mathematical theory was orders of magnitude greater than in gas theory. Mathematical theories abounded to

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121 See Maxwell, *On the Stability of the Motion of Saturn's Rings* (Cambridge: Macmillan and Co., 1859), reprinted with commentary in *Maxwell on Saturn's Rings,* Stephen G. Brush, C. W. F. Everitt and Elizabeth Garber, eds. (Cambridge MA.: MIT Press, 1983).

122 These early kinetic theory papers are reprinted with commentary in *Maxwell on Molecules and Gases,* Elizabeth Garber, Stephen G. Brush and C. W. F. Everitt, eds. (Cambridge MA.: MIT Press, 1986). His experimental work on viscosity became the Bakerian Lecture of the Royal Society in 1866.

123 Maxwell, "Illustrations of the Dynamical Theory of Gases," *Phil. Mag.* 19 (1860): 19–32, 20 (1861): 21–37, and, "On the Viscosity or Internal Friction of Air and other gases," *Phil. Trans. R. Soc. London,* 156 (1866): 249–268.

124 Details of Maxwell's model and its mathematical expression are in Siegel, *Innovation,* chap. 3. Maxwell, "On Physical Lines of Force. Part I The Theory of Molecular Vortices Applied to Magnetic Phenomena," *Phil. Mag.* 21 (1861): 161–175, "Part II The Theory of Molecular Vortices Applied to Electric Currents," same journal (1861): 281–291, "Part III The Theory of Molecular Vortices Applied to Statical Electricity,"same journal 23 (1862): 12–24, "Part IV The Theory of Molecular Vortices Applied to the Action of Magnetism on Polarized Light," same journal (1862): 85–95. Reprinted as Maxwell, "On Physical Lines of Force," in Maxwell, *Scientific Papers*, vol. 1, 451–513.

cover aspects of these phenomena but no one theory unified all of them. Michael Faraday had shown that galvanic and static electricity were the same and extended the work of Oersted and Ampère to show how mechanical force, current electricity and magnetism were inextricably linked. Maxwell began his mathematical constructions in magnetism as he had with his fluid analogy. In working on the mathematical expression of the mechanics of vortices, Maxwell found that the mathematics representing the centrifugal force of the rotating vortices acted as the magnetic force. The mathematical form representing the motions of the particles acting as idler wheels between the vortices behaved as the electric current. The mathematics that represented the changes in the velocity of the vortices was the same as that for electromagnetic induction.

To complete the mathematical description of the full range of phenomena, Maxwell needed a mathematics of some mechanical property of his vortices that could represent the phenomena of electrostatics and then link those to the equations representing current electricity.[125] To do this Maxwell endowed his vortices with elasticity. With a medium endowed with elasticity, Maxwell could bring to bear all the mathematics of the ether and capture light in his mechanical net by tracing the propagation of elastic waves through his mechanical medium. His mechanical model from which he could deduce the mathematics of known electromagnetic, electrical, and electrostatic phenomena led him to predict the velocity of propagation of these elastic waves (the ratio of electrostatic to electromagnetic units) as the velocity of light. He extended his mechanical model in other directions that led to a relationship between the refractive index of a dielectric with its dielectric constant. Neither prediction was brought to the experimental fruition Maxwell had hoped for.[126]

The mechanical model from which Maxwell developed a unified mathematical theory of electricity, magnetism, and light depended on Stokes' understanding that solids and fluids differed only in the degree of their mechanical properties, not in kind. Simultaneously Maxwell used fluid-theory mathematics in his parallel development of electromagnetism and a second version of his theory of gases. In his gas theory he avoided the specifics of a molecular model until it was necessary to deduce expressions for the transport coefficients. He then used a centers-of-force model that allowed him to integrate a crucial equation and bring about mathematical and physical closure.[127] While uniting a broad range of phenomena mathematically it is unclear that Maxwell accepted these molecular models as representing physical

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125 This was a crucial development as Wilhelm Weber's theory, the only serious rival to Maxwell's, had accomplished just that.

126 Siegel documents Maxwell's responses to the relevant experiments in Siegel, *Innovation*, 155–158.

127 On the importance of the mechanics of fluids in Maxwell's physics see *Maxwell on Molecules and Gases*, Garber, Brush and Everitt, 23–26.

reality. It was heuristic. In the manipulations of his equations, the mechanics of the model allowed him to follow physical processes that were mechanically defensible and consistent. Only mathematical manipulations with such mechanical counterparts were recounted and followed through in his papers. This was a search for an understanding of the processes of nature, not a coherent mathematical description of a broad, experimentally connected set of phenomena. Mathematical extensions were through the development of the mechanical properties of the medium, not the manipulation of equations. Mathematics was subordinated to the extensive exploration of a mechanism.[128]

Maxwell did not take these specific mechanical models as the image of nature. He later reformulated both his gas theory and his electromagnetic theory of light. He reworked them so that all his results depended on the general principles of mechanics, not on the particular characteristics of any one model.[129] He also explored the shortcomings of theories based on mechanical models. Maxwell preferred a more Stokesian approach, basing his theories on general hypotheses that simply assumed the phenomena only depended upon the configuration and motion of a material system.[130]

He used models to generate mathematical relations and guide the development of the mathematics along physically defensible lines. The mathematics and model together also allowed Maxwell to reach beyond known experimental evidence to create new encounters between mathematics and experiments. Only mathematics guided by possible mechanical processes could lead to plausible outcomes. Mechanical models were necessary for him to structure his physical theories. As an image of nature and her operations, he found mechanical models less than satisfactory.

Maxwell was both a theoretical and experimental physicist who made few contributions to pure mathematics. Those he did make were in his *Treatise* and his final paper on gas theory. In the *Treatise,* pure mathematics was confined to two

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128 For his mechanical models in electromagnetism see Maxwell, "Physical Lines of Force." For gases see Maxwell, "Illustrations," and, "On the Dynamical Theory of Gases," *Phil. Trans. R. Soc. London* 157 (1867): 49–88, reprinted in *Scientific Papers*, vol. 2, 26–78, and *Maxwell on Molecules and Gases*, 419–470.

129 For Maxwell's reformulation of his electromagnetic theory see Maxwell, "A Dynamical Theory of the Electromagnetic Field," *Phil. Trans. R. Soc. London,* 155 (1865): 459–512, and for the papers leading to his form of statistical mechanics, with commentary see *Maxwell on Heat and Statistical Mechanics: On "Avoiding all Personal Enquiries of Molecules"* Garber, Brush and Everitt, eds. (Bethlehem PA.: Lehigh University Press, 1995).

130 For Maxwell on mechanical models in general see Maxwell, *Treatise on Electricity and Magnetism* (New York: Dover reprint of third edition, 1945), vol. 2, chap. 5. Maxwell added a chapter, "On the Equations of Motion of a Connected System," in the second edition of his treatise. For Maxwell's criticisms of specific molecular models including vortices, see Garber, "Molecular Science in Late Nineteenth-Century Britain," 275–279.

chapters separated from those on the physics of electricity and magnetism.[131]

By the 1870s Maxwell was sufficiently aware of the difference between the enterprise he, Thomson, and Tait were engaged in and that of the mathematicians to see a new relationship between mathematics and natural philosophy. In his review of Thomson and Tait's *Elements of Natural Philosophy,* Maxwell pointed out two ways in which natural philosophy was usually taught. The first was as training in pure mathematics where the student "at once appreciates the language if not the ideas of the new subject and where progress is equated with bringing that science under the power of the calculus." The other method was as training in experiment. Neither led to the development of powers of reasoning or the development of new powers of thought. Mathematics was a transfer of effort from thinking about natural phenomena to contemplating equations. In his opinion all mathematicians should put their ideas into words to enlighten the world and themselves. Maxwell himself doubted "whether the ideas as expressed in symbols had ever quite found their way out of the equations into their minds." He briefly described the contents of the text and noted, with regret, that the authors had not used vectors, although one of them was "a follower of Hamilton." Maxwell expressed his sympathy with their efforts to divest "scientific truths of that symbolic language in which mathematicians have left them," and put them in precise, vernacular terms. The experimentalist was bogged down in details that obstructed their ability to reach for "higher forms of thought." Yet there was a third method,

> where each department in turn is regarded, not merely as a collection of facts to be coordinated by means of the formulae laid up in stone by the pure mathematicians, but as itself a new mathesis by which new ideas may be developed.[132]

In keeping both experiment and pure mathematics together and acknowledging that both required hypotheses, a new form of mathematical knowledge could emerge. For Maxwell, mathematics was symbolic and unless the physical meanings of those symbols were kept before the mind, the student would gain little understanding of the physical universe promised by the mathematics. Nevertheless in understanding the mathematical characteristics of the quantities being signified, mathematical

131 Maxwell *Treatise,* vol. 1, chap. 5, "General Theorems," where there is a generalized discussion of Green's theorem, and chap. 9, "Spherical Harmonics." Spherical harmonics are also the subject of an addition to Maxwell, "On Stresses in Rarified Gases from Inequalities in Temperature," *Trans. R. Soc. London,* 170 (1880): 231–256. Reprinted in Maxwell, *Scientific Papers*, vol. 2, 681–712. See *Maxwell on Heat and Statistical Mechanics*, Garber, Brush and Everitt, 77–78 for a discussion. Maxwell also published on reciprocal statics, curvilinear coordinates, and the calculus of variations.

132 Maxwell "The *Elements of Natural Philosophy* by Professors William Thomson and Peter Guthrie Tait (Macmillan and Co., 1873)," *Nature,* 7 (1873): 324–328, reprinted in Maxwell, *Scientific Papers*, vol. 2, 324–328.

analogs were quickly built.[133] He based his proposed classification of these analogies on the physical effects he saw signified by the mathematical quantity. Curl thus signified a rotation, convergence, a focussing at a point.

By the time of Maxwell's death in 1879, several approaches to and goals for the study of nature using mathematics had emerged, several of which he had defined, shaped and made credible. Mathematicians whose forays into analysis began in problems of physics continued to use the new domains of physics for their own purposes. They frequently inverted the direction of the development of these fields by using physical problems to illustrate the uses of new mathematical domains. This constituted "mathematical physics" as practiced by mathematicians. This type of mathematics was quickly being reclassified as a branch of "applied mathematics," and relegated to a secondary place in a changing hierarchy of research in mathematics. The newer pursuit of research into foundations constituted the highest rung of the research ladder in "pure" mathematics. Mathematicians also developed mathematical theories based in physical hypotheses which they claimed were theories of physical phenomena that bore no relationship or points of contact through experiment with any aspect of physical reality. These were not confined to James Challis; it was even a characteristic British exercise in mathematics. Some German mathematicians indulged in the same kind of research that Maxwell found irksome and fundamentally useless.

Not all explorations of the purely mathematical kind that began in physical problems were without interest for those using mathematics to interpret the functioning of nature. Mathematical conclusions, especially using particular cases, were examined for their physical content. These examinations were no longer done with the casual assumptions of Poisson et al., but required that the behavior of the original physical entities encoded in mathematical symbolism be traced through the mathematical manipulations. This was to make sure that those manipulations consistently represented plausible physical processes and the conclusions were a truly idealized version of real experimental circumstances. Such examinations began with Herschel and Whewell and culminated in the work of Stokes who used such a generalized approach masterfully.

To go further and generate physical meanings, not merely seeing them retrospectively in mathematics already developed, required that each step in the evolution of the mathematical operations, going from one equation to the next, carried physically consistent meanings. A certain mathematical operation always represented a rotation, etc. In addition, this operation was only of interest if and when the physical results of it were consistent with the general physical characteristics of

133 He extended this idea of mathematical analogy from algebra to geometry and formally began the transition from quarternions to vector analysis. See Maxwell, "On the Mathematical Classification of Physical Quantities," *Proc. London Math. Soc.* 31 (1869–71): 224–232, reprinted in Maxwell, *Scientific Papers,* vol. 2, 241–266.

the system under study. Conservation laws for example and other accepted principles had to be obeyed. Even if only such generalities were followed, the results of mathematical manipulations might be unexpected. If these results were physically plausible, they were allowable. The paths taken by the mathematician were hedged about by the needs of physical plausibility and consistency in the meanings attached to mathematical operations. The meanings of mathematical operations, as well as its symbols, were becoming infused with physical meanings. Every mathematical result was reached to make a physical point, or bypassed. Or such results might be passed over in silence and explored later before a mathematical audience.

Further subjugation of the languages of mathematics to the purposes of physical interpretation came by using the specifics of mechanical models. The range of both mathematical and physical validity was more limited, although they might be physically suggestive and insightful. Thomson's growing preference for this approach meant that his work in physics was piecemeal, while the mathematics that he used was consistent. He produced insights into physical problems without developing any broad theories of any type of physical phenomena.[134]

There were also cases where both physical imagery and the logical outcomes of mathematical manipulation joined together more equably. The logical structure of the mathematical manipulation suggested directly how a physical system ought to behave. This was true in the development of thermodynamics and is most easily seen in English in Maxwell's *Theory of Heat.*[135]

Practitioners of all these different methods shared the assumption that mathematics was central to understanding nature. All three approaches led to predictions of new phenomena through the extension of mathematics guided by physical principles, or models, beyond the confines of known behavior and experimental results. Maxwell understood the power of this new use of mathematics that was independent of the older experimental methods of natural philosophy. He also exploited the potential of these predictions to justify this new discipline. He also understood that the failure of mathematical results to meet those of experiment doomed the theoretical enterprise and was a forceful argument against the use of theory in the study of nature. It was however this new combination of mathematics, detailed physical imagery, and experiment that characterized the new discipline. Experimental results were integrated into the justification of using mathematics in one way rather than in another through the behavior of postulated physical systems.

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134 For the popularity of Thomson's vortex atom see Robert Silliman, "William Thomson: Smoke Rings and Nineteenth-Century Atomism." For other atomic and molecular models see, Garber, "Molecular Models."

135 Maxwell, *Theory of Heat* (London: Longmans Green, 1871). This text went through several editions in Maxwell's lifetime. See the section in the later editions on Gibbs's thermodynamic surface. See also *Maxwell on Heat and Statistical Mechanics*, Garber, Brush and Everitt, eds. 48–51, 232–247.

For Maxwell and his contemporaries, predictions did not necessarily mean that the physical imagery that led to the experimental result represented the actual workings of nature. Such philosophical subtleties were lost on following generations who took for granted the power of models and hypotheses to mimic and predict the operations of nature. This heady combination of mathematics, physical imagery, and experiment characterized a new discipline. Yet the mix and actual usage of these elements depended on the individual and even the specific problems under study. In Britain this range remained within the practices of theoretical physicists throughout the nineteenth and into the twentieth century. Mechanical models of the ether were not the only characteristic of British physical thought as Pierre Duhem would have us accept.[136] The centrality of mathematics to this new discipline meant that new developments within mathematics might have immediate implications for theoretical physics. And within a generation, vector analysis had such an effect in the reformulation of electromagnetic theory by Oliver Heaviside and Josiah Willard Gibbs.[137]

In Britain the inheritance of French mathematical physics was transformed through the traditions of natural philosophy. The descriptive function of French mathematical physics was inadequate. Philosophical issues were fundamental to and necessary for the development of theoretical physics, as was training in the mathematical languages. All practitioners of this first generation of theoretical physicists shared training in French mathematical physics and natural philosophy. For those educated at Cambridge, especially at Trinity, the philosophical interests of Whewell were never far from the surface of their experience and their letters. This reenforced the philosophical discussions within the demonstration courses in natural philosophy that all were exposed to both before and during their mathematical training. These factors, together with the contingencies of developing careers in the middle of the nineteenth century in the new professions within the sciences, converged to give a few extraordinary individuals opportunities for the

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136 For example, in the 1890s George Hartley Bryan argued from general principles that no mechanical model could represent the behavior of the second law of thermodynamics. In Britain, at least, mechanical mimicry of thermal systems ended. Through the same papers efforts to propose mechanical models to avoid the implications of the equipartition theorem came to a similar end. See Brush, *The Kind of Motion we Call Heat* (New York: North-Holland, 1976), 2 vols., vol. 2, chap. 10.

137 On Oliver Heaviside see Bruce Hunt, *The Maxwellians,* chap. 3. For Gibbs on vectors see Gibbs, *Elements of Vector Analysis, arranged for the Use of Students of Physics* (New Haven, 1881). Reprinted in Gibbs, *The Scientific Papers of J. Willard Gibbs* Henry Andrews Bumstead and Ralph Gibbs Van Name, eds., (New York: Dover reprint of 1906 edition) 2 vols., vol. 2, 17–90. While printed privately Gibbs' text on vectors fell into the hands of Tait and Gibbs defended vectors in a series of papers in *Nature* in 1891 and 1893. Reprinted in his collected papers. On the mathematical history of vectors see Michael J. Crowe, *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System* (Notre Dame: University of Notre Dame Press, 1967).

contemplation of and the solution to long standing problems of natural philosophy.

Britain was not the only society with a long history of natural philosophy, or an academic community struck by the stunning success of French mathematical physics which they then sought to annex for themselves. The Germans, like the British, transformed French mathematical physics, combined with experiment and physical imagery to produce their own particular forms of theoretical physics. This was accomplished within the same time period in Britain, although the specific content was intellectually different and the professional outcomes unique to German society.

Chapter VIII

Physics and Mathematics in the German States, 1830–1870

By 1830 in the German States the new opportunities to make careers within the sciences and mathematics were secure within the reformed universities. Teaching was no longer the only expected activity of faculty. Research had become a defining quality for appointment to and promotion within universities across the German states. This ideal was complicated by the justification for continued state support for these renewed institutions, the training and certification of future teachers in the lower echelons of the education systems within each state.[1]

Entry into the research community in the sciences required a student to navigate a series of formal steps. He had to be trained and certified as competent in a specialty by a university.[2] The specialist training was offered in demonstration lectures and seminars, and the first laboratory courses in the experimental sciences. Seminars and laboratories were apprenticeships in the practices of the disciplines. Ultimate certification was through the acceptance of a PhD dissertation. The dissertation certified the ability of the author to understand a body of material, identify a problem, then bring to bear and manipulate the methods of that specialty in the search for its solution.[3]

These general remarks outline the formalized academic training at any German university in any scientific discipline. What redefined both mathematics and

1 Jungnickel and McCormmach, *Intellectual Mastery of Nature* give the details of the formalized, German academic system as it affected the members of the discipline of physics, and then the problems of physicists in obtaining adequate support. The first volume covers the period of this chapter.

2 It was no longer possible for someone like August Crelle for example, who studied mathematics privately, to obtain their PhD. Nor were the cases of Friedrich Wilhelm Bessel and Joseph Fraunhofer repeated, where two men who began their lives as craftsmen overcame social and intellectual barriers to gain appointments as professors at universities. Hermann von Helmholtz was trained as a physician, i.e., in a learned profession, so that his transition to physics was possible.

3 Certification in teaching in a university required a further dissertation, the *Habilitation*.

physics lay in the details of the practices developed by mathematicians and physicists as they confronted specific research problems and passed the skills of solutions on to their students. Precisely what the practices of mathematics and physics were to become depended upon the subject matter of the lectures, the methodologies taught in the seminars and laboratories of these same universities. There was no consensus about these crucial, defining elements. What specifically was taught and how depended on the occupants of the chairs, the heads of seminars, and the directors of the research institutes. They introduced their experiences and practices to their students and passed on to them their particular sense of the important problems in mathematics and physics, as well as how to attack them. Distinct traditions in mathematics were crystallized in the same decades.[4] Equally distinct traditions of research arose in physics in the universities of Königsberg, Berlin and Bonn. The older historiography of physics in the German university system placed the mathematics-physics seminar at Königsberg, begun in the 1830s, as the unique source for the development of research in German physics and mathematics. It was the first, but its influence on the subsequent development of the disciplines is more difficult to trace. The actual impact of the physics half of this seminar and the laboratory, also conducted by Franz Neumann, is harder to judge than even the impact of Jacobi's training in the mathematics seminar.[5] Neumann trained future gymnasium teachers as he was creating the methods to do so. He inspired many students but student publications emerging from that seminar were few compared to those from the mathematics seminar. Recent research indicates that modern forms of physics developed more gradually and in several centers.[6]

The model from which both physicists and mathematicians began was that of

4 The main rival traditions in mathematics developed at the universities of Berlin and Göttingen. Joseph Dauben, "Mathematics in Germany and France in the Early Nineteenth Century: Transmission and Transformation," in, *Epistemological and Social Problems,* Jahnke and Otte, eds. 371–399. Dauben restricts his comments on German mathematics to Berlin. For later decades see Thomas Hawkins, "The Berlin School of Mathematics," in *Social History of Nineteenth-Century Mathematics,* Mehrtens, Bos, and Schneider, eds. 233–245. On Göttingen see David E. Rowe, "Klein, Hilbert and the Göttingen Mathematical Tradition," *Osiris,* 5 (1989): 186–213.

5 The number of Jacobi's students were small but some lived the same ideal of research within the German university system, even though the next generation of mathematics seminars was not established until the 1860s. His research approach was repudiated by later generations of mathematicians centered at the University of Berlin who dominated the profession from the 1860s to the 1890s. Assessment is also clouded by the symbolic value of Jacobi for liberal historians and for mathematicians. During the revolution of 1848 Jacobi ran afoul of the King of Prussia and lost the support of other academics in the Prussian university system. His jewish background added to his problems, although that is softpedalled in all the accounts. See Alexander von Humboldt and Carl Gustav Jacob Jacobi, *Briefwechsel,* (Berlin: Akademie Verlag, 1987).

6 The range of the training in physics offered by different universities is given in Jungnickel and McCormmach, *Intellectual Mastery of Nature.* See Kathryn Mary Olesko, The

the French. The French gave German experimental physicists a model of practice, from measurement techniques to interpretations of experimental results. French mathematics and mathematical physics offered both German physicists and mathematicians a series of research problems and forms for their solutions.[7]

Mathematical Physics as Mathematics

Carl Gustav Jacob Jacobi epitomized the opportunities of the reform university and redefined the mathematics of the university as "pure," then successfully trained students in this new brand of mathematics. Jacobi captured the ideology of the new system and then demonstrated his commitment to this ideal in his early research into transcendental functions, his methods for the solution of partial differential equations, and his teaching in his mathematical seminar through research. Students in the seminar produced publishable research that usually appeared in Crelle's journal. The origins of Jacobi's research were in the mathematics of Euler, Lagrange, and Legendre, among others. Physical problems and their direct expression in mathematical form was not the starting point for his mathematics.[8] Physics was reduced to the role of supplying illustrative examples for his methods.

Jacobi was not initially interested in mechanics and was only drawn to it by the work of William Rowan Hamilton. Jacobi had labored for some years to develop a general method of solving partial differential equations of the first order in n unknowns. Building his methods as he went he generalized his findings for three, five, then more unknowns. In 1834, just as his research was beginning to bear fruit, it looked as though he had been anticipated by Hamilton. Hamilton began with Lagrange's analytical formulation of mechanics, based upon the principle of Least Action, restricting himself to time-independent central forces. He then examined the variation of the time-Action integral V written as a function of the initial and final coordinates of the path of a particle. He then varied the end-points of the path and found linear, partial differential equations in V, the initial and final coordinates of the system, and H, a constant. Hamilton reduced the problem of

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Emergence of Theoretical Physics in Germany: Franz Neumann and the Königsberg School of Physics. Unpublished PhD dissertation, Cornell, 1980, and *Physics as a Calling,* for Königsberg. Two other traditions of training in physics have been studied in detail in Jungnickel, "Teaching and Research in the Physical Sciences and Mathematics in Saxony, 1820–1850," *Hist. Stud. Phy. Sci.* 10 (1979): 3–47, and Schubring, "The Rise and Decline of the Bonn Natural Science Seminar," *Osiris* 5 (1989): 57–93.

7 The uses nineteenth-century German mathematicians made of physical problems is explored in Mikolai N. Stuloff, "Die mathematischen Methoden im 19. Jahrhundert und ihre Wechselbezeichungen zu einigen Fragen der Physik," *Technikgeschichte,* 33 (1966): 52–71. This is a survey, from Leibniz to Weierstrass, of the development of "pure" mathematics in Germany through the consideration of problems originating in physics.

8 Jacobi only attacked such problems successfully in the company of an astronomer or physicist.

finding V, the "Characteristic Function" of the system to solving two, simultaneous, partial differential equations for the endpoints of the path.[9] Hamilton developed a "Calculus of Principle Relations." The principal function S was related to V by the expression,

$$S = V - Ht.$$

He used this method in his examination of a number of the mathematical problems presented by the wave theory of light and electromagnetism.

Jacobi's reaction to Hamilton's papers was negative. A solution to Hamilton's equations might not necessarily exist, and why did Hamilton need two partial differential equations? The most general solution was obtainable from one such equation, and this reduced by half the number of arbitrary constants in the solution. His concern was to achieve the the most general solution possible, and establish sufficient and necessary conditions for that solution.[10]

For Jacobi, the equations of mechanics were but one example of the class of partial differential equations he was exploring. One of the last series of lectures Jacobi delivered at the University of Königsberg was on these methods of solution. Mechanics served as examples of their use. The examples were simple, the solutions well known, and were used only to demonstrate how to obtain the principal and characteristic functions and how to compose the one partial differential equation that required solution.[11] It is difficult to see what use any of the mechanical problems, many taken as simple exercises in Keplerian astronomy, and the proposed solutions would have been to any physicists in their research during that decade. The lectures were addressed to the pedagogical needs of the discipline that Jacobi was helping to define. Besides his work on transcendental functions, Jacobi imposed more rigor in various areas of the calculus and demonstrated tech-

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9 Hamilton's final version was also reached through successive attacks on a specific problem, the mathematical problem of the perturbed orbit of a planet. His goal was to develop a conceptually simpler mathematical method than those currently available. See Robert Percival Graves, *Hamilton.*

10 Jacobi, "Über die Reduction der Integration der Partielle Differentialgleichungen erster Ordnung zwischen irgend einer Zahln Variabeln auf die Integration eines einzigen Systems gewöhnlicher Differentialgleichungen," *J. Reine Angew. Math.* 17 (1837): 97–162, and, "Note sur l'intégration des équations différentielles de la dynamique," *Comptes Rendus* 5 (1837): 61–67. The paper was translated in full as, Jacobi, "Sur la réduction de l'intégration des équations différentielles partielles du premier ordre," *J. Math. Pures Appliqués* 3 (1838): 60–96, 161–202. It was still important for German mathematicians to have their worked noticed by the French. Jacobi was careful to send his earlier work to Legendre, whose informed appraisal of it eased Jacobi's appointment at Königsberg.

11 These lectures were edited and then published posthumously as, "Lectures on Dynamics." This title might well be that of the editor rather than Jacobi. See Carl Gustav Jacob Jacobi, *Vorlesungen über Dynamik,* Alfred Clebsch, ed. In Jacobi, *Gesammelte Werke,* Karl Theodor Wilhelm Weierstrass, ed. (New York: Chelsea Pub. Co., reprint of second edition, 1969), vol. 8.

nical brilliance in transforming intractable integrals or differential equations into simpler, well-known and soluble forms. He also saw the implications of his own work in the calculus for geometry and brought new perspectives to bear on both these fields.

We also need to assess the work of other German mathematicians and the relationship of their result with research work in contemporary physics. Historians have claimed that the research of some mathematicians, among the most prominent being Gustav Peter Lejeune Dirichlet, was important for physics during these crucial mid-century decades.[12] We cannot simply accept methods that became useful to theoretical physicists later as useful in this time period. We have to examine whether the results sit within mathematics or physics and whether they were used or useful for Dirichlet's contemporaries in physics. Dirichlet, a close friend of Jacobi, spent the years from 1822 to 1826 as a student in Paris meeting many prominent mathematicians and scientists. One of the most important and influential was Fourier.[13] Many of Dirichlet's later research papers drew on the work of French mathematicians and hence, from the point of view of later mathematicians, from physics.[14] However, in these and other papers Dirichlet's attention was directed to the mathematical flaws, or to extensions of the mathematics, not to its physical significance or its possible use by physicists.

All his papers on mathematical physics during the 1830s are addressed to the mathematical issues in previous solutions to the same problems. In his work on the stability of the solar system, he criticized the mathematical methods of Poisson and Laplace. Dirichlet noted that they had neglected terms of order higher than two without sufficient mathematical justification, and approached the problem from a new perspective that avoided the issue.[15] In his examination of boundary-value problems, his attention was on extending the analysis of the potential function to any number of dimensions. While boundary-value problems were to become important in physics, Dirichlet did not connect his mathematical work directly to

12 Such claims were made initially in histories of mathematics written largely by mathematicians themselves. Many of those mathematicians were strongly allied to Felix Klein's views of mathematics and its relationship with physics. See also Paul L. Butzer, "Dirichlet and his Role in the Founding of Mathematical Physics," *Arch. Int. Hist. Sci.* 37 (1987): 49–82. Butzer's argument cannot be sustained on two grounds. In considering only the German context the French origins of mathematical physics is ignored. In addition, given the definitions of mathematics of the era, the work is mathematical, not physical.

13 Not even Fourier as secretary of the Académie was able to obtain a post for Dirichlet in Paris and he returned to Berlin.

14 For the interaction of German mathematicians with Fourier's work, see Garber, "Reading Mathematics, Constructing Physics," in *No Truth Except in the Details,* Kox and Siegel, eds. 31–54.

15 Dirichlet, "Bedingungen der Stabilität des Gleichgewichts," *J. Reine Angew. Math.* 32 (1846): 85–88.

physics. His work lay within mathematics.[16]

Similarly, Dirichlet's lectures on inverse square forces introduced students to progressively more general mathematical cases of the potential function. He used physical cases to illustrate how such mathematical forms could be collapsed into a more general mathematical method of treatment. He also focussed on the conditions under which the potential had mathematical meaning. His remarks at the end of his chapter on "static electricity" were on the mathematical significance of the material in the chapter, not its physics. It would take some work and understanding on the part of physicists to select those aspects of the text of interest to them or their students, and decipher them in physical terms.[17]

Dirichlet began by deducing expressions for the component of the resultant force of a system of point masses on a single mass. When all the masses were reduced to unity, as was the gravitational constant, each of these forces became the partial differential quotients of a single function. The mathematical expressions he deduced for the gravitational case were also valid for the magnetic and electrical cases, except that there were, as he noted, cases of the "masses" being -1. As a mathematician he found the physical differences between gravitation, electrostatics and magnetism uninteresting. He also examined the potential function at the surface of a sphere where the value of the function inside the surface was not the same as that on the outside, although the function was continuous across the surface. Thus he examined $\partial V/\partial x$ and $\partial^2 V/\partial x^2$ and found that the first partial differential of V was continuous throughout space. These lectures explored the theory of functions where electrostatics, magnetism, and gravitation served as demonstration exercises.

Although Dirichlet followed his French models by often rooting his mathematics in physical problems, he did not attempt to compare specific numerical results of his mathematics to the results of experiment. Mathematics needed no support from physics to justify its methodologies or results. His mathematics began in physical problems, but their analysis quickly became and remained exercises in mathematics, or explorations of new mathematical territory. Some of this math-

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16 Dirichlet, "Über ein neuen Ausdruck zur Bestimmung der Dichtigkeit einer unendlich dünnen Kugelschale, wenn der Werth des Potentials derselben in jedem Puncte ihrer oberfläche gegeben ist," *Ber. Berlin* (1850): 99–116, published in full in *J. Math. Pures Appliqués* 2 (1851): 57–80. Dirichlet also improved the methods of the new calculus. In 1829 he critiqued Cauchy on his work on the convergence of trigonometric series and then made convergence tests more rigorous. Dirichlet, "Sur la convergence des séries trigonométriques qui servent à représenter une fonction arbitraire entre des limites donnés," *J. Reine Angew. Math.* 5 (1829): 157–169, and, "Solution d'une question relative à la théorie mathématique de la chaleur," *J. Reine Angew. Math.* 5 (1829): 287–295.

17 Dirichlet, *Vorlesungen über die im umgekehrten Verhältnis des Quadrats der Entfernung wirkenden Kräfte*, F. Grube, ed. (Leipzig: B. G. Teubner, 1876).

ematical work was recognized later as important for physics. This subsequent importance cannot be used in retrospect to color either his intentions or our own reading of his papers.[18]

Bernhard Riemann's goal of producing a mathematics that united the whole of physics was a mathematical exercise and seems remote from the mathematical needs of physicists in the middle decades of the nineteenth century. Riemann studied physical questions to develop a "self-contained mathematical theory" that encompassed all of mechanics, thermodynamics, electricity and magnetism and he did not distinguish between them.[19] Physicists were left to themselves to decipher the physics locked within this mathematical physics and reorient that language for their own purposes.

Much the same can be said of Riemann's text on partial differential equations and "their application to physical problems." These were lectures on definite integral solutions to the partial differential equations of Fourier's theory of heat. Riemann initially established all the required mathematical techniques, then launched into a section on ordinary differential equations before turning to the issue of linear partial differential equations of the second order. He turned his attention to Fourier by setting up the most general form of the partial differential equation for the flow of heat in three dimensions. He took a series of mathematically special cases that lead to definite integral solutions. All were mathematically defined through systematic restrictions–mathematical boundary conditions on the generality of the equation he first started with. None of these mathematical developments were accompanied by a hint of a physical explanation. The physicist would have to figure out what the various sets of boundary conditions meant in physical terms, then chase down the physical meanings of the processes represented by the accompanying mathematical details. What use this might be to a physicist in this era is far from obvious and rather is a counterexample to the claims of historians that many German mathematicians worked on physical problems or in areas that physicists would find useful.[20]

A more accessible example of Riemann's approach to the solution of physical problems was in the essay of 1860 submitted in a prize competition of the

18 As a antidote to this see Oystein Ore, "Gustav Peter Lejeune Dirichlet, 1805–1859," *Dict. Sci. Bio.*, vol. 4, 127, where the mathematical focus of Dirichlet's work is emphasized, even as the writer noted where the solutions were important for physics, without stating where or how.

19 See Thomas Archibald, "Riemann and the Theory of Electrical Phenomena: Nobili's Rings," *Centaurus,* 34 (1991): 247–271, 259.

20 B. Riemann, *Partielle Differentialgleichungen und deren Anwendung auf physikalische Fragen,* K. Hattendorff, ed. (Brunswick: Friederich Viewig und Sohn, 1896) The text consisted mainly of Riemann's lectures for the winter semester of 1860-1861. Because they were edited after Riemann's death, the title may or may not have been his choice. The lectures are another example of the impact of Fourier and physical problems on the development of mathematics.

Académie des Sciences in Paris on the theory of heat. The essay demonstrates the cryptic nature of Riemann's mathematics, criticized by the Académie, and the reason he was not awarded the prize. Riemann stated the general equation for the conduction of heat in three dimensions but did not obtain particular solutions to the general partial differential equation as the starting point of the mathematical exercise. Instead he investigated, on the most general level possible, methods to reach particular solutions. He was interested in the characteristics of classes of particular solutions rather than the solutions themselves. These classes were then described in geometrical terms. In some cases Riemann demonstrated how a certain type of solution might be obtained in principle. Riemann apologized for the incompleteness of his solutions and blamed his already deteriorating health. This approach shows Riemann's concerns. His interests lay in function theory and mathematics, not the physical meaning of the solutions he obtained.[21]

These arguments can be extended to a discussion of Rudolph Friedrich Alfred Clebsch's work in physical subjects, particularly elasticity, one subject of interest to contemporary physicists working on light and the properties of the aether.[22] No contemporary physicist working in this area refers to, or appears to use, Clebsch's work.

Finally we must consider the reputation of Gauss for publishing significant results in "theoretical" physics. If we look at Gauss's mathematical work on physical problems, we find that they are mathematical exercises. His paper on inverse square forces was an exercise in the mathematics of the potential function. After working on a mathematically general level, Gauss turned to the particular cases of gravitation electrostatics and magnetism and lumped them all together. The mathematical results covered all physical cases as a "special case of a particular solution," a phrase only a mathematician could use.[23] In the body of the paper

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21 Ruth Farwell and Christopher Knee, "The missing Link: Riemann's 'Commentatio', Differential Geometry and Tensor Analysis," *Hist. Math.* 17 (1990): 223-255. The authors' main contention is that this essay is not an early intimation of his work in differential geometry and tensor analysis. They present an English translation of the essay in an appendix.

22 Alfred Clebsch, *Theorie der Elasticität fester Körper* (Leipzig: Teubner, 1862).

23 Gauss, "Allgemeine Lehrsätze in Beziehung auf die im verkehrten Verhältnisse des Quadrats der Entfernung wirkenden Anziehungs- und Abstossungs-Kräfte," *Resultate* (1840): 1–51, reprinted in Gauss, *Werke* vol. 5, 196-242, 241. Translated in Taylor's *Scientific Memoirs* 3 (1843): 153-196. Ch.-J. de la Vallée Poussin, "Gauss et la théorie du potentiel," *Rev. Quest. Sci.* 23 (1962): 315-330, also sees Gauss' work in potential theory as a branch of "pure mathematics." Kenneth O. May, "Gauss, Carl Friederich, 1777–1855," *Dict. Sci. Bio.*, vol. 5, 298–314, argues that this was "the first systematic treatment of potential theory as a mathematical topic." May also considers that it was important for the rigor Gauss introduced into the subject. More recently Thomas Archibald, in "Physics as a Constraint on Mathematical Research," in *The History of Modern Mathematics*, David Rowe and John McCleary, eds. 2 vols, vol. 2, 29–75, argues that this

were a number of theorems proving the existence and continuity of the potential function. There were also theorems for transforming volume integrals into integrals of functions over corresponding surfaces. The paper was the formal demonstration of the mathematics he had published on the characteristics of the magnetic potential over the surface of the earth.

Gauss' work in geomagnetism fell into two types. The first, done in conjunction with Wilhelm Weber, was in the development and use of more accurate methods for observing the components of the earth's magnetic field. In reporting these methods, Gauss included long tables of the results of these observations. Along with these, copious tables of scrupulously observed data were exhaustive discussions of the errors of observation of very small quantities and methods to minimize them. These demonstrate the general German concern with accuracy and Wilhelm Weber's passion for "measurement physics." The papers also convey a sense of a developing confidence in data that grew from such discussions of errors and their careful minimization. Gauss was one of the first to initiate such extensive discussions of observational error in his work on geodesy for the Hanover government.[24] At the same time Gauss was supervising the establishment of the observatory at Göttingen. Until the year of his death Gauss observed regularly, reduced his own data, and reported his results.

Gauss was unusual in that he undertook imaginative work in physics, considered as the complex of observational and experimental sciences, and mathematics.[25] This eighteenth-century definition of disciplinary boundaries fitted Gauss' own sense of his relationship to the State of Hanover and his attitude towards the publication of his mathematical work.[26] None of these activities suggest that Gauss sought, let alone established, any changes in the disciplinary boundary between the observational and the mathematical sciences. Studies within the observational sciences gave him ample opportunity to explore mathematical problems. To change the boundaries required the injection of specific physical notions that Gauss rejected.

This division of labor was reinforced in his mathematical paper on the earth's magnetic field. Given that there were two magnetic poles, Gauss discussed the mathematical characteristics of the magnetic potential. His discussion included the general, closed form of the equipotential lines on the surface of the globe. Algebraically he represented the potential by an infinite series of spherical functions.

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paper is important physically as well as mathematically. However, he does not specify what that physical importance was, or why.

24 Gauss, "Bestimmung der Genauigkeit der Beobachtungen," *Zt. Astron.* 1 (1816): 185–197.

25 His practical mathematical work in observational astronomy is covered in chap. III.

26 May, "Gauss," sees Gauss as an eighteenth-century servant of the state who never made the transition to a nineteenth-century academic. He analyses Gauss' geodesic work in the context of Gauss' sense of his social place in the state of Hanover.

The lengthy tables of data included from the world-wide network of magnetic observations enabled Gauss to evaluate the first twenty-four coefficients in this series. Thus, the mathematical mapping of the magnetic potential was possible. Its physical meaning remained a mystery. Gauss quite specifically separated his theory from all current models of magnetism that might have led to the specific form he explored mathematically.[27]

We can perhaps now understand the mixed reception given his work in terrestrial magnetism. The carefully collected data and his observational methodology fitted the criteria for quantitative work within "Humboltian" science. He did not offer the physical qualitative explanatory interpretation that usually accompanied such observational results. The "theory" was not a physical theory at all but an exercise in mathematics and a piece of research at that. No wonder the group interested in the geosciences were mystified.[28]

The one area in which Gauss was actually a physicist in the modern sense was in his work on the measurement of the earth's magnetic force in absolute units. In this he acknowledged the help of Weber who extended his own interests in the measurement of non-mechanical phenomena in his work on electricity. This was, however, a paper on the problems of experimental physics and included no theory on the nature of the earth's magnetic field. Terrestrial magnetism remained a mystery.[29] Therefore, the arguments made by Jungnickel and McCormmach cannot be sustained with respect to Gauss, Dirichlet, or Riemann. They give only general statements as to the necessity of higher mathematics being recognized by physicists, but no statements from specific physicists on this subject. Nor do they give specific examples of physicists using the mathematical methods they claim as important for them in this era.[30]

Most historians of mathematics and physics follow the statements of the earliest historical accounts and accept all work in mathematical physics as related to and pertinent for the development of physics. This is not the case for physicists working in the middle decades of the nineteenth century and the research of their mathematical colleagues using the results of their experiments to generate mathematical puzzles. Even if that mathematical research began with a problem of interest to contemporary physicists, the solution was not the source for the development of physical understanding of the problem. Only in retrospect was such abstract math-

27 Gauss, "Allgemeine Theorie des Erdmagnetismus," *Resultate* (1839): 1–57, 146–148, translated in *Taylor* 2 (1841): 184–251, 313–316.

28 James Gabriel O'Hara, "Gauss and the Royal Society. The Reception of his Ideas on Magnetism in Britain, 1832–1841," *Notes Record Roy. Soc. London* 38 (1983): 17–78.

29 Gauss, "Intensitas vis magneticae terrestris ad mensuram absolutam revocata," *Gött. Comment* 8 (1833): 3–44, *Ann. Phy.* 28 (1834): 241–273, 591–614. This was not, contrary to May's contention, the first time this problem had been discussed within physics. Coulomb had faced this issue in his experiments on electricity.

30 Jungnickel and McCormmach, *Intellectual Mastery,* vol. 1, chap. 7.

ematics useful in restructuring physicists' understanding of a domain in physics, not while the subject was under active research consideration in physics itself.[31] Most of the above mathematicians were involved in the transformation of mathematics in the middle third of the nineteenth century that amounted to a revolution. An important aspect of the transformation of mathematics in the German states was still rooted in exploring the mathematical implications of physical relationships. What we have in the same decade in mathematics and physics is a phase in which both German mathematicians and physicists adopted French methods. The mathematicians quickly surpassed the French and injected new standards into mathematics to such an extent that they changed the core of the discipline from solving problems to the examination of "foundational" issues.[32]

While the content of the discipline of mathematics was changing, its relationship to experimental physics was unaltered by the work of mathematicians in their versions of mathematical physics. And in the career and research of of Julius Plücker for example, we find the same general pattern that we see in mathematicians of his generation. His research was both in mathematics and experimental physics. His experimental work was initially inspired by Faraday. While Plücker is usually considered as a mathematician, his work does not fall easily into the categories available through modern definitions of either field. This was also true of other members of his generation who confronted French mathematics and physics. Franz Neumann is usually considered a physicist on the basis of his own definitions of his work and from his official position. However, his research and teaching do not easily fit into later images of physics. Yet his research and teaching did much to create one particular strand of physics within Germany to which we need now to turn in detail.

The Transformation of Physics: The First Generation

In common with German mathematicians wanting to annex the research work of the French and create their own discipline, physicists needed to understand, replicate, and then develop the methods of the French. Some German physicists accepted that the mathematical physics of the French was indeed relevant to understanding nature. These physicists interpreted French mathematical physics directly as physics. They therefore had to decipher the mathematics to unearth the

31 Later in the century, Felix Klein wrote of the work of mathematical physics done by the above mathematicians as a part of mathematics. He regarded the work of Clausius, Kirchhoff, and Helmholtz as research that required mathematics but was of a different order and did not call it mathematics. Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im neunzen Jahrhundert* (Berlin: Springer, 1927.) Klein did not investigate those differences.

32 By foundational issues I mean the examination of the mathematical meanings of the foundational ideas of mathematics, such as number, space, function etc.

physics within, teach themselves those methods and how to use them in their own areas of research. They also needed to create the institutional forms to train the next generation of German physicists in those methods and interpretations. While experiment remained the disciplinary core of physics, the relationship between experiment, speculation, and the mathematics that grew out of those experiments and speculations was about to change dramatically.

Some of the first indications of change came in Gustav Theodor Fechner's *Repertorium der Experimental Physik.*[33] This was a report on the state of research in physics. Fechner focussed upon those references that were difficult to acquire, and he had clearly as he claimed read the originals. The table of contents promised a survey that seems extremely modern. The layout of the text betrayed his own preferences for French methodology and Kantian metaphysics. Yet the foundation of physics remained as experiment coupled with vernacular interpretations. He recounted Poisson's theory of matter as atomic, with bodies made up of particles of "imperceptible size" which even in the aggregate were still "imperceptible" and between which were the material stuff of electricity, magnetism, and heat.[34] These particles attracted each other and the particles of heat, but this aspect of the theory was not emphasized. Cauchy's ideas on matter were dealt with similarly. Fechner's account of the mathematics of heat, electricity, magnetism, and mechanics were given separately. However, Fechner claimed that Poisson and Cauchy treated the equations of motion of bodies on the basis of their physical assumptions.

Fechner's description of the physical ideas and their mathematical development were necessarily short and schematic, and he did not address how the two were connected. What Fechner did give was a comparison of the content and the results of various mathematical theories. He also related the mathematical benefits of the various approaches. Cauchy treated elasticity very generally and never got down to "particular" problems. Fechner reported that Navier obtained differential equations without bothering to integrate them to find further cases for application.

33 Gustav Theodor Fechner, *Repertorium der Experimentalphysik, enthaltend eine vollständige Zusammenstellung der neuen Fortschritte dieser Wissenschaft. Supplement zu neuen Lehr- und Wörterbüchern der Physik.* (Leipzig: Leopold Voss, 1832), 3 vols. Very few historians have considered Fechner as a physicist. "Gustav Theodor Fechner, 1801–1887," *Dict. Sci. Bio.* vol. 2, 556–559 focuses on his importance in psychology not physics. Jungnickel and McCormmach, *Intellectual Mastery*, vol. 1, focus on his experimental work on Ohm's law and in electrodynamics, 58–61, 137–138. See also Wolfgang Schreier, "Gustav Theodor Fechner als Physiker," *NTM* 24 (2) (1987): 81-85. He had no students and his career in physics was relatively short. The *Repertorium* was published as an effort to earn a living since the salary from his academic position was meager.

34 This was a possible interpretation of Poisson as he never clarified the notion of electricity and magnetism as fluids, the imagery with which he began his physical account. The imagery with which he started his mathematical development of the theory of both electricity and magnetism was that of forces.

In comparison Poisson put these differential equations into a more general form, then integrated them anew and found various applications for them although the results were "less than practical." Fechner also included a bibliography of the literature on the integration of partial differential equations.

Fechner began to compare the results of the mathematics directly with those of experiment. He confronted the results of French mathematics and tried to extract physical significance from that mathematics. In this he had to argue which physical cases were encapsulated in the mathematics of Cauchy, Navier, and Poisson without giving the analytical details. He claimed to get mathematically deduced frequencies of the various oscillation of bodies from Poisson and Cauchy. He used one or the other as it suited his needs without telling the reader how he got the frequencies he reported. From Poisson he took the expression for the ratio of the longitudinal to translational elastic moduli and frequencies of oscillation. Fechner took data from various experiments and directly compared the mathematical and experimental numbers, without giving the criteria he used to decide whether the mathematical cases actually matched the idealized experimental conditions. The numbers deduced from the two sources were listed in tables without comment.

It is clear that Fechner worked through the mathematics to extract the physics. However, he used results from any and all mathematical sources without referring back to theory and passing on a sense of a theoretical image carried through the mathematical deductions. He had taken mathematics and transformed it where possible into a discussion about physical entities. Mathematical results were made over into physical characteristics and compared directly with what physicists actually measured in their experiments. There is no sense that mathematical physics is actually a part of physics not mathematics. Lest we expect too much, many pages of the *Repertorium* is unremitting reportage of experiments, especially in the areas of his own research, galvanic electricity and its connection with chemistry.[35]

This was an important change but was not echoed in the work of his immediate contemporaries. Both Henri-Gustav Magnus and Heinrich Dove, two of the more prominent examples, were experimentalists. Helmholtz's biographer noted that Magnus,

> regarded experimental and mathematical physics as separate departments and warned him [Helmholtz] repeatedly against undue partiality for mathematics, and the attempt to bring the remote provinces of physics together by its means.[36]

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35 In this respect it is interesting that Fresnel's work on light appeared only in the experimental section, not in his account of the mathematics of light where Fechner discussed only the work of Poisson and Cauchy.

36 Leo Koenigsberger, *Hermann von Helmholtz* (New York: Dover Pubs., reprint of 1906 edition, 1965). Translated by Francis A. Welby, 38. See Jungnickel and McCormmach,

At the University of Berlin, Magnus was instrumental in establishing laboratory training as a crucial aspect of the education of physicists, even though he had to teach these laboratories and accompanying seminars privately.[37] In 1845 the Berlin Physics Society emerged from the meetings and the "Physics Colloquium" at his house. However, we cannot see this as a specialist, scientific society whose members' work matched that of later societies of similar title. Many of its first members, including Hermann von Helmholtz, were interested in physiology. Physics still retained the eighteenth-century meaning of experimental philosophy.

Franz Neumann

It is not with Fechner that we see a sustained confrontation between the needs of German experimental physicists and the works of the French mathematicians. Franz Neumann succeeded in connecting mathematical to experimental results in the context of a new form of mathematical physics.[38] Yet it took Neumann decades to develop a method that made the characteristics of the physical phenomena limit the mathematics and guide its development. He developed one approach to bending the mathematics of the French to the needs of the physicist, based on his assumption that physical meaning lay in the mathematics without the need for speculations about the operations of nature. He rejected the hypothetical ramblings of Naturphilosophie and eschewed, as far as he could, all speculations about physical processes. If he used hypotheses, he kept them to a minimum and they were of the most general kind. While Fourier was clearly a major influence on his mathematics, he used the results and methods of Poisson and others as it suited his immediate purpose. Physics was more important than mathematical consistency.

Despite his development of a new form of mathematical physics, for Neumann the core of physics was measurement. Mathematical physics was a complement to this core. His first accomplishment in mathematical physics was the extension of Fourier's methods into the related physical domain in crystals. Fourier generalized his mathematics through idealized physical examples. Neumann took the physical specifics from the laboratory. He first generated a mathematical expression of an experimentally defined relationship or process, then moved on to a more general physical and geometrical arrangement. The physical elements changed from linear

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Intellectual Mastery, vol. 1, 119–126.

37 The political complexities of the establishment of experimental physics at the University of Berlin are outlined in Armin Herman, "Von Paul Erman zu Hermann von Helmholtz: Die Anfänge der Physik an der Universität Berlin," in *Berlinische Lebensbilder Naturwissenschaftler,* Wilhelm Treue and Gerhard Hildebrandt, eds. (Berlin: Colloquium Verlag, 1987). Jungnickel and McCormmach, *Intellectual Mastery,* vol. 1, 15–18 discuss the economics of the physics department. See also, "Life and Labors of Henri-Gustav Magnus, 1802–1870," *Smithsonian Report* (1872): 223–230.

38 Neumann's work in the 1820s is covered in chap. V.

to circular to a more general shape and their relationships changed from planar to three-dimensional. Neumann put these experimental, geometric possibilities directly into mathematical form. In general he did not explore the mathematics any further than necessary to delve into those particular physical cases.

Neumann was practicing a form of mathematical physics grounded in physics. Yet there was, for example, no discussion of flux as a physical concept. All such physical ideas were treated as mathematical functions. In crystals these were dependent on the geometric symmetries of the crystal. Unlike Fourier, Neumann usually could not develop mathematical expressions from which all physical cases would then follow. He hedged the mathematics about with the limitations of his chosen physical cases. Neumann stuck closely to possible experimental configurations. His examples were physically connected yet sat in mathematical isolation from one another. This necessarily limited what he allowed the mathematics to accomplish. The mathematics was also dense, less than elegant, and always tied to particulars.

Neumann repeated this pattern in his work on crystal optics in which he relied upon the experimental results of Fresnel and others. He could no longer avoid using some hypothesis about the nature of light, but he avoided any assumptions about the interaction of light and matter in the interior of solids. Neumann could only replicate the experimental results established by Fresnel. He also concluded, as had his predecessors, that Fresnel's wave front was necessary for the production of such phenomena. Neumann's mathematical attempts at understanding the polarization phenomena of solids shared the problem of physical implausibility with those of the mathematicians that preceded him whose work became the starting point for his own research.[39]

Neumann chose Navier's hypothesis that the displacement force acting on a particle of a solid was proportional to its displacement. For crystals Neumann generalized this force to a function of the angle between the direction of the displacement and the crystal axis. He then treated this function with all mathematical generality, apart from the mathematical simplifications introduced by assuming the displacement was very small.[40] He arrived at a generalized equation of motion for

39 For a statement of the generic problems of Neumann's approach to the theory of light see Whittaker, *A History of the Theories of Aether and Electricity* vol. 1, 136-139. Whittaker points to the arbitrary physical nature of theories that tried to reconstruct experimental results mathematically, beginning with Cauchy and finishing with McCullagh and Neumann. To see the physical content of Neumann's argument, see his rejection of Fresnel's assumption that refraction is due to changes in the density of the aether in different media. Neumann, "Theoretische Untersuchung der Gesetze, nach welchen das Licht, an der Grenze zweier vollkommen durchsichtigen Medien reflectirt und gebrochen wird," *Abh. Akad. Wissen. Berlin* (1835): 1-160, 7-8.

40 Neumann, "Theorie der dopplten Strahlenbrechung, abgeleitet aus den Gleichungen der Mechanik," *Ann. Phy.* 25 (1832): 418-454.

a disturbance propagated through the medium which was characterized by six constants. He deduced the relationships between the constants, but not their numerical values. The latter Neumann noted was done through experiment. When Neumann examined the case of a homogeneous solid, the equation of motion reduced to Navier's. So far this was mathematics.

Neumann explored the problem further only for physically plausible cases. He investigated homogeneous, non-crystalline and crystalline solids where functions reflected the symmetry characteristics of particular materials. He then faced integrating experiment with mathematics more directly. He began with empirical laws and the same assumptions with which he structured his mathematics and tried to establish Fresnel's expression for the polarization of light reflected off metals.[41] Neumann hoped to replicate Fresnel's trigonometric expressions for the ratio of the intensities and amplitudes of the incident and reflected rays along with their known directions of relative polarization. As with previous mathematicians Neumann had to replicate Fresnel's expression for the shape of the wave front within the solid. Neumann then extended this to crystalline substances by generalizing the behavior of the elasticity of the aether. Elasticity became a function of direction.

What separated Neumann's work from earlier mathematical theories of light was the way he constructed his mathematics. He considered a series of specific physical cases, each more complicated than the last. He moved from homogeneous solids, where he could directly replicate Fresnel's results, to crystals. In both he dealt with incident, reflected, refracted rays with concomitant intensities, amplitudes and polarization directions. While more complicated to visualize and keep track of on the page, Neumann constructed those quantities for some of the solids whose characteristics Fresnel had already measured. Neumann could compare his mathematical results directly with experiment. Mathematically, he did not go beyond reconstructing these quantities. Thus his mathematics was limited.

Neumann's method becomes clearer in his 1841 paper on light. His handling of the problems of optics was surer when he connected them, first analogically, then directly with the conduction of heat. Here there were mathematically more general cases. But because Neumann would not investigate hypothetical mechanisms that might account for the change in the character of the light in its passage through the solid, many critical constants in his equations were taken from observations already at hand. Only the consistency of these equations could be tested. It was by no means a complete theory or mathematical description.[42]

The same limitations surfaced in his work on electromagnetic induction. Neu-

41 Neumann, "Theorie der elliptischen Polarisation des Lichtes, welche durch Reflexion von Metallflächen erzeugt wirden," *Ann. Phy.* 26 (1832): 89-122.

42 Neumann, "Die Gesetze der Doppelbrechung des Lichtes in comprimirten oder ungleichformig erwärmten uncrystallinischen Körpern," *Abh. Akad. Wissen. Berlin* (1841) Part II: 1-254.

mann developed mathematical expressions for the interaction of primary and secondary circuits and of circuits and moving magnets of evermore geometrical complexity. He did this through consideration of the specific geometries of the circuits, or magnets, and the geometries of their motions. His cases were limited to closed circuits, or their equivalent magnetic fields. The induced emfs were from the movement of either circuit or magnet. Neumann assumed that the induced current was proportional to the velocity of the motion. The proportionality factor, L, was a function of the Amperean force on the secondary circuit because it changed its sign with any change in direction of the motion of the moving element. The simplest form of dependence of L on this force was linear. Therefore, "the intensity of the induced current is proportional to the component of the electrodynamic force in the direction of motion." His justification for this assumption was that it worked. Neumann could reproduce the known laws of induction.[43]

Lenz's and Ampère's experimental laws allowed Neumann to set up a general equation for a series of relationships between the elements of two circuits moving with respect to one another in a completely general geometrical sense. The result of the movement of one or both of the elements was an induced emf. Neumann took the simplest case of linear circuit elements. Having got an expression for the induced emf for that case, he considered what happened when each expression for the induced emf changed in time, that is, if the velocity of the moving element was not constant, or the current in the primary was a function of time. He then escalated the cases to non-linear circuits. Here his expression for the induced emfs were all local. To obtain the effect of the whole circuit line integrals were necessary. This took his mathematical case from infinitesimal elements to the physically probable and observable. His analysis began with carefully chosen directions to the motions, and coordinate system etc., to give him known currents in known directions. Any physical constants in these initial equations came from experiment. However, any coefficients introduced as the mathematics developed or constants introduced into the integration were defined mathematically, their physical significance remaining unexpressed. Their values are left in general integral form. Neumann deduced

43 Neumann, "Allgemeine Gesetze inducirten elektricischer Ströme," *Abh. Akad. Wissen. Berlin* (1845): 1–88. Abstract in *Ann. Phy.* 67 (1845): 31–44. Neumann's arguments are difficult to follow in large part because of his idiosyncratic mathematical notation not used elsewhere in his mathematical physics. It seems to have a point in that it kept the mathematical operations on both the electrical and geometrical elements of the circuit quite distinct from one another. At this date notation within the calculus was by no means standardized. Previously, Neumann had distinguished partial, full derivatives and variations none of which was done here. His son Carl commented on the paper and translated Neumann's notation into more conventional form. Franz Neumann, *Über ein allgemeines Princip der mathematischen Theorie inducirter elektrischer Ströme,* Carl Neumann, ed. (Leipzig: Wilhelm Engelmann, 1892). For the history of notation in the calculus see Florian Cajori *A History of Mathematical Notations* (Chicago: Open Court Pub., 1928–1929), 2 vols.

known, physical cases and could replicate known experimental results. His work here paralleled his earlier accomplishments in mineralogy using geometry. What Neumann did accomplish was the transformation of the physical problem directly into differential form without the intervention of any physical hypothesis.

In this first paper he did not rise above the consideration of particular cases until he reworked the whole by following the mathematical implications of one of his examples. Here Neumann's use of Fourier was explicit and successful. He constructed an expression for the induced current, then reconstructed it by considering the flow of electricity into an element, ds, of a closed circuit, in this case a ring, in a short period of time. The primary induced a difference in "electric tension," U, between two elements of the circuit that produced an emf, E. The fluid thus had a tendency to move from a region of high to one of lesser tension, and

$$E = -\frac{dU}{ds}.$$

The quantity of electricity traversing a cross section q of the element ds in unit time was, $-q\,k\,(dU/ds)$, where k was the velocity of flow of the electricity for a unit emf in unit time.

Neumann then used Fourier's argument for constructing the flow of heat in a ring, substituting electricity for heat, and tension for temperature.[44] He then constructed an expression for the accumulation of electricity in this section as

$$q\,k\left(\frac{d^2U}{ds^2} - \frac{dE}{ds}\right)ds,$$

where, $U = U(s,\ t)$, was the potential effective at ds at time t. Here U was analogous to temperature in Fourier's work. At this point Neumann parted company with Fourier. He did not examine the flow out of this element. Neumann simply equated the increase in electricity to $q\,ds(dU/dt)$ and equated the two expressions, obtaining

$$\frac{dU}{dt} = k\left(\frac{d^2U}{ds^2} - \frac{dE}{ds}\right).$$

He then constructed mechanical expressions that related the induced current to the electrodynamic force that produced the induced current. This was one of the simpler examples where the circuit and its motion were geometrically restricted. More general motions and circuit geometries led to more intricate mathematical expressions.

When he examined the expression for the current induced in a circuit in motion in the field of a magnet, Neumann found that it depended only on the changes in

44 Compare, Fourier, *Heat*, 86–87, for the flow of heat in a closed ring, and Neumann, "Allgemeine Gesetze," (1845), 18–19.

a function caused by the motion itself.[45] This function represented the potential between the circuit and the magnet, when a unit of current traversed the circuit. Neumann therefore reformulated his ideas on the basis of this potential function.[46]

By the end of the 1840s, Neumann rose above the mathematical particulars of each physical case when he recognized a mathematical function the potential which allowed him to reorganize his description of induction. He used this function as the organizing principle to recreate the series of physical appearances which he had previously used to construct his mathematical cases. This is an example where Neumann was able to shake off the limitation of the particular physical cases that produced only isolated mathematical results. It remained the only example where a consistent mathematical point of view generated a connected series of physically significant and known examples. Elsewhere, his insistence upon a ready reference to the physically particular and the needs of the empirical referent stifled investigations into the physical questions of his research. His mathematical descriptions of physical processes was dogged by the particular and evidential.

Neumann did not produce traditional mathematical physics. It was not possible through particular physical examples to arrive at a mathematical theory that would cover all possible cases without jettisoning the particularities of the physics. Neumann achieved this just once. He did refashion mathematical physics to the needs of physicists by considering only those cases of known physical significance. Despite these limitations, his legacy to his colleagues and students was important. Neumann deciphered the mathematically particular cases of his French predecessors in physical terms. However, his approach did not allow him to develop the physical implications of some of the mathematics developed. He limited his ideas on theory to the improvement of experiment and enmeshed the results of the latter in a context of mathematical description. The focus of his work in physics was experiment, with the mathematics too tightly controlled to enable Neumann to explore regions of physics not already visualized experimentally.[47]

Neumann was exploring new ways of doing physics. Much of it was in the realm of the mathematical expression of specific, complicated physical cases in which both physical cases and mathematical descriptions of them were obscure. This is one reason why he had a marginal role in developing theoretical physics as a distinct domain of research. The other reason was the development of electrodynamics that was more in keeping with metaphysical principles accepted by German physicists,

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45 Neumann, "Allgemeine Gesetz," (1845), sect. 9, 57. The equation to which he refers is on p. 56.

46 Neumann, "Über ein allgemeines Princip der mathematischen Theorie indukirter electrischer Ströme," *Abh. Akad. WissenḂerlin* (1847), Part II: 1-72.

47 See Carl Neumann in his commentary on Franz Neumann's paper on induction in Franz Neumann, *Allgemeines Princip.*

even while they denied that they used those principles at all.[48]

In his published lectures on mathematical physics, his closeness to the French mathematical tradition reasserted itself. In the section on hypotheses from his lectures on elasticity, Neumann assumed bodies were made up of moving mass points between which forces act. Yet after setting up a molecular level expression for pressure and tension, he dropped this approach and only considered macroscopic pressures and forces. This was the starting point for his mathematical theory. When he got down to physical specifics, he discussed cases that emerged from mathematical particulars, not physically significant cases that imposed limits on the mathematics.[49] In the case of Elasticity, Neumann followed the French model of constructing a highly abstract mathematical theory with few connections to experiment and no systematic pattern of connecting the two. It is immaterial here that, in the eyes of his students who were better trained in mathematics than he, his mathematics was not "the most elegant or the most general from the point of view of the mathematician."[50]

In his research he gave his students and contemporaries an example of how to confront the problem of relating the constructs of mathematicians with the empirical evidence of physicists. For the next generation, better trained in mathematics and in the measurement physics he helped to create, Neumann's was a legacy that was discarded in detail but one that was richly suggestive.

Tracing Neumann's influence within the work of his younger colleagues and students is more than usually problematic. There is no question that he inspired many students, and in general established a model of how to be a physicist. The particular kinds of mathematical methods and solutions he developed in his research left few traces in the work of others.

The one exception was in the research approach of Gustav Kirchhoff and particularly in his use of the potential. Kirchhoff's work on current networks was done while he was a student in Neumann's Physics Seminar. Even as a student Kirchhoff was better prepared mathematically than Neumann. His first paper was published in the form of a general mathematical theory of the distribution of electricity in a

48 Olesko, *Physics as a Calling* notes that we must largely ignore accounts of Neumann's work published by his students. They overstated his importance in an effort to gain what they thought was his rightful place in an already crowded profession in the late nineteenth century. While they, better trained in mathematical physics, could see physical implications in his work, we cannot attribute those physical insights to Neumann himself. As an example of such a student assessment see Woldemann Voigt, "Gedächtnissrede," in Neumann, *Werke,* vol. 1, 3–19. Voigt claimed that many of Neumann's papers were groundbreaking but was unable to state exactly what ground they actually broke.

49 Franz Neumann, *Vorlesungen über mathematische Physik gehalten an den Universität Königsberg,* 7 vols (Leipzig: 1883).

50 Voigt, "Gedächtnissrede," 10, Wangerin, "Neumann als Mathematiker," in Franz Neumann, *Werke,* vol. 1.

thin infinite plane. This paper was followed by an account of his own experiments to establish the form of the curves that represented equipotential surfaces across a disc. Kirchhoff tried to use Neumann's method of literally using experiment to establish the forms of the mathematical functions that represented the physical entity he would then examine mathematically. In ways strongly reminiscent of the French and Neumann, his physical concepts were measurables, and relationships with which he began his analysis were those that emerged from his own experiments.

His mathematical analysis was not limited to just those experimental cases. His work also covered the distribution of current electricity in three-dimensional networks.[51] Kirchhoff assumed only the validity of Ohm's law rewritten in terms of a function u that determined the flow of the current. This function had the properties of the potential function and allowed Kirchhoff to use some of the mathematical results of Gauss' paper on the subject. He then looked for closed curves of equal "tension." To get results back down to physically plausible cases and experiment Kirchhoff took the example of a circular plate. Details of his measurement techniques and results followed together with their comparison with his mathematics.

Kirchhoff extended this work to the physical conditions under which Ohm's law was valid and mathematically generalized these conditions. He started by asserting that Ohm's demonstration of his law was only true when the density of electricity within the conductor was the same in all directions. He considered an electrical conductor in equilibrium and the force of "free" electricity on a point within the conductor. Kirchhoff mathematically identified Ohm's electroscopic force and electrostatic potential. He also demonstrated mathematically that Ohm's condition was the only one possible. What Kirchhoff achieved was to show the mathematical compatibility between electrostatics, Ohm's work on current electricity, and his own work in the same domain.[52]

Separated from this mathematical exercise were some of Kirchhoff's ruminations on the various conjectures about the nature of electricity.[53] These conjectures

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51 Gustav Robert Kirchhoff, "Über der Durchgang eines elektrischen Stromes durch eine Ebene, insbesondere durch eine kreis förmige," *Ann. Phy.* 64 (1845): 497–514. Kirchhoff, "Nachtrag zu dem Aufsatze: Über den Durchgang eines elektrischen Stromes durch eine Ebene, insbesondere durch eine kreisförmige," *Ann. Phy.* 65 (1846): 344–349, Kirchhoff, "Über die Anwendbarkeit der Formeln für die Intensitäten der galvanischen Ströme in einem Systeme linearer Leiter auf Systeme, die zum Theil aus nicht linearen Leitern bestehen," *Ann. Phy.* 75 (1818): 188–205.

52 Kirchhoff, "Über eine Ableitung der Ohm'schen Gesetze welche sich an die Theorie der Electrostatik anschleisst," *Ann. Phy.* 76 (1849): 506-513, *Phil. Mag.* 37 (1850): 463–468.

53 Jungnickel and McCormmach, *Intellectual Mastery*, vol. 1, 154–155, see these papers as physics and hence consider the conjectures of Kirchhoff at the end of his paper as part

played no role in the mathematical parts of the paper. Kirchhoff's later work in electrodynamics and elasticity show the same reluctance to use specific hypotheses about molecular processes in nature. He preferred to base his mathematical physics on general principles and statements of laws deduced from experiments. His was mathematical physics directed towards the needs of physicists.[54]

Kirchhoff combined the two paths along which Neumann's students appear to direct their careers. Some became "measurement" physicists who replicated his concern with error analysis and carefully constructed experiments to measure small quantities. Others used the emerging methods of mathematics to construct a mathematical physics that soared far above the concerns of the empirical base that gave them the starting point for their mathematical developments. What they did share with Neumann was an aversion to hypotheses. While his students might succumb to the blandishments of highly abstract theory, Neumann's insistence that the physically plausible lay at the root of mathematical physics meant that this discipline became the mathematics of physicists and was no longer the exclusive province of mathematicians.

Wilhelm Weber

Wilhelm Weber began his career in the same decade as Neumann with the same French models and resources before him for both experimental and mathematical physics. However, because of his chosen research problems, Weber followed other models of performing experiments and mathematical physics. The interests of Neumann and Weber coincided in the 1840s when both converged on the problem of induction and the work of Ampère. The end results of that particular convergence of interests were very different. They presented to their colleagues contrasting ways of approaching an experimental and theoretical problem and visions of what constituted their solution.

Weber's interest in physics began in exact experiment without seriously branching out into mathematical physics. He analyzed the results of Poisson, Cauchy, and Navier in their theories of elasticity to extract physically meaningful results. He explicitly compared the results of the mathematicians with the experiments of physicists. However, Weber's passion was measurement. Beginning with the papers published with his brother, he examined every aspect of the act of observation, measurement, and the reduction of data. They extended this research into consideration of the psychological aspects of the relationship between observer and observed. Both Weber and Neumann were struggling with the same problems of interpreting the French and creating a methodology for a discipline.

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of his argument within the paper.

54 This is seen most systematically, not in his research papers but in his lectures on mathematical physics. See Kirchhoff, *Vorlesungen über mathematische Physik,* Kurt Hensel, ed. (Leipzig: B. G. Teubner, 1876–1891), 4 vols.

Weber's training at the University of Halle was more systematically focussed in mathematics and physics than Neumann's. He studied mathematics with J. F. Pfaff, one of the few mathematicians of his generation to understand and add to the development of analysis, and experimental physics with J. S. C. Schweigger. Weber frequently worked with collaborators in his research throughout his professional life. This began with his brother and a careful experimental examination of past work on waves, both mathematical and experimental.[55] In their experiments the Weber brothers examined both water and sound waves. They carefully defined their terminology and developed methods of producing certain kinds of oscillations on strings, plates, and in hollow cylinders. This work culminated in Wilhelm's dissertation in 1826 on organ pipes (1826) and his *Habilitation* on coupled oscillators (1827). The "laws" developed in this research were all phenomenological and descriptive, and expressed in non-mathematical language.

The brothers faced the issue of extracting physically meaningful results from the morass of mathematics on elasticity and wave propagation. Wilhelm Weber continued this in his investigation of the results of Poisson's theory of elasticity. He examined the ratio Poisson had deduced for changes in thickness and length for wires under tension. Further, Weber examined Poisson's ratio of the latitudinal to transverse vibrations of thin plates and compared them with his own measurements. Weber expressed this as, $n' = 2.05610ne/\ell$ where ℓ was the length, e the thickness of the plate, and n and n' the frequencies of the vibrations of the longitudinal to transverse vibrations. There was no extended analysis of why these particular results of Poisson's could be directly related to experiment. The results Weber deduced from his experiments were simply listed alongside the results derived from Poisson without comment. Weber's immediate papers were similarly experimental, although increasingly sophisticated in their assessment of the mathematics of elasticity and its relationship to experimental physics. Weber's ultimate publication on this subject was a review paper on experiments in the elasticity of wires and a discussion of new experimental methods.[56]

In work with Gauss at Göttingen (1831-1839) Weber's experimental energies shifted to terrestrial magnetism, its instrumentation, improvement of both instrumentation and measurement techniques for the measurement of small quantities. This included a discussion of the bifilar magnetometer and improvements in its design, and the precise layout of the terrestrial magnetism observation station in Göttingen.[57]

55 This resulted in Wilhelm Eduard and Ernst Heinrich Weber, *Wellenlehre, auf Experimente gegrundet, oder über die Wellen tropfbarer Flüssigkeiten mit Anwendung auf die Schall- und Lichtwellen,* Leipzig: Fleischer, 1825.

56 Wilhelm Weber, "Über die Elasticität fester Körper," *Ann. Phy.* 54 (1841): 1–17. This work emerged from Weber's sustained examination of the operation of the magnetometers used at Göttingen to measure the earth's magnetic field.

57 Weber, "Bemerkung über Einrichtung magnetischer Observatorien, und Beschreibung der darin aufzustellenden Instrumente," *Resultate* (1837): 13–33. "Beschreibung eines kleinen Apparats zur Messung des Erdmagnetismus nach absolutem Maass für Reisende," *Resultate* (1837): 63–89, "Bemerkungen über die Einrichtung und den Gebrauch des

Weber's interests did not remain with instrumentation, measurement and data reduction. His role in the development of Gauss' ideas on absolute measurements is unclear but seems substantial.[58] The issue of absolute measurement became more important as he investigated how to use the magnetometer as a galvanometer using the phenomenon of induction.[59] This led him to a study of induction and the question of the measurement of electrodynamic quantities that converged in his first massive paper on the subject.[60]

Within "measurement physics" Weber continually confronted the problem of making plausible connections between what he observed with his instruments, motions due to mechanical forces, and what he was supposedly measuring, magnetic then electrical quantities.[61] Weber confronted the need for a theory connecting the three domains of physics–the mechanics of measuring instruments, magnetism and then galvanic electricity.[62] The phenomena that drew these three domains of experimental physics together were those of electrodynamics and induction. The systematic expression of their relationships were in Ampère's empirical laws on electrodynamical phenomena and Faraday's on induction. Weber began his search for a theoretical net that connected these different domains of physics together in a theory of the measurement in absolute, i.e., mechanical terms, of the measurables of Ampère's and Faraday's experiments. This was a theory that originated in the new measurement physics. Measurement did not merely serve to present mathematicians with expressions that formed the initial and/or final points of their

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Bifilar-Magnetometers," *Resultate* (1837), "Über den Einfluss der Temperatur auf den Stabmagnetismus," *Resultate* (1838): 38–57.

58 See May's remarks in May,"Gauss," *Dict. Sci. Bio.* and Archibald, "Tension and Potential," *Centaurus,* 31 (1988): 141–163.

59 Weber, "Der Induktor zum Magnetometer," *Resultate,* (1839): 86–101, "Der Rotationsinductor," *Resultate,* (1839): 102–117, and "Unipolare Induktion," *Resultate,* (1840): 63–90, and "Messung starker galvanischer Ströme bei geringem Widerstande nach absolutem Maasse," *Resultate,* (1841): 83–90.

60 Weber, "Elektrodynamische Maassbestimmungen," *Abh. Leipzig* (1846): 209–378, and in Weber, *Werke,* 6 vols., (Berlin: Springer, 1892–1894) vol. 3, 27–214. Abstracted in *Ann. Phy.* 73 (1848): 193–240. The abstract was translated as Weber, "On the Measurement of Electrodynamic Forces," in *Taylor's Scientific Memoirs* 5 (1852): 489–529. The painstaking experiments to examine Ampère's law were cut short in the abstracts as were Weber's descriptions of his instrumentation. Only the skeleton of his theory remained.

61 This is different from Neumann's measuring problems in his work on heat and light because thermal and optical properties were measured directly by the instrumentation.

62 For an account of the political complications of Weber's academic career and his relationship with Gauss and the role of Gauss in the development of his ideas, see Jungnickel and McCormmach, *Intellectual Mastery,* vol. 1, 130–140. Their account pps. 140–144, of the contents of Weber's first electrodynamics paper discussed here is different from my own and assumes a great deal that I am exploring as problematic.

analysis. The analysis of the problems of measurement themselves were the stepping stones into a discussion of a theory that joined distinct types of phenomena together with imagery and mathematics. Experiment was enmeshed finally in specific physical imagery, expressed mathematically, whose physical meaning was investigated using mathematics as the language of exploration. Physical imagery, as Weber knew, did not come with measurement; it was hypothetical and necessary.

In the abstract to his paper Weber made this explicit by comparing the development of electrodynamics with astronomy. In the former, no principle linked the laws of magnetism and electromagnetism together. This was unlike astronomy where Kepler's laws were joined together through Newton's principle of gravitation. Newton's theoretical leap led to much research in astronomy. In electrodynamics Ampère's work had not led to such research. The known induction phenomena were discovered independently of Ampère's research. To press the point home Weber cited Faraday's work. If one could develop the "true" laws of Electrodynamics, they would serve as a "guide to different classes of phenomena" as had happened in astronomy. Unlike astronomy, there was no serviceable combination of theory and observation available in electrodynamics. While Ampère provided the mathematics, there was no idea equivalent to gravitation that joined disparate aspects of electrodynamics together. A vital link was missing,

> in the development of electrodynamics no such combination of observation with theory has occurred as in that of the general theory of gravitation. Ampère who was rather a mathematician than an experimenter, very ingeniously applied the most trivial experimental results to his system and refined this to such an extent, that the crude observations immediately in question no longer appeared to have any direct relation to it.[63]

Ampère's mathematics was remote from its experimental foundations. Its origins could no longer be traced through the refinements of the mathematics.

Part of Weber's objection was that Ampère did not actually measure the quantities he examined in his experiments because his were null-point experiments. Ampère had not demonstrated what he claimed. The solution lay in improving methods of observation and in the careful comparison of specific points of theory with experiment. This would provide for the introduction of the "spirit of theory in observation, without the development of which no unfolding of its powers is possible." This meant that Weber was committed to the development of a theory that did not lose sight of its physical, empirical foundations. In addition Weber had further ambitions, to join into one theoretical whole electrostatics and electrodynamics as well as induction.

His massive paper had many layers to it. To characterize it as the presentation

63 Weber, "Elektrodynamische Maassbestimmungen," 214, *Werke*, 30–31, and *Taylor's Scientific Memoirs*, 490.

of a theory of electrical action misses many of Weber's intentions, and most of the text. Weber devoted much space to reestablishing Ampère's law experimentally. His problem was to construct the fundamental laws of electrodynamics through the actual measurement of electrodynamic forces, not through the precarious balancing of such forces against one another. By investigating the rotation of one coil within another, while both carried currents, Weber reconstructed Ampère's law.[64]

Weber immediately inverted the role of Ampère's law. It became the foundation for the measurement of all electrodynamic quantities. To do this Weber had to demonstrate that using Ampère's law he could construct the moment of rotation for a coil in motion induced by a current. By comparing experimental results to an extended form of Ampère's law, Weber established that the moment of inertia for this rotation gave a direct measure of the current.[65] Mechanical quantities, movements, rotations, moments of inertia, were a direct measure of electrical quantities. Weber then launched himself into an empirical examination of induction. What he hoped to establish, although he could only state it not prove it, was that the induction phenomena Faraday had uncovered could not be used as a foundation for understanding Ampère's law. Only the inverse was possible.[66] Weber thus secured the absolute status of mechanical measurement and quantities over electrical or magnetic ones. Weber contended that if we begin with Faraday's results and induction, we could only reach Ampère's law on a case by case basis by replacing the electrodynamic activity of the current by an equivalent magnet. Constant current phenomena were not encompassed in this analysis. In contrast, Ampère's law assumed constant currents but offered the possibility of striking out beyond this case by assuming the general mathematical case of currents as functions of time, and hence contained the possibility of explaining induction.

Weber approached the problem of induction in several steps, while also introducing his fundamental assumptions about the nature of electricity and electric currents in as low key and unobjectionable a way as possible. At the same time he developed more and more general mathematical cases as his argument became less tied to laboratory visualization. In so doing Weber drew together all three aspects of electrical science. At work in this section of the paper are three levels of concern, mathematical abstraction, physical hypotheses, and the development of the imagery made possible by expressing the hypotheses in the language of mathematics.

It was no accident that Weber began in electrostatics and from there deduced

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64 After his series of experiments Weber concluded that the agreement between observed and calculated values could not have been better. Ampère's fundamental law was confirmed in its most general and important consequences. Weber, "Elektrodynamische Maässbestimmungen," 249, *Werke*, vol. 3, 69.

65 Weber, "Elektrodynamische Maassbestimmungen," 249–268.

66 Weber, "Elektrodynamische Maassbestimmungen," sect. 18, 305–307, *Werke,* 132.

Ampère's law. The introduction of electric charge as a center of force was unobjectionable to his German audience. Weber was not the first to assume that electric current was the motion of oppositely charged particles within a conductor. Fechner had introduced the idea a year earlier. Fechner's unlike charges attracted when they moved in opposite directions. Fechner had explained induction for the case of parallel wires starting in Ampère's electrodynamic law.[67] Weber set his charges in motion parallel to one another and then took only their relative motions into account. The resultant force that emerged from all possible combinations of moving charge interactions was

$$+8\frac{e\,e'}{r^2}\,a^2\,uu',$$

where a^2 was a constant, e and e' were charges with velocities u and u' at a distance r apart. He then generalized this particular case to one with relative accelerations of his point charges as well as velocities. A term of the form,

$$1-a^2\left(\frac{dr}{dt}\right)^2+b\frac{d^2r}{dt^2},$$

had to be multiplied into the expression for the force.

To get from electrostatics to induction, Weber introduced mathematical results deduced from Faraday's work to show the plausibility of the approach, if not a cast-iron mathematical case. Weber was looking for a general differential mathematical expression for induction. Therefore, he started with the most general expression of Ampère's law, reworked into a form deduced from his starting point, electrostatics. He had to construct expressions for each possible moving charge in one conducting element acting on the two possible moving charges in the other element separating from one another to produce the current. By translating both the velocities and the cosines of the angles involved into differentials of distance and manipulating them algebraically, Weber obtained an expression for the force between two charges that contained accelerations along the conducting elements

$$\frac{ee'}{r^2}\left(1-a^2\left(\frac{dr}{dt}\right)^2+2a^2\,r\,\frac{d^2r}{dt^2}\right).$$

Weber claimed this expression as the basic law of electrical action that stemmed from Ampère's laws. It was a "precise expressions for an extensive class of cases" and was "without hypotheses."[68]

He followed this by reconstituting Ampère's law from electrostatics in the case for constant currents.[69] He concluded that the important elements were relative

67 Fechner, "Über die Verknüpfung der Faraday'schen Induktion-Erscheinungen mit den Ampèreschen elektro-dynamischen Erscheinungen," *Ann. Phy.* 64 (1845): 337–344. Weber appraised Fechner's ideas in his section on induction, Weber, "Elektrodynamische Maassbestimmungen," 347.

68 Weber, "Elektrodynamische Maassbestimmungen," 327, *Werke,* 157.

69 Weber, "Elektrodynamische Maassbestimmungen," sect. 22, 327–334.

motions and accelerations of the charges as well as their masses and distances apart.

Weber required one additional assumption to deduce the laws of induction from his basic imagery of moving electric charges; the induced emf was measured as the difference between the forces acting on the moving charges of the secondary by the moving charges of the primary. He took the simplest case, the imbalance of force from the unequal motions, however produced, of the charges moving in the primary circuit induced the emf in the secondary circuit. Since the emf was measured along the circuit element of the secondary, he needed only that component of the resultant force.[70] Assuming constant currents and one circuit moving with respect to another and through some ingenious algebraic manipulations of the velocities as derivatives with respect to time of their distances from one another, Weber obtained an expression for the induced emf for parallel closed circuits where currents were flowing parallel and antiparallel. In the last section of his paper Weber confronted the results of Fechner's paper and Neumann's first paper on induction.

Both Weber and Neumann built their mathematical case through the consideration of physical particulars. Yet, Weber's is truly a **physical** theory of electrical and electrodynamical phenomena, developed in the language of the calculus. The physical imagery was powerful, developed mathematically as far as was necessary to demonstrate a particular known physical case, and in certain circumstances the general case. Like Neumann, Weber started from empirical laws and constructed other, known laws directly. However he used the specifics of a physical model and the actions of moving electric charges. Neumann's path was mathematically convoluted and offered no sense of the underlying physical process. In contrast, Weber constructed a specific physical model and traced physical processes through that model. Beginning with his ideas on electric currents as the separation of charges, Weber constructed known empirical laws. The consistency, simplicity and Kantian familiarity of the imagery were powerful. Finally, Weber kept mathematics subservient to the requirements of the physical cases and the physicality of the mathematics was obvious. He introduced into physics that which Neumann avoided, specific imagery. In his commentary on his mathematics, Weber wrote in terms of the physical content of the mathematics, velocities, distances, forces, emfs, not in terms of functions, coefficients, etc. The latter were part of Neumann's commentary on his developing mathematics even as the physical case that set up the mathematics is visually explicit.

One of Weber's criticisms of Neumann's induction paper was physical. While Neumann could replicate the known laws of induction, there was essentially no physical sense of what was actually going on. To Weber, induction arose from a "mutual exchange of electric currents" and any explanation of these phenomena

70 Weber, "Elektrodynamische Maassbestimmungen," sects. 24–25, 336–346, and *Werke,* 116–126.

"must be based on the consideration of this mutual exchange." This pointed to the foundations of what physical theory should consist of and its relationship to process. There was, for Weber, no "inner coherence" in Neumann's work on induction. The inner coherence of mathematics was no longer sufficient. For Weber it was "remarkable" that Neumann's law agreed with known empirical rules.[71] Whether we take this as irony or not, Weber had not spotted the coherence and conjunction between the mathematics of their approaches that lay in the potential.

More importantly, Weber was calling for a new foundation upon which to build physical theory. What Weber constructed here, through simple powerful imagery based on philosophically familiar assumptions, was a theory of the processes of nature that drew together a wide range of phenomena that had, until then, remained isolated on an explanatory level. Neumann's mathematical description also drew some of these same phenomena together, but as Neumann acknowledged, there was a mystery at the bottom of his mathematics which eluded him.

There was only one aspect of theoretical physics not encompassed in Weber's massive paper, prediction. There was also a price to pay for using specific imagery, alternatives that appeared quickly and often after the publication of Weber's work. The basis of Weber's work remained an hypothesis that could, at best, be compatible with phenomena, but could never be established as necessary beyond its usefulness.[72]

Whatever the ontological difficulties of using specific physical imagery, or the direction of Weber's career after this 1846 paper, the terms of this achievement were the basis for theory within German physics.[73]

The terms of the debate shifted. Arguments were about specific physical models, their adequacy as images of the operation of nature, their implications as images of nature, and whether they were understood by their protagonists or not. Mathematical prowess was no longer an issue within the community of physicists. It was assumed as a prerequisite for entry into the discipline. The grounds upon which its use were judged had also changed. Was the mathematics suitable and employed well enough for the physical cases at hand? Physicists had established criteria of judging theories within physics expressed in the language of mathematics that were no longer dominated by the criteria of mathematicians. German physicists used mathematics in a variety of ways to explore the structures and processes of nature, from the formal and highly abstract to the exploration of the implications

71 Weber, "Elektrodynamische Maassbestimmungen," and *Werke*. 178.

72 See, Maxwell, *Treatise on Electricity and Magnetism*, vol. 2, 486-487. Even though Maxwell was writing about an approach that rivaled his own, he had already moved away from specific imagery as a foundation for his electromagnetic theory. He chose the foundation of the general laws of mechanics.

73 Measurement remained Weber's passion and he explored absolute measurement in other areas of electrodynamics. For details of Weber's later research see, Jungnickel and McCormmach, *Intellectual Mastery*, Vol. 1, 144–148.

of very specific mechanical models.

From the publication of Weber's paper, hypotheses and imagery were accepted and became integral facets of German physics. By 1870 a full range of such approaches was exploited in this community, and two important figures that consolidated the use of mathematics, imagery combined with experiment within the discipline of physics were Rudolph Julius Emmanuel Clausius and Hermann von Helmholtz.

Clausius and Helmholtz

As Weber and Neumann reached the midpoints of their careers, a new generation of physicists, some of whom they trained, began to publish their first pieces of research. This later generation had before them a series of research problems, together with examples of solutions that incorporated the new standards and methods of physics. This younger generation also could take advantage of the systematic training now offered in both the experimental and mathematical aspects of this new discipline, together with training in higher mathematics offered at German universities. However, the profession and discipline of physics was not so well defined that the unorthodox might not enter. Hermann von Helmholtz, one of the most important members of the generation that came to maturity in the late 1840s, received no systematic training in higher mathematics or mathematical physics. At the other end of this educational spectrum, Rudolf Julius Emmanuel Clausius received all the training now offered for an aspiring physicist. At the University of Berlin he worked with Dove and Magnus in experimental physics and heard Dirichlet's courses in mathematics.[74]

Clausius' research was never the familiar nineteenth-century mix of experiment and mathematics. He was a theoretical physicist and never published any experimental research although he was always well aware of it. Experimental results were the starting point of all his research, and incorporated into his explanatory compass.[75] His career and the courses he taught reflect the deviant and difficult professional path he chose.[76] He was also one of the first German physicists to be fully

74 The courses from Dirichlet included potential theory and differential equations. He also heard Dirksen's lectures on analytical functions and Steiner on function theory.

75 He included in his research the problems of engineers, publishing a text on the steam engine and on the design of electric motors. See Clausius, "Über die Anwendung der mechanischen Wärmetheorie auf die Dampfmaschine," *Ann. Phy.* 97 (1856): 441–476, 533–558. Clausius, "Zur Theorie der dynamoelectrischen Maschinen," *Ann. Phys.* 20 (1883): 353–391.

76 From 1844–1850 Clausius taught at a Gymnasium in Berlin and from 1850 was professor of physics at the Royal Artillery and Engineering School. He received a call as Ordinarius to the Polytechnic in Zurich in 1855 and then Würzburg in 1867, returning to Prussia and the University of Bonn in 1869. In Berlin and Zurich he taught physics courses to engineers.

competent in contemporary mathematics, and to manipulate it for his own ends. The paramount importance of mathematics for physics in the 1850s is illustrated by Clausius' reaction to criticism of the quality of the mathematics in his theory of heat. In a subsequent edition he published a chapter on the differential equations of thermodynamics.[77] Even in his dissertation and first publications, the explanation of the physical processes behind the mathematics was important to Clausius. He traced the pathways of the light within hollow vesicles in the atmosphere that formed rainbows and other meteorological color phenomena.[78] However, Clausius soon became bolder and his work more central to the changing range of research problems and methods of mid-century German physics. Clausius was matched in his mathematical and theoretical ability by Helmholtz and throughout their careers they dogged each other's theoretical steps.

Helmholtz's training lay in medicine. The only mathematics or physics he heard formally was that appropriate to his future career as a physician. His doctoral dissertation was a thorough exercise in systematic observational, microscopic physiology. His observations on the nervous systems of invertebrates was important and workmanlike. As with Clausius' equally workmanlike dissertation in physics, this research established his competence in his field. As a medical student Helmholtz's training appears to have been thorough, thoroughly academic, and requiring sheer physical stamina to stay the course. Recounting the well-known details of his early career Helmholtz, along with his mentor Johannes Müller and his other students, were determined to make physiology an exact, experimental and quantitative science. Beyond that they shared a commitment to explanatory schemes that relied on physics and chemistry. This determination to excise vitalism from physiology and substitute a mechanistic explanation of life was fraught with political overtones that instantly labeled the whole group radical. This label persisted despite the fact that neither Helmholtz nor Du Bois-Reymond supported such political visions.[79]

In the first decade of his career Helmholtz pursued academic research while a practicing physician for the army. Although Helmholtz attacked vitalism directly, his research depended on his army postings. The subject of his early research was a study of putrefaction and fermentation. With respect to the question of vitalism, the outcome of these experiments was ambiguous. This line of research

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77 See Clausius, "Über die mechanische Wärmetheorie," *Poly. J.* 150 (1858). The mathematical parts of this paper were expanded and published as Chapter 1 of Clausius, *Abhandlungen über die mechanische Wärmetheorie* (Braunschweig: Vieweg, 1864–1867) 2 vols, translated into English by T. Archer Hirst as *The Mechanical Theory of Heat* (1867).

78 Clausius, "Über die Lichtzerstreuung in der Atmosphäre," *J. Reine Angew. Math.* 34 (1847): 122–147: "Über die Intensität des durch die Atmosphäre reflectirten Sonnenlichts," same journal 36 (1848): 185–215.

79 See Frederick Gregory, *Scientific Materialism in Nineteenth Century Germany* (Hingham MA.: Reidel, 1977).

was cut short by his posting from Berlin to Potsdam, far enough to cut him off from necessary laboratory resources of Müller. Helmholtz then turned to the study of animal heat and the heat generated by muscular action. Here he demonstrated that chemical changes occurred in working muscles and a year later, 1848, that heat was generated by muscle contractions.

In 1847, in the middle of this research Helmholtz published "On the Conservation of Force." The extent of his physicalist image of life became clear, as well as the Kantian foundation of his physics. His physical explanations were based upon the idea that matter was made up of point masses between which were attractive or repulsive forces. Helmholtz's work in the 1880s on the Principle of Least Action served to reemphasize his commitments.[80]

In 1848, the considerable pressure friends at the center of Prussian academic life in Berlin brought to bear on the army and the Kultus-Ministerium, together with Helmholtz's research, led to his release from his army obligations. He never practiced medicine again, although his research over the next twenty years was mainly in physiology beginning in his experimental work on the velocity of nerve impulses. His research again was guaranteed to undermine confidence in vitalism and build his own physicalist ideas. It also led him into the instrumentation of electrical experiments and their improvement to measure small time intervals and electric pulses. Helmholtz treated physiological problems with the quantitative methods of experimental physics.[81]

These same physiological problems became the starting point for many of Helmholtz's forays both experimental and theoretical physics. The experiments on nerve impulses led him to consider electric pulses and this expanded into an

80 Helmholtz, *Über die Erhaltung der Kraft, eine physikalische Abhandlung vorgetragen in der Sitzung der physikalischen Gesellschaft zu Berlin am 1847* (Berlin: G. Reimer, 1847), translated as "On the Conservation of Force," *Taylor's Scientific Memoirs* 2 (1853): 114–162, trans., John Tyndall. This essay was republished throughout Helmholtz's life. In the edition of 1881 Helmholtz reaffirmed his commitment to Kantianism. See Helmholtz *Über die Erhaltung der Kraft, eine physikalische Abhandlung* in Helmholtz, *Wissenschaftliche Abhandlungen* (Leipzig: Barth, 1882–1895) 3 vols., vol. 1, 12-68, Appendix. On Helmholtz's Kantianism see, Peter Heiman, "Helmholtz and Kant: The Metaphysical Foundations of *Über die Erhaltung der Kraft*," *Studies Hist. Phil. Sci.* 5 (1974): 205–238. On Helmholtz's monocycles see Günther Bierhalter, "Zu Hermann von Helmholtzens mechanischer Grundlegung der Wärmelehre aus dem Jahre 1884," *Arch. Hist. Exact Sci.* 25 (1981): 71–84, and "Die von Helmholtzschen Monozykel-Analogien zur Thermodynamik und das Clausiussche Disgregationskonzept," same journal 29 (1983): 95–100.

81 Helmholtz, "Über die Dauer und den Verlauf der durch Stromesschwankungen inducirten elektrischen Ströme,"*Ann. Phy.* 83 (1851): 505–540. Helmholtz needed a theory of his measuring instruments to convince his colleagues, even Müller, of the physiological validity of his work. His work using this instrumentation led later to a specific image of color reception. See Timothy Lenoir, "Helmholtz and the Materialities of Communication," *Osiris,* 9 (1994): 185–207.

interest in the problems of induction and his critical overview of the whole domain of electrodynamics of the 1870s. He followed the same general path in his investigation of the sense of hearing. Hearing led him to the consideration of the actual motion of air in open-ended pipes to the motion of air at the end of those pipes and problems of gases with internal friction and hydrodynamics. For Helmholtz, physiology led to more general experimental physical issues and more abstract questions of "theoretical" physics.

It was not until 1870 that Helmholtz received the call to Berlin and a chair in Physics. His research work in physiology ceased. This is a reminder of just how long it took him to formally enter the profession he had published in with great distinction for twenty years. Helmholtz's career is also a reminder of how fluid the term "Physics" still was in mid century. All of this is to set Helmholtz's research in physics in the context of the constraints of his education, then opportunities that his research gave him to transcend those limitations and encroach on the turf of the field that he worked to make his own. Throughout the 1840s Helmholtz educated himself in higher mathematics, initially to understand how to use mathematics in physiology. Helmholtz realized that his vocation was physics and took every opportunity to drive his research into physics.

The closeness of his work in physiology and physics is illustrated in his work on conservation of force. The principle itself was actually stated in a review paper on animal heat of 1845.[82] After surveying work done by Davy and Lavoisier on the issue, he examined Leibig's paper on the origins of animal heat. Stating that it was of interest to physics in general as much as physiology, Helmholtz asserted that the principle of the constancy of force-equivalence was already used as the foundation of mathematical theories. As examples he cited Carnot's and Clapeyron's determination of the work contained in a given quantity of heat and Neumann's theory of currents induced by moving magnets. Helmholtz took his principle of "conservation of force" as empirically grounded and "theoretically stated and well known." The material theory of heat was doomed. Helmholtz then conjectured that, "if we substitute the motion theory of heat for the material theory of heat, we see heat as originating from mechanical force." From this it followed that chemical, electrical, and mechanical force were equivalent, and Helmholtz cited some of the experimental evidence he would use in his 1847 paper to demonstrate this.

> If we accept heat as motion we can firstly assume mechanical, electrical, and chemical forces as equivalent to one another, as complement to a kind of transformation of one force into another. For mechanical force exists yet no experiment demonstrates this; the work of Carnot and Clapeyron

82 Helmholtz, "Bericht über die Theorie der physiologischen Wärmeerscheinungen für 1845," *Fort. Phys.* (1845) [1847]: 346–355. Reprinted in Helmholtz, *Wissenschafte Abhandlungen,* vol. 1, 1–11.

> and Holtzmann seem not to point to the production but to the diffusion of heat. In the case of chemical forces the heat equivalent (latent heat) has been determined for a series of chemical processes and the law of the constancy of heat production which bind two substances together is known. For constant electric current it follows from the law of Ohm and Lenz and established empirically by Becquerel from the heat developed during electromechanical change.[83]

This was the outline of his argument in the paper on the conservation of force. What was missing, and added to the 1847 paper, was a vision of matter that allowed him to illustrate mathematically what he believed was the universality of his principle. Helmholtz's paper on the conservation of force was ambitious. In describing this paper as one in physics, Helmholtz was using the term in its older sense, not in the sense that had been built up through the research, discipline and profession called physics over the previous two decades. "Physics" included all the experimental sciences and he drew on evidence from chemistry etc., in arguing for the conservation of force. Helmholtz read his paper on the conservation of force to the Berlin Physical Society. Similar papers were read after he left the city. Even though the physicists from the University of Berlin also attended, few understood the implications of the paper until it was explained to them by Dubois-Reymond. Reactions to the paper seem to reflect the idea that this was a paper in a discipline not defined by the contents of the *Annalen der Physik* to which it was submitted. While Poggendorff was busy narrowing the definition of the discipline, Helmholtz was trying to urge a broadening of it in directions that Poggendorff had repudiated as a young editor in the 1820s.

Mathematical physicists might begin with the generalized mathematical solution to a problem from which particular solutions were extracted, Helmholtz began from the most general metaphysical principles, from which he extracted specific physical results in mathematical form. This led him from mechanics to other important research areas within physics, namely electricity and the nature of heat. Forces brought about change yet they were themselves conserved. For Helmholtz the physical question was the measurement of this conservation. In his model force was reduced to the mechanical forces of attraction and repulsion. In a closed system of mass particles, change was measured by alterations in *vis viva*. Helmholtz reexpressed this change in terms of the changes in the "intensity of the force," that is in the potential of the forces that acted between such particles,

$$\frac{1}{2}mv_1^2 - \frac{1}{2}mv_2^2 = \int_r^R \phi dr,$$

where, m is the mass of the particle, whose velocity changes from v_1 to v_2. ϕ was the intensity of the force constructed by considering changes in v^2. Both

83 Helmholtz, "Bericht über physiologische Wärmeerscheinungen," (1845), 6.

the velocity and force X, Y, Z were functions of the coordinates x, y, z only and Helmholtz expressed this as

$$\frac{1}{2}md(v^2) = Xdx + Ydy + Zdz.$$

Defining the x-component of the intensity of the force as $X = (x/r)\phi$ Helmholtz showed that for central forces, if the *vis viva* was conserved, then so was the intensity of the force. He then generalized this to a system of an arbitrary number of such centers of force.

In his discussion of heat Helmholtz used examples from physiology, chemistry, and electricity to argue that heat was not a substance but a measure of the *vis viva* of thermal motions. (Latent heat, a measure of the forces between atoms that changed with the changes in position of those atoms.) The nature of atomic motions were unknown and unknowable. It was sufficient simply to understand that heat was motion. To make this plausible, Helmholtz turned to systems in physiology in which *vis viva* was not conserved, and Joule's experiments on friction. In both cases heat was generated but until now neither the increase in "tensional" force within the body, nor the extraction of mechanical effect, had been taken into account. Helmholtz asked whether in these cases the "force" developed equaled the mechanical force lost and when mechanical force disappeared was a definite amount of heat always developed? If so, then there was a quantity of heat equivalent to mechanical force. The evidence Helmholtz introduced for this argument included experiments on exothermic chemical reactions and Joulean heating. To counter the objection that in induction no heat was generated, Helmholtz stated that there was no source of heat because there was no transfer of material substance. To reach an estimate for the mechanical equivalent of heat, he cited Joule's and Holtzmann's experiments on the compression of gases, those on the velocity of sound, latent heat, and the expansion of water vapor with temperature.

The unifying concept throughout this paper was "tensional" force and its intensity. Helmholtz used this concept to extend his argument into electricity to obtain the "force equivalent" of electrical processes. Thus, using his principle, the change in the *vis viva* of two charges moving from distance r to R apart was

$$\int_1^2 \frac{1}{2}md(v^2) = -\int_r^R \phi dr,$$

He identified ϕ with Gauss' potential function. With his paper Helmholtz gave the potential function physical meaning. He then drew into this new conceptual net of force equivalence the heat generated by galvanic currents and a physical analysis of the results of Neumann's first paper on induction.

The physiological intention of the paper surfaced in Helmholtz's afterword. He wanted to address issues for live matter, but could only show such principles for inanimate processes. Hypotheses, and the condensation of the meaning of disparate phenomena into one general principle, had once again entered German physics. In this paper Helmholtz displayed a pattern of argument that he repeated throughout his life. He surveyed an existing set of known phenomena and their explanations in a domain replete with conceptual ambiguity and explanatory confusion and contestation. He then cut through the confusion to bring out the physical essentials of the cases and pinpointed a method, experimental, mathematical, or in this case conceptual, to sweep away the ambiguities and confusions and open up new ways of dealing with whole domains of physics. He then explored these new domains himself, mathematically and experimentally.[84]

Sweeping metaphysical principles, and undemonstrable models of matter even if they led to useful results were the antithesis of the systematic, experimental quantitative approach and the mathematical description of these results that constituted many pages of the *Annalen der Physik.* Weber, in the theoretical sections of his electrodynamics paper, had kept close to his own experimental results. He had introduced his moving, charged particles as a principle only after discussing the problems of the approaches of physicists that denied the necessity for hypotheses. He also used hypotheses only to unify phenomena he had investigated himself. Helmholtz had done none of the experiments he cited. The mathematical content of his paper was minimal, just enough to demonstrate the physical point but no more. There were no sophisticated developments of its implications, and certainly not of the mathematical caliber already displayed by Clausius.[85] When Clausius addressed the same mechanical problem of deducing a conservation law for a system of mass points his mathematical understanding of the problem was much deeper and more carefully stated.[86]

The place of Helmholtz's paper within the body of the discipline of physics in the 1840s was peculiar. It simply set aside all the standards of the discipline and was a measure of Helmholtz's distance from it. Reaction to the paper was less than enthusiastic. It was not surprising that this brilliant but rambling paper was rejected for the *Annalen der Physik.* Many physicists could not follow his argument.

84 See his work in hydrodynamics, of the 1850s, that had implications for both mathematics, as well as acoustics, and the physics of gases. He performed experiments on the behavior of gases and examined fluid behavior mathematically. This was repeated in his reexamination of electrodynamics. He was less successful in his attempt to bring unity to the physical sciences using the Principle of Least Action.

85 This is not to say that Helmholtz was incapable even at this early stage in his career of understanding or producing such mathematics. See his analysis of Challis' theory of sound published the following year. Helmholtz, "Bericht über die theoretische Akustik betreffenden Arbeiten von Jahre 1819–1848," *Fort. Phys.* 4 (1849): 101–118, 124–125, and 5 (1850): 93–98. He recognized Challis' work as an exercise in mathematics rather than physics. Challis was chasing a particular integral in the equations of hydrodynamics.

86 Clausius,"Über das mechanischen Aequivalent einer elektrischen Entladung und die dabei stattfindende Erwärmung des Leitungsdrahles," *Ann. Phy.* 86 (1852): 337–375. Translated in *Taylor's Scientific Memoirs* (1853): 1–32.

Helmholtz's use of the term force was ambiguous. It was a multipurpose word to cover much the same ground as Ohm's usage of it thirty years beforehand. By force Helmholtz meant *vis viva*, potential, a term he took from Gauss but only saw in relation to galvanic electricity, and mechanical potential. Kirchhoff had only recently explicated Ohm's law in terms of potential. Helmholtz had to personally win over Neumann to his ideas.

> ...after a severe struggle, I have converted a bold mathematician, who gets confused over non-mathematical logic, and is himself a lecturer in mechanics, to the doctrine of conservation of force, so that it is now official doctrine in this University. Neumann is rather difficult to get at; he is hypochondrical and shy, but a thinker of the first order.[87]

Older physicists such as Magnus were dubious, even hostile to his work.[88]

In the period immediately following its publication, Helmholtz's memoir was not mentioned in the debates over the nature of heat and the mathematics in which to express it. Of the younger physicists Clausius was the one who understood Helmholtz's work, its limitations, and the challenge that it offered his own work in the domain of heat theory. Clausius' approach to the theory of heat was similarly dependent on the experiments of others. His analysis of the problems with current ideas on heat depended on the ambiguities inherent in the experimental record itself. In addition, he did not leap from these contradictions to a grand principle of nature. Instead he built up his case for his assumptions about the nature of heat through a series of well considered, special physical cases presented in a succession of mathematical papers.

In these mathematical papers Clausius reinterpreted Clapyeron's mathematical version of Carnot's work.[89] His reworking meant that Clausius kept much of the mathematical analysis developed by Clapyeron. Its physical foundation required reexamination and Clausius gave many of Clapyeron's results physical meaning. For Clausius the mathematical characteristics of the functions that entered his equations determined the physical characteristics of his system. The only functions and equations he pursued were those he saw as having physical utility. In his analysis of the Carnot cycle, Clausius chose the ideal gas as his physical system. He reduced the general problem of the ideal heat engine to analyzing the changing state of a gas as it traversed an infinitely small cycle. Clausius constructed the expression for the heat added or expelled for each leg of this cycle.[90] He then

87 Letter from Helmholtz to DuBois-Reymond in, Königsberger, *Helmholtz*, 64.

88 Those enthusiastic about his work were physiologists, such as DuBois-Reymond already committed to a mechanical vision of life.

89 The mathematical character of Clausius' papers is examined in Eri Yagi,"Clausius' mathematical Method and the Mechanical Theory of Heat," *Hist. Stud. Phys. Sci.* 15 (1984): 177–195.

90 Clifford Truesdell, *The Tragicomical History of Thermodynamics* argues that Clausius'

constructed the expression for the inverse of the mechanical equivalent of heat, A, as the ratio of the heat expended over the work produced, "the equivalent of heat for the unit of work." He then added all his expressions for the heat added going around the cycle. Using the accepted mathematical expression for the work done by an ideal gas, the area within the cycle in the P-V diagram, A became

$$\frac{d}{dt}\left(\frac{dQ}{dv}\right) - \frac{d}{dV}\left(\frac{dQ}{dt}\right) = \frac{A.R}{v},$$

where dQ was the heat added, $a + t$ the absolute temperature of the gas, and V its volume. R was the gas constant. Clausius continued that the above expression showed that,

> Q cannot be a function of V and t as long as the two latter are independent of one another. For otherwise, according to the known principles of the differential calculus, that when a function of two variables is differentiated according to both, the order in which this takes place is a matter of indifference, the right side of the equation must be equal to zero.[91]

The equation could be brought under the form of a complete differential,

$$dQ = dU + A.R\frac{a+t}{V}dV,$$

where U was an arbitrary function of volume and temperature. The above expression was not integrable until the relationship between V and t was established. Clausius gave both U and the other terms in this equation physical significance. U was the heat necessary for internal work, and depended only on the initial and final condition of the gas. The second term, the external work, depended on the initial and final states of the gas and the path taken between those two states. In his succeeding papers on the second law, Clausius introduced $1/T$, where T was the temperature, as a multiplier of dQ to make a complete differential of the form $Xdx + Ydy$.[92]

This was the antithesis of Helmholtz's approach. Clausius avoided philosophical explanations of any kind and hid his particular theory of matter. He drew his theoretical conclusions on the nature of heat directly from the results of experiment

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analysis is less general than Thomson's two years later. Mathematically this is true. Physically it is irrelevant because Clausius reasoned, as had Carnot, that there was a unique maximum to the mechanical work equivalent of a unit of heat. The working substance in the ideal engine was irrelevant.

91 For an ideal gas V and t were related through the ideal gas laws. Clausius, "Über die bewegende Kraft der Wärme und die Gesetze, welche sich daraus für die Wärmelehre selbst ableiten lassen," *Ann. Phy.* 79 (1850: 368. Translated in *Phil. Mag.* 2 (1851): 1–21, 102–120, 12.

92 Clausius, "Über eine veränderte Form des zweiten Haupsatzes der mechanischen Wärmetheorie," *Ann. Phy.* 93 (1854): 481–506.

and a physical interpretation of the properties of the terms in his equations. The mathematical path of a function became the physical path of the physical entity represented by the mathematical symbol. Like Helmholtz, Clausius had to make this reevaluation of Carnot plausible. He therefore turned to experiments on the latent heat of vapors and the velocity of sound to draw them into a single explanatory net.

On a more abstract level Clausius showed that the mechanical theory of heat was conceptually better than Carnot's. Carnot had to assume that perpetual motion could not exist to argue for the conservation of heat. The mechanical theory of heat ruled out the possibility of perpetual motion from the beginning. If the mechanical theory was not accepted, heat could move from a colder to a hotter body, which went against all observational evidence. Arguing mathematically from these assumptions Clausius established that Carnot's function C was simply the absolute temperature. To make this conclusion plausible Clausius compared values of C deduced from his theory with William Thomson's experiments. To drive the point home he examined the behavior of vapors as they deviated from the gas laws, comparing temperatures of maximum density from his theory and Regnault's experiments.

This long excursion into Clausius' methods demonstrates that his papers on the mechanical theory of heat were as speculative as Helmholtz's. However, his presentation and methodology lay well within the standards of German physics while extending those standards into new domains of explanation. In the next three years Clausius published a series of papers exploring various phenomena to demonstrate the range and significance of the principle underlying his work. This included both electrical and thermoelectrical phenomena. Helmholtz, meanwhile, pursued physiological research and only began publishing review articles on the mechanical theory of heat after 1855.

In Clausius' pursuit of the mechanical theory of heat there was a consistency of perception and a systematic methodology tied closely to experimental results. This makes Clausius' argument more compelling than Helmholtz's of 1847 that he did not follow up until much later.[93]

Clausius criticized the work of Helmholtz and Holtzmann in the theory of heat over the consistency of their physical interpretations and their usage of mathematics. In 1853 Clausius argued that Helmholtz's demonstration of his conservation

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93 Helmholtz later claimed that he saw his 1847 paper as simply a review of the literature; his principle not being so remarkable a thing to come by. However, while ceding the credit for the idea to Mayer he accepted all the credit for its development. See, Helmholtz, "Erhaltung der Kraft," (1881), Appendix. See also his letter to Tait used by the latter as an "impartial" account of the early history of thermodynamics to counter Clausius' complaints of mistreatment in Tait *Sketch of Thermodynamics* (Edinburgh: David Douglas, 1877), chap. 1. For more details see Garber, Brush and Everitt, *Maxwell on Heat and Statistical Mechanics* 34–44.

law was only valid for his particular model of matter. Helmholtz had not, mathematically, established it in general. In addition Helmholtz had not understood the notion of the potential or used it consistently in the electrical examples he chose to illustrate his principle. Clausius added an illustration, inherent in a paper Helmholtz had missed.[94]

In his criticism of Helmholtz on the potential, Clausius separated the physical potential from its mathematical expression, Gauss' potential for which Clausius preferred Green's expression the potential function. Helmholtz was stung by the criticism. It undermined his position within physiology as well as the profession he was fast discovering he ought to be in, physics. His reply then acknowledged the validity of Clausius' criticism, while demonstrating he could do his sums as well as anyone. On the question of his model being particular, Helmholtz could only reply that he was concerned with "real" forces, not those abstract, generalized concerns of "mechanicians [Mechaniker]," i.e., mathematicians. A nice putdown but hardly an answer to the nub of Clausius' point.[95]

Clausius clearly knew both the mathematical and physical aspects of this question. He also had a particular vision of the structure of matter that surfaced at the end of the decade. He had suppressed it earlier for the sake of an analysis based on more acceptable foundations and experimentally demonstrable assumptions. Later Clausius was to explore the actual molecular motions that constituted heat through a theory of gases. He developed the concepts of disgregation and the virial while deepening his own understanding of the physical significance of the second law of thermodynamics.[96]

In the early 1850s both men were equally able to manipulate modern mathematics and express the results of their mathematics in the language of a chosen physical imagery. While Helmholtz had begun this in a manner guaranteed to disturb the very audience he wanted to reach, by 1860 he demonstrated his control of their methods in such a way that he could no longer be ignored. In 1858 Helmholtz published a paper in Crelle's journal that put together a mathematical argument through physical illustration. This was not new, but the implications he drew from the solutions to the partial differential equations were put in purely physical terms.

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94 Clausius, "Über einige Stellen der Schrift von Helmholtz 'Über die Erhaltung der Kraft'," *Ann. Phy.* 89 (1853): 568–579, and 91 (1854): 601–604. Helmholtz's reply is sandwiched between in Helmholtz, "Erwiederung auf die Bemerkungen von Her. Clausius," *Ann. Phy.* 91 (1854): 241–261. Clausius' criticism of Helmholtz's inconsistent understanding of the idea of the potential was stated in his earlier papers on galvanic electricity. Clausius' criticism of Holtzmann appeared as, "Erwiederung auf die im März-hefte der Annalen enthaltenen Bemerkungen des Hrn Holtzmann," *Ann. Phy.* 83 (1851): 118–125. On both counts Clausius was correct and later Helmholtz acknowledged this.

95 Helmholtz, "Erwiederung ," and "Erhaltung," (1881), Appendix.

96 See Garber, Brush and Everitt, *Maxwell on Heat and Statistical Mechanics*, 45–46, and the literature cited there.

The problem was in hydrodynamics. Helmholtz argued that the solutions to the general hydrodynamical equations offered from Euler to Stokes ignored friction, both internal to the fluid, and between the fluid and fixed bodies. Helmholtz demonstrated mathematically that if a "velocity potential" existed, that is a perfect fluid, there could be no rotations within the fluid. While not being able to investigate the question in general, Helmholtz was able to demonstrate the mathematical and physical characteristics of such rotations, should they exist.[97] Helmholtz had no notion of the mathematical forms of two types of friction he identified and it was unlikely, even if he could do so, that the resulting differential equation would be integrable. For the particular cases that he could investigate, Helmholtz used both Green's theorem and the analogies he could draw from the forms of his equations when they resembled those of electrodynamics. At each stage in the development of his mathematical cases, Helmholtz referred to a physical description of what was going on in the fluid, sometimes in analogy to an electrodynamical case. And he was only interested in physical cases. Purely mathematical ones were not his concern, although he knew full well that he was solving a previously unsolved mathematical problem.

A more limited domain for mathematics was evident in Clausius' later work, none more so than in his 1859 treatise on the potential. This was a text in mechanics in which conservation of energy was seen as a less general way of understanding physical processes than the potential. For both Helmholtz and Clausius, mechanics and electrodynamics were expressed in terms of potential and force, and eventually least action, in preference to energy. In this text we see Clausius' training in mathematics, especially his understanding of Dirichlet's work on the potential.[98] Just as Helmholtz had done the previous year, Clausius developed the mathematics only so far as it was useful for making his physical point. In fact the mathematics is somewhat sloppy, although Clausius argued in such a way that mechanics, electricity,and magnetism were separated simply by the value of a constant in his fundamental equation for the force law. The potential as a physical concept unified

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97 Helmholtz, "Über die Integrale der hydrodynamischen Gleichungen welche den Wirbelbewegung entsprechen," *J. Reine Angew. Math.* 55 (1858): 25–55. Helmholtz then investigated fluids and their internal friction experimentally. See, Helmholtz and G. von Piotrowski, "Über Reibung tropfbarer Flüssigkeiten," *Ber. Berlin* 40 (1860): 607–658. This was followed by Helmholtz, "Über discontinuirliche Flüssigkeitsbewegungen," same journal (1868): 215–228, translated into *Phil. Mag.* 36 (1868): 337–346. This list does not include the work that followed on gases, acoustics and sound.

98 Thomas Archibald, "Physics as a Constraint on Mathematical Research," has argued that the development of understanding of the potential was hampered by its investigation by physicists rather than mathematicians. My point is that physicists had their own purposes for investigating the potential. Those of mathematicians they now left to mathematicians. Perhaps it would be more useful to ask why mathematicians during the 1850s and 1860s did not see the potential function as offering them interesting mathematical problems to solve.

these fields of physics.[99]

He characterized his force components as $X = -(\partial V/\partial x)$ and so on. Force could then be represented by a function of the coordinates only. Clausius merely stated that from this we obtain

$$\frac{\partial X}{\partial y} = \frac{\partial Y}{\partial x}, \qquad \frac{\partial Y}{\partial z} = \frac{\partial Z}{\partial y}, \qquad \frac{\partial Z}{\partial x} = \frac{\partial X}{\partial z}.$$

There was no generalized pursuit of an expression for a function with particular mathematical characteristics in terms of generalized coordinates, etc. The force function was developed only in terms of Cartesian coordinates. What we have is the pursuit of a physics of central forces with frequent illustrative mechanical examples to demonstrate the physical significance of the mathematical points.

Clausius preferred Hamilton's terminology of force and force-function to potential and potential function. The force function was the function whose partial differentials were the components of the force. It was a function of the coordinates, and was the physical equivalent of the potential.[100] He dealt with the components of force along arbitrary directions in space to show the form of the relationship between force and the force-function. This led into an example to show that knowledge of the force-function at any point in space contains within it knowledge of the force itself. Yet Clausius only dealt with inverse-square force laws. He made it clear that he knew there was mathematically more to what he was doing than he chose to display in his text.

This was Clausius' attempt to rework mechanics on the foundation of the potential. And he presented it as a mathematical resource, an approach to the problems that physicists had to solve in many different areas of research. In his case it served him well. From this approach he developed a particular form of the potential, the ergal, and from this the virial theorem. In the mechanics of a closed system the conservation of energy was a subordinate result. It was the the sum of the ergal and the *vis viva* that was a constant.[101] It was through this form of mechanics that Clausius understood and explored the nature of gases and the actual motion that was heat.

By 1860 there existed in German physics a theoretical physics, pursued along similar lines by Helmholtz and Clausius, in which mathematics was understood and developed for the physics it contained. Mathematics as a language was tamed. It was not investigated for the physics that could be deduced from a generalized form or function. The function spoke directly to the physical process, or physicists were not interested. If they were they pursued and published it as mathematics

99 Clausius, *Die Potentialfunction und das Potential: ein Beitrag zur mathematischen Physik* (Leipzig: Barth, 1859), 14. The text went through four editions in Clausius' lifetime.

100 Clausius, *Die Potential*, 8, 10.

101 Clausius *Die Potential*, 149–158.

not physics. In Clausius' case we have a prolific author who published in every field of importance in research during the three decades from 1850 to 1880. With Helmholtz there was a clarity of physical vision that allowed him to control the mathematical language lacking in previous generations of German physicists. After having made the mathematical point the physical one had to follow. If it did not, the mathematics was of no interest to them. The adopted languages of mathematics now described physical processes.

In the German States we also have a series of approaches to the issue of the interpretation of natural processes and structure that parallel those of British physicists, yet were set upon different foundations. Force and potential were the conceptual basis for German physics, energy conservation and engineering mechanical models for the British. In both groups there were individuals who appreciated the need to rise above the particulars of models to establish more defensible grounds for this new enterprise of theoretical physics. The British and the Germans produced complementary, yet quite different visions of the same processes when solving shared research problems. In their work they collectively created a range of possible approaches to the interpretation of physical phenomena for which mathematics, and especially the calculus, was the crucial, common language, reshaped to the needs of the discipline they were at the same time creating.

While we might disagree with some of their premises, much of their methodology is familiar. Too familiar, for we forget that it had first to be recognized, practiced, then molded to purposes that were defined by the very research problems they chose to pursue. Twentieth-century physicists extended and developed the power of these practices, and their success obstructs our view of the very processes through which this physics came into existence. This is the process that this exploration has attempted to render visible once again.

Part IV

Conclusions and Epilogue

Chapter IX

Physics About 1870 and the "Decline" of French Physics

Theoretical physics did not come into existence as a subfield of physics until the 1860s. By 1870 physicists had accepted mathematics as the natural language of physics and put into place their own ways of training and using the diverse languages of mathematics. Physicists such as John Tyndall were anachronisms within the profession. While he performed quantitative experiments, he was not obsessed with accuracy, even though trained within the German academic system. He also did not deduce algebraic relationships from his results that were by that time expected of physicists.[1] Tyndall's statements about the structure and functioning of nature were qualitative and in the vernacular. And his audiences consisted of the general public, as well as his colleagues within the profession. His career harkened back to the era before the formalized, academic and professional structure of the discipline which he entered in the 1860s. Physicists had withdrawn into a profession of peers that largely addressed each other. The general public was not privy to the research process as they had been in the first half of the nineteenth century. The mathematics now necessary to penetrate the theories of physicists meant that only the most general of ideas and sketchiest of plans of their understanding of nature were available to the vast majority of the general public.

However, theoretical physics was not just mathematics. Mathematics encapsulated a physical situation, or process in symbolic form. It represented a relationship between physical concepts, or an operation, interpreted in a particular case expressed in algebraic or geometrical form. The direction and depth in which the symbolic forms of this language were developed through mathematical operations and transformations were now firmly controlled through the physical meanings embedded in the symbolic forms and operations of the mathematics. The possible

1 Tyndall's experimental results on the intensity of radiation emitted at a fixed wavelength were the starting point for Josef Stefan's deduction of the Stefan-Boltzmann law for black bodies. See Brush, *The Kind of Motion we call Heat,* vol. 2, 513–517.

changes to the physical system encapsulated in mathematical operations had to be compatible with the expected behavior of the physical system under examination. Physical interpretation was, therefore, imposed on the mathematics, to narrow its focus to the physically meaningful or significant. It was no longer sufficient that mathematical results were compatible with experimental results. The physical conditions represented by the mathematical forms must necessarily match, in detail, plausible, physical possibilities. Results of mathematical interest were only of secondary importance for physicists, and presented to the appropriate audience of mathematicians in separate publications.

While possible directions for the development of solutions to the equations that emerged from consideration of physical processes were broad, most were of no interest within physics. Self-consistent, mathematically developed worlds were not a part of physics. Theories needed to include within their explanatory net known, quantitative, experimental results. Experiments were now integrated into the body of theory. This meant that while experiments were the core of physics, they were now caught within theoretical nets and also critical to the plausibility of those theories. Experiments were still the starting point for theoretical explorations of physical phenomena, known results were required to be a necessary result of mathematically expressed theories, and were the predicted outcomes of the physical processes visualized mathematically.

Experiment thus served several critical functions within this new subdiscipline. As in the earlier mathematical physics, experiment was the starting point for theoretical physics. Known experimental results could be used in the development of theory to guide the mathematics along physically plausible paths. However, standards had shifted and known experimental results had to be derived from the mathematical specifics of theory in such a way that the conditions of the experiment, in idealized form, were mirrored within the structure of the mathematics. Mathematical technicalities to reduce the generalities of mathematical deductions to forms paralleling known experimental results were no longer sufficient indication of the validity of the deductions. Physicists had to be able to trace the physical processes in detail through mathematical manipulations. The outlines of the experiment, idealized in abstracted form, had to be visible through the mathematical language.

Conversely, mathematical results needed to be structured in such a way that experimentalists could develop models of the operation of nature under specific conditions and mimic those mathematical results. Language had to be replicable in the laboratory. And that replication needed to be precise. Mathematical theories were now expected to lead to numerical results that could be visualized and repeated in the laboratory. It was also possible to directly visualize, through the imagery encapsulated in mathematical form, how a physical system might perform under possible conditions not yet established in laboratories. Predictions, taken directly from the physical extrapolations of a theory, were now possible. Analogies developed from the mathematical forms appearing within distinct physical situations were no longer satisfactory as a guide to predictions.[2] Maxwell was surprised with the results of his kinetic theory of gases. In general, the internal friction of gases appeared to be independent of pressure and varied as the square root of the temperature. This prediction did not include any details of the specific geometry of an experiment to mimic this result. Maxwell had to develop the details of his experiment through the mathematical example of circular plates oscillating in a horizontal plane about a vertical axis.

The elegance of the experiment and the precision with which he captured the results of his deductions did much to validate his ideas on gases.[3] They also were instrumental in changing the function of experiments. Because theories now contained precise physical imagery and led to results that were directly reproducible in the laboratory, experimentalists could literally test the predictions of theories. Their work became necessary for the validation of theories.[4] Mathematicians' dismissals of experimental results were no longer sufficient to stifle criticism of mathematical derivations. If an expected phenomenon was not detected, the theory was in more trouble than if it was found to be of the wrong order of magnitude. Yet theories might survive, given ongoing disputes between experimentalists over methods and accuracy.[5] However, experiment could refute theory.[6]

Experiment was the other half of a new enterprise of physics. While problematic experiments reflected upon theories, not mathematics: Mathematics had become a given. The power that the mathematics of the calculus brought to physics changed the very nature of the theoretical structures physicists could use to interpret the operations of nature. The range of phenomena that could be encompassed within the net of an hypothesis broadened. The most dramatic example was a theoretical

2 For such an example see Garber, "Poisson," 162.

3 Maxwell, "On the Viscosity, or Internal Friction, of Air and Other Gases," *Phil. Trans. R. Soc. London,* 156 (1866): 249–268. For an account of the impact of this result and other deductions from his theories of gases and the experiment that resulted see, *Maxwell on Molecules and Gases,* Garber, Brush and Everitt, eds. 18–37.

4 For an example of an experimental physicist who lived through and understood the change in the use and function of experiment see David Cahan, "From Dust Figures to the Kinetic Theory of Gases: August Kundt and the Changing Nature of Experimental Physics in the 1860s and 1870s," *Ann. Sci.* 47 (1990): 151–172.

5 See Bruce J. Hunt, *The Maxwellians,* chaps. 2, and 7 for an account of the prediction of, then search for, electromagnetic waves. For the differences in interpretation given the experimental methods that arose from Maxwell's predicted behavior of the transport coefficients of gases see *Maxwell on Molecules and Gases,* Garber, Brush, and Everitt, eds. 18–40.

6 See the fate of Joseph Larmor's vortex ring model of the ether at the hands of Oliver Lodge, in Hunt, *Maxwellians,* 212–215.

tissue created to encompass phenomena that ranged from electrostatics, through current electricity, to electrodynamics. Theory in the modern sense of the term came into existence with the creation of these theories, while at the same time exemplifying the powers of this new approach to understanding physical processes.

Despite the power the languages of mathematics brought to physicists, the calculus was not sufficient to create modern theoretical physics. Hypotheses and the physical imagery generated from them were also necessary and central to the new enterprise. Conflicting imagery could and did use and generate the same mathematical forms. There were two major theoretical structures covering electrical and magnetic phenomena, if we ignore the variations on the action-at-a-distance and field theories. However, the mathematical forms used in field theory led to mathematical results, and physical conclusions that were not obvious from the imagery and mathematics in use in action-at-a distance theories. In this case experiment might validate one imagery over another, or, spur theoreticians to replicate the results mathematically within the alternative imagery. This is seen clearly in the case of the mathematics of electrodynamics and that of the caloric theory of heat that were reinterpreted in the terms of the rival mechanical theory of heat.

Sometimes, experiment would not, or could not, decide the issue between rival imageries. Standards introduced into physics, that lay beyond both experiment and mathematics, were brought to bear to decide the case. Hypotheses were incorporated back into a discipline whose members had previously decried their use in the empirical enterprise of experimental physics. On several levels hypotheses might be used to guide the structure and then the direction of development of the mathematical expression of those hypotheses. Specific detailed models of the structure of matter and the processes of nature were modeled mathematically. Some German physicists favored center of force and action-at-a-distance imagery; British physicists preferred engineering models of mechanical devices for matter. These models were valid over a much more restricted domain of phenomena than those now based on far more general, yet just as mechanical, principles of nature.

Quite how much hypothetical imagery was allowed into the mathematical exposition was a decision made by individuals and was one of almost personal preference. And, one approach did not necessarily exclude any other. Maxwell, Helmholtz, and Clausius found mechanical models of matter useful, yet each seemed to work towards the goal of basing the results of their theories on more and more abstract and generalized mechanical principles. Maxwell, in both his theories of electromagnetism and gases, moved from specific models to Hamiltonian dynamics. Clausius, in his theory of heat, moved from abstract principles to specific models to abstraction and Hamilton's Principle of Least Action. Helmholtz also followed this same general path from a specific model of matter to Hamilton's principle of Least Action as an organizing principle of nature in his theory of heat. This path might be reversed. Mechanical models offered the guidance of specifics that made the mathematics physically visible with a specificity that generalized principles did not offer.

The use of specific mechanical models to constrain mathematical language led to compromises in mathematicians' definitions and understanding of the terms and operations that were the elements of that language. The extent of the compromises the physicist allowed himself also was a matter of choice. Yet even in the case where, after 1870, in Boltzmann's work in statistical mechanics, physics again became mathematics, the context of that choice changed how the work was received and what was then done with it. Boltzmann's work on the second law was controversial within physics because there was no mechanical imagery to visualize the physical processes that the H-theorem was meant to express.[7] Boltzmann himself felt the need to reexpress his theorem in mechanical terms, to give it a real physical meaning. He finally abandoned these efforts.[8]

Mathematics was both structural and expressive of ideas. It was structural in that it was used to express relationships. At the same time mathematics also allowed for the exploration of what could happen, given the limitations of mathematical structures and operations, and the processes allowable through the consideration of physical hypotheses. Mathematics also limited and tamed speculations with the necessity for mathematical consistency. The characteristics of mathematical functions or coefficients could suffer immediate physical interpretation with subsequent consequences for the visualization of how the physical system could behave. Clausius' understanding of internal energy, mechanical work and heat are obvious cases in point. Mathematics was also itself used as the source for analogies for understanding the mathematics of one domain of physics from another. Mathematically identical structures were also richly suggestive of physical behavior in physically isolated cases. Both could and did evolve together and were thus processes of interpretation in mathematical language and physical imagery simultaneously.[9]

Experiment, hypotheses, and mathematics were the foundations for new ways of investigating and interpreting the processes of nature. The fusion of these aspects of physics can be seen in the changes in textbooks during the last third of the nineteenth century. German texts of lectures in mathematical physics barely mention experiment. Mathematical methods, consistency and their manipulation to obtain theoretically interesting results were the focus of attention. Those in theoretical physics, starting with Thomson and Tait's text, joined concepts, experiment and mathematics together. They offered students an introduction to both concepts and

7 For the H-theorem see, Brush, *The Kind of Motion we Call Heat,* vol. 1, chap. 6.

8 See Martin J. Klein, "Boltzmann, Monocycles, and Mechanical Explanation," in *Boston Stud. Phil. Sci.* 11 (1974): 155–175.

9 In this sense theories are about themselves as well as the external world. See Enrico Bellone, *A World on Paper: Studies in the Second Scientific Revolution* (Cambridge MA.: MIT Press, 1980).

mathematical methods. This was coupled to numerous examples of how the language of mathematics could and should be manipulated and interpreted to yield physically meaningful results. The practice, used within French mathematical physics, of beginning in specific examples was taken over into theoretical physics. However, the purpose of the mathematical exercises was now the investigation and interpretation of nature, not the generation of mathematics.

The "Decline" of French Physics

French physics, and most of the other sciences, fit awkwardly into any account of nineteenth-century science under the assumptions that historians make about the markers of excellence or intellectual development. What French scientists did in the nineteenth century does not easily fall into line with the work of scientists in the same fields in either Germany or Britain. The easy way out is to omit them altogether, or, simply mention those men and their work who are necessary in marking the intellectual development of a field. Either way historians avoid the issue of French Science altogether. However, French names and research crop up too often to ignore the question of what makes French science different from that of Germany or Britain in the nineteenth century?

Accepting the sociologist's solution of labeling French science as in "decline" hardly solves the problem. Sociological categories define the sciences using twentieth century criteria. These categories are not the best instruments for understanding the intellectual differences that existed in the sciences across national and cultural boundaries over a century ago. Sociological factors and political circumstance dominate theories on nineteenth-century French science, although the adequacy of this approach has recently been questioned.[10] These factors and circumstances are used to measure the intellectual place of French scientists and mathematicians amongst the other European nations.[11]

One recent examination of physics and mathematics of France in the nineteenth century focussed upon research productivity in an effort to draw together an integrated picture of French science during this era.[12] The political economy of science can point to restrictions in opportunities, and hence decline in the numbers of scientists and their productivity. However, quality is not necessarily equal to quantity.

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10 MaryJo Nye, "Scientific Decline: Is Quantitative Evaluation Enough," *Isis,* 75 (1984): 697–708.

11 For a discussion of the issue of decline see Harry W. Paul, "The Issue of the Decline in Nineteenth-Century French Science," *French Hist. Stud.* 7 (1971): 416–450. Paul also criticizes the theories that explain "decline" in political and social terms without taking the intellectual quality of French science into account.

12 Terry Shinn, "The French Science Faculty System, 1808–1914: Institutional Change and Research Potential in Mathematics and the Physical Science," *Hist. Stud. Phys. Sci.* 10 (1979): 271–332.

Putting the issue of how to measure "quality" aside, a decline in quantity of published research does not address the continuing importance of the research results of French scientists to scientists in Germany and Britain in the nineteenth century. British and German physicists had to take the work of French experimentalists into account especially in the study of heat and light. To learn the practices of experimental physics, William Thomson spent months toiling in Regnault's laboratory in the 1840s. German experimentalists and mathematicians still made pilgrimages to Paris to meet their peers. French assessments of German experimentalist's work were germane in their files for promotion. This was even more true in mathematics and mathematical physics. A measure of the importance of French physics to their German and British colleagues can be made by looking at the reports carried in German and British journals of French scientific work in French scientific journals and the publications of scientific societies across France. It is also vividly reflected in the footnotes to, remarks and reports on, and uses made by British and German physicists of those works in their own journals. They also pepper their private correspondence. French journals were still required reading. French experimentalists and mathematicians were important colleagues.

The work of French physicists still mattered to their British and German colleagues. Simply to point out that before 1830 there was one major center for scientific research in Europe, namely Paris, and after that date there were several, namely London, Cambridge and the Scottish Universities as well as the Universities of Berlin, Heidelberg, Bonn and Königsberg, names the phenomenon without explaining it. Perhaps we need to consider how French scientists practiced their crafts during the nineteenth century before we declare "decline."

During the first three decades of the nineteenth-century French physicists and mathematicians developed a highly successful set of practices that defined the disciplines of experimental physics and mathematics. The intellectual boundaries of these disciplines were fixed even as the institutional setting for their pursuit changed from a vocation and the Académie to the Université and the École Polytechnique and a profession. These intellectual boundaries were stable throughout the nineteenth and into the twentieth century. French physics and mathematics did not decline. The practices of the members of the disciplines conformed to the highly successful practices developed during that first third of the century.[13]

French experimental physicists were skeptical of "speculation" and hypotheses, other than those that had become so accepted within the community of French physics as to not seem hypothetical at all. The subject matter of their experiments also related to areas that had been successfully explored during those decades of the early nineteenth century. These areas included the phenomena of light, within the

13 The social institutions also formed a continuum with those of the early nineteenth century. See Maurice Crosland, *Science under Control: The French Academy of Sciences, 1795–1914* (Cambridge: Cambridge University Press 1992).

context of the wave theory, and heat, within the context of the caloric theory of heat. Many experiments also related to other areas of strength developed by the French in those same early decades of the century, including astronomy.[14] Astronomy and precision experiments in optics were closely related. Nineteenth-century French emphasis on particular aspects of optics begins to make sense.

Within France, the ideological and social barriers erected in the German States between research that was esoteric and "pure" and that which was practical and by implication of lesser intellectual value, did not exist. While universities were teaching institutions, research both esoteric and useful was pursued there even before the 1870s. After the Franco-Prussian War stringencies of the budgets from Paris required that physicists and chemists seek local sources of support. The utility of science was pursued and made manifest in the work of physicists in industry and for industry. There was a constant flow of scientists from industry to the university and back again.[15] Henri Victor Regnault's work on the physical constants of gases was commissioned by the French government in an effort to improve the design of steam engines. Regnault completed this research at the Collège de France as professor of physics. Regnault was by training and previous research a chemist. Regnault's experiments included redetermining the composition of air and respiration, a remarkably broad range of experiments that crossed the disciplinary lines being drawn in both Germany and Britain, yet remaining inside physics within the borders of France.

In France, the foundations for the practices and standards of experimental physics established in the early decades of the nineteenth century deliberately excluded hypotheses. Knowledge was based on observation and measurement. Experimental physicists in France regarded their work as purely empirical and devoid of all hypotheses. These physicists were reduced to narrow domains of endeavor because they could not embrace the speculations being investigated as fast as possible by British and German experimentalists. This trait was particularly marked in experiments on, and speculations about, the nature of heat. Clapeyron's mathematical explorations of Carnot's ideas on heat of the 1830s had not contained any deductions that drew them into contact with experiment, or observation, or the development of the caloric theory of heat. Regnault's work was empirical and his conclusions based on phenomenological reasoning. While French experimentalists reported work that indicated the equivalence of mechanical work and heat they were largely

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14 John L. Davis, "The Influence of Astronomy on the Character of Physics in Mid-Nineteenth Century France," *Hist. Stud. Phys. Sci.* 16 (1986): 59–82. Davis notes the intellectual precedents for French superiority in astronomy that were built into institutions that offered paths to careers for experimentalists. These were important factors in the choice of research subject matter for experimentalists.

15 Harry W. Paul, "Apollo courts the Vulcans: The Applied Science Institute in Nineteenth-Century French Science Faculties," in *The Organization of Science in France, 1808–1914,* Fox and Weisz, eds., 155–181.

ignored, as was that of the engineer, F. Reech.[16]

The molecular theory of matter was another area in which hypotheses proved fruitful for experimental and theoretical research for both German and British physicists. It was still a point of contention within the French scientific community in 1900. Finally the experiments of Jean Perrin, who claimed to map the actual motions of molecules, convinced his reluctant colleagues of their existence.[17] Perrin actually established the validity of Einstein's formula for the mean displacement of molecules and Paul Langevin's law of horizontal displacement. Einstein was either unaware, or unconcerned with the problem of whether molecules existed or proving that they did. Perrin and Langevin were.[18]

French physicists as experimentalists worked within a distinct set of practices that they had defined earlier in the nineteenth century. In tandem with this French mathematicians continued their own practices defining their research problems and solutions according to the tradition defined in the same period of time. They could not come to terms with some aspects of the research being accomplished by both British and German physicists. Mathematics based in physical problems continued as an important research node of the French discipline.

These differences extended into the philosophies of science developed by scientists. Pierre Duhem's philosophy can be seen, in part, as an attempt to validate French practices and standards in physics. Experiment remained the core of the discipline. Physical theory was mathematics, and at best an exercise in "natural classification." Mathematics described but could not interpret nature. Attempts by the German and British to uncover the realities behind the appearances revealed by experiment were wrongheaded and futile. Essences could never be known.[19] Hypotheses were necessary, and built into experimental practices. However, only certain types of hypotheses were legitimate. Those based in mechanical images of nature, whether based on action-at-a-distance forces or the mechanical mon-

16 For a discussion of Reech's work see Clifford Truesdell, *The Tragicomical History of Thermodynamics,* chap. 10, and, Appendix.

17 See MaryJo Nye *Molecular Reality: A Perspective on the Life of Jean Perrin* (New York: American Elsevier, 1972). These were also the standards of French chemists who only reluctantly accepted atomism by 1900. See Terry Shinn, "Orthodoxy and Innovation on Science: The Atomist Controversy in French Chemistry," *Minerva,* 18 (1980): 539–555.

18 See Perrin, "Mouvement brownien et grandeurs moléculaires," *Ann. Chim. Phys.* 18 (1909): 1–114, translated by Frederick Soddy as *Brownian Movement and Molecular Reality* (London: Taylor and Francis, 1910). See also Perrin, "Rapport sur les preuves de la réalité moléculaire," in *La théorie du rayonnement et les quanta,* Paul Langevin and Louis de Broglie, eds. (Paris: Gauthier-Villars, 1912), 153–250.

19 Pierre Duhem *The Aim and Structure of Physical Theory*, P. Wiener, trans. (Princeton: Princeton University Press, 1954.) We have omitted any reference to the religious goals of Duhem's philosophy of science. See also Bruce Eastwood, "A Second Look: On the Continuity of Western Science from the Middle Ages, A. C. Crombie's *Augustine to Galileo,*" *Isis,* 83 (1992): 84–99, 88.

strosities of Maxwell's electromagnetic theory, were insufficient. Theory must be grounded in postulates, whose consequences could be developed along any path, provided there were no logical inconsistencies. Only the conclusions of those theories were required to conform to the results of experiments. And only those conclusions stood or fell. The intermediate steps from postulate to conclusions were immune from such a fate. Mathematical theories could take flight as long as they were logically consistent; only their conclusions were subject to the test of reality.

Duhem was also describing the relationship currently existing between mathematics and experimental physics in France. After 1830 mathematicians constantly used the problems of physics as a source for their mathematical explorations. Fourier and the analytical theory of heat served generations of French mathematicians as the starting point for their research, from J. M. C. Duhamel and Joseph Liouville to Henri Poincaré.[20] They also returned to mechanics, and the theory of elasticity and light as well, and, in the case of Poincaré, to celestial mechanics.[21]

Poincaré's philosophy of science reemphasized the central place of mathematics within the sciences, echoing French mathematicians of almost a century earlier. His conventionalist ideas of natural law were a restatement of the idea that the search for essences was a wild goose chase. All that scientists could achieve in their expressions of natural law were more and more efficient descriptions of phenomena. The laws themselves were expressions of a consensus among experimentalists. In this context mathematics was the most efficient and effective descriptive language available. And, mathematical descriptions subsumed those of physics.

After 1870 this reaffirmation of values that had defined physics as experiment, and mathematics that encompassed physics as mathematical physics still separated the work of French physicists from their colleagues in Britain and Germany. French science was not in "decline." In France, particularly in physics and mathematics science was defined and practiced according to quite different and older criteria. The goals and expected outcomes of the work of these mathematicians and physicists were also distinct from those of experimentalists and mathematicians elsewhere on the continent. There was no place for the detailed, specific theories of physical processes that guided the research of both British and German theoretical physicists in the late nineteenth century. General principles, such as the wave nature of light or the principle of conservation of energy, and, eventually the second law of thermodynamics, became the foundations for extended forays into mathematics. Theoretical physics, as it existed in Germany and Britain, could not exist within the prevailing culture of French science.

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20 See Jesper Lützen *Joseph Liouville*. On French mathematicians' use of Fourier, see Garber, "Reading Mathematics, Constructing Physics."

21 Poincaré's work in celestial mechanics focussed on the three-body problem and examined the mathematical properties of recurrent orbits. See, Henri Poincaré, "Sur le problème des trois corps et les équations de la dynamique," *Acta Math.* 13 (1890): 1–270.

Some Conclusions

We can no longer assume that physics, with its modern standards and practices, has existed since Newton, Galileo or any one person. Nor can we claim, as did Cannon some twenty years ago, that physics "was invented by the French between 1810 and 1830." Historians of physics now agree that social institutions shape the lives of their practitioners and the functioning of disciplines. The institutions and standards of modern physics were not in place in Europe until the 1860s. In addition, the research practices that shaped the discipline into its modern form were created in the nineteenth century. Those practices, together with the institutional forms in which they functioned, were the keys to making modern physics.

In this study we have focussed upon the ways in which theories about the structure and functioning of nature shaped the practices of what we call theoretical physics. Foundational ideas, the general principles upon which speculations about nature rested, are insufficient to define what theories are and what physicists did in creating theoretical physics. Meaning is conveyed only through the exploitation of those principles in the context of specific problems. The implications of mechanical principles were interpreted and reinterpreted through the results of explorations of the behavior of bodies under well defined circumstances. General principles needed often to be coupled with sets of subsidiary hypotheses to bring those principles to bear upon the solutions of particular problems. Specific analyses of these particular problems are the hallmarks of theory.

The language that eased the development of such detailed working out of the implications of general ideas was the calculus. Without the investigation of how mathematics became the language of physics, any account of the development of theory is hollow. Mathematics was necessary to the development of theory. Before its widespread use within physics, natural philosophy was speculative and closer to metaphysics than the experiments that formed the core of the discipline in the eighteenth century. Mathematics has shaped and reshaped physicists' interpretations of nature. Different forms of mathematics have allowed physicists to reinterpret, to literally, envision phenomena and their interpretation in new ways.[22]

Theory also encompassed experiment in ways that the older speculative natural philosophy and mathematical physics did not. Experiments were integrated into the very body, into the detailed implications, of the physical ideas making up that theory. Mathematics gave physicists the flexibility to develop ideas on high levels of abstraction, while also allowing them to descend into the detailed functioning

22 The most dramatic nineteenth century example of this lies in the introduction of vector analysis into the theory of electromagnetism. In another context Ana Millán Gasca has discussed how different mathematical approaches affected the biological sciences and the images of biological systems the mathematics brought with them in Gasca, "Mathematical Theories versus Biological Facts: A Debate in mathematical population Dynamics in the 1930s," *Hist. Stud. Phys. Sci.* 26 (1996): 347–403.

of specific cases where experiment might be mirrored, in ideal terms, within the compass of the formalisms of mathematics. Mathematics could be reduced to particular, numerate cases. As a language mathematics could be used to extrapolate beyond the confines of known experimental results to predict the results of specific, theoretically visualizable, yet still unrealized experimental conditions. This was no longer a deduction from mathematical analogy but specific, physical juxtapositions that might be put into experimental form. Both the use of mathematics as the language of speculation and the ability to integrate experimental findings into the body of theory changed the nature of speculations about nature and what was acceptable as speculation about nature.

At the same time that physicists were creating theoretical physics, experiment was reconfirmed as the center of the discipline. Theoreticians did not take over the discipline. They remained a minority in numbers and their output was subject to the searching probes of experimentalists who were apt to mold theoreticians results to their own purposes. They were also apt to modify, if not deny, the validity of theories. And theoretical physicists felt compelled to follow the dictates of experiment.[23] The dominance of experiment was also reaffirmed in the 1890s with the detection and exploration of x-rays and radioactivity. It was not yet plausible to declare the independence of theory from experiment. Even in the 1930s it was still possible to state that physics was experiment, the rest was only mathematics.[24]

The complex of methods that made up theoretical physics by 1870 and the tangled relationships that developed between mathematicians and physicists can only be clearly understood if we distance ourselves from the concerns of philosophers and the histories of physics written by physicists. In the late nineteenth-century philosophers took as their model of physics descriptions of physics and its development written by contemporary physicists. Philosophers have defined the essentials of physics for historians of physics ever since. And, as these essentials have changed, so have the narratives of historians. During the late nineteenth century, physicists in Germany and Britain remade their history to conform to the new disciplinary boundaries and practices they had created. This makeover was done both in formal histories of physics and in the reinterpretation of the content of technical papers written in the eighteenth and early nineteenth centuries. In this they were aided and abetted by mathematicians also busily rewriting their own history which discounted the standards of eighteenth-century mathematical practices. Physicists were able to claim as physics many of the papers written within mathematics in the eighteenth century, by interpreting the mathematical results of those paper in physical terms where none existed in the original. Tait gave the notion of the conservation of energy a pedigree that reinterpreted the meaning of

23 Witness Lorentz's reaction to the results of the Michelson-Morley experiment.

24 See Laurie Brown and Helmut Reichenberg, "The Development of the Vector Meson Theory in Britain and Japan, 1937–38," *Brit. J. Hist. Sci.* 24 (1991): 401–433, 417.

Newton's work that was historically and technically dubious. In general, Newton's significance was redrawn to conform to late nineteenth-century standards of physics as a discipline.[25] Euler, Lagrange, Poisson, Fourier and a host of others were soon accepted as working within physics as well as mathematics and became prodigious heros with deep physical insight as well as exulted mathematicians. Physics was redefined by their inclusion in its pantheon.[26] Within these narratives physics, since Galileo and Newton, was a discipline driven by theory and expressed mathematically.

Historians of physics have taken these histories far too seriously. They have assumed that throughout the eighteenth and nineteenth centuries papers, treatises, and textbooks bearing titles that place them within the boundaries of late nineteenth-century physics were written as physics papers, treatises and textbooks. And because their mathematical methods became part of the practice of theoretical physicists, historians accepted their designation as "physics" at their time of publication.

We have to discard the idea that once a method was introduced into physics It remained part of the practice of the discipline. We also have to rethink The notion that what we regard as theoretical physics was always read as such in the past three centuries. Specifically, we need to consider what mathematics meant in the eighteenth and early nineteenth centuries to judge whether, mechanics for example, was indeed an aspect of physics or a branch of mathematics in those eras. We must consider the practices of mathematicians and physicists simultaneously before such assumptions become historically reasonable. Perhaps mathematics has only been seen as the "natural" language of physics for the last century and a half.

By 1900 mathematics had become so essential and integrated into the practices of theoreticians that the idea of a preestablished harmony between the the two

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25 Perhaps the last history of physics published that took experiment as its core was that of Poggendorff's in the 1870s. See J. C. Poggendorff, *Geschichte der Physik* (Leipzig: Zentral-Antiquariat of the DDR reprint of 1879 edition, 1964). The history of Maximilien Marie, *Histoire des sciences mathématiques et physiques* (Paris: Kraus reprint of Gauthier-Villars edition of 1883–1888, 1979) is a history familiar from eighteenth-century France. The narrative is biographical with some technical discussion of what the list of characters did. All biographies are treated strictly chronologically. The only explicit value judgments that enter are those directed against astrologers and alchemists. This is another indication of the uniqueness of French mathematics and physics in this era and their connections to their eighteenth-century roots.

26 Rachel Laudan, "Definitions of a Discipline: Histories of Geology and Geological History," in *Functions of Disciplinary Histories,* Loren Graham, Wolf Lepenies and Peter Weingart, eds. (New York: Reidel, 1983) has followed the same pattern of the appropriation of history by geologists. See also Paula Findlen, *Possessing Nature* on the rewriting of the history of natural history in the eighteenth century that rendered the work of Renaissance naturalists invisible.

disciplines made invisible the actual appropriation of mathematics into physics.[27] Mathematics became the "natural" language of physics. In the historical development of physics, mathematics disappeared as a factor that required explanation. Following this new history philosophers and historians of physics relegated mathematics to the status of a tool. It was and is always there in a myriad of forms that miraculously fitted the needs of the job at hand, to be used and replaced upon the shelf. Its function in structuring the very ways in which physicists interpreted nature became invisible. Some thoughtful physicists continued to ask themselves why mathematics was so effective in describing then interpreting the operations of nature. Others spoke, and still speak, of mathematics as their language, thus acknowledging it as a constituent of their very thinking about the operations of nature.

We have tried to make mathematics visible again to historians and physicists alike. In the process we also begin to understand yet again that physicists in previous centuries neither acted as we expect, nor were they striving to become twentieth century versions of physicists. Rather than impose on physicists of the eighteenth and nineteenth century categories of behavior derived from twentieth-century expectations, it was instructive to simply let the actors speak, then try to figure out what precisely physicists thought they were doing. From this anthropological stance, the richness and diversity of the development of physics and the creation of theoretical physics along multiple paths revealed itself. The range of practices that are encompassed within current theoretical physics now have roots that make them historically understandable. As a bonus, this methodology has added to our understanding of the diversity and complexity of practices in contemporary versions of Physics.

27 See Lewis Pyenson, "Relativity in Late Wilhelmian Germany: The Appeal to a Preestablished Harmony between Mathematics and Physics," *Arch. Hist. Exact Sci.* 27 (1982): 137–155. The author thanks David Cassidy for pointing out this argument and its importance for twentieth century physicists.

Chapter X

Epilogue: Forging New Relationships, 1870–1914

By 1870 both physics and mathematics had become distinct academic specialties within universities across Europe and, in these forms, spread to the United States, Japan and elsewhere. The research center of physics was in the laboratory and in the pursuit of quantitative experiments of increasing accuracy tied consciously to the development of theory. Theory was an accepted research activity and its language was the calculus, that is, ordinary and partial differential and integral calculus as it stood within mathematics in the 1830s. This might be the end of the beginning except that theoretical physics as an accepted subfield within physics, with theoreticians forming a distinct subcommunity within physics, did not coalesce until the twentieth century. Theoretical physicists formed a loosely connected set of individuals within the discipline and profession of physics itself. To form a subfield within academic physics, theoreticians needed to develop a set of practices that were distinct from those of their experimental peers. They also had to replicate themselves by training students as theoreticians, rather than students merely taking courses in theory. Even if they did not develop their own specialist societies and journals, their work needed the recognition of experimentalists as valuable and complementary to their own research. These processes as well as the development of a sense of collective identity as theoreticians unfolded within physics during the forty odd years between 1870 and World War I. This growing awareness was fostered also through a series of intense, competitive interactions with mathematicians in the 1890s and early 1900s. While in 1870 both disciplines reached a maturity marked by autonomy, within forty years members of both disciplines had forged new relationships across their respective disciplinary boundaries. These were not so much alliances as sometimes fierce competitive interactions that have marked the development of theoretical physics throughout the twentieth century.

In the first two decades after 1870, the center of research activity in physics shifted decisively to universities of the newly established German Empire. However, within German universities the disciplines of mathematics and physics drew steadily apart.[1] Although this separation was only temporary, the terms under which mathematicians and physicists interacted with one another in the early twentieth century were different from those of the mid-nineteenth century. By 1900 mathematics was taken for granted as a part of physics and included along with laboratory courses in the training of physicists. In the same decades physicists developed their own versions of the calculus for their students and, more significantly, had begun to develop mathematics beyond the calculus, directed to their own needs without the mediation of mathematicians.

In the same decades mathematicians in Germany focussed on research problems that emerged from mathematics, not the problems of physics. Not all mathematicians engaged in this form of research, yet, those involved in "foundational problems" and pure mathematics dominated the departments of the prominent universities, influenced professional appointments, and sat on the editorial boards of the major mathematical journals. This state of affairs changed only in the first decade of this century through the efforts of Felix Klein and others with their belief that their interests in certain types of mathematical problems coincided with those of physicists. Thus began a series of interactions of mathematicians and physicists that were mutually beneficial, yet shot through with mutual misunderstandings.

The patterns discernible in the institutions and discipline within Germany cannot be superimposed upon the profession or the research produced within France, Britain, or the United States.[2] These three communities followed their unique paths of development where theoretical physics held an even less prominent position than in the German universities.

One common characteristic of all academic disciplines in this era, whatever the national differences in their internal organization, was their international character. Both mathematicians and physicists addressed their respective international research communities.[3] A second common characteristics was that articles in journals addressed a small international audience of mathematicians or physicists that excluded all but the authors' immediate colleagues engaged in the same cluster of research problems. Addresses to colleagues across physics was becoming more difficult except in general terms and those to colleagues across the academic campus were reduced to philosophical issues with minimal technical content.

Significant aspects of the emergence of theoretical physics to a central position within the discipline of physics lay in the solutions to research problems that

1 The reasons for this isolation were both institutional and intellectual. The relationships between the disciplines also depended heavily on the particular institution under discussion. See Jungnickel and McCormmach, *Intellectual Mastery of Nature* vol. 2 chaps. 21–23.

2 This account is skewed towards German universities because the institutions and intellectual development of physics and mathematics elsewhere have been less studied.

3 We should remember the advent of International Conferences in mathematics and physics began in the early twentieth century.

required physicists to develop mathematical languages that led them beyond the calculus, the mathematical language that seemed to define their subfield at its inception.

The subject matter of this chapter falls naturally into three overlapping themes; the range of ways in which physicists used mathematics as the languages of theory and how these languages related to both general laws of nature and specific models; the kinds of mathematics they used and/or developed in their research and the kinds of mathematics that they taught to the next generation of physicists; the development of a new set of relationships between mathematicians and physicists within the context of the modern professions in the first two decades of this century. We confine ourselves to how physicists appropriated the languages of, as well as developed new ones useful for their own purposes. While it is usual to trace changes in theory through ideas, imagery and experiments, these enter into this account only on its margins. We trace mathematics, the language of theory that had become so thoroughly integrated into practice that it was almost invisible, a "tool." A tool it has remained in historical accounts. Supposedly the real work of theorists was in the development of ideas about nature, not the development of languages in which to express those ideas.[4]

The Limitations of Autonomy

After 1870, research produced by the group within physics that we recognize as theorists integrated mathematics, physical imagery and experimental results into a form familiar in the twentieth century. The autonomy enjoyed in the German university systems by mathematicians and physicists changed the ways in which research within their disciplines was structured, to whom it was addressed, and the training available for the next generation of professionals. These activities were geared to maintaining the autonomy of the discipline and to training students as future professionals. These processes had the cumulative effect of isolating mathematicians and physicists from one another. Another component aiding isolation was limited resources, the budgets of even Prussia could not expand indefinitely as the university system expanded. The competition for limited resources and the standards used in distributing those resources led inexorably to several characteristics shared by mathematics and physics.

In general, career patterns depended on both teaching and research. For advancement to chairs at major universities it was necessary to develop an international reputation within a disciplinary research field. For a successful career the physicist or

4 Throughout this chapter a distinction is understood between mathematical physics and theoretical physics. This lies essentially in the level of abstraction to which the mathematics is carried and its relationship, drawn up by the author, to the physical problem under consideration. Added to this is the institutional place of the author and the disciplinary character assigned him by his colleagues. While being somewhat arbitrary this distinction is however useful.

mathematician had to be well aware of the type of research that would actually lead to recognition, and hence promotions, and the teaching and research support that went with them. In physics this meant minutely analyzed precision experiments, costly in themselves in terms of equipment and human resources. Excursions into theory needed to be coupled to experience in the laboratory.[5] Although courses in theoretical physics developed in all universities, systematic training specifically for theorists with courses of increasing sophistication and difficulty still lay in the future.

Since theory was an accepted aspect of the research enterprise, it was integrated into the training of physicists. However, the research interests of the teachers of such courses often lay in experiment not theory. As significant, the occupants of chairs in theoretical physics were, *extraordinarius* not *ordinarius,* professors. Even as the sense of identity of "theoretical physics" within physics grew, "theoretical physicists" were still a minority within the discipline and profession. For many physicists their primary field of research was experiment and the theory they explored was particular to their experimental concerns. Experiment was still considered the core of the discipline.[6] In the German Empire at the end of the century some chairs in theoretical physics were offered to experimentalists if they agreed to teach the smaller, specialized classes in theoretical physics. The larger, more lucrative classes were for the full professors, and those classes were in general and experimental physics.[7] Even though theoretical physics flourished during the last decades of the nineteenth century, we should not overestimate the numbers or importance of the field in physics during the closing decades of that century.

Experimental precision remained the key that separated the professional from the amateur. To gain the necessary skills, systematic training was required. Education in the manual skills of precision offered moral training for the specialists

5 Max Planck was the first physicist to develop a career with no experimental research. Helmholtz, and Boltzmann did experimental research as well as theoretical even though only Helmholtz contributed significantly to experimental physics. Clausius taught experimental physics and controlled the resources of an experimental, research laboratory. Hemholtz also was the only one of this group to head a major research laboratory. During his career in Berlin Helmholtz's experimental skills were well appreciated while his research throughout the 1870's was theoretical. Boltzmann and Clausius spent time trying to restrict their duties to teaching theory rather than overseeing the time consuming laboratory courses. Jungnickel and McCormmach, *Intellectual Mastery* vol. 2., chaps., 14, 16, vol. 1, chap., 8, 12.

6 David Cahan, "The Institutional Revolution in German Physics, 1865–1914," *Hist. Stud. Phys. Sci.* (1985): 1–65 demonstrates that the resources poured into the physics institutes built at the end of the century was for experimental, not theoretical, research and teaching.

7 See Paul Forman, John Heilbron and Spencer Weart, "Physics *circa* 1900. Personnel, Funding and Productivity of the Academic Establishment," *Hist. Stud. Phys. Sci.* 5 (1975).

needed in the developing industrial economies, or so the arguments for support for these training laboratories and institutes went.[8] Most of the research reports in the pages of the major physics journals of Europe were experimental, or theoretical investigations limited to points that emerged from experimental work.

However, by the 1890s young physicists such as Ernest Rutherford abandoned the precise methods of their immediate predecessors and were ready to risk their careers in the exploration of the newly discovered phenomena of radioactivity or x-rays.[9] These phenomena emerged from experimental research and were remote from the concerns of contemporary theoretical physicists.

Physics was still institutionally one field, and theory a decidedly secondary aspect of it.[10] Support, within the university and at the ministerial level was luke-warm. Theory had to prove itself to the experimentalists and they had to convince the various departments of education of its intrinsic worth. This did not come together until after 1900.

Mathematics as an integral part of theory was also an aspect of the introduction of students to theoretical physics. Students of physics were even encouraged to attend lectures in mathematics departments. Yet, after 1870, with few exceptions, less and less that was taught in mathematics departments seemed relevant or even vaguely connected to the needs of physicists, mature or neophyte. During the same decades that physicists reconstructed physics, mathematicians in both Germany and Britain changed their own discipline. Mathematicians turned away from the eighteenth-century practice of using the solution of problems to generate mathematics. Direct examination of the foundational concepts of mathematics and development of their linguistic possibilities from within became the hallmark of a first class mathematician. What constituted the foundations of mathematics that now required such extensive examinations differed in Britain and Germany. This new vision of mathematics was shared only by a minority of mathematicians in Britain and also split the German mathematical community into two hostile camps.[11]

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8 See Cahan, "Institutional Revolution." This was also true in Britain, see Graeme Gooday, "Precision measurement and the Genesis of physics teaching laboratories in Victorian Britain," *Brit. J. Hist. Sci.* 23 (1990): 25–51.

9 Isobel Falconer, "J. J. Thomson and 'Cavendish Physics'," in *The Development of the Laboratory*, James, ed. 104–117, argues that Thomson abandoned precision experiments as head of the Cavendish Laboratory in the 1880s. However, he made no attempts to recruit students or convert them to his approach to experimental physics.

10 See Jungnickel and McCormmach, *Intellectual Mastery* vol. 2, chap. 15.

11 See David E. Rowe, "Klein, Hilbert, and the Göttingen mathematical Tradition," *Osiris,* 5 (1989): 186–213 and Lewis Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany* (Philadelphia PA.: American Philosophical Society, 1983). For the debates and tensions the various avenues of developing mathematics created amongst mathematicians in the late nineteenth and early twentieth centuries see

Mathematics had likewise become a profession driven by research and the needs of younger mathematicians to make research reputations in fields recognized as significant by the leading faculty at major universities across Germany. After about 1860 the kinds of mathematical problems deemed important, and the solutions regarded as significant, were defined by the mathematicians associated with the Berlin "school." This included Karl Weierstrass in analysis, Ernst Kummer in algebra, and Leopold Kronecker in number theory. Absent from their approach to mathematics was geometry or any concern with a mathematics that might be generated through the consideration of problems that originated outside the boundaries of mathematics itself. Collectively these mathematicians narrowed the important research problems for mathematicians. Kronecker ultimately narrowed the foundations of mathematics down to arithmetic to which all other branches of mathematics were subordinate. The development of this approach was coupled with, then justified by, a neo-Kantian philosophy of mathematics. Arithmetic was a product of the mind and the only purely intellectual foundation for mathematics. Space and time had a reality that lay outside of their intellectual contemplation, and were contaminated sources. Mathematical proofs had to stand on rigid evidential grounds that only arithmetic met. This philosophical grounding was not new but was well suited to the development of an autonomous profession within the Prussian university system.[12]

The dominance of the Berlin approach to mathematics had more than an intellectual impact on the field. Their standards began to affect the assessment of research and the effectiveness of the teaching of individual mathematicians throughout the German Empire. Professorships at the more prominent universities went to those mathematicians whose research met the expectations of the Berlin mathematical faculty.[13] This did not mean that all mathematicians abandoned other lines of research, or that the Berlin mathematicians agreed completely on how or what research issues to pursue. However, some research possibilities were judged as secondary and it was increasingly difficult for their practitioners to gain appointments to other than provincial universities.[14]

Herbert Mehrtens, *Moderne Sprache Mathematik: Eine Geschicte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme* (Frankfurt: Suhrkamp Verlag, 1990).

12 For the development of these changes see Umberto Bottazzini, *The Higher Calculus: A History of Real and Complex Analysis from Euler to Weierstrass* (New York: Springer, 1986), Ivor Grattan–Guinness *Foundations,* and Harold M. Edwards "Kronecker's Views on the Foundations of Mathematics," in *The History of Modern Mathematics,* Rowe and McCleary, eds. 2 vols. (New York: Academic Press, 1989), vol. 1, 67–78.

13 See Gert Schubring, "Pure and Applied Mathematics in divergent institutional Settings in Germany: The Role and Impact of Felix Klein," in *Modern Mathematics* vol. 2, 171–220.

14 Bernhard Riemann and Hermann Grassmann are cases in point. Their lives and careers were not blessed by easy appointments to major university departments of mathemat-

As in other academic disciplines, mathematicians in the German universities focussed on teaching the next generation of mathematicians rather than the mathematics needed by students in other disciplines. Given the developing professional dynamics within their discipline, mathematicians were less likely to want to teach the kind of mathematics for even advanced students in physics. While physics students always entered mathematics courses on terms set by mathematicians, in the last third of the nineteenth-century mathematicians almost ceased to teach mathematics that physicists might be able to relate to their own research problems. There was a tendency to downgrade and neglect by attrition courses of study and areas of research in mathematics that might interest physicists, engineers, or indeed any other professional whose research interests intersected those of mathematicians.

In their turn physicists set out to teach their students the mathematics they required to become physicists. Beginning with Thomson and Taits' text in mechanics, treatises, and volumes of lectures established the mathematical foundations for the solutions to physical problems.[15] In Thomson and Tait's text mathematics was dispersed throughout. Until mathematics became necessary to further the discussion, physics developed in the vernacular. The authors only introduced enough mathematics from the appropriate field to solve the physical problem. Mathematics entered the middle of the discussion of kinematics. Similarly with simple harmonic motion and Fourier analysis, with only enough Fourier to solve the problems of "sonorous vibrators." More extended discussion of mathematical issues were in a series of appendices extending Green's theorem, spherical harmonics and so on. Mathematics was in the context of specific physical problems, for the solution to those problems.

Textbook writers assumed that physics students needed to be familiar with differential and integral calculus. However, it was calculus molded to the needs of solving problems. In the case of Thomson and Tait, the problems were in mechanics.[16]

Unlike Thomson and Tait's text, Maxwell's *Treatise* was in a new field of theoretical physics. It served as a survey of the fields of electro- and magneto-statics and the whole of electrodynamics. In addition the text presented his own theories

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ics. For Riemann's geometry see J. J. Gray, *Ideas of Space: Euclidean, non-Euclidean, and Relativistic* (Oxford: Oxford University Press, 1989). For a recent assessment of Grassmann and his influence into the twentieth century see *Hermann Günther Grassmann (1809–1877),* Gert Schubring, ed. (Dordrecht: Kluwer Academic, 1996). For the philosophical aspects of his mathematics see A. C. Lewis, "Hermann Grassman 1844 *Ausdehnungslehre* and Schleiermacher's *Dialetik*," *Ann. Sci.* 34 (1977): 103–162.

15 Helmholtz oversaw and wrote the introduction to the German translation of Thomson and Tait's text in the 1870s.

16 William Thomson and Peter Guthrie Tait, *Treatise on Natural Philosophy* (New York: Dover reprint, 1962). The mathematics for kinematics begins on p. 3, that for simple harmonic motion is between, 38–59.

on these subjects, current electricity and the theory of the electromagnetic field. However, Maxwell treated mathematics in a similar manner. After a description of the phenomena of electrostatics came a discussion of its mathematics and proofs of some fundamental theorems. In other chapters the characteristics of the potential function were explored and then used in the solution of physical problems. Green's theorem appeared elsewhere and another section was devoted to spherical harmonics and surfaces. In the second volume mathematics was inserted again where and how much Maxwell judged necessary for the physics that surrounded it.[17]

In the above examples mathematics and physics occur in immediate contact. Less intimate relations between the two occurred in texts from the continent. Clausius presented the mathematics of the potential function in a chapter separate from the physics of the potential. Helmholtz and Kirchhoff introduced the mathematics necessary for the physics in separate lectures.[18]

Hendrik Antoon Lorentz took this trend further. He produced a textbook in mathematics for physics students whose needs he claimed were not met by existing textbooks. Mathematical definitions and proofs were rigorous enough. However, Lorentz only took the mathematics as far as was necessary to solve the physical problems that form the problem sets at the end of each chapter. He built the subject matter from one chapter to the next with repeated use of mathematical techniques in examples.

Lorentz used definitions of the derivative and integral of limited use to a mathematician. They were sufficient for him to then deduce expressions for the motion of falling bodies. In the rest of the text mathematics was presented as a series of techniques to solve physics problems. The physics presented a unified whole, the mathematics was fragmented. As important, the mathematics is used then reduced to a numerical expression to demonstrate the behavior of physical phenomena.

Lorentz began in algebra, and ended with Fourier series and differential equations. Because he used Taylor series his chapters on calculus from a mathematical point of view were of limited use. He also found it necessary to defend his extended presentation of Fourier series and complex analysis for physics students. The text, clearly for undergraduates, demonstrated the distance between the calculus of the physicist in the 1870s and that of the mathematician of the same era. Much of this mathematics harked back to a mathematics that was sixty years

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17 James Clerk Maxwell, *Treatise on Electricity and Magnetism* (New York: Dover reprint of third edition, 1954), vol. 1 chap. xii on electrostatic equilibrium and conjugate functions where Maxwell puts various functions to particular physical uses, 284–316.

18 Clausius, *Die Potentialfunction und das Potential: ein Beitrag zur mathematischen Physik* (Leipzig: Barth, 1859), Helmholtz, *Vorlesungen über die theoretische Physik* 3 vols., (Leipzig: Barth, 1903). In the volume on electrodynamics there is a chapter on the potential function. Kirchhoff, *Vorlesungen über mathematische Physik* 4 vols. (Leipzig: Teubner, 1876–1895).

old.[19] In physics textbooks the standards of solution to differential equations, convergence criteria, and other mathematical processes, remained those of an earlier era in mathematics, that of the first third of the nineteenth century. These were well below the standards demanded by mathematicians of the middle of the nineteenth century. Mathematics was becoming codified into a skill, on a par with the manipulative ones routinely taught students for successful work in the laboratory. Students needed only that amount of mathematics to demonstrate the validity of the physics expressed mathematically. Beyond that mathematics could be safely ignored.

In the decades after 1870 physicists, along with courses in laboratory practice, developed parallel courses to develop fluency in the language of theory, independent of mathematicians. Most physicists found contemporary mathematics irrelevant for their research or their teaching. Some deplored the loss of the close relationship that physicists once had with their mathematical colleagues. Simultaneously they lamented that recent research by mathematicians had become useless for physicists. Indeed some physicists declared that mathematical research ceased to be of interest to physicists after 1830.[20] Yet to even study the subject of theoretical physics, students of the 1870s and 1890s needed a knowledge of ordinary and partial differential and integral calculus. Whether the students were taught these subjects within the physics department or in the mathematics department by mathematicians depended largely on the interests and talents of both physicists and mathematicians and their interactions at specific institutions.[21]

Another measure of the separation of these two disciplines was the attempts of theoretical physicists to express the differences between theoretical physics, mathematical physics, and mathematics. Even given the formal context in which many of these nineteenth-century ruminations were made and the limitations of addressing a general audience, some factors remain as commonalities. Mathematics was taken for granted. There was no need to defend its use or explain its place within theoretical physics. What required more attention was the status of certain hypotheses, their uses within theoretical physics and the necessity for hypotheses in physics.[22] The use of hypotheses and mathematics together, as Maxwell remarked in the 1870s, marked the emergence of a new discipline.

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19 Hendrik Antoon Lorentz, *Lehrbuch der Differential- und Integralrechnung und der Anfangsgründe der Analytischen Geometrie* (Leipzig: Barth, 1900). The original Dutch edition appeared in 1882.

20 See Jungnickel and McCormmach, *Intellectual Mastery,* vol. 2.

21 For the variety of such arrangements, see Jungnickel and McCormmach, *Intellectual Mastery* vol. 2.

22 Boltzmann, "On the Development of the Methods of Theoretical Physics in Recent Times," in Boltzmann, *Theoretical Physics and Philosophical Problems,* Brian McGuinness, ed. (Dordrecht: Reidel, 1974), 77–99.

Mathematics in Physics

Mathematics had become almost taken for granted as a skill that was now simply necessary to become a physicist. That mathematics was in a form that spoke directly to the needs of physicists and to the solutions to their problems. For mature physicists and students alike, only aspects of theorems and areas of analysis or results directly pertinent to the solution of physics problems received detailed attention. Green's and Stokes' theorem and Fourier analysis were tailored to the needs of physicists. Mathematics had been domesticated.

No physicist, experimentalists included, could afford to ignore mathematics. It was the language required to understand the operation of their instrumentation, apparatus, and the meaning of their results. Also physicists now expected theoretical discussions to be a combination of mathematical language, with aspects of mathematical methods of proof, together with a measure of physical insight coupled with experimental evidence. Yet the balance of mathematics, imagery and empirical evidence remained a matter of individual choice. In theory development there remained a spectrum of uses of mathematics. This included a mathematical physics in which there was more concern with mathematical standards that troubled most other colleagues within physics. Some physicists would not have been out of place in a mathematics department.[23] At the other extreme there were experimentalists who used and required of others only the minimum of mathematics.

In the 1870s for some theoreticians, such as Helmholtz, physical results sprang directly from mathematics without the intermediary of models. Others used models to create then guide the development of the mathematics and interpret the results of their manipulations. Maxwell moved freely from one end of this spectrum to the other. He was well aware of the limitations of using models and the added credibility results acquired when grounded in a mathematically expressed physics based in the general principles of mechanics.

In German physics departments during the last third of the nineteenth century we can see a trend away from theory based in the specifics of models towards one based in principles.[24] This trend is illustrated through the physical work of Helmholtz during this period. By 1870 Helmholtz had acquired the reputation of an intellect in a class by himself. This was further enhanced by his institutional position as recently appointed director of the new physics institute at Berlin university. During the next decade Helmholtz's research focussed in one of most intensely researched and competitive areas of physics, electrodynamics.[25] While

23 Examples include Ludwig Boltzmann and Carl Neumann. For a short period of time in the 1870s Boltzmann was professor of mathematics at Vienna University.

24 One notable exception to this is Rudolph Clausius, but his career began in the 1840s. The other is Ludwig Boltzmann.

25 This was closely connected with the increasing economic importance of the telegraph, the

most historians of physics have focussed on the physical content of Helmholtz's papers few have looked seriously at the language within which those physical arguments were expressed and the role that mathematics played in establishing the ideas they zealously explored.[26]

For Helmholtz mathematics functioned both as a mediator between principles and experiment and a means to rise above the particularities of the different approaches to electrodynamics of Weber, Franz Neumann and Maxwell. His investigations into electrodynamics began as a critical survey of the field put into general mathematical form. They continued both as a defense of his interpretations and an attempt to improve what others had begun. Helmholtz explicitly recognized the validity of other approaches, specifically of using models, having "no essential objections" to them. However, he noted his aversion to the inexplicable in physics, although mechanics should not be merely a "field for mathematical exercises." He found far more satisfactory "the simple representation of physical facts and laws in the most general form, as given in systems of differential equations," and adhered to the latter which he found also safer.[27] He chose to construct an electrodynamics that was based on the results of experiment and the laws deduced directly from them (Ampère's law for example). Expressing these laws with mathematical generality, he attempted to encompass in one expression all known experimental results, and this allowed him to judge the validity of the particularist theories of Weber, and Maxwell, and the mathematical theory of Franz Neumann.[28]

Beginning with Ampère's and Coulomb's laws, Helmholtz constructed an expression for the potential between two current elements, or at least reached an expression that behaved as a potential and then treated it as such. The potential was equivalent to work done. This allowed him to reach an expression for the conservation of force in a domain of physics outside of mechanics, without assuming a mechanical character to the system. The potential law was fundamental to electrodynamics and brought to its study a unity not available with any other

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telephone, electric motors, and finally the development of the grid system. See Thomas Hughes, *Networks of Power*.

26 For example Jed Z. Buchwald, *The Creation of Scientific Effects: Heinrich Hertz and Electric Waves* (Chicago: University of Chicago Press, 1994), Part I, and Appendices 2 and 3 where Helmholtz's mathematics is transposed into vector form, and Buchwald, *From Maxwell to Microphysics* (Chicago: University of Chicago Press, 1985), Part IV. See also S. D'Agostino, "Hertz and Helmholtz on Electromagnetic Waves," *Scientia* 106 (1971): 637–648, and "Hertz's Researches on Electromagnetic Waves," *Hist. Stud. Phys. Sci.* 6 (1975): 261–323, 273–279. These are examples, the practice is commonplace.

27 Helmholtz, "Preface," in Heinrich Hertz, *The Principles of Mechanics,* D. E. Jones and J. T. Walley, trans. (New York: Dover reprint of 1896 trans., 1956).

28 Jungnickel and McCormmach, *Intellectual Mastery,* vol. 2, 22, discuss the links between Helmholtz's electrodynamics papers and his earlier, experimental work on the timing of nerve impulses.

approach.[29] Also it reduced some second order differential equations to first order making their solution feasible.

His general expression for the potential between two current elements was

$$-\frac{1}{2}A^2\frac{ij}{r}\Big([1+k]cos(Ds, D\sigma)+[1-k]cos(r, Ds)cos(r, D\sigma)\Big)DsD\sigma,$$

where current intensities i and j pass through circuit elements Ds and $D\sigma$ at a distance r apart. Of the two constants in this expression, A was the reciprocal of the velocity of light and the value of k identified the expressions for the potential from the different theories of Weber, Neumann and Maxwell. The value of k, $k = -1, 0, +1$, carried physical significance and the physical consequences were hunted down and judgments made as to the validity of the three approaches. If $k = -1$ the velocities became infinite, a result incompatible with the conservation of force. Hence his judgment against Weber's theory.[30]

From experiment and physical principles Helmholtz moved to mathematics, and from the mathematics deduced physical significances in terms of the entities from which he began his argument. In the case of electrodynamics, mathematics allowed him to demonstrate the superficial character of many of the details of the modelling he encountered and show such modelling was irrelevant to the physical analysis he was seeking.[31]

Helmholtz made judgments in terms of the physics represented by the functions, terms, coefficients, etc. The mathematics carried physical significance.[32]

29 See Helmholtz, "Kritisches zur Electrodynamik," *Ann. Phy.* 153 (1874): 545–556.

30 Helmholtz, "Über die Bewegungsgleichungen der Electricität für ruhende leitende Körper," *J. Reine Angew. Math.* 72 (1870): 57–129. This judgment reinforced an earlier one made in Helmholtz, *Erhaltung der Kraft.* Other papers in this series include, Helmholtz, "Über die Fortflanzungsgeschwindigkeit in elektrodynamischen Wirkungen," *Monats. Akad. Berlin* (1871): 292–298. In "Über die Theorie der Elektrodynamik," *J. Reine Angew. Math.* 75 (1873): 35–66, Helmholtz considered induction, in "Die elektrodynamischen Kräfte in bewegten Leitern," same journal 78 (1874): 273–324 where he introduced ponderomotive forces. Shorter versions of the arguments of these papers appeared elsewhere.

31 While it is true here and elsewhere, this is only useful after the fact; it was a pattern that Maxwell followed about the same time. Both were seeking a physics based in general, physical principles. See Maxwell, *A Treatise on Electricity and Magnetism,* vol. 2, chap. vi for his attempt to put his electromagnetic theory in Lagrangian form. The limitations of Maxwell's attempts are discussed in Tetu Hirosige, "Origins of Lorentz's Theory of Electrons and the Concept of the Electromagnetic Field," *Hist. Stud. Phys. Sci.* 1 (1969): 151–209, 192.

32 This differs from the analysis of Buchwald in, *Scientific Effects,* of Helmholtz's work in electrodynamics. Buchwald interprets Helmholtz's work as resting on a physical argument that is "implicit" in Helmholtz, that has a "natural energetic interpretation." Maybe, but only from the perspective of the twentieth century.

Helmholtz tied his mathematics in this series of papers directly to principles; in the first paper to that of the "conservation of force." Until 1873 the dominant physical language of the papers in this series is about force and potential, not energy.[33] However, energy arguments gave Helmholtz further points upon which to criticize Weber's electrodynamics.. The terms in any expression for the conservation of energy must be positive. From the mathematical form of a two-part term in his interpretation of Weber's work, Helmholtz argued that in Weber's theory energy could become negative and perpetual motion was possible. The argument hinged on the mathematical representation of physical quantities that, because they were physical had to behave according to general physical principles. While appearing excessively mathematical, and Helmholtz did construct the most general mathematical cases, he followed only the implications of physically significant cases.

There were serious limitations to this approach. Constants appearing in equations could only be determined by experiment, if such coefficients were amenable to experiment. There was no direct connection between mathematics and the laboratory because the differential equations and their solutions were so general. To go further than present experiments required some kind of modelling. Furthermore, mathematics even in known cases was not an infallible guide to physical consequences. Helmholtz's original argument against Weber's theory was that the function representing the potential could, in certain circumstances, become negative. This implied that the velocities of the electrical particles could become infinite. In the ensuing exchange, others pointed out that any increase in velocity would lead to an induced force that would decrease the particles' velocities.[34] To clinch his argument and choose between these theories required experiment. Devising these, then carrying them through, or at least getting his students to do so, was a difficult task that consumed much of the decade of the 1870s.[35]

Helmholtz passed this potent combination of experiment and mathematics to all his students and in particular to Heinrich Hertz. Eventually Hertz realized the limitations of Helmholtz's approach. Physical imagery was necessary and in this area Helmholtz's mathematical approach led to inconsistent physics. With this insight Hertz began to understand experimental results that had puzzled him and he was

33 While Helmholtz had remarked in 1869 that his conservation of force had been renamed conservation of energy, potential rather than energy remained the mathematically preferred form in German physics into the 1870s. See Norton Wise, "German Concepts of Force, Energy, and the electromagnetic Ether: 1845–1880," in *Conceptions of Ether,* Cantor and Hodge, eds., 269-307.

34 See Weber, "Maasbestimmungen," *Ann. Phy.* 4 (1878) : 366–373. This was finally recognized by Helmholtz in an afterword (1881) added in his collected papers. See Helmholtz, "Über die Theorie der Elektrodynamik," *J. Reine Angew. Math.* 75 (1873): 35–66, reprinted in *Abh.* vol. 1, 647–687, "Zusatz (1881)," 684–687.

35 For a discussion of this aspect of Helmholtz's work, see Jungnickel and McCormmach, *Intellectual Mastery,* Vol. 2, 25–30.

free to explore his own theoretical investigations into Maxwell's theories. Under limiting conditions Helmholtz's equations replicated Maxwell's, but they were not physically equivalent. There was only mathematics. In this case "the physical basis of Helmholtz's theory disappears."[36] Physical imagery was necessary to clothe the mathematical forms. He set about rethinking the physical underpinnings of his own and Maxwell's theories. However, Hertz was finally reduced to accepting the situation that he saw as a flaw in Helmholtz's work. Hertz was unable to reconcile Maxwell's ideas and his mathematics, and he was resigned to accepting Maxwell's equations as Maxwell's theory. He then went on to claim that the "inner significance" of his own and Maxwell's equations, although different in form, were the same. To explain his experimental results in terms of electromagnetic waves in the ether, Hertz simply assumed Maxwell's equations. His focus then shifted to explaining their physical implications and the legitimacy of his experimental results.[37] A fuller version of his theoretical ideas followed, in which he gave general mathematical expression to a physically consistent image of the origins of electric and magnetic forces in the ether.[38]

Many German theoretical physicists did not accept Helmholtz's judgments on Weber's electrodynamics and long a lasting schism opened up between supporters in both camps that affected careers into the next generation. For some, the issue became Helmholtz's mathematics and whether his differential equations represented Weber's fundamental laws. Beyond the mathematics was a physical imagery, which if successfully challenged, would destroy more than Weber's work.[39] Despite these tensions Helmholtz renewed a pattern of doing theoretical physics in Germany that was universal. Mathematics offered a unity for physics through its abstractions as the analytical language of physics.[40] The principles of mechanics was the center, the means to draw together the other branches of physics, heat,

36 For the most detailed consideration of Hertz's break with Helmholtz and the development of his experimental work and its relations to his rethinking and reformulation of electrodynamics, see Buchwald, *Scientific Effects.*

37 Hertz, "The Forces of Electrical Oscillations treated according to Maxwell's Theory," (1889) in Hertz, *Electric Waves,* D. E. Jones trans. (New York: Dover reprint of 1893 edition, 1962), 137–159.

38 Hertz, "On the Fundamental Equations of Electromagnetic Bodies at Rest," (1890) in Hertz, *Electric Waves,* 195–240. This demand for the generality of mathematics together with a defensible, consistent physical imagery also drove Hertz in the production of his text on mechanics. See Hertz, *The Principles of Mechanics,* D. E. Jones and J. T. Walley, trans. (New York: Dover reprint of 1896 trans., 1956).

39 For a discussion of this see Buchwald, *Scientific Effects,* Appendices 6 and 16. This was in the same period that Helmholtz was arguing with mathematicians and philosophers over the foundations of geometry. Helmholtz, "The Origin and Meaning of Geometrical Axioms," (1870) in *Science and Culture: Popular and Philosophical Essays,* David Cahan, ed. (Chicago Ill: University of Chicago Press, 1995), 226–248.

40 For the theme of unity in science and physics in particular and its professional and

electrodynamics, light, the study of solids, and gases. The creation of a mathematical language that transcended the particularities of these separate domains was sometimes the explicit goal of theoretical physicists. In others it can be seen in the pattern of their work throughout their lifetimes.[41]

This same approach to theory through the grand unifying principles of physics was reflected in the next generation in the work of Max Planck. While not rederiving equations familiar to his readers, he discussed only those aspects of the physical issue at hand that were new. It is a spare style that was unusual in the late nineteenth century. Theoreticians were apt to rederive in their own way all the equations they might need. Use of mathematics was an indication of the seriousness and the professionalization of the theoretical enterprise. Planck simply imported them as needed. He developed just enough mathematics necessary to make his physical argument. No superfluity of generalization to demonstrate mathematical prowess.

Planck claimed later that mathematics was a mere instrument, and that his focus was on a physics based in the most general principles possible.[42] However, all his physical arguments depend directly on the mathematical forms in which those principles were expressed. Because his physics was one of principle there was no intermediary models, or particularist assumptions. The only medium for the development of his ideas was in the structure of the language he used–mathematics. And contrary to later statements, he appears to have understood this. A theory stood or fell with its equations. The physical imagery was flexible, and might be omitted altogether; the proof of the equations was another matter. He paraphrased Heinrich Hertz stating that Maxwell's electromagnetic theory of light was his equations. Planck went further to claim that equations were essential, all else besides the mathematics could be discarded, hardly a tool-like judgment.[43] This was especially the case in his early papers on thermodynamics where he focussed on entropy. Here sophisticated mathematics was of far less use to Planck than to his contemporaries enmeshed in the complexities of electrodynamics.[44] Math-

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cultural meaning in late nineteenth-century Germany see Peter Galison, "Introduction: The Context of Disunity," in *The Disunity of Science,* Peter Galison and David J. Stump, eds. (Stanford: Stanford University Press, 1996), 1–33.

41 This was not unique to Helmholtz. This was true of J. J. Thomson and Joseph Larmor who also used Hamilton's principle to achieve such a unity.

42 See D. de Causabon, "Le rôle des mathématiques dans la physique, Planck, 1894–1900," *Fund. Sci.* 6 (1985): 281–197, and Jungnickel and McCormmach, *Intellectual Mastery.*

43 Max Planck, "Die Maxwellsche Theorie der Elektrizität von der mathematischen Seit betrachtet," *Jahresber. Dtsch. Math. Ver.* 7 (1899): 77–89.

44 In the last three decades of the nineteenth century, the bulk of theoretical papers in the *Annalen der Physik,* although the majority of them were experimental, was in electrodynamics. This was also true of the mathematical and theoretical papers by physicists in the *Journal für Reine und Angewandte Mathematik.* See Jungnickel and McCormmach, *Intellectual Mastery,* vol. 2, chap. 1.

ematical complexity would not lead him into physically fruitful directions. His initial work was on the significance of entropy and the implications of the first and second laws of thermodynamics in specific cases.[45] Planck was after physical connections but gaining those connections directly from the mathematics. And Planck managed to squeeze as much physical significance as possible out of the partial differential equations at his disposal. No mathematical expression was left without physical comment or explanation. He also made no attempt to generalize them. His mathematics referred only to physical cases and conditions, preferably linked directly to numerical data. In his papers on critical states Planck specified those states without any assumptions about the inner structure of matter; thus the numerical aspects of the papers lent him the specificity necessary for theory in the 1870s.

Even as Planck turned his attention to electromagnetism and black body radiation, these same characteristics mark his papers from those of his colleagues. He imported equations where he could, using mathematics itself sparingly. His physical explanations were as spare as his mathematics and stuck to the essential points of principle and their implications in the example at hand. Planck made as few assumptions as possible about the nature of his resonators or the electromagnetic radiation with which they interacted.[46] He introduced the notion of "natural radiation" in his fourth paper on the subject. In the last paper he pulled together the work he had accomplished thus far on the problem, and here began to use Fourier series to characterize the radiation impinging on a resonator, as well as investigating the energy and entropy of the same resonators.

His first two papers in quantum theory exemplify his approach. Initially he stated only the new mathematical expression for the energy distribution and the new thermodynamical foundations on which the expression was based. He had previously established that the energy distribution was determined once the entropy, S of a resonator was known as a function of its vibrational energy U. As he had also already determined, the second law was not sufficient to calculate this function. To arrive at his new law, Planck constructed "completely arbitrary expressions for the entropy" that would not lead to Wien's law and satisfied both thermodynamical and electromagnetic considerations. Not specifying what this expression was, Planck

45 See his dissertation, Max Planck,"Über zweitzen Haupsatz der mechanischen Wärme Theorie," Munich 1879 in *Physikalische Abhandlungen und Vorträge* 3 vols. (Braunschweig: Vieweg und Sohn, 1958), vol. 1, 1–60, and Planck, "Verdampfen, Schmeltzen und Sublimiren," *Ann. Phy.* 15 (1882): 446–475, *Abh.* 1, 134–163.

46 The series of papers Planck produced on black body radiation begins with Planck, "Absorption und Emission elektrischer Wellen durch Resonanz," *Ann. Phy.* 57 (1896): 1–14, and continues with "Über elektrische Schwingungen, welche durch Resonanz erregt und durch Strahlung gedämpf werden," same journal 60 (1897): 577–599, and a five part series, "Über irreversible Strahlungsvorgänge," *Sitz. K. Preuss. Akad., Berlin* (1897): 57–68, 715–717, 1122–1145, (1898): 449–476, 440–480.

stated he was led to it from

$$\frac{d^2S}{dU^2} = \frac{\alpha}{(U(\beta + U)},$$

where α and β are constant and U is the energy of the resonator. From this the radiation law followed,

$$E = \frac{C\lambda^{-5}}{e^{c/\lambda T} - 1},$$

where E is the energy density between the wavelengths λ and $\lambda + d\lambda$.[47]

While this might be taken as a short statement establishing Planck's priority for deriving this expression, his follow up was only a mathematical skeleton of those aspects of the theory that were new and presented just to give physical meaning to the above expression. Planck found it necessary to turn to Boltzmann for a new physical understanding of entropy. Entropy was disorder, and that meant irregularity in the changes in amplitudes, and phases of the radiation of the oscillators even in a stationary radiation field. This disorder could only be understood using probability, introduced into thermodynamics by Boltzmann.

The complete deduction of his final equation for the energy density would require Planck to recapitulate much of his work in electromagnetism along with the full thermodynamical deduction of the energy of a resonator. Planck merely sketched what was new. Viewing resonators as groups as before, he constructed the distribution of energy not by considering the resonators themselves, but by looking at the distribution of energy over the frequencies of the oscillations to find the energy of the whole as a function of the vibrations and the temperature of the system. Unlike Boltzmann, Planck did not, as was still usual in statistical arguments in physics, treat energy as a continuum, and thus introduced the element of discontinuity into the mathematics of physics, pushing that mathematics further beyond the calculus.[48] Planck assumed that his audience was familiar with permutations and probabilities. He gave only one simple numerical example before presenting the general expression for the number of permutations of P "energy elements" among N resonators. He then looked for the most probable distribution of the total energy among all the ways of distributing that energy in P amounts among N resonators. He further adds the necessary thermodynamic expressions to reach

$$u_\nu d\nu = \frac{8\pi h\nu^3}{c^3}\frac{d\nu}{e^{h\nu/k\theta} - 1},$$

47 Planck, "Über eine Verbesserung der Wien'schen Spektralgleichung," *Verh. Dtsch. Phy. Gesell.* 2 (1900): 202–204.

48 The most extensive examination of Planck's use of discontinuity in his expression for the energy distribution function was that by Thomas Kuhn, *Black–Body Theory and the Quantum Discontinuity, 1894–1912* (New York: Oxford University Press, 1978). Kuhn revised his argument in Kuhn, "Revisiting Planck," *Hist. Stud. Phys. Sci.* 14 (1984): 231–252.

for energy distributed between the frequencies ν and $\nu + d\nu$.[49]

The systematic treatment promised by Planck was not forthcoming until his text on heat radiation was published six years later.[50] By this date, there were several areas in physics in which the calculus no longer sufficed as a satisfactory, descriptive language. Those areas included both electrodynamics and the behavior of gases. And in both of these subfields of theory, models and particulars played a crucial role in their conceptual and linguistic development.

Beyond the Calculus

Two points need to be made about this era in nineteenth century physics. First, mathematics was taken as a natural aspect of physics. Secondly, the skill of theoretical physicists was judged on their creative development and manipulation of physical imagery. Especially valued was imagery on the highest level of abstraction that unified ever broadening types of phenomena across different domains of physics. However, during the 1890s these two characteristics were challenged. Mathematics came to the foreground. The calculus no longer seemed adequate to describe some of those domains in physics that were the most promising for unifying physics, that is electrodynamics and thermodynamics. Greater unification in physics was not reached through abstraction but through the specificity of models, much of it the work of Lorentz and Boltzmann.

Lorentz worked and then reworked his electrodynamics throughout his career.[51] Like the structure of his papers, his return to the same central issues in theoretical physics were systematic and driven by the need to incorporate new experimental work, to clarify, then extend previous work.[52] His basic model was of a continuum ether with only electromagnetic properties in which were embedded charged particles. All interaction between matter and the ether were through these particles. Lorentz began with a physical case that led to equations that were then added to term by term as he developed the physical situation. He translated physical

49 Planck, "Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum," *Verh. Dtsch. Phy. Gesell.* 2 (1900): 237–245.

50 Planck, *Vorlesungen über die Theorie der Wärmestrahlung* (Leipzig: J. A. Barth, 1906). A second edition that was reworked in terms of quantum theory and published in 1913 and this edition was translated into English. See Planck, *The Theory of Heat Radiation,* Morton Masius, trans. (Philadelphia: P. Blakiston's Son and Co., 1914).

51 This focus of research emerged with his dissertation in 1875, Lorentz, " Sur la théorie de la réflexion et de la réfraction de la lumière," Leiden, 1875 in *Collected Papers,* Pieter Zeemann and A. D. Fokker, eds. (The Hague: Martinus Nijhoff, 1935–1939), vol. 1 193–383. Lorentz constructed the laws of reflection and refraction using the electromagnetic theory of light.

52 Here we are focussing on Lorentz's use of mathematics. For the development of his physical ideas, see Jungnickel and McCormmach, *Intellectual Mastery,* vol. 2, 232–236 and the sources cited there.

characteristics and processes directly into separable mathematical entities that carried with them identifiable physical outcomes. The details of his physical model were added as the complexity of the interactions of ether and matter developed. Thus he mathematically and physically separated electrostatic phenomena from those involving the motion of particles, "ions," within their "holes," i.e., dielectric displacement, and those from the motion of the molecules themselves, i.e., electrodynamical effects.[53] Each state led to forces that were represented as distinct terms that were added one to the other. [54]

Once the physical situation was represented, Lorentz brought all the devices of the calculus to solve the equations. He expressed sets of partial differential equations in terms of potentials, thus reducing them from second order to first order and possible solution. He used Taylor expansions that might later be simplified to conform to physical conditions, and Green's theorem, and so on.

He also constructed his papers like mathematical papers. His first conclusions were a set of straightforward mathematically deduced physical results. These were called upon later as he built his argument from electrostatics to the motion of "ions" through the ether and the appearance of electromagnetic waves. While the mathematics was elegant and might be developed with some generality, it was also tightly bound to the pursuit of his physical quarry. A function represented a physical quantity that was named and followed throughout his mathematical excursions. The only cases he pursued were those that led to physically significant results. However, mathematics imposed its own limitations on the behavior of the entities they represented. The functions representing the ether were continuous and the ether therefore defined rigorously.

With the acceptance of Hertz's experimental findings and the reality of electromagnetic waves established, Lorentz and other theorists could take that aspect of Maxwell's theory as an experimental given. In 1892 Lorentz's work on electrodynamics was reoriented and put into a unified mathematical structure where he continued Maxwell's effort to put field theory into a "dynamical" Lagrangian formulation. Using the energy equation and the Principle of Least Action (here reduced to d'Alembert's principle) Lorentz brought mathematical simplicity to his theory. In this new mathematical form, he replicated previous results and integrated dieletric displacement into this view. While claiming its "dynamical" character Lorentz had actually developed a formal, mathematical theory in which physical

53 See Lorentz, "Concerning the Relation between the Velocity of Propagation of Light and the Density and Composition of the Media," *Verhand. K. Akad. Weten. Amsterdam* 18 (1878): 1, in *Collected Papers,* vol. 2, 1–119. His ions had only electromagnetic properties from p. 23 until part III of the paper when he introduced dispersion and his particles were then endowed with inertia.

54 See Lorentz, "Concerning the Relation," p. 35–36 where he reconstructs the forces generated in the ether from moving "molecules" that he has built up in pieces especially over pps. 21–35.

entities were treated as mathematical ones, as functions, terms, and constants. However, once in these mathematical forms, they were not translated back into physical form unless Lorentz was at a point where he could compare his theoretical speculations directly with experiment. He needed many mathematical tricks to replicate known results, and one of these was the retarded potential. The retarded potential kept the form of the function for electricity and magnetism complete analogs with potentials from the simpler case without charged particles moving through the ether.[55]

In this 1892 paper Lorentz's derivations were obscure as was the physical meaning to be attached to some of them. This changed in his next reworking of electrodynamics of 1895. For us, much of the clarification was due to the mathematical form in which the physics appeared–vector analysis.[56] He used this recently developed algebra to simplify and emphasize the physical content of an argument that only three years before was wrapped in the algebra of differential calculus. It is here that Lorentz presented a form of electrodynamics that is physically recognizable to us. The origin of electrodynamical effects lay in his "ions" i.e., electrons, not in the field.[57] Given that he still worked within the physical framework of the ether, the physical point to this long papers is far more visible and sensible to the late twentieth century.[58] His assertions about the existence of ions, from chemical evidence, was firmer and he simply assumed the validity of Maxwell's equations in vector form and no longer sought to derive them. However, in 1895 his notion of ions was still controversial. "Ions" were posited only through indirect, chemical means. J. J. Thomson's experiment that demonstrated their physical characteristics were three years in the future. Equally unusual was the mathematical form in which he presented his physics. Lorentz did sprinkle the older, component forms of some crucial relationships alongside their vector forms, especially in the earlier chapters. This may be in recognition of the novelty of his use of vectors, and the need to reassure colleagues. The vector form of his argument may well account for the lack of immediate enthusiasm of his contemporaries for this work.

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55 Lorentz, "La théorie électromagnétique de Maxwell et son application aux corps mouvants," *Arch. neerl.* 25 (1892): 363, *Collected Papers* vol. II; 164–343, p. 299. While this paper is important for the clarification of Lorentz's physical ideas on the nature of the interaction of matter and the ether, it is its mathematical character that concerns us here. For the physics see, Hirosige, "Origins," and Buchwald, *From Maxwell to Microphysics,* 194–196.

56 Lorentz *Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern* (Leiden: Brill, 1895), reprinted in *Collected Papers*, vol. 5, 1–137.

57 For a discussion of the transition from Maxwell's electromagnetic theory of light to Lorentz's "microscopic" theory see, Buchwald, *From Maxwell to Microphysics.*

58 Because of his theoretical commitment to seeing the origins of electromagnetic effect in electrons and their motions Lorentz had to reinterpret, yet again, crucial aspects of Maxwell's theory such as the displacement current.

He also focussed on one general issue, the motion of ponderable matter through the ether, not on the front burner for most other theoretical physicists working in electrodynamics.[59]

Lorentz demonstrated a mastery of vectors, yet his use of mathematics itself and its relationship to his physical ideas remained the same. Maxwell's equations and common relationships in electrodynamics were all translated into vector form. His focus on the problem of charged bodies moving through the ether meant that Lorentz only sketched in the aspects of electrodynamics that did not pertain to this main problem. However, when he reached the main issue, mathematics still carried much of his argument. Mathematical devices maintained Maxwell's laws when he transformed his reckoning of them from the "stationary" coordinates of one set of "ions" to the set of "ions" moving through the ether.[60] The relationship between time and the coordinates between the two systems were introduced as "new variables." Only the time-transformation was commented upon physically. Time measured in the moving system is "local," and he referred the reader back to the mathematical relationship for the conversion from one to the other. The relationship is mathematical rather than physical.

Vectors, the algebra that seems to us to make the physics of Lorentz's arguments visible in ways in which his earlier mathematical treatments do not, were still a marginal mathematical form for physicists in 1895, and of no interest to mathematicians. Along with statistical arguments, they had been developed by physicists strictly for their own uses within the context of their own problems. It was only later that mathematicians were to take up either again as mathematics and generalize both statistics and vector algebra.

While combinatorial methods were considered a legitimate aspect of mathematics in the nineteenth century, the calculus of probability and statistics challenged the prevailing thrust of mathematicians towards rigor. Rigor implied that proofs were either correct or incorrect, with truth established in some absolute form. Arguing probabilities did not fit this search for certainty and firm foundations. Additionally, these methods studied by mathematicians in the eighteenth and early nineteenth centuries arose in the context of social and experimental concerns in other disciplines, also recently repudiated as a source for research problems in mathematics. As recent work has demonstrated, the mathematical development of statistics shows a hiatus between the work of Laplace and the English biometricians of the late nineteenth century. [61]

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59 On this point see Buchwald, *From Maxwell to Microphysics*, p. 198–199.

60 This is stated deliberately to capture Lorentz's concerns to replicate Maxwell's equations, rather than those of the twentieth century by not using the term "invariance."

61 See Lorraine Daston, *Classical Probability in the Enlightenment* (Princeton NJ: Princeton University Press, 1988) for mathematicians in the eighteenth century. Stephen M. Stigler, *The History of Statistics: The Measurement of Uncertainty before 1900* (Cambridge MA:

The advent of probability to describe then explain the regularities in phenomena opened up new relationships between physical imagery and the mathematical languages in which they were expressed. This ultimately forced physicists to abandon the continuity inherent in the calculus and embrace a physics of the discontinuous in quantum physics. This final break was avoided during the nineteenth century. While both Maxwell and Boltzmann worked from the interactions of particular mechanical systems in collisions, they realized that probability expressed the frequency with which events occurred in a collection of systems and did not reflect the inner workings of the systems that made up the collection. Both men began by considering particular interactions between particles of certain types, then constructed the average of the characteristics, such as density of particles, and the distributions of velocity, momentum, or kinetic energy, in a system in equilibrium. The averages of these quantities reflected the macroscopic behavior of the systems, in this case of gases.

Merely specifying that probability arguments were introduced into physics is inadequate to describe precisely how they were used. This general description does not determine how probability was defined and then expressed mathematically and what properties of molecules in motion Boltzmann or Maxwell chose to follow, or the regularities they chose to express and link to macrophenomena. From his first papers Maxwell developed expressions for the density, and the distribution of velocity, momentum, kinetic energy, etc. He then argued from the macroscopic behavior of diffusion, viscosity, and heat conduction which particular integral expressing what average linked the behavior of the collection of molecules to their observed phenomena. He went further and developed expressions for the mean free path of a molecule and an estimate of Avogadro's number. The consistency of his own and others' experimental results on the transport properties of gases in the 1860s argued for the plausibility, not just the convenience, of considering matter as made up of molecules.[62]

While they influenced each other, there were technical differences beginning with their definitions of probability. Maxwell and Boltzmann had distinct purposes in studying gases that centered on different physical issues. Maxwell's interests were on transport properties and his remarks on the second law of thermodynamics and its relationship to probability were casual and illustrative, not mathematical.

Harvard University Press, 1986) is a study of how a field, statistics, came into being from its origins in several other areas of study, scientific and social. His work overlaps that of Daston somewhat for mathematicians in the late eighteenth century. Theodore Porter, *The Rise of Statistical Thinking* (Princeton: Princeton University Press, 1986), traces the use of statistics in the social sciences from Quetelet through Galton and Pearson.

62 For Maxwell's papers on kinetic theory and statistical mechanics with a critical introduction, see *Maxwell on Molecules and Gases*, Garber, Brush and Everitt, eds., and *Maxwell on Heat and Statistical Mechanics: On "Avoiding all Personal Enquiries of Molecules,"* Garber, Brush and Everitt, eds. (Bethlehem PA: Lehigh University Press, 1995).

Boltzmann's major focus was on the meaning of the second law while his remarks on transport phenomena, mathematically more thorough, were peripheral to the main thrust of his research.

There were further differences in the level of mathematical apparatus brought to bear on their various problems. Maxwell's proofs reflect the mathematics of Cambridge at mid-century where the curriculum incorporated standards of early nineteenth-century France. Boltzmann's proofs reflect the mathematical standards of Germany through Andreas von Ettingshausen at the University of Vienna. His proofs were more rigorous, and eventually physics became mathematics.[63] Boltzmann realized more clearly and quickly than Maxwell that the mathematics actually removed the necessity of considering the particulars of the molecules' interactions. The molecules in motion in the gas become just so many "individuals" in different states of motion and "if the number which on average have known states of motion stays constant, the characteristics of the gas remain constant." During a collision, not specified, the states of the molecules involved changed, represented by a change in their kinetic energy from x to ξ but in equilibrium the energy, E, of the gas remained constant. Boltzmann constructed the quantity E, an integral of functions of the coordinates, velocities and density of all the molecules in the gas. Through mathematical manipulations Boltzmann expressed E as[64]

$$E = \int_0^\infty f(x,t)\left[\frac{log f(x,t)}{\sqrt{x}} - 1\right]dx.$$

To examine the rate of change of E, Boltzmann reconstituted E, then dE/dt in the form

$$\frac{dE}{dt} = \int_0^\infty \int_0^\infty \int_0^{x+x'} \frac{log f(x,t)}{\sqrt{x}}\left[f(\xi,t)\frac{f(x+x'-\xi,t)}{\sqrt{\xi}\sqrt{x+x'-\xi}} - \frac{f(x',t)}{\sqrt{x'}}\frac{f(x,t)}{\sqrt{x}}\right].$$

He examined the behavior of the right hand side of this equation, and through his use of particular integration techniques and changes of variables, the above was reduced to

$$\frac{dE}{dt} = \frac{1}{4}\int_0^\infty \int_0^\infty \int_0^{x+x'} \frac{log(ss')}{\sigma\sigma'}\left[\sigma\sigma' - ss'\right]r\,dx\,dx'\,d\xi.$$

where

$$s = \frac{f(x,t)}{\sqrt{x}}, s' = \frac{f(x',t)}{\sqrt{x'}},$$

63 The development of Boltzmann's statistical ideas are discussed in Brush *The Kind of Motion we call Heat,* vol. 1, 566–616, and Kuhn, *Black Body Radiation*.

64 Boltzmann, "Weitere Studien über das Wärmegleichgewicht unter Gasmolekülen," *Ber. Wein* 66 (1872): 275–370, translated in Brush, *Kinetic Theory,* 3 vols. (New York: Pergamon, 1966), vol. 2, 88.

$$\sigma = \frac{f(\xi, t)}{\sqrt{\xi}}, \qquad \sigma' = \frac{f(x + x' - \xi, t)}{\sqrt{x + x' - \xi}},$$

and

$$r = \sqrt{xx'}\psi(x, x', \xi).$$

The function ψ depended on the type of collision and remained unspecified. $f(x, t)$ represented the number of molecules in unit volume whose kinetic energy lay between x and $x + dx$. The product

$$log \frac{ss'}{\sigma\sigma'}\left[\sigma\sigma' - ss'\right]$$

was intrinsically negative. Whatever mechanical system one chose, a quantity existed that had the properties of the thermodynamic relation $-\int dQ/T < 0$, for irreversible cycles. For such a system in equilibrium,

$$f(x, t) = C\sqrt{x}e^{-hx}.$$

The properties of the physical system came directly from the behavior of the mathematics.

From criticisms of his gas theory, Boltzmann was goaded into rethinking the determination of thermal equilibrium in terms of probability alone. He reinterpreted the expression for entropy in terms of the number of ways the total energy of a gas could be distributed among the molecules constituting the gas.[65] Boltzmann assumed that the probability of the energy state for the gas was proportional to the number of ways it could be constituted on a molecular level. Entropy was directly related to probability without any considerations of the structure of individual molecules or their interactions with their fellow molecules in the gas.[66] Even if the gas was initially in an improbable energy state, the system would pass to a more probable one, and finally, if left undisturbed, to the most probable state, that of thermal equilibrium.[67]

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65 Boltzmann, "Über die Beziehung zwischen dem zweiten Haupsatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung, respective der Sätzen über des Wärmegleichgewicht," *Ber. Wien* 76 (1877): 73. See also Boltzmann, *Lectures on Gas Theory*, Stephen G. Brush, trans. (Berkeley CA: University of California Press, 1964) chap. 1.

66 Boltzmann also published papers on the mathematical aspects of kinetic theory and statistical mechanics. See Boltzmann, "Über die Integral linearer Differentialgleichungen mit periodischen Koeffizienten," *Ber. Wien* 18 (1868): 54–59, "Einige allgemeine Sätze über Wärmegleichgewicht," same journal 63 (1871): 679–711, "Über die Aufstellung und Integration der Gleichungen welche die Molekularbewegungen in Gasen bestimmen," same journal 14 (1876): 503–552.

67 Objections to the physical implications of Boltzmann's argument in his 1872 and later papers are discussed in Brush, *The Kind of Motion*, vol. 2, 602–608.

Boltzmann's papers were carefully constructed mathematically. He also appreciated Kirchhoff's, and what he understood to be Hertz's, goal "to represent directly observed phenomena in basic equations, without the colourful wrappings of hypotheses that our imagination lends them."[68]

Boltzmann was unsatisfied with a purely statistical interpretation of entropy as he explored mechanical models in the 1880s.[69] Nor was he consistent in his understanding of the relationship between probability and the second law. However, even in popular lectures on the second law, Boltzmann retained its probabilistic explanation.[70] His careful mathematics was not pedantic but came from the training he received at the university of Vienna, and in a real sense, for Boltzmann mathematics was always the center of physics, although he never denigrated or abandoned the use of models and imagery. However, he never confused the help they offered in reaching the "bare equations" with their actual existence in nature. He combined a vigorous defense of the use of atoms and mechanical models against the Energeticists coupled with equally pointed comments on the inadequacy of their mathematics.[71] Yet for Boltzmann, the intellectual attraction and beauty of mathematics paled before physics precisely because mathematics did not deal with the real world.

In a similar fashion, and in the same decade, physicists began to see the need for mathematical representations of vector quantities and an algebra that distinguished

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68 Boltzmann, "On the Methods of Theoretical Physics,"5–12, 8–9. This argument is repeated in Boltzmann, "On the Development of the Methods of Theoretical Physics in Recent Times," 77–100, 87–91 along with arguments about the dangers of relying on mathematics alone in Boltzmann, *Theoretical Physics and Philosophical Problems, Selected Writings,* Brian McGuinness, ed. (Boston: Reidel, 1974).

69 Boltzmann, "Über die Eigenschaften monozyklischer und anderer damit verwandter Systeme," *J. Reine Angew. Math.* 98 (1884): 68–94: 100 (1887): 201–212. The examination of these mechanical systems, some of whose internal motions did not affect the system's macroscopic thermodynamic properties, was begun by Helmholtz, "Principien der statik monozyklischer Systeme," *J. Reine Angew. Math.* 97 (1884): 111–140, 317–336. See Günther Bierhalter, "Die von Hermann von Helmholtzschen Monozykel–Analogien zur Thermodynamik," *Arch. Hist. Exact Sci.* 29 (1983): 95–100, "Zu Hermann von Helmholtzs mechanischer Grundlegung der Wärmelehre aus dem Jahre 1884," same journal 25 (1981): 71–84; "Boltzmanns mechanische Grundlagung des zweiten Hauptsatzes der Wärmelehre aus dem Jahre 1866," same journal 24 (1981): 195–205, 207–220.

70 See Boltzmann, "Relationships of Applied Mathematics," in *Physics for a New Century: Papers presented to the 1905 St. Louis Congress,* K. R. Sopka, compiler (New York: AIP, 1986), 267–279, Boltzmann, "On the Significance of Theories," and "The Second Law of Thermodynamics," in *Theoretical Physics*, 33–36, 12–32 respectively.

71 On Energetics see Boltzmann, "Zur Energetik," *Ann. Phy.* 58 (1896): 39 and "Über die Unentbehrlichkeit der Atomistik in der Naturwissenschaft," same journal 60 (1897): 231.

their behavior from that of scalars.[72] For this we again need to return to Maxwell and his examination of mathematics from the point of view of the physicist.[73] Maxwell pointed to an important distinction for physicists between scalar and vector quantities, recognized by Hamilton in his work on quarternions. He indicated the mathematical results from quarternions and what they indicated about the properties of scalars and vectors. However, while quarternions were important for geometry they were not what was needed by physicists because the distinctions between scalars and vectors needed to be kept in mind at all times. Using quarternions, kinetic energy for example was always negative. Maxwell focussed on Hamilton's operators and what they represented physically as well as presenting to the mathematicans his own nomenclature for these operators, convergence (divergence), curl, and the combination of the two.

Maxwell persisted. His correspondence with Peter Guthrie Tait over the next decade frequently included comments or questions about quarternions or results he had deduced. They appear most conspicuously in Maxwell's *Treatise*. He introduced them in the mathematical sections of his introductory chapter, along with the operators above and Laplace's operator. Throughout both volumes Maxwell noted the quarternionic equivalent of results. In his second volume vectors were introduced into his mathematical arguments themselves and he began to stress the type of quantity being dealt with. However, extensive mathematical manipulations were in Cartesian form, and where appropriate, with references to Hamilton or Tait's work on quarternions. His most extended discussion was in his chapter on the electromagnetic field. Maxwell equations appear in vector form. Yet although appearing throughout the text, vectors were marginal; the main mathematical arguments were in Cartesian form, their results expressed in the equivalent vector format.[74]

For over a decade work proceeded in electrodynamics without the benefit of vector algebra. Heaviside's introduction of them into the subject was piecemeal. Judging from reactions from his colleagues their usage was neither obvious nor easy for physicists accustomed to thinking in Cartesian, or quarternionic terms.[75] Responding to Heaviside's demonstration of the usefulness of vectors in electrody-

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72 This section depends heavily on Michael Crowe, *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System* (New York: Dover reprint of 1967 edition, 1985), although I disagree on some specific points.

73 Maxwell, "On the Mathematical Classification of Physical Quantities," *Proc. London Math. Soc.* 3 (1871): 224–232. He had begun this discussion in Maxwell, "Address to the Mathematical and Physical Sections of the British Association," *Rep. British Assoc.* (1870): 1–9, reprinted in *Scientific Papers,* vol. 2, 215–229, and *Maxwell on Molecules*, Garber, Brush and Everitt, eds. 89–97.

74 Maxwell *Treatise*, vol. 1, 10–32, vol. 2, 247–259.

75 See also Crowe, *Vector Analysis,* on George Francis FitzGerald's review of Heaviside's *Electromagnetic Theory*, and his use of vectors, 175–176.

namics, Hertz replied that it was difficult to follow Heaviside's symbols, especially as he did not use the vector potential at all. He could not understand Heaviside's symbols and "mode of expressing yourself. You know mathematical symbols are like a language and your writing like a remote dialect of it..."[76] By this date in his papers on electromagnetism, Heaviside had published on vector analysis and its use in electromagnetism. However, it was not until 1893 with the publication of Heaviside's *Electrical Theory* that he treated vector analysis systematically, if polemically. From this date vectors began to make some impact on physicists.[77]

Heaviside's vector analysis was taken up, not in Britain but in Germany in the textbook of August Föppl on Maxwell's theory. The first part of this text is a systematic exposition of vector analysis necessary for electrodynamics. The definition of a vector is followed by that for the unit vector, the vector product and the transformation of coordinates. Föppl also introduced examples, using as his vector the velocity of a particle, an indication that he saw extensions of their use in mechanics. He introduced the key differential operators and, again taking examples from mechanics, demonstrated their importance and also illustrated their relationships with one another. In the last section to this chapter he dealt with the integration of vectors and the potential as a scalar. His final words on the subject were on the Laplacian operator. The whole text was written in vector form, using Maxwell's nomenclature throughout.[78]

Lorentz's *Versuch* appeared the following year. Despite the popularity of Föppl's textbook, vectors did not replace Cartesian methods easily or quickly even in the domain of electrodynamics. In 1904 Lorentz still published his equations in both Cartesian and vector forms.[79] Albert Einstein's initial papers on electrodynamics and relativity were similarly in Cartesian form. Vector analysis seeped into the research and probably the teaching of physics in Germany in the last decade of the nineteenth and the first decade of this century.[80] It was not until German mathemati-

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76 Quoted in J. G. O.'Hara and Willibrand Pricha, *Hertz and the Maxwellians,* (London: Peter Peregrinius, 1987), Hertz to Heaviside, March 21, 1889, 62–63.

77 See Crowe *Vector Analysis* 169–174 for Heaviside's system and 174–177 for its reception. I leave any account of Gibbs' and Grassmann's work on vector analysis until the next section.

78 August Föppl, *Einführung in die Maxwell'sche Theorie der Elektricität: mit einem einleitenden Abschnitte über das rechnen mit Vectorgrössen in der Physik* (Leipzig: B. G. Teubner, 1894). See also Crowe, *Vector Analysis*, 176, 226–227.

79 Lorentz, "Electromagnetic phenomena in a System moving with any Velocity less than that of Light," *Proc. Amsterdam K. Akad. Sci.* 6 (1904).

80 Neither Woldemar Voigt, *Kompendium der Theoretischen Physik* (Leipzig: Veit, 1895) 2 vols., nor Paul Drude, *The Theory of Optics* C. Riborg Mann and Robert A. Millikan, trans. (New York: Dover reprint of 1902 edition) and other theoretical physics texts used vectors. However, Larmor, *Aether and Matter* (Cambridge: Cambridge University Press, 1900) devoted a section to vectors and their advantages for electrodynamics. However,

cians turned their attention to the mathematical possibilities of physical problems that the power and possibilities of vector, then of tensor analysis, began to shift the mathematical language of physics decisively away from that of Descartes, in a process which forged a new relationship between mathematicians and physicists.

Physicists Versus Mathematicians

The transitions to these new relationships were neither smooth nor easy. There were early and acrimonious confrontations between mathematicians and physicists. Tait and G. G. Knott, ardent quarternionists, took Gibbs to task because of his misuse of Hamilton's mathematical invention. Their criticism was that Gibbs had merely invented a new notation and had actually made mathematical matters worse by not using Hamilton's quarternions. From the point of view of the mathematician's, Gibbs had missed the mathematical point of quarternions. And from the point of view of the history of mathematics Gibbs work has been judged as "not highly original."[81] Yet from his first publications Gibbs went beyond Tait and others in his ability to treat physical problems. He demonstrated how vector analysis could be used in astronomy, even when his main purpose was to teach its methods to students.[82]

Gibbs did not produce a systematic study of vector algebra to really bring out its power to make physical processes visible. In contrast Oliver Heaviside both transformed vector algebra and Maxwell's electromagnetism. The latter was simplified into a geometrically vivid form centered on the notions of electric and magnetic force rather than on the analytical potential functions. Heaviside introduced a much improved notation and developed vector algebra in more detail than Gibbs.[83] While he derived his vector analysis from Hamilton's quarternions he did it for the purpose of expressing electromagnetic theory simply and graphically. During the 1890s quarternionic mathematicians argued against this truncated algebra. In Great Britain Heaviside prevailed while the independent development of vectors by Hermann Grassmann gained acceptance on the continent.[84]

Heaviside was not so fortunate in his development of operational calculus. The Royal Society refused him publication in its *Proceedings* even though this was an accepted perk for society fellows. To explain this refusal Hunt invokes the shifting boundaries of mathematics, and that mathematicians now claimed the

mathematics was not a prominent aspect of his argument here.

81 See Michael J. Crowe, *History of Vector Analysis.*

82 Gibbs, "On the Determination of the Elliptical Orbits from three Complete Observations," *Mem. Nat. Acad. Sci.* 4 part II (1889): 79–104.

83 Heaviside, criticized for the compactness of his arguments noted that Gibbs' text on vectors was "a condensed synopsis." See Crowe, *History of Vector Analysis,* 158.

84 Crowe, *History of Vector Analysis,* chap. 6, and Hunt, *The Maxwellians,* for the specifics of the arguments over vectors and Heaviside's polemical skills.

self-generation of mathematics from within. Mathematics generated from physical problems was no longer rigorous enough.[85] The boundaries of mathematics had certainly shifted. However, those trying to practice a form of mathematics that did not spring from the consideration of physical problems formed a besieged minority in the discipline within the British mathematical community.[86] Mathematical physics, that is mathematics that developed out of physical problems, was still a dominant tradition in Britain, especially at Cambridge. Heaviside's work in operational calculus was mathematically unimportant, despite its future use by engineers and physicists.[87] In the case of Heaviside and operational calculus, the opposition was from one mathematician, William Burnside, on the Council of the Royal Society and politically positioned to thwart Heaviside's ambitions.

In the early twentieth century German mathematicians reannexed vectors from the physicists, and rewrote the history of its development. Their historical accounts subsumed theoretical physics within the mathematician's forms of mathematical physics. Mathematicians' systematizations of recently developed domains of theoretical physics such as electrodynamics, and later special relativity, brought clarification and gave physicists new physical insights into the possibilities of that theory. However, mathematicians were unconcerned with the operations of nature and aimed at mathematical systematization or the exploration of a mathematical language. They were therefore able to systematize theories already in mathematical form, yet could not, much to the frustration of Hilbert, actually encompass theoretical physics. Mathematicians did not have to ponder the implications of their results for the structure and functioning of nature. This was the dividing line between the two disciplines.

In the 1890s the isolation of mathematics and physics, and between mathematicians and physicists, was the subject of comment in German academic departments, then of some concern. This anxiety came from different sources within the two disciplines but led ultimately to a greater interest in the research interests of each other.[88] Just as physicists publicly bemoaned the growing isolation of mathematics from their field and the abstractions of mathematicians' research and teaching German mathematics changed yet again.

Symbolizing this reorientation of mathematics as a discipline is the career of Felix Klein. Klein was a student of Julius Plücker and had been his laboratory

85 Hunt, "Rigorous Discipline: Oliver Heaviside versus the Mathematicians," in *The Literary Structure of Scientific Argument,* Peter Dear, ed. 72–96.

86 See Ivor Grattan-Guinness, "University Mathematics at the Turn of the Century," *Ann. Sci.* 28 (1972): 369–384. See also G. H. Hardy *A Mathematician's Apology* (Cambridge: Cambridge University Press, 1920).

87 Paul J. Nahin, *Oliver Heaviside: Sage of Solitude* (New York: IEEE Press, 1988).

88 For the place of mathematics within the German university system see Gert Schubring, "Germany to 1933," in *Companion to History and Philosophy of Mathematics* Grattan-Guinness ed., vol. 2 Part II Higher Education and Institutions, 1442–1456, 1448–1453.

assistant in physics in the 1860s. From these beginnings Klein's research lay in the then unfashionable and professionally disastrous field of geometry. In this he joined other mathematicians such as Alfred Clebsch who shared Klein's interest in the mathematical problems that emerged from technical and physical problems.[89] Klein combined extraordinary mathematical ability with equally forbidding political and organizational talents that forced long-term changes in the profession. With the rapid development of an industrial economy, Technische Hochschule began to replace universities as the mathematical training ground for engineers. In an effort to halt the hemorrhage of students and maintain a future for mathematics, Klein set out to recapture the teaching and what he considered the proper place of mathematics with respect to the exact sciences and engineering.[90]

In his efforts Klein successfully led university mathematicians in retaining control of the teaching of their subject in higher education. He also cultivated and broadcast the idea that mathematics was the key to all the sciences and engineering. Physics and engineering became "applied mathematics," although Klein's meaning of this term was crucially vague.[91] However, Klein's notion of mathematics as being key to the development of these other disciplines did not lead him to see them as equal in this endeavor. Mathematics was not at the center of some vast horizontal network binding all the specialist studies together. It was an hierarchical relationship. Klein saw the connections of mathematics to engineering and the sciences as the key to revitalizing the discipline and becoming the intellectual center for rapidly fragmenting technical fields that were quickly losing touch with one another. However, mathematics subsumed all these other fields within itself. While the applied sciences were a source of new discoveries in mathematics, they were intrinsically inexact. Only pure mathematics developed from axiomatic foundations could lead to the structure required by the other technical knowledge bases.

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89 Klein's interest in physical problems as a source for mathematics can be traced back to his Erlanger Program of 1871. See David Rowe, "Felix Klein's 'Erlanger Antrittsrede'," *Hist. Math.* 12 (1985): 123–141, which was his inaugural lecture as extraordinary professor at Erlangen. The text of his lecture is reproduced here. For more details on Klein's career and its personal costs see Rowe, "Klein, Hilbert and Göttingen Mathematical Tradition."

90 The details of Klein's twenty year campaign is complicated by fundamental changes in German higher education. How this entwines with his vision of mathematics and its position with respect to the exact sciences and engineering is detailed in Lewis Pyenson *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany* (Philadelphia PA: American Philosophical Society, 1983). See also Rowe, "Klein, Hilbert, and Göttingen," Gert Schubring, "Pure and Applied Mathematics in Divergent Institutional Settings: the Role and Impact of Felix Klein," in *History of Modern Mathematics,* Rowe and McCleary, eds. vol. 2, 171–220, and Lewis Pyenson, "Mathematics, Education, and the Göttingen approach to physical Reality, 1890–1914," *Europa* II (1970): 91–127.

91 Schubring has examined Klein's writings on this term. See Schubring, "Pure and Applied Mathematics," 192–197.

While statements like these in the context of changing educational policies led to clashes with the very groups Klein hoped to cultivate, he was able to convince both government and private resources to underwrite these intellectual visions in more concrete forms. He also gathered a group of mathematicians around him at Göttingen that seriously investigated the mathematics of the physical sciences and developed a program that attracted many bright students to this newly invigorated study. These efforts, begun in the 1890s, came to fruition during the second decade of the twentieth century.

Whether physicists or engineers would accept his vision and that of likeminded mathematicians at Göttingen, among the most outspoken of which was David Hilbert, of the role of mathematics in their respective specialities is another matter.[92] Physicists in particular would more than once echo Einstein's complaint that the mathematical axiomatization of an area of physics could be useful but could only be done after the fact. So far as the development of physical theory in an unchartered domain, the axiomatic approach offered by mathematicians was useless.[93]

In their renewed encounters between roughly 1900 and the first world war, a changed relationship between these two disciplines would emerge that would reshape both disciplines. One of the symptoms of this renewed relationship was the growing interest of mathematicians in the mathematics physicists now needed for their research. During the early decades of this century texts by mathematicians appeared on vectors, the partial differential equations of physics, etc.[94]

However, the first example that we find of a mathematician seriously involved in the scrutiny and then reworking of a contemporary field of physics lay not in Germany but France. This is not surprising seeing that the structural changes that the universities underwent in France in the nineteenth century did not include the separation of the "pure" from the practical. If anything, after the Franco–Prussian War, political changes in the funding of higher education in France reinforced the mutual support of theoretical and practical interests of French academics in the

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92 See David Hilbert, "Die Grundlagen der Physik," *Math. Ann.* 92 (1924): 1–32, in *Gesammelte Abhandlungen,* 3 vols. (New York: Chelsea reprint, 1965), vol. 3, 258–289 on the general theory of relativity, and, "Naturerkennen und Logik," *Naturwissenschaften* (1930): 959–963, *Abh.* vol. 3, 378–387.

93 For such an axiomatic approach see Hilbert, " Bemerkungen über die Begründung der elementaren Strahlungstheorie," *Göttingen Nach.* (1912): 773–789, (1913): 409–416, (1914): 275–298 in *Gesammelte Abhandlungen* 3 vols. (New York: Chelsea reprint of 1935 edition, 1965), vol. 3, 217–257.

94 See Heinrich Weber, *Die partiellen Differential-Gleichungen der mathematischen Physik nach Riemann's Vorlesungen* (Braunschweig: F. Vieweg, 19900–01), 2 vols., Richard Gans, *Einführung in der Vektoranalysis mit Anwendungen auf die mathematische Physik* (Leipzig: Teubner, 1905). Also see the articles in *Encyklopädie der mathematischen Wissenschaften. Mit Einschluss ihren Anwendungen,* Felix Klein ed. (Leipzig: Teubner, 1904–1922).

sciences.[95] Henri Poincaré followed in a long line of nineteenth-century French mathematicians who found in physical and other problems a fruitful source for research in mathematics. He was also used in the early twentieth century by German mathematicians as a contemporary example of what mathematicians could accomplish for physics.

Poincaré's involvement with theoretical physics lasted from the 1880s until his death. In his examinations of the problems of physics he chose those of current importance to theoretical physicists. Poincaré saw his work in mathematical physics as of two kinds: Work on the differential equations of physics and criticism of physical theories.[96] In his examination of the differential equations of physics he examined equations of various forms, the simplest of which was $\nabla u = ku$, where ∇ was the Laplacian, then moved to solutions of equations of the form $\nabla u = k(du/dt)$ and $\nabla u = k(d^2u/dt^2)$. Most problems of physics could be reduced to solutions of equations of these forms of increasing mathematical difficulty. His solutions were in the form of functions with particular properties under certain mathematical conditions. He did not specify the physical significance or meanings of these mathematical conditions.

Poincaré's work lay within the tradition of French mathematical physics that we have discussed previously. However, to determine the significance of his work across disciplinary boundaries is not necessarily straightforward. Until 1908 his contemporaries in physics saw his work in electrodynamics as that of an astute critic not as a developer of an alternative theory of electrodynamics or the electron. And this needs consideration, given the judgments made about his importance in the development of the theory of special relativity.

His most sustained work was on the theories of electrodynamics and optics.[97] He initially critiqued existing physical theories as he reduced them to mathematical form. However, his criticisms were based upon logical and mathematical criteria, not their physical content.[98] He compared and judged electrodynamical theories

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95 Terry Shinn, "The French Science Faculty System, 1808–1914: Institutional Change and Research Potential in Mathematics and the Physical Sciences," *Hist. Stud. Phys. Sci.* 16 (1979): 271–332. See also Maurice Crosland, *Science Under Control.*

96 Poincaré, "Analyse de ses travaux scientifiques," *Acta Math.* 38 (1921): 116–125, in *Oeuvres,* 9 vols. (Paris: Gauthier–Villars, 1954), vol. 9., 1–14.

97 Poincaré also published papers on the mechanical foundations of thermodynamics, thermal conduction, elasticity, capillarity and the theory of errors as well as on electricity and optics. Aspects of Poincaré's work on electrodynamics and optics initially appeared as a series of papers, then reappeared as published lectures in the 1890s and early 1900s. The papers took up particular aspects of the subject matter (electric waves) or the work of particular physicists including Weber, Larmor, Lorentz and Hertz. The lectures were published as Poincaré, *Électricité et Optique. La lumière et les théories électrodynamiques* Lectures at the Sorbonne, 1888, 1889, 1899 (Paris: Carré Naud, 1901).

98 Olivier Darrigol, "Henri Poincaré's Criticism of *Fin de Siècle* Electrodynamics," *Stud.*

through their differential equations, reducing them in many cases to mathematical equivalence. Their physical incompatibilities or differences were irrelevant.[99] This made a difficult subject deceptively simple and disguised the very real conceptual differences between the authors he discussed. For Poincaré their essential content lay in their mathematics.

No matter what their age or interest in current physics Poincaré examined theories within an area of his interest as if they were of equal significance to his contemporaries in physics. Thus he examined both mechanical and electromagnetic theories of light with the same seriousness. He did not seem to understand that by 1890, many of the former had been abandoned for physical reasons.[100] In these ongoing studies experiment and mathematical consistency was of paramount importance, physical imagery malleable. Assumptions made by physicists were sometimes turned directly into mathematical forms, sometimes were the result of convoluted mathematical manipulations. He introduced Lorentz's transformation without its physical justification, the constants within its terms were designated as arbitrary and yet to be determined. For Poincaré, Lorentz's electromagnetic field equations were "not altered by a certain transformation (which I will call the *Lorentzian*)."[101] In the fuller version of this paper Poincaré identified this type of transformation as a group.[102]

In Lorentz's papers the constants in his transformations were physical. Poincaré cast them adrift and argued at length to demonstrate that Lorentz had assumed certain values for these constants. From this he further argued that Lorentz's electrodynamics was the only form consistent with the inability to detect absolute motion. His main target here was Max Abraham.[103] Such a procedure was mathematically allowable, physically it was arbitrary unnecessarily convoluted and

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Hist. Phil. Mod. Phys. 26 (1995): 1–44, notes this foundation for Poincaré's judgments, although he still treats his work as physics, not mathematics. Darrigol also notes the clarity and the quality of his language throughout his papers and lectures.

99 Darrigol, "Poincaré," also notes that he conflated physical ideas and reduced one theory (Maxwell's) to a special case of another (Helmholtz's).

100 This was, and still is a strong tradition in mathematics where the mathematics of the eighteenth century can still be a stimulus for research in mathematics whereas in physics such is not the case. In the twentieth-century physics Newton's mechanics is of historical but not of research interest.

101 Poincaré, "La dynamique de l'électron," *Compte Rendu,* 140 (1905): 1504–1508) in *Oeuvres,* vol. 9, 489–493, 490.

102 Henri Poincaré, "Sur la dynamique de l'électron," *Rendiconti del Circolo matematico di Palermo* 21 (1906): 129–176, in *Oeuvres* vol. 3, 494–550, 513–515. This long paper was written in reaction to Lorentz, "Electromagnetic Phenomena in a System moving with any Velocity smaller than that of Light," *Proc. Amsterdam K. Akad. Sci.* 6 (1904): 809–831, and as an attempt to improve it. As Darrigol, "Poincaré," indicates Poincaré and Lorentz corresponded closely throughout this decade.

103 Poincaré, "l'électron," *Rendiconti.* He compares Abraham's and Lorentz's hypotheses,

seriously muddied the physical arguments.

Poincaré's shortcomings should not stop us from noting his importance as a critic that lead Lorentz and Abraham to redirect their own ideas. His place outside of the physical mainstream allowed him to develop some radical ideas, whether reached on mathematical or physical grounds. He announced these ideas, the principle of relativity, the speed of light as the upper limit of all velocities, and the death of the ether before developing them systematically.[104] For Poincaré local time is a consequence of the principle of relativity, that we can only measure relative not absolute velocity. Einstein developed a kinematics based upon this principle raised to a postulate and added another, that the velocity of light is the same in all measuring systems. These assertions affected the expression and interpretation of other key phenomena involving light and moving bodies, Doppler effect and radiation pressure. The range of implications for physics in Einstein's case was much broader than those from Poincaré's theory of the electron.

In his criticisms Poincaré returned repeatedly to a mathematical test, to understand whether or not a particular theory, or his mathematical expression of that theory, conformed to Lorentz's transformation, a mathematical judgment in his case, or whether they conformed to his experimental counterpart, "the impossibility of demonstrating absolute motion." Not that physicists did not also use the same arguments but they were enmeshed, as Lorentz's, in a net of physical arguments. Poincaré's net was mathematics and observation, not physical theory.

This plunge into Poincaré's work has two goals. First, to illustrate the different ways in which a mathematician deeply and on a long term basis went about mathematical physics, and secondly to illustrate that to claim a rivalry in the history of the development of relativity theory between Einstein and Poincaré makes no historical sense. They were going about two different sorts of business. Their goals in these enterprises were also quite different.

To make electrodynamics into a deductive, mathematically consistent theory is one enterprise. To just think of Einstein's theory as developing a consistent physical theory of electrodynamics without mentioning his broader goals in examining the electrodynamics of moving bodies was quite another. Poincaré's search was dominated by the standards of mathematics: Einstein was creating a physical theory. Poincaré's began with Lorentz's theory of the electron and ended in trying

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523–529. The Abraham paper Poincaré refers to is Abraham, "Dynamik des Electrons," *Göttingen Nach.* pt. 1 (1902): 20–41.

104 See Poincaré, "The Principles of Mathematical Physics," *Physics at St. Louis*, 281–299. The argument he made for the upper limit on velocity was based on the limitations of measurement techniques. Because measurement techniques were visual and dependent on the velocity of light we cannot measure any velocity greater than that. See also, Poincaré, "La dynamique de l'électron," *Rev. gen. sci. pures et appliquées* 19 (1908): 386–402, in *Oeuvres*, vol. 9, 551–586, 574. He argued for the elimination of the ether, 575–576.

to improve on and them develop his own. From the beginning Einstein was out to refashion both mechanics and electrodynamics. Both based the idea of the death of the ether on experimental grounds, the relativity of measurement that leads to the notion of local time and the velocity of light as an upper limit. In addition, and not incidentally, there was Einstein's commitment to a particular aesthetic for physical explanation with which he opened his first relativity paper. Explanations should reflect the symmetries of the phenomena they are developed to explain.

What then did they accomplish with these ideas? Poincaré could not release himself from the notion that somehow time was a coordinate different from the other space coordinates. He explored local time and discussed a four–dimensional space with coordinates x, y, z, $t\sqrt{-1}$, but in relation to his discussion of the Lorentz transformation group and the invariance of certain functions that after manipulation demonstrated the velocity of propagation of gravitation. Einstein merely remarked on the group character of his transformations, nothing more. He discussed his transformation equations in a section on "The physical meaning of the equations concerning moving rigid bodies and clocks."[105] Einstein made the rest of his first paper on special relativity an exploration of some of the implications for mechanics, electrodynamics, and optics of using his postulates, as well as placing time on the same footing as space coordinates. The mathematical apparatus was minimal. He imported equations as he needed them. The steps that drove the argument and the mathematics forward were physical. While the mathematical expression of electrodynamics might be the same as in earlier theories the physical meanings of those equations needed rethinking.[106] This was not an exhaustive investigation but an exploration using particular physical cases such as the Doppler effect, and radiation pressure.

At this point Whittaker's judgments, and those that followed him, on the respective merits and claims of the mathematician Poincaré versus the physicist Einstein were not wrong but wrongheaded.[107] His account only stands if the mathematical form **is** the solution, no further explanation being necessary. One must also assume that the statement of physical implications at the end of a string of analysis is entirely equivalent to its statement as a matter of physical principle. That equivalence encompasses the theory that follows, worked out in detail, with respect to the changes in the interpretation of physical processes expressed in the equations that follow from those assumptions. Assuming Poincaré's priority here

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105 Poincaré, "l'électron," *Rendiconti*, 541–543. Albert Einstein, "On the Electrodynamics of Moving Bodies," *Ann. Phy.* 17 (1905): 891–921, in *The Collected Papers of Albert Einstein,* vol. 2, Anna Beck, trans. (Princeton NJ: Princeton University Press, 1989), 140–171, 156.

106 Einstein, "On the Electrodynamics," 159.

107 Edmund Whittaker *History of the Theories of Aether and Electricity,* vol. 2, chap. II The Relativity Theory of Poincaré and Lorentz. In this chapter Whittaker consistently diverts attention and credit from physicists to mathematicians.

is to equate foundational ideas with theory development itself. This is no longer feasible. Principles do not uniquely define the theories based upon them. Even if Einstein had taken Poincaré's statements with respect to the velocity of light, the impossibility of knowing absolute motions, and the death of the ether, the theory of special relativity is still not specified. Neither Poincaré nor Lorentz developed it. Physics is more than mathematical deductions. Einstein explored the implications of what were for Poincaré conclusions. The physical issues that Einstein realized was of paramount importance, simultaneity, needed more exploration and explanation of its mathematical expression for its physical implications for both mechanics and electrodynamics to emerge. Poincaré's mathematics was still tied to an older form of mechanics.

Therefore while their physics might be expressed in the same mathematical forms, as was Lorentz's for that matter, the meanings that each author drew from these equations were quite different. This does not rule out the possibility of shared concerns and interests between mathematicians and physicists, or that they see each other as rivals, or that the mathematicians might not see physics as simply a branch of mathematics, that is as applied mathematics. Nor does this get rid of the problem that physicists did not at times appreciate the concerns or potential of mathematics until the work of mathematicians so overlapped their own research interests that mathematical solutions become obviously relevant. All these dynamics developed in the German mathematics and physics community in the first two decades of this century. From across their disciplinary boundary, mathematicians and physicists could benefit each other, yet think and work in terms of standards and expectations of their respective fields.

Throughout the first decade of his career in physics, Einstein's output was prodigious. His attention was focussed on one issue, the disparity in physics between descriptions of mechanical versus electromagnetic phenomena. He examined this disparity from several different points of view, largely from that of mechanics.[108] In addition he wrote many review articles on thermodynamics, as well as papers on the size of molecules, and the implications of quantum theory. By comparison the papers in special relativity were few in number. He addressed some specific points that emerge from his 1905 paper, but it was Max Planck who began to explore relativistic dynamics. Einstein was working in other directions.

The paper that began to turn his attention back to relativity was that of Hermann Minkowski of 1908. However, this was not because Minkowski in his work was addressing Einstein's papers on relativity directly.[109] His starting point was the

108 His early commitment was to the "molecular–kinetic" theory of heat and the fundamental nature of the laws of thermodynamics, as interpreted statistically. For a discussion of mechanics at the turn of the twentieth century and the role of thermodynamics in Einstein's work see Martin Klein, "Mechanical Explanation at the End of the nineteenth Century," *Hist. Stud. Phys. Sci.* 1 (1969): 127–149.

109 Lewis Pyenson, "Hermann Minkowski and Einstein's Theory of Relativity," *Arch. Hist.*

work of Poincaré and Lorentz.[110] Minkowski located the "theorem of relativity" in the covariance of the original equations of Lorentz's electrodynamics of 1895 through the transformations explored by Poincaré. He took this as "purely mathematical." This theorem depended on the form of the differential equations for the propagation of waves with the velocity of light.

Minkowski claimed that no one had as yet followed through the implications of this theorem for matter, hence it was as yet a postulate. Minkowski recognized Einstein's 1905 paper as the clearest on the postulate. However, Einstein's argument was based on phenomena and new ideas about the concept of time. The principle of relativity had not yet been formulated for the electrodynamics of moving bodies. Then, in vector form he stated the Lorentz equations for the electrodynamics of a moving body. These were rewritten to demonstrate their symmetry with respect to the indices attached to the four coordinates x_1, x_2, x_3, x_4. Minkowski redefined the Lorentz transformation as a rotation and demonstrated that the equations of electrodynamics are invariant under such a rotation. For Minkowski the Lorentz transformation introduced a modification of the coordinate x_4, the "time parameter." He then plunged into a short section on simultaneity. The Lorentz transform permits us to consider time exactly as we do the other three space coordinates. This, Minkowski claimed, should be easier for mathematicians as they were used to dealing with four dimensional and non-Euclidean geometry. Minkowski referred the reader to Einstein's 1905 paper for an account of the physical explanation.

Minkowski constructed two types of vectors invariant under the Lorentz transformation, then demonstrated that the electromagnetic equations for a body at rest could be rewritten in terms of these types of vectors and were therefore themselves invariant. He turned to moving bodies and transposed the coordinate system to the moving body, with respect to which the electrodynamic equations for a body at rest must hold. He had already demonstrated that these equations were invariant under such a transformation, hence so were those for the moving body.

Minkowski generalized his mathematical treatment of the Lorentz transformation, putting the argument into matrix form.[111] Theorems on particular matrices followed and their invariance under the Lorentz transformations. He pointed out that many results of electrodynamics simply fall out of the algebraic characteristics

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Exact Sci. 17 (1977): 71–96, established how little Minkowski knew or understood of Einstein's special relativity.

110 In his first paper Minkowski referred to Lorentz, *Versuch*, and his piece for Klein's *Encyklopädie der mathematischen Wissenschaften* on Maxwell's theory and electrons and and Poincaré's *Rendiconti* paper. See Hermann Minkowski, "Die Grundgleichungen für die elektromagnetischen Vorgänge in bewegten Körpern," *Göttingen Nach.* (1908): 53–111, in *Gesammelte Abhandlungen,* 2 vols (New York: Chelsea, reprint of 1911 edition, 1967), vol. 2, 352–404, 352.

111 The format of the equations in the first sections of the paper give away the direction in which Minkowski would take the argument.

and manipulations of these matrices. In a short addition Minkowski addressed the issue of mechanics. Newtonian mechanics was equivalent to mechanics with the relativity postulate if we assume the velocity of light is infinite. In the context of turning to the case when the velocity of light was finite, Minkowski introduced the geometric analog to the algebraic form he had worked with thus far.

This first paper again subsumes the electrodynamics of moving bodies within an algebra and it was this form of Minkowski's paper to which Einstein and Jacob Laub, his first collaborator, responded.[112] Their response was to particular electrodynamical, not mechanical results in the paper.

The reworked geometrical version of Minkowski's paper was to claim far more for mathematics than he ventured in his initial work.[113] Space and time were joined in space-time, no longer could they be thought of separately. Orthogonality remained and within this geometry all the laws of mechanics and electrodynamics were invariant under particular group G_c transformations.[114] In space-time the relativity postulate as the requirement for invariance under these transformations became a much more grandiose claim.

> The postulate comes to mean that only the four-dimensional world of space and time is given by phenomena, but that the projection in space and in time may still be undertaken with a certain degree of freedom, I prefer to call it the *postulate of the absolute world* (or briefly the world-postulate).[115]

The laws of physics were written in the language of the geometry of this four dimensional space because "the validity of the world postulate, I like to think, is the true nucleus of the electromagnetic view of nature, which discovered by Lorentz, and further revealed by Einstein, now lies open to the light of day."[116] Minkowski also conjectured that further mathematical development of the mathematical consequences of the world postulate would lead to suggestions for experimental verifications of the postulate.

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112 For Jacob Laub and Einstein's work in relativity in this era and its mathematical, physical and institutional context see Pyenson, *The Young Einstein: The Advent of Relativity* (Boston: Adam Hilger, 1985).

113 Peter Galison, "Minkowski's Space-Time: From Visual Thinking to the Absolute World," *Hist. Stud. Phys. Sci.* 10 (1979): 85–121, sees Minkowski as a "visual thinker," and like previous scholars emphasizes the geometrical aspects of his thought. However, there is only passing mention of the geometrical representation in his first published version of his work on space–time.

114 Lorentzian rotations.

115 Minkowski, "Raum und Zeit," *Phys. Zt.* 10 (1909): 104–111, in *The Principle of Relativity,* W. Perrett and G. B. Jeffery, trans. (New York: Dover reprint of 1923 edition, nd), 75–91, 83. This was an address given to the Naturforscher–Versammlung September 1908.

116 Minkowski, "Raum und Zeit," 91.

Physics had become geometry, and it was a stunning achievement.

For mathematicians and Minkowski this work on relativity became **the** example of the relationship between mathematics and physics. There was a preordained harmony between mathematics and physics. Mathematicians held the key to this harmony, for mathematics provided deeper meanings to the gropings of physicists forever doomed to reinvent the mathematics they needed.[117] Mathematicians became convinced that they could infuse physics with the rigor and clarity of mathematics, and demonstrate how physics should be done in the future. Einstein later bemoaned his early neglect of mathematics and accepted its immense powers as keys to understanding phenomena. However, experience remained "the sole criterion of the physical utility of a mathematical construction."[118] More than once Einstein reminded mathematicians that their intellectual constructions were all very well and very elegant but did not "correspond to reality." Physicists contested this intrusion and rudely reminded mathematicians that there was more to physics than the formulation of a generalized, mathematical coherence. Physical imagery distinguished the physically significant from the myriad mathematical possibilities. Minkowski had chosen his group G_c rather than any other group of transformations because of its significance already indicated in mechanics and electrodynamics.

In the development of general relativity, the interactions and rivalries between mathematicians and physicists intensified. Mathematicians developed the mathematics at the same time that physicists required it in their research. David Hilbert, following Minkowski's lead, regarded physics as a derivative of mathematics. However, he was dependent upon physicists to interpret and accept the physical implications of his deductions. While Einstein called upon Marcel Grossmann's mathematical skills his judgment of their collaborative work was made on physical grounds.[119] Einstein felt that he needed tensor analysis and absolute differential calculus to develop a physics that encompassed electrodynamics and mechanics independent of any coordinate system. To accomplish this required that he reinterpret the meaning of measurement, and this impacted the very foundations of physics. The subsequent tensor law that replaced Newton's law of gravitation contained a set of mathematical functions. The specific forms of these functions

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117 Minkowski made this last remark, repeated later by Klein, in the draft notes to a lecture on relativity in 1907. See Galison, "Minkowski," 95–96, and Pyenson, "Relativity in Late Wilhelmian Germany: The Appeal to a Preestablished Harmony between Mathematics and Physics," *Arch. Hist. Exact Sci.* 27 (1982): 137–155, 147. The idea of preestablished harmony can be traced back to Leibniz.

118 Einstein, "The Methods of Theoretical Physics," in *The World as I see It* (London: 1935), quoted in Pyenson, *Einstein*, p. 153.

119 It is interesting to note that their paper of 1913 is in two parts, a physical one by Einstein, and a discussion of the mathematics and proof of crucial theorems by Grossmann. See, "Entwurf einer verallgemeinerten Relativitätstheorie und einer Theorie der Gravitation," *Zt. Math. Phys.* 62 (1913): 225–261.

were limited by physical laws and postulates that set the criteria the mathematics needed to satisfy.[120] The mathematical choices were still large. His subsequent misgivings and then abandonment of his 1913 formulation were based on the argument that the gravitational field equations were not themselves covariant, but also that the motion of the perihelion of Mercury deduced from it was too small.[121] His goal, a physics that was independent of all coordinate systems. *"The laws of physics must be of such a nature that they apply to systems of references in any kind of motion."*[122] His concerns were physical, expressed in mathematical form.

The problem of gravitation and general relativity attracted numerous others besides Einstein, many of them mathematicians, especially those educated or associated with Göttingen. One of the most active and intent was David Hilbert.[123] Hilbert recognized that mathematics and physics had drawn together, and both had changed. Previously mathematics had treated physical problems too mathematically and physicists had only taken necessary formulae from mathematics. With the example of his close colleague Minkowski before him, Hilbert concluded that physics needed pure mathematics. However, since mathematicians could learn physics easily while physicists found it impossible to follow modern mathematical papers, mathematicians would complete the union by solving physicists' problems for them. He would invite prominent physicists, such as Einstein, to give lecture series, and thus informed mathematicians would solve their problems.

Einstein gave such a series of lectures on the status of general relativity theory at Göttingen in the summer of 1915. Hilbert's solution to the problem of general relativity was based on axioms, the first of which was that "the laws of physical phenomena are determined by a world function H" that had certain mathematical properties. Hilbert treated H as a generalized Hamiltonian that was invariant under any transformation of any world coordinate.[124] Hilbert went further in defining H as the sum of two other functions. Relationships between these functions, K and L, contained all of electrodynamics and the "equations of gravitation." While Hilbert explored the mathematical properties of H, the physical conclusions that he drew

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120 See Einstein, "Zum gegenwärtigen Stande des Gravitationsproblems," *Phy. Zt.* 14 (1913): 1249–1262, in *Collected Papers,* vol. 4, 198–222, 198–200. See also the discussion in Jungnickel and McCormmach, *Intellectual Mastery*, vol. 2, 325–328. For a full account of Einstein's path to his general field equations of 1915 see Abraham Pais *"Subtle is the Lord..." The Science and Life of Albert Einstein* (Oxford: Oxford University Pres, 1982), chaps., 12–14.

121 See John Norton, "How Einstein found his Field Equations: 1912–1915," *Hist. Stud. Phys. Sci.* 14 (1984): 253–316, 298–299.

122 Einstein, "Die Grundlage der allgemeinen Relativitätstheorie," *Ann. Phy.* 49 (1916): 769–822, in *The Principle of Relativity*, 109–164, 113. Emphasis is in the original.

123 This account relies heavily on Pyenson *The Young Einstein*, 183–193.

124 Hilbert's world function clearly derived from Minkowski but its immediate predecessor was an equally mathematical function in Gustav Mie's electrodynamics.

from these manipulations were minimal.[125]

Einstein would voice regrets at his youthful neglect of mathematics and understood his own debts to mathematicians, but he was skeptical of these mathematical efforts.[126] He countered most of these theories with criticisms grounded in physical considerations.[127] He was equally critical of Hilbert's attempts to reduce gravitation to mathematics. Physical considerations overruled mathematical formulations no matter how comprehensive they seemed to be. While he was deeply interested in joining together the seemingly disparate parts of physics, it had to be done through the consideration of physical principles, even if it destroyed the mathematical unity of the effort.

These interactions between mathematicians and theoretical physicists in these few years set a pattern for their future throughout the twentieth century. Hilbert was right, members of both disciplines needed those of the other. Physicists needed mathematics as it was developing in the research of mathematicians. Mathematicians often found the subject matter of their research once again in the problems of physics. However, the tensions inherent in the exchanges of these early decades in the twentieth century would resurface later.

With the achievements of special then general relativity, Einstein and theoretical physicists and their subfield were thrust to the center of their discipline where their position was reinforced by the development of quantum mechanics in the 1920s. By this time theoretical physicists had grown in numbers and their speciality in importance. In the 1920s training in that specialty meant that they began to reproduce themselves, rather than taking one or two courses in the subject. Theoretical physics was at last a respectable and respected disciplinary partner to experimental physics. Part of that partnership was also a commitment to mathematics as the powerful languages in which theorists now expressed themselves. The cost was a distancing of their work even from their colleagues, except other theorists equally equipped with the mathematics they now required. They could no longer safely trust in a decades old mathematical tradition but had to maintain a close relationship with mathematicians, even sharing the same facilities and buildings. For their part, mathematicians would no longer ignore problems that physics or other sciences might present them. They might be the occasion for new mathematical insight.

However, tensions would remain, and physicists and mathematicians continued warily to court one another, even as the standards and values of their respective

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125 See David Hilbert, "Die Grundlagen der Physik," *Göttingen Nach.* part I (1915), reprinted in *Gesammelte Abhandlungen* vol. 3, 258–289. Hilbert discussed the motion of mass points, 285–289.

126 For a discussion on Einstein on mathematics, see Jungnickel and McCormmach, *Intellectual Mastery*, vol. 2, 334–340.

127 See, "Discussion" following the lecture on Einstein "Zum gegenwärten Stande," reported in *Phy. Zt.* 14 (1913): 1262–1266, in *Collected Papers,* vol. 4, 223–230, on Gustav Mie, Max Abraham, and Gunnar Nordström's efforts.

fields drove them in opposite directions. No matter the aesthetic pleasures of mathematics in the end physicists had to side with Einstein.

The instrument that mediates theory and praxis, thought and experiment is mathematics: it binds them together and forms their inner essence. Therefore, it appears that our contemporary culture in as far as it rests on the contemplation and manipulation of nature, depends on mathematics. — David Hilbert[128]

It still seems to me that you very much overrate the value of purely formal points of view. These are quite precious if there is an already-discovered truth finally to be formulated, but they almost always fail as a heuristic aid —Einstein to Felix Klein[129]

128 David Hilbert, "Naturerkennen und Logik," *Naturwissenschaften* (1930): 959–963, in *Gesammelte Abhandlungen,* 3 vols (New York: Chelsea Publishing, reprint of 1935 edition, 1965), vol. 3, 378–387, 385.

129 Einstein to Felix Klein 15, Dec. 1917. Quoted in Lewis Pyenson, "Mathematics, Education, and the Göttingen Approach to physical Reality, 1890–1914," *Europa* II (1979): 91–127, 125.

Bibliography

A full bibliography for a monograph such as this is a monster, even while listing only those items cited. Since footnotes appear at the bottom of the pages, it seemed less taxing on the reader to expand the index. Works cited are entered by page number at their first entry and by author(s) and subject matter separately. This bibliography is, therefore, a short introduction to available sources that will lead the reader into the topics covered in the text. The sources are grouped by topic area. My only regret is that Charles Gillispie, *Laplace* (Princeton NJ.: Princeton University Press, 1998), Christa Jungnickel and Russell McCormmach, *Cavendish* (Philadelphia: American Philosophical Society, 1997) and Robert E. Schofield, *The Enlightenment of Joseph Priestley* (University Park: University of Pennsylvania Press, 1997) came to my attention too late to be consulted for this volume. That these three volumes are biographies indicates the importance of the lives of individuals play in the history of science where the diversity of approaches to addressing research issues in both mathematics and physics are highlighted.

The best introduction to how different science was in the eighteenth century intellectually, socially and culturally and how historians have grappled with those differences is George S. Rousseau and Roy Porter, eds., *Ferment of Knowledge* (New York: Cambridge University Press, 1980). For eighteenth-century physics, see John Heilbron, *Electricity in the Seventeenth and Eighteenth Centuries: A Study of Early Modern Physics* (Berkeley CA: University of California Press, 1979) despite its subject matter being restricted to electrostatics. For physics as natural philosophy, see Casper Hakfoort, *Optics in the Age of Euler: Conceptions of the Nature of Light* (Cambridge: Cambridge University Press, 1995) and Geoffrey Cantor, *Optics after Newton: Theories of Light in Britain and Ireland, 1704–1840* (Manchester: University of Manchester Press, 1983). On caloric theories of heat, see Robert Fox, *Caloric Theories of Gases from Lavoisier to Regnault* (New York: Cambridge University Press, 1971). For mathematics in the eighteenth century there are the essays in the volumes (still emerging) of Euler's papers that set his work in their intellectual context. For example, see Clifford Truesdell, "The Rational Mechanics of Flexible, or Elastic Bodies, 1638-1788," in Euler, *Opera Omnia,* series 2, vol. 11, part 2. For the development of the calculus, see Ivor Grattan-Guinness, *The Development of the Foundations of Mathematical Analysis from Euler to Riemann* (Cambridge MA.: MIT Press, 1970), Judith Grabiner, *The Origins of Cauchy's Rigorous Calculus* (Cambridge MA.: MIT Press, 1981) and Niccolo Guicciardini, *The Development of Newtonian Calculus in Britain, 1700–1800* (Cambridge: Cambridge University Press) for the development of the calculus in Britain. There is no overview of the social history of the sciences in the eighteenth century. The standard on the most important eighteenth-century scientific society is still Roger Hahn, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666-1803* (Berkeley CA.: University of California Press, 1971). The place of science in the consumer societies of western Europe has been studied for the case of Britain in Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760–1820* (Cambridge: Cambridge University Press, 1992) and Larry Stewart, *The Rise of Public Science: Rhetoric, Technology and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992).

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The nineteenth-century sciences become more complicated to try and cover. For the development of physics in the German states, see Christa Jungnickel and Russell McCormmach, *The Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein* 2 vols., (Chicago: University of Chicago Press, 1986). John Purrington, *Physics in the Nineteenth Century* (New Brunswick NJ: Rutgers University Press, 1997) is an intellectual history of physics from the point of view of a late twentieth-century physicist who has read widely in the historical sources. No modern history of nineteenth-century theories of electricity and magnetism exists, and older accounts are no longer viable. Aspects of this history are in Geoffrey

Cantor and M. J. S. Hodge, eds., *Conceptions of Ether: Studies in History of Ether Theories, 1740-1900* (Cambridge: Cambridge University Press, 1981), Kenneth Caneva, "From Galvanism to Electrodynamics: The Transformation of German Physics and Its Social Context," *Hist. Stud. Phys. Sci.* 9 (1978): 63-159 and Jed Buchwald, *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century* (Chicago: University of Chicago Press, 1989). Studies on Maxwell's electromagnetism are many, the most recent, detailed and useful being Daniel M. Siegel, *Innovations in Maxwell's Electromagnetic Theory: Molecular Vortices, Displacement Current and Light* (Cambridge: Cambridge University Press, 1991). Later, crucial developments are in Bruce Hunt, *The Maxwellians* (Ithaca NY.: Cornell University Press, 1991), and Jed Buchwald, *From Maxwell to Microphysics* (Chicago: University of Chicago Press, 1985). Histories of nineteenth-century mathematics are centered on Germany, see Grattan-Guinness, *From Calculus to Set Theory* (London: Duckworth, 1980), and the early volumes *The History of Mathematics* published by the American Mathematical Society. The development of vector analysis is in Michael Crowe, *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System* (Notre Dame: Notre Dame Press, 1967). However, the distinctive mathematical traditions of Britain are seen in Elaine Koppelman, "The Calculus of Operations and the Rise of Abstract Algebra," *Arch. Hist. Exact Sci.* 8 (1971): 155-242, and Joan Richards, *Mathematical Visions: The Pursuit of Geometry in Victorian England* (Boston: Academic Press, 1988). The volume edited by Robert Fox and George Weisz, *The Organization of Science and Technology in France, 1808-1914* (New York: Cambridge University Press, 1980) explores the social history of French science in the nineteenth century. The Académie is scrutinized during the same period in Maurice Crosland, *Science under Control: The French Academy of Science, 1795-1914* (Cambridge; Cambridge University Press, 1992). The relationship between political power and scientists in Germany is in C. E. McClelland, *State, Society and the University in Germany, 1700-1914* (New York: Cambridge University Press, 1980), and in Britain in Jack Morrell and Arnold Thackray, *Gentlemen of Science: The Early Years of the British Association for the Advancement of Science* (Oxford: Clarendon Press, 1981).

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Cartan-Albert Einstein: Letters on Absolute Parallelism, 1929-1932 (Princeton NJ.: Princeton University Press, 1979).

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