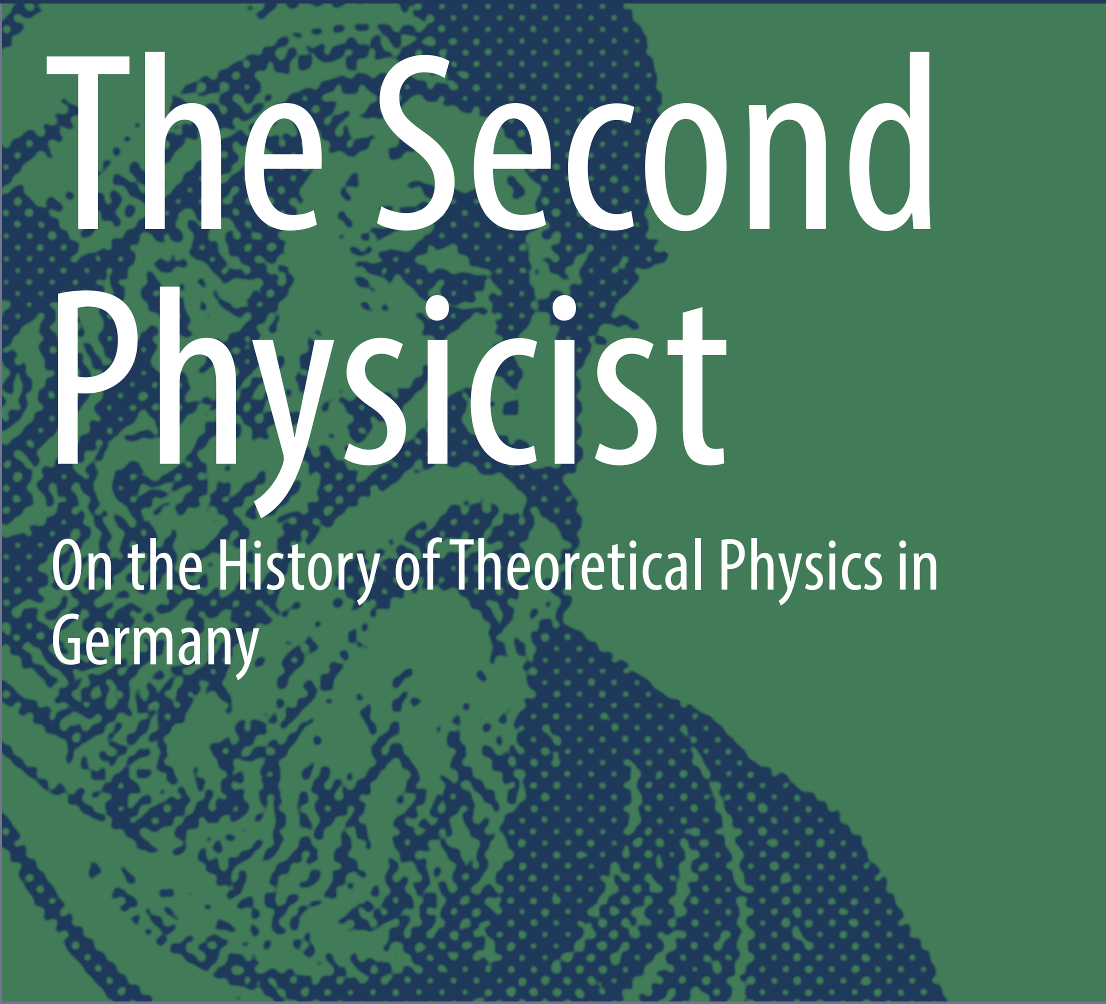
Archimedes 48 New Studies in the History and Philosophy of Science and Technology

Christa Jungnickel
Russell McCormmach



The Second Physicist

On the History of Theoretical Physics in Germany

Springer

The Second Physicist

Archimedes

NEW STUDIES IN THE HISTORY AND PHILOSOPHY OF SCIENCE AND TECHNOLOGY

VOLUME 48

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On the History of Theoretical Physics in Germany

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Christa Jungnickel
Eugene, Oregon
USA

Russell McCormmach
Eugene, Oregon
USA

This book is a revised and shortened version of *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, a two-volume work by the same authors, which has been published by The University of Chicago Press. Copyright © 1986 Christa Jungnickel and Russell McCormmach

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For our parents

When Germans start being accurate, there is no end to it.
—*Tolstoy,* War and Peace

I was beside myself with ecstasy for days.
—*Einstein to Paul Ehrenfest, January 1916*

Preface

"Even the formulation of this concept is not entirely without difficulty," Ludwig Boltzmann wrote in 1895.[1] He was referring to the concept of a "theoretical physicist," which he introduced so that his readers would understand what he had to say about his teacher Josef Stefan. We have taken on a task similar to Boltzmann's, only with reference to the many physicists who did research in and taught theoretical physics in Germany.

The interest of one of us—we use the first-person singular—was that of a student of the mathematical sciences who wanted to see the history of science integrated into more general history. My interest in the problem arose out of my studies and work with the German historian Jacqueline Strain, whose example impressed me with the need for historians to incorporate the work of specialists in the history of science into the materials of general history. I wanted to test the idea by studying nineteenth-century German educational and cultural institutions and, at the same time, science. In particular, I wanted to study physics, one of the earliest sciences to partake of what, in other contexts, might be considered a characteristically German cultural aspiration, the attainment of a unified and comprehensive vision. Together with many others who are concerned with German history, I also turned to the general subject of German culture with the wish to contribute, however remotely, to the understanding of what went wrong, of what produced the German culture that, despite all of its intellectual and artistic accomplishments, in many respects failed so conspicuously in the middle years of the twentieth century.

This book is a result of the complementary, as well as overlapping, interests of its authors. The early interest of the other author in theoretical physics goes back to a demanding senior course on the subject I took at Washington State College. Its teacher was the theoretical physicist William Band, who observed that in recent decades—I now refer to the textbook based on his lectures—theoretical physics had

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[1] Ludwig Boltzmann, "Josef Stefan. Rede gehalten bei der Enthüllung des Stefan-Denkmals am Dez. 1895," in *Populäre Schriften* (Leipzig: J. A. Barth, 1905), 92–103, on 94.

moved from its "classical" emphasis on mechanical constructions toward a more abstract mathematical approach, and his course accordingly surveyed "theoretical physics from a modern unified point of view." There was at the same time a continuity from the classical period in one of the modern theorist's primary goals: he sought, Band said, the one "universal law embracing the whole of physical reality," borne by a "faith that such a universal law does exist and is comprehensible to the human mind."[2] When I took up graduate work in physics, my thesis supervisor handed me a large stack of reprints of papers on superconductivity, a daunting problem in twentieth-century theoretical physics, which Niels Bohr, Albert Einstein, Max Born, Werner Heisenberg, and Richard Feynman, among others, tried to solve and failed. (As it turned out, that same year, the problem was solved by John Bardeen, Leon Cooper, and Robert Schrieffer, for which they received Nobel Prizes, complicating my intended thesis area.) I was struck by what Einstein said at the beginning of his paper on superconductivity: "nature is a merciless and harsh judge of the theorist's work. In judging a theory, it never rules 'Yes,' in the best case says 'Maybe,' but mostly 'No.' In the end, every theory will see a 'No'."[3] One reason I later chose the history of science as my field of study was to understand the history of theoretical physics, which in Band's presentation had impressed me with its power to describe widely different phenomena in a uniform way and which in Einstein's characterization had impressed me by its challenge. As I studied early works in theoretical physics, I became increasingly interested in the "theoretical physicist"—Boltzmann's difficult concept—and in his search for universal laws encompassing the physical world. I have undertaken this study in part to answer my questions about the historical circumstances that gave rise to the theoretical physicist and about what it meant for someone to become a specialist of that kind and to do that kind of work.

Our book treats the development of theoretical physics in one country, Germany. This is not to imply that the subject was invented only there; German physicists played an important part, but so did physicists from Britain and other countries. To avoid a misrepresentation of our subject, we show the many points at which German physicists brought together their work with the work of their colleagues abroad. Nevertheless, there is a proper sense in which we may define our subject as German theoretical physics: in the course of the nineteenth century, German physicists organized their field within their universities, a native institutional framework, which was also found in a few German-speaking locations outside of Germany. We give a historical account of the work of theoretical physics in its institutional setting in Germany.

At the beginning of the nineteenth century, the German universities had no function that called for more than elementary physics lectures for students seeking a general education before going on to professional studies in medicine, law, or

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[2] William Band, *Introduction to Mathematical Physics* (Princeton, NJ: Van Nostrand, 1959).

[3] Albert Einstein, "Theoretische Bemerkungen zur Supraleitung der Metalle," in *Het Natuurkundig Laboratorium der Rijksuniversiteit te Leiden in de Jaren 1904–1922*, ed. Heike Kamerlingh Onnes (Leiden: Eduard Ijdo, 1922), 429. The crisp translation of the Einstein quotation is by Jőrg Schmalian, "Failed Theories of Superconductivity," in *BSC: 50 Years*, ed. Leon N. Cooper and Dmitri Feldman (Singapore, Hackensack NJ, and London: World Scientific Publishing Co., 2010), 40–55, on 43.

theology. Consequently, they made no provisions for what we have come to associate with a scientific field such as physics: a comprehensive course of study, advanced training in research, and research by the established scientists who are the mentors (the model of scientific education at German universities that in time would spread round the admiring world). The idea that the production of new knowledge could lift these state institutions to a higher plateau and thereby raise the cultural and political reputation of the German states that maintained them supplied the main argument for educational reform during and for a time following the Napoleonic wars. It was also an idea that at least where the natural sciences were concerned had almost no hope of being formally realized then, since providing the means for research would have required relatively large amounts of money, which the then beleaguered and impoverished German states had more urgent uses for. It would also have required an understanding of whose business it was to provide the money and what, in fact, research in the natural sciences consisted of. That information was sought not from scientists but from bureaucrats and educators who had little interest in promoting experimental science. In time, the idea that research belonged at the German universities succeeded, becoming commonplace, and the union of research and teaching became a defining characteristic of a "German" university and, by implication, of a "German" scientific field. The practice of physics in Germany was influenced by the universities' system of organization.

The development of theoretical physics into a separate field is, of course, an instance of specialization within the natural sciences. The designation of new specialized fields was common at German universities during the period of our study. The specific form that specialization took within physics was a division into theoretical and experimental physics based on the partially different methods they applied to a common subject. Although every empirical science was understood to require theoretical guidance, as one of our physicists observed, it was only in physics that theoretical work developed into a major teaching and research specialty in its own right with its own methods.[4] The division of physics into theoretical and experimental physics perpetuated arrangements that had arisen out of practical necessity and elevated them into something of an ideal of scientific cooperation.

To draw as complete a portrait as possible of the working lives of physicists, we divide their activities into two categories.[5] First, we study the individual physicist and his relations with others working with him in the same field. In German universities of our period, physics was usually represented by only one senior physicist, and this meant that his interactions with fellow physicists of equal rank occurred by correspondence, visits, occasional formal meetings, and publications in specialized journals or academy proceedings. Second, we study the individual physicist and his relations with others within the institution employing him, usually his university, and with the bureaucracy of the state in which he worked. In the university, he dealt with

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[4] Wilhelm Wien, "Ziele und Methoden der theoretischen Physik," *Jahrbuch der Radioaktivität und Elektronik* 12 (1915): 241–259, on 241.

[5] As done, for example, in Edward Gross, *Work and Society* (New York: Thomas Y. Crowell, 1967), 12.

fellow faculty members who were not physicists but who nevertheless could influence important decisions regarding his subject. These decisions included appointments and promotions that affected physics, distribution of funds between physics and other subjects taught by the faculty, and allocation of university teaching and, in some instances, research. As an employee of the state—that is, as a university teacher and as director of the university physics instrument collection or physics institute—he dealt with a hierarchy of persons ranging from the laboratory servant to his state's minister of education or even his monarch. We outline these relations in a few words here; in practice, as we show in this book, they varied considerably with each German state, often with each university, and certainly with time. If the German university physicist was to succeed in both kinds of relationships, he had to arrange his work so that the practical task of his employment served the scientific task he pursued as a researcher, and conversely he had to represent his scientific work as important for carrying out his teaching obligation. This balancing of the parts of the physicists' work is central to our study; by tracing the ways in which physicists achieved it, we show how they ultimately arrived at a well-defined university field with well-understood and generally accepted goals and needs.

Of equal importance to us as the physicists' institutional activities is their scientific research, which shaped their relations with one another and suggested the courses they offered to advanced students. We do not give a complete and continuous history of their work, since many studies do that and little purpose would be served by going over the same ground; in any case, we could not do the subject justice in a book of this length. Instead, we discuss a limited number of theoretical researches by physicists in the several principal branches of physics. Innovation in methods of research is characteristic of the period we consider, and the individual researches we choose to discuss illustrate this and, as well, the wider conceptions of physics; in each case, we take care to locate the research within the workplace. Through surveys of the literature of physics, especially publications in journals, we give summary accounts of the work of physicists in and out of the universities and at all levels within the universities. In addition, we discuss lectures and textbooks, showing how physics was presented to the next generation of physicists and how the teachers integrated the branches and the whole of their science at different times.

We have organized our subject chronologically. In the development we describe, there are no sharply defined turning points; even an important scientific discovery requires time to prepare and time to affect the rest of physics. There are, however, certain periods during which institutional changes or scientific ideas that once needed to be advocated or justified are no longer discussed but are treated as a matter of fact, while their consequences now occupy the scientists, faculties, and government officials.

We divide our subject into three periods. The first, which begins with the new century and extends to about 1830, we include primarily to characterize the state of physics in Germany to which we refer subsequent changes. The disruptions of war, political reforms, and social realignments affected the German universities as they did most aspects of German life, and physics professors proceeded as best they could. Outside of Germany, particularly in France, these same decades produced an important body of mathematical physics together with major experimental work.

Physics from abroad entered German physics as it appeared, though its important consequences came only toward the end of our first period. It was then that the first German physicists who had grown up with a new physics began to acquire university professorships and scientific independence. The period saw important changes in the way physics professors saw their subject and themselves.

Early in our second period, 1830–1870, German physics was affected by changes in the way that positions in universities were filled and rewarded, with research becoming the primary criterion. We show how universities acquired elementary physics laboratories and advanced, or "higher," physics instruction, how certain forms of specialized physics activities came about, and how major physical concepts and laws were introduced. Over time, the teaching of physics became demanding for one professor, calling for another physicist at a junior rank. He was the "second physicist," who came to be identified with theoretical physics. In the middle of this period, the first important German physicist to specialize in theoretical physics appeared, Rudolf Clausius.

Our third period, extending from 1870 to around the turn of the twentieth century, marked the end of an era in German physics in the sense that physicists who embarked on their careers in the 1830s and 1840s with the goal of making a place for their experimental science at the universities now reached the end of their careers, concluding their successful efforts. Scarcely anyone any longer questioned that physics laboratories belonged at universities, or that students of physics should do more than just watch experiments and listen to lectures, or that physics was an intellectual subject comparable in value to the humanistic sciences. Physics came into the hands of men who built on what had become accepted practice, which had consequences: physicists could make a more effective case than before for physics education for different kinds of students, strengthening their claim on government support, and more students for physics meant more teachers in subordinate positions, which were usually designated for theoretical physics. As a complement to the specialization of physics teaching, theoretical physics was increasingly recognized as a specialized area of research. In 1875, Gustav Kirchhoff moved to Berlin University as the second physicist in a newly created position for a full professor of mathematical physics, joining Hermann von Helmholtz who held the professorship of experimental physics there. This signaled the impending systematic separation in German universities of the two parts of physics at the highest level, even though Kirchhoff's move did not have that design. Kirchhoff's successor at Berlin, Max Planck, was the first physicist who did not perform experiments to direct an institute designated for theoretical physics. By the end of the century, a degree of separation of the two parts of physics, if not yet equally realized at all German universities, was fully acknowledged. Theoretical physics acquired great distinction through the work of Planck and his later colleague in Berlin Albert Einstein, with whom we conclude our study.

The physicists whose work we discuss individually were leaders in the field. We recognize that much of the development of physics in the nineteenth century was carried out by physicists whose names are remembered only by historians of science, if by them. However, at any given time, the number of productive physicists in Germany was not large, and our focus on the work of a selection of them does not seriously bias our account. We give particular attention to 11 men, who belong to four generations. The earliest three—Georg Simon Ohm, Franz Neumann, and Wilhelm Weber—were born in the 15 years from 1789. The next three—Hermann von Helmholtz, Clausius, and Kirchhoff—were born in the 1820s. The next three—Woldemar Voigt, Heinrich Hertz, and Planck—were born in the 1850s. Born in 1844, Boltzmann falls between groups. The last man, Einstein, was born in 1879. The inclusion of Boltzmann and Einstein needs an explanation. Boltzmann was an Austrian, who held positions in theoretical physics in Germany. Einstein was born in Germany and received his early education there; he received his higher education and held his first positions in Switzerland, and after the end of our period, he moved to a position in Germany. None of the physicists came from a scientific family, though in most cases the father was a professional man. The fathers of two of them were professors, of theology and law. Two were lawyers. Occupations of the others were secondary schoolteacher of classical languages, school official and preacher, tax commissioner, farmer and estate agent, manufacturer of electrical equipment, and master locksmith, and one is uncertain. Most of the physicists were brought up in Protestant families. Boltzmann was a Catholic. Hertz's father converted from Judaism to Christianity. Einstein's parents were non-observant Jews. All of the physicists had university educations and all eventually held university positions. They all began publishing in their 20s, usually by around age 25, and they normally did their best work in that decade and in their 30s, which they sometimes extended. Planck was over 40 when he introduced the quantum theory, unquestionably his most important work, but he achieved his "first great scientific accomplishment," in Einstein's words, when he was 29.[6] If we exclude Hertz, who died at 37, the physicists lived on the average to age 67, and with few exceptions, they published fairly regularly until nearly the end of their lives. Throughout the book, we introduce researches by the above physicists (and by others) as examples of the work of theoretical physics. We give weight to theories concerned with the foundations of physics for the reason that physicists considered them to be the most important. We also recognize that in their publications, physicists applied their theories to specific problems of many kinds, which we illustrate. The work of theoretical physics inspired dedication, which was often obsessive. Voigt in an activity of over a half century published physics papers in at least a dozen journals, over one hundred in the *Annalen der Physik* alone. At age 52, he wrote to a colleague that he had been working too hard, his nerves were not right, and he was planning a vacation. He said that his last research had brought him "much joy," but it also did not leave him alone, "day and night," explaining that it is "my misfortune that I am so entirely passionate with my work."[7]

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[6] This was the third paper in his series, in 1887, "On the Principle of the Increase of Entropy," which dealt with the general theory of chemical equilibrium. Albert Einstein, "Max Planck als Forscher," *Naturwiss.* 1 (1913): 1077–1079, on 1077.

[7] Woldemar Voigt to Carl Runge, 1 March 1902, DM.

This book is a revised and shortened version of our two-volume *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, published by the University of Chicago Press in 1986. Christa Jungnickel, my coauthor and wife, died in 1990. I have written entirely new first and last chapters and have made substantial changes throughout. Although I draw on several excellent histories of physics published since our book came out, I do not attempt to survey the field again. What is offered here is a reformulation of our study, adding necessary explanations and other new materials, sharpening discussions, and making clearer what its goal is and what it accomplishes. It includes what is most important in the original book, develops it further, and makes our research more accessible. As in the original edition, we discuss the scientific publications of the time, but our object is not to offer new interpretations of the researches of the physicists we treat, nor is it to discuss the researches and their evolution in their entirety; for our account of them, we rely heavily on the many historical works on nineteenth-century physics. Our intent is to give a sufficient account of the researches and teaching to show the main features of theoretical work in physics.

In this revision of our book, our focus remains the same as in the original edition: the nature of the *work* of theoretical physics. We find that this approach leads us naturally to answers to our other questions. A word of caution: the work of theoretical physics and the work of theoretical physicists are not always the same, and neither are the authors of work on theoretical physics and theoretical physicists. In varying ways, experimentalists and mathematicians contributed to the work of theoretical physics. We make the distinctions as needed.

I am indebted to Rob Deltete for his advice with the revision.

Contents

List of Figures

List of Tables

Abbreviations of Archives

AR.	Algemeen Rijksarchief, Den Hague
A. Schweiz.	Archiv des Schweizerischen Schulrates, ETH Zürich
Bad. GLA.	Badisches Generallandesarchiv Karlsruhe
Bay. HSTA.	Bayerisches Hauptstaatsarchiv, München
Bay. STB.	Bayerisches Staatsbibliothek München
Bonn UA.	Archiv der Rheinischen Friedrich-Wilhelms-Universität Bonn
Bonn UB.	Universitätsbibliothek Bonn
Breslau UB.	Biblioteka Uniwersytecka Wrocław (Breslau)
DM.	Bibliothek des Deutschen Museums, München
DZA, Merseburg.	Deutsches Zentralarchiv, Merseburg
EA.	Einstein Archive, The Hebrew University of Jerusalem
Erlangen UA.	Universitäts-Archiv der Friedrich-Alexander-Universität Erlangen
Erlangen UB.	Universitätsbibliothek Erlangen-Nürnberg
ETHB.	Bibliothek der ETH Zürich
Freiburg SA.	Stadtarchiv der Stadt Freiburg im Breisgau
Freiburg UB.	Universitäts-Bibliothek Freiburg i. Br.
Giessen UA.	Universitätsarchiv Justus Liebig-Universität Giessen
Göttingen UA.	Archiv der Georg-August-Universität Göttingen
Göttingen UB.	Niedersächsische Staats- und Universitätsbibliothek Göttingen
Graz UA.	Archiv der Universität, Graz
Heidelberg UA.	Universitätsarchiv der Ruprecht-Karls-Universität Heidelberg
Heidelberg UB.	Universitätsbibliothek Heidelberg
HSTA, Stuttgart.	Württembergisches Hauptstaatsarchiv Stuttgart

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East Germany refused us permission to consult its state archives.

Jena UA.	Universitätsarchiv der Friedrich-Schiller-Universität Jena
LA Schleswig-Holstein.	Landesarchiv Schleswig-Holstein, Schleswig
Leipzig UA.	Archiv der Karl-Marx-Universität Leipzig
Leipzig UB.	Universitätsbibliothek der Karl-Marx-Universität Leipzig
Munich UA.	Archiv der Ludwig-Maximilians-Universität München
Münster UA.	Universitäts-Archiv der Westfälischen Wilhelms-Universität Münster
N.-W. HSTA.	Nordrhein-Westfälisches Hauptstaatsarchiv Düsseldorf
Öster. STA.	Österreichisches Staatsarchiv, Wien
STA K Zurich.	Staatsarchiv des Kantons Zürich
STA, Ludwigsburg.	Staatsarchiv Ludwigsburg
STA, Marburg.	Hessisches Staatsarchiv Marburg
STPK.	Staatsbibliothek Preussischer Kulturbesitz, Berlin
Tübingen UA.	Universitätsarchiv Eberhard-Karls-Universität Tübingen
Tübingen UB.	Universitätsbibliothek Tübingen
Würzburg UA.	Archiv der Universität Würzburg

Images of Researchers

Fig. 1 Carl Friedrich Gauss, 1777–1855

Fig. 2 Georg Simon Ohm, 1787–1854

Fig. 3 Franz Neumann, 1798–1895

Fig. 4 Wilhelm Weber, 1804–1891

Fig. 5 Hermann von Helmholtz, 1821–1894

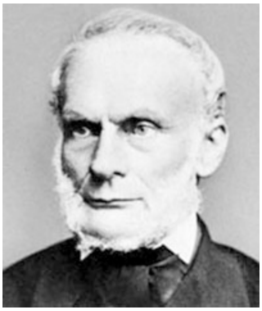

Fig. 6 Rudolf Clausius, 1822–1888

Fig. 7 Gustav Kirchhoff, 1824–1887

Fig. 8 Ludwig Boltzmann, 1844–1906

Fig. 9 Woldemar Voigt, 1850–1919

Fig. 10 Heinrich Hertz, 1857–1894

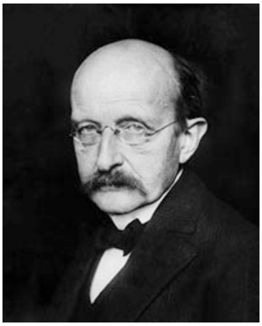

Fig. 11 Max Planck, 1858–1947

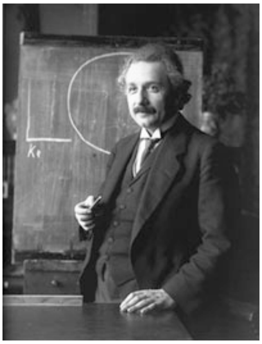

Fig. 12 Albert Einstein, 1879–1955

Map of German Universities

Abhandl. d. K.S. Gesellsch. d. Wissensch., phil.-hist. Kl. XXIV. II.

Map 1 German universities in the beginning of the twentieth century. Founded before 1700: *light type*. Founded since 1700: *dark type*. Universities in parentheses have ceased. Universities underlined are not German-language universities (Reprinted from Franz Eulenburg, “Die Frequenz der deutschen Universitäten”)

Chapter 1
Toward a Characterization of Theoretical Physics in Germany

In the first edition of our book, we did not define theoretical physics, intentionally, for we did not want to prejudge the issue: the examples of research we discussed in the book we chose because they were considered at the time to be important work in theoretical physics. One critic so misread our book that we are said to have identified theoretical physics with jobs in theoretical physics.[1] Most of the physical theories we discuss were by men who were not working in theoretical jobs, and not all of them ever had one. To forestall any such misreading in the future, and for other reasons, we lay out features of theoretical physics in our period in this chapter. Although we agree with Boltzmann that it is by no means easy to characterize our subject, much less give a precise definition of it, we can offer the reader some orienting guidelines.

Boltzmann and other physicists in our study saw theoretical physics as beginning in the seventeenth century, and given what they meant by theoretical physics they were right. Across changes in definitions of physics, methods, philosophies, goals, concepts, and advances in physical knowledge over time, the activity of making scientific theories of physical phenomena retained a core identity. What Newton looked for and what Einstein looked for had a common object: concepts for understanding the physical world and laws embodying the concepts, and theories were part of the work. Historians advance their understanding by making distinctions, which they call periods; they also recognize that a satisfactory understanding of an activity may require giving attention to developments that span periods, as defined by other criteria. Theory in some form has regularly been a part of physics, and the specific ways in which it has been made and presented have depended on historical factors such as the stage of physics at the time, the examples of other

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[1] Elizabeth Garber, *The Language of Physics: The Calculus and the Development of Theoretical Physics in Europe, 1750–1914* (Boston, Basel, Berlin: Birkhauser, 1999), 17. With the exception of this note and two later ones, we do not call attention to our differences with this author, as they are peripheral to our purpose.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_1

theories then, and what was expected of a theory then. We see theoretical physics as evolving with the rest of physics.[2]

In this chapter, we establish a framework for discussing the researches of German physicists. A primary source for this purpose is the ways the first theoretical physicists, who appear in the latter part of our period, characterized their field. From our study of their work and of their predecessors' work, we supply further characteristics. In the next chapters where we discuss physical work in the early part of our period, we point out correspondences between it and the theoretical work discussed in this chapter.

1.1 Physics, Theoretical Physics, and Experimental Physics

Physics is the science of the general properties of all bodies, a common definition, of which there were variants. When this study opens, in the early nineteenth century, the parts of physics look familiar to us. Paul Erman, the physics professor at Berlin University, gave a complete cycle of lectures covering the parts of physics every year. He began with a lecture course on general physics, which included mechanics, followed by a course on magnetism, electricity, and electric current, then by a course on heat and light, and finally by a course on meteorology.[3] There were questions about the contents of physics and whether or not border subjects such as meteorology should be added to the standard ones, which took time to work out.

"Theoretical physics" could refer to several things, each of which we take up. One is work that has to do with theoretical issues, as set out in lectures and publications and as used in ongoing researches; detached from its first appearance, it may or may not have been reformulated or reinterpreted in the meantime. Another meaning is a piece of original theoretical research usually in the form of a paper. Another is the activity of doing theoretical research. A fourth meaning is a part of physics and an academic field, which German physicists often called a "discipline." A fifth meaning is a specific profession, a term which applies at the end of our period.[4]

Physicists we treat in this book often worked on problems in chemistry and other sciences as well as in their own. This does not mean that they did "interdisciplinary"

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[2] An ample defense of the evolutionary approach to history of science, currently somewhat out of step with academic fashion, is given in the fine history of physics by Robert D. Purrington, *Physics in the Nineteenth Century* (New Brunswick, NJ and London: Rutgers University Press, 1997), ix–x.

[3] Max Planck, "Das Institut für theoretische Physik," in *Geschichte der Königlichen Friedrich-Wilhelms-Universität zu Berlin*, by Max Lenz, 4 vols. in 5 (Halle a. d. S.: Buchhandlung des Waisenhauses, 1910–1918), vol. 3, *Wissenschaftliche Anstalten. Spruchkollegium. Statistik* (1910), 276–78, on 276.

[4] Although we have not come across the use of the term "profession" in this connection at the time, Woldemar Voigt, Gustav Kirchhoff, and especially Max Planck were professional physicists who could also be considered the first professional theoretical physicists in Germany because of the nature of their research and of their jobs.

work; rather it means that they applied their methods to phenomena arising in, or shared with, other sciences. In place of "discipline," German physicists also used the word "field," which is commonly used by English speakers today, and it is our preference; we mean by it a science or branch of a science or scientists who worked in it, depending upon the context. Our object is not to analyze a discipline or a field or a profession, proper subjects for sociology, but to examine the *work* of German physicists who made and used theories, recognizing that it like all human activities has a history. Our subject is admittedly narrow, but so is most history, depending on the perspective from which it is judged.

Physics is divided into two "great departments," "theoretical physics" and "experimental physics," which are distinguished by their "intellectual work," according to the physicist and physiologist Hermann von Helmholtz.[5] In an address in 1874, he discussed the relations between mathematical physics and experimental physics. Proceeding inductively, experimental physics opens up a field of phenomena and arrives at an empirical law connecting them, and the completion of what has been found inductively relies on the deductive work of "conceptual, and preferably of mathematical analysis." The passage from inductive uniformities or laws to general laws requires a mathematical way.[6] Thoughtful physicists held sophisticated views on the relation of theory and experiment, and none more so than Heinrich Hertz. In one of his papers on electric waves and electromagnetic theory, he wrote that although explanations of the formulas of the theory are "given as facts derived from experience, and experience must be regarded as their proof . . . each separate formula cannot be specially tested by experience, but only the system as a whole." In another paper, he wrote that his experiments on electric waves establish conclusions independently of any given theory, but they also "amount to so many reasons in favor of that theory of the electromagnetic phenomena which was first developed by Maxwell from Faraday's views." In Hertz's understanding, no one experiment can be taken as decisive either as confirmation or as falsification of a theory. Consideration of the theory and experiments as a whole is required. Boltzmann, whose epistemological views were equally sophisticated, disagreed with Hertz, maintaining that theories can be tested piece by piece against experience.[7] The physicists who enter this book raised questions that philosophers of science still debate.

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[5] Hermann von Helmholtz, *Vorlesungen über theoretische Physik*, vol. 1, pt. 1, *Einleitung zu den Vorlesungen über theoretische Physik,* ed. Arthur König and Carl Runge (Leipzig: J. A. Barth, 1903), 3.

[6] Leo Königsberger, *Hermann von Helmholtz,* trans. F. A. Welby (Oxford, 1906), 284–85. Helmholtz's address was "On the Attempt to Popularize Science," which was published as his preface to the translation of John Tyndall's *Fragments of Science* (London, 1879).

[7] Heinrich Hertz, "On the Fundamental Equations of Electromagnetics for Bodies at Rest," in *Electric Waves; being Researches on the Propagation of Electric Action with Finite Velocity through Space*, trans. D. E. Jones (New York: Dover Reprint, 1962), 195–240, on 197; "On Electromagnetic Waves in Air and Their Reflections," in *Electric Waves*, 124–36, on 136. We base this discussion of Hertz's holistic view of the relation of theory and experiment on Salvo D'Agostino, *A History of the Ideas of Theoretical Physics: Essays on the Nineteenth and Twentieth Century Physics* (Dordrecht: Kluwer Academic Publishers, 2000), 100–101, 211.

The experimental physicist Gustav Magnus, who belonged to the generation before Helmholtz, recommended maintaining a "strong separation of the work between mathematical and experimental physicists."[8] Helmholtz disagreed, and in his address as successor to Magnus he characterized mathematical physics as no less empirical than experimental physics. In his subsequent lectures on theoretical physics, he continued the thought: "experimental physics entirely without mathematical physics is a very narrowly bounded science and gives little insight into the course of physical phenomena, while the reverse, mathematical physics without experimental physics, would be a rather lame and unfruitful science because one cannot make theories of natural processes before one has come to know the processes from a proper point of view."[9] According to Paul Drude, who carried out both experimental and theoretical physics, the researcher should not separate the work of the writing desk from that of the laboratory, since the experimenter does not succeed without theory, and the theorist's work becomes grey if not nurtured by the "life tree" of experiment. He said that the development of physics owed most to individuals who "united experimental and theoretical schooling in equal measure" such as Helmholtz Hertz, Kirchhoff, and James Clerk Maxwell.[10] The reason Ludwig Boltzmann gave for working in Helmholtz's laboratory was to enter into a relationship with Helmholtz and also to confound the view that a "calculating physicist" can never experiment.[11]

Experimental physicists held the same view of the subject as their theoretical colleagues. In his inaugural lecture at the Prussian Academy of Sciences, Helmholtz's successor August Kundt said that when advancing into the region of unknown facts and when inventing new ways of discovery, the experimental physicist makes use of theory while recognizing that his own experimental work is equally significant. In his reply to Kundt's address, the secretary Emil du Bois-Reymond said that Kundt's own work achieved a balance between "both directions of physical research," and as evidence he referred to a research he had carried out with Emil Warburg, establishing that the "molecule" of mercury gas is really a single atom, in agreement with theory.[12]

In their experimental publications, physicists usually included some theoretical discussion. In their theoretical publications, they made experimental predictions when they could, which was not always. Hertz's work on new principles of

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[8] Hermann von Helmholtz, "Zur Erinnerung an Rudolf Clausius," *Verh. phys. Ges.* 8 (1889): 1–7, on 2.

[9] Hermann von Helmholtz, "Gustav Magnus. In Memoriam," in *Popular Lectures on Scientific Subjects*, trans. E. Atkinson, 2nd ser. (London, 1881), 1–25, on 17–19; Helmholtz, *Einleitung . . . theoretische Physik*, 4.

[10] Paul Drude, *Die Theorie in der Physik. Antrittsvorlesung gehalten am 5. Dezember 1894 an der Universität* (Leipzig, 1895), 14.

[11] Boltzmann to "Herr Director" (Stefan?), 2 February 1872.

[12] August Kundt, "Antrittsrede," *Situngsber. d. preuss. Ak. d. Wiss.* (1889), 679–83, on 682. Du Bois-Reymond's response, 683–85. Wilhelm von Bezold, "Gedächtnissrede auf August Kundt," *Verh. phys. Ges.* 13 (1894): 61–80, on 74.

mechanics is an example. While preparing the theory for publication, he wrote to a colleague that he was occupied with a work that "unfortunately has a purely theoretical interest and no practical [experimental] interest at all."[13]

Boltzmann said that the experimentalist makes complex calculations in evaluating his observations, but he is not doing theoretical physics, which is to order the phenomena and describe them clearly and simply.[14] We might qualify this by another statement by Boltzmann: the distinction between the theorist and the experimentalist is not profound, and the "breaking up of physics into theoretical and experimental is merely a consequence of the prevalent division of methods and will not last forever."[15] The separation has proved more enduring than Boltzmann probably expected, but the theoretical reasoning and competence that experimentalists exhibited in their work showed that the separation was not unbridgeable. If we take theoretical physics to encompass both the derivation of natural laws and experimental predictions, we recognize that some of the work of theoretical physics was done by experimentalists as well as by theorists; experimentalists extended the theorists' deductive work of conceptual and mathematical analysis in preparation for their investigations in the laboratory. In addition to their specialized work, theoretical physicists and experimental physicists had a region of shared work, and when experimental work and theoretical work were carried out by the same physicist, the line could be hard to draw. In addition, certain kinds of laboratory work were understood to be part of the practice of theoretical physics. In their recommendation of Boltzmann for the chair of theoretical physics, the Munich philosophical faculty described the relationship between theoretical and experimental physics. They accepted the need for special positions for theoretical and experimental physics, but they did not think that physics would split into two completely separate branches. Instead, experimental and theoretical physics would "supplement and penetrate one another,"[16] a perceptive prophesy.

1.2 Theoretical Physics, Mathematical Physics, and Terminology

Because we are concerned with the work of men who made theories of physics in Germany in the nineteenth century, we need to become familiar with the terminology of the time. The recognition of a division of physics into experimental

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[13] Tertz to Sarasin, 19 May 1893, Ms. Coll., DM, 3149.

[14] Boltzmann, "Stefan," 94.

[15] Ludwig Boltzmann, "The Relations of Applied Mathematics," in *International Congress of Arts and Science: Universal Exposition, St. Louis, 1904*, vol. 1, *Philosophy and Mathematics* (Boston and New York: Houghton, Mifflin, 1905), 591–603, on 596.

[16] Dean von Baeyer of Section II of the Munich U. Philosophical Faculty to Munich U. Senate, 24 November 1889, Munich UA, E II-N, Boltzmann.

and mathematical parts in the first half of our period is evident from titles of lecture courses, which often contain the terms “experimental physics” and “mathematical physics.” The latter subject was taught by physicists and also by mathematicians, in recognition of the practical separation of the methods of physics and also of the interests, competences, and audiences of the lecturers.

The mathematical methods of physics were acknowledged when the term “mathematical physics” was attached to the position for the second physicist at German universities. This is illustrated by the early appearance of a second physics professorship at Göttingen in the 1840s. When after an absence of several years the professor of experimental physics Wilhelm Weber returned to Göttingen, J. B. Listing who had replaced him was made full, or “ordinary,” professor for mathematical physics, a new position, and he was given his own rooms and apparatus in the physics institute alongside Weber’s. Weber had been willing to accept the mathematical physics professorship, but he preferred the one for experimental physics, because except for his own researches he had done little with mathematical physics.[17] He understood that to represent mathematical physics properly required appropriate knowledge and experience.

The border between mathematics and physics was not always sharply drawn, though the main objectives of the two fields naturally differed. With the physicist, whose object was to connect theory with experiment and observation, mathematical rigor was lax. By contrast, the mathematician who worked in mathematical physics was drawn to examples of difficult mathematical problems, with physics supplying the inspiration, and in treating the problems he gave attention to mathematical rigor. The mathematician might also take an interest in what mathematical physics reveals about the workings of the physical world, and on occasion he might contribute to the work of making theories: he did so in an obvious way when he assisted a theorist with a mathematical problem or developed a mathematical method of use in physics, and he did so again when he derived a mathematical law of physics. The mathematicians Carl Neumann, Bernhard Riemann, and Hermann Grassmann developed electrodynamic laws, which the theoretical physicist Clausius examined in his electrodynamic researches.[18] In an address in 1912 on the last hundred years of physics research and teaching in Germany, the theoretical physicist Woldemar Voigt surveyed physics branch by branch, highlighting the major German contributions in each. The expected physicists appear: Ohm, Neumann, Weber, Hertz, Kirchhoff, Helmholtz, Planck, and others we meet in this book. The mathematician Carl Friedrich Gauss also appears, both for his theoretical and experimental work on magnetism and for his principle of least constraint in mechanics. Hertz made use of Gauss’s principle, with a small modification, in his reformulation of mechanics, clarifying this branch of physics

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[17] This episode is discussed in chap. 7.

[18] Rudolph Clausius, *Die Mechanische Wärmetheorie,* vol. 2, *Die mechanische Behandlung der Electricität* (Braunschweig, 1879). In chap. 9, on the derivation of a new electrodynamic fundamental law, Clausius discusses the electrodynamic laws developed by the mathematicians.

with the intent of adapting the theory of mechanics to the theory of the ether, and Helmholtz used the principle in developing electrodynamic theory.[19]

Just as some mathematicians took an active interest in physics, some physicists took an active interest in mathematics. Boltzmann, who for a time held a professorship in mathematics, said that Kirchhoff in addition to his theoretical work also published papers of "merely mathematical interest, that is, their importance lay not in results, but in the completion of mathematical methods." Boltzmann had a full appreciation of mathematical methods of theoretical physics, and persons who did not share that appreciation he likened to "the Greek philosopher who said of Archimedes' investigations of the properties of the ellipse that they had no importance beyond their pleasing form." We know that Archimedes' work was the basis of later astronomy, which guided ships at sea. Whoever understands the difficulty of finding mathematical formulas for "describing exactly natural phenomena," Boltzmann said, should value the "greatest discovery of our century," the mathematician G. L. Dirichlet's calculation of the class number of all quadratic forms.[20] Whatever their motivations, mathematicians did work that theoretical physicists valued for their own work.

Theoretical physicists lectured and published on mathematical methods, a tradition that went back at least as far as Newton's treatise on the "mathematical principles of natural philosophy" in 1687, the *Principia*. In 1859, Clausius published a book on the "potential function" and its integral, the "potential," as "a contribution to mathematical physics." He explained that it was hard for physicists to learn what they needed to know about the "theory, significance, and properties" of this important mathematical method. Written to fill the gap, his book went through several editions.[21] Physicists were responsible for much of the development of another important branch of mathematics for physics, vector analysis. Taught with physics and used in physics research, mathematical methods such as the potential function and vector analysis were a part of physics as well as of mathematics. Because mathematicians could not be expected to formulate the concepts for describing the physical world, the need, which was continuous, was met through our period by physicists who were capable in mathematics and physical in their thinking.

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[19] Woldemar Voigt, *Physikalische Forschung und Lehre in Deutschland während der letzten hundert Jahre. Festrede im Namen der Georg-August-Universität zur Jahresfeier der Universität am 5. Juni 1912* (Göttingen, 1912), 4; Heinrich Hertz, *Die Prinzipien der Mechanik, in neuem Zusammenhange dargestellt*, ed. Philipp Lenard (Leipzig, 1894); *The Principles of Mechanics Presented in a New Form*, trans. D. E. Jones and J. T. Walley (London, 1899; New York: Dover Reprint, 1956), 31–32.

[20] Ludwig Boltzmann, *Gustav Robert Kirchhoff* (Leipzig, 1888), 28.

[21] Rudolph Clausius, *Die Potentialfunction und das Potential. Ein Beitrag zur mathematischen Physik* (Leipzig, 1859). The "potential function" is the "function of force" of an agent that acts according to the inverse square law: $V = \varepsilon \int dq^{\prime}/r$. The "potential" is $W' = \varepsilon \iint dq dq^{\prime}/r$. Work is the difference in W' in the initial and final states.

Over the course of the nineteenth century, the meaning of the term "theoretical physics" changed. At the beginning, it was commonly used for titles of university lectures and textbooks that presented physics without experiments. By the time of Baumgartner and Ettingshausen's textbook on physics in 1839, "theoretical physics" meant "higher physics," for which elementary physics was a preparation. "Elementary physics" was taught as "experimental physics," in which physical laws were justified by experiments, whereas in the teaching of "theoretical physics," the laws of phenomena were deduced mathematically from first principles.[22] From the 1830s, Franz Neumann taught a lecture course, "Introduction to Theoretical Physics," covering mainly mechanics, followed by courses on the theories of the other branches of physics. In the 1850s at Heidelberg Kirchhoff gave a regular 6-h lecture course on experimental physics, and over the first 10 years he also gave a three-hour lecture course in the winter on "theoretical physics," which treated mainly mechanics in the "wider sense." In his last years, the lecture announcement dropped "theoretical physics," reading simply "mechanics," and the lectures that followed were entitled mechanics of elastic and fluid bodies, theory of heat and electricity, and optics.[23] In an irregular manner, the term "theoretical physics" gradually came to replace the term "mathematical physics" for most uses in physics. The following examples give an idea of how the change came about. As a private teacher, or "Privatdocent," at Berlin from 1850, Clausius taught 4 h a week, treating physics "from a mathematical-theoretical point of view."[24] In a letter to the Baden minister of interior in 1854, Robert Bunsen said that Clausius's works on heat and optics used well-grounded views, but they were of a "purely mathematical physical nature, supported by the observations of others."[25] In 1855, in reference to his research, Clausius informed an official of the Zürich Polytechnic that previously he had applied himself to "theoretical physics with special preference."[26] That same year Poggendorff recommended Clausius as the best "candidate especially for mathematical physics."[27] In 1868 the Bonn University science faculty used both terms in recommending Clausius, pointing out his "outstanding gift for mathematical-physical speculation," and as

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[22] Andreas von Baumgartner and Andreas von Ettingshausen, *Die Naturlehre nach ihrem gegenwärtigen Zustande mit Rücksicht auf mathematische Begründung*, 6th ed. (Vienna, 1839), 7.

[23] *Anzeige der Vorlesungen ... auf der Grossherzoglich Badischen Ruprecht-Carolinischen Universität zu Heidelberg*, the published list of courses offered each semester at Heidelberg. Friedrich Pockels, "Gustav Robert Kirchhoff," in *Heidelberger Professoren aus dem 19. Jahrhundert*, ed. Karl Friedrich (Heidelberg: C. Winter, 1903), vol. 2, 243–63, on 248; Wilhelm Lorey, *Das Studium der Mathematik an den deutschen Universitäten seit Anfang des 19. Jahrhunderts* (Leipzig and Berlin: B. G. Teubner, 1916), 72–73.

[24] Georg to Sidler, 31 March 1855; also Rudolph Clausius's Vita, 17 June 1855.

[25] Robert Bunsen to Baden Min. d. Innern, 26 July 1854, Bad. GLA, Sign. 235/3135.

[26] Rudolph Clausius to the Schulratstpräsident, 31 May 1855. Quoted in Grete Ronge, "Die Züricher Jahre des Physikers Rudolf Clausius," *Gesnerus* 12 (1955): 73–108, on 80.

[27] Poggendorff to Brunner, 18 May 1855; A. Schweiz. Sch., Zurich. "Auszug aus dem Protokoll," 24 August 1855.

one of the founders of the mechanical theory of heat, he was “one of the best theoretical physicists.”[28] In 1870 Helmholtz said that he was tired of physiology, and that he was now interested only in “mathematical physics.”[29] The two terms were used as alternatives at Göttingen when Voigt was appointed “ordinary professor for theoretical (mathematical) physics” in 1883.[30] The same occurred in 1875, when Kirchhoff was appointed to a newly founded ordinary professorship for “mathematical physics” at Berlin University, although in his negotiation with Berlin, he spoke of wanting to lecture on “theoretical physics.”[31] We see it again in the lectures on theoretical physics Helmholtz gave at Berlin late in his career, in which he said that “theoretical physics” has also been called “mathematical physics.”[32]

By 1895 the separate usages of the terms “theoretical physics” and “mathematical physics” had become sufficiently acknowledged that Boltzmann said that it was inappropriate to characterize the work of the theoretical physicist as “mathematical physics.” The theorist has to have a sure command of mathematics, but he is not doing mathematics.[33] Experimentalists too acknowledged the change in terminology. In 1900, Otto Wiener explained to the Leipzig faculty that Boltzmann’s work belonged to “theoretical physics” rather than to “mathematical physics,” since it stressed “physical content” and the “connection with experimental physics” and did not treat mathematics as an end in itself, which was done by others.[34] Still, with regard to individual physicists, the terminology could be a grey area. After his graduation from the Zürich Polytechnic, Einstein wrote to Wilhelm Ostwald at Leipzig asking if he had use for a “mathematical physicist,” enclosing his first paper, published in the *Annalen der Physik*; he did not introduce himself as a “theoretical physicist,” and this was in 1901.[35] Both before and after the term “theoretical physics” was used in the modern sense, physicists who made theories differed in their mathematical skills and emphases. To take an example from a time after this book, the Göttingen theoretical physicist Max Born said that if the distinction between mathematical physics and theoretical physics had any meaning, his colleague Arnold

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[28] Evaluation of the Bonn University science faculty, July 1868, Bonn U, Archive, Plücker Personalakte.

[29] Helmholtz to Du Bois-Reymond, 7 April and 17 May 1870, STPK, Darmst. Coll. F 1 a 1847.

[30] Prussian Ministry to Göttingen U. Curator von Warnstedt, 3 September 1883; Voigt to Curator, 4 October 1883; Curator to Voigt, 9 October 1883; Voigt Personalakte, Göttingen UA, 4/V h/203.

[31] The Prussian ministry of culture told Kirchhoff that he did not have to give prescribed lectures. Kirchhoff to Du Bois-Reymond, 13 December 1874.

[32] Helmholtz, *Einleitung . . . theoretische Physik*, 4.

[33] Boltzmann, “Stefan,” 94.

[34] Otto Wiener to Dean Eduard Sievers of the Leipzig U. Philosophical Faculty, 10 March 1900; letter to the Saxon Ministry of Culture and Public Education, drafted by Wiener and signed by Ostwald, Wilhelm Wundt, Heinrich Bruns, Otto Hőlder, Carl Neumann, and Sievers, 12 March 1900; Ostwald, “Beibrief,” 12 March 1900; Ministry to Philosophical Faculty, 4 August 1900; Boltzmann Personalakte, Leipzig UA, PA 326.

[35] Banesh Hoffmann, *Albert Einstein Creator and Rebel* (New York: Viking Press, 1972), 32.

Sommerfeld was a “mathematical physicist.”[36] Sommerfeld was director of theoretical physics at Munich and a major contributor to atomic theory in the early years of the twentieth century, and when the theoretical physicist Max Planck’s professorship at Berlin was vacated, Sommerfeld headed the list of eminent candidates to replace him. In Sommerfeld’s case, the older term “mathematical physics” meant a strongly mathematical approach, which had an honored place in theoretical physics.

“Mathematical physics” encompassed work that took its starting point in physical theory but had only or mainly mathematical interest and also work that agreed with what later would be called “theoretical physics.” With the introduction of the term “theoretical physics,” this ambiguity was largely removed. Because experimental work was quantitative, it went without saying that physical theories were mathematical, and because there was more to the methods of theoretical physics than mathematics, “theoretical physics” was a better description of the work of a researcher whose field was physics. There was a conceptual justification for the change as well. Over the previous three decades, university physicists had lectured on “theoretical physics,” laying down the concepts, laws, and methods of mechanics applicable in all parts of physics. Because throughout this time and nearly to the end of our period, mechanics was understood to provide the foundation for the rest of physics, the term “theoretical physics” acknowledged the base. In due course, the position of the second physicist in German universities came to be called “theoretical” rather than “mathematical” physics. The understanding of the work of physics that went with the change of wording was that mathematical elaborations of theory having no current or eventual connection with experimental physics would have no place. A practical benefit of this was the justification for a second physicist, since a mathematician in the second position would not be the same.

The change from “mathematical” to “theoretical” physics had a wider setting. In German universities in the middle of the nineteenth century the ideal of pure mathematics came to dominate mathematical education. Physicists had reason to distinguish their work from this version of mathematics, as their change of wording suggests. The word they adopted, “theoretical,” was not limited to physics. Around the same time that “theoretical physics” began to appear, physical chemistry came to be recognized as a distinct field. In 1893 Walther Nernst, who held the first professorship in the new subject, published *Theoretische Chemie*, the text that had the greatest influence on the professional training of physical chemists.[37] In his

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[36] Max Born, “Arnold Johannes Wilhelm Sommerfeld, 1868–1951,” *Obituary Notices of Fellows of the Royal Society* 8 (1952): 275–96, on 282.

[37] F. A. Lindemann, Lord Cherwell, and Franz Simon, “Walther Nernst 1864–1941,” *Obituary Notices of Fellows of the Royal Society* 4 (1942): 101–6, on 103. Just as in physics, with its alternatives “mathematical physics” and “theoretical physics,” chemistry had “mathematical chemistry” and “theoretical chemistry.” In 1894, the year after Nernst’s book on theoretical chemistry, Georg Helm published *Grundzüge der mathematische Chemie*, a theoretical work in which he set out to develop physical chemistry as a branch of mathematical physics, organizing and unifying the results of research within a single framework based on a small number of fundamental principles. He was trained in mathematics and physics and did research in both fields, and he had definite ideas on what mathematical physics could and should do. Rob Deltete, “Georg Helm’s Chemical Energetics,” *HYLE-International Journal for the Philosophy of Chemistry* 18 (2012): 23–44.

lectures on heat theory around the same time, Helmholtz treated thermochemistry, referring to the "recent development of theoretical chemistry."[38] In astronomy "theoretical" positions appeared alongside "practical" ones. In proposing Helmholtz as a foreign member of the Prussian Academy of Sciences in 1870, Du Bois-Reymond said that Helmholtz contributed to all areas of "theoretical natural science" including "the highest, mathematical physics."[39] There have been times in history when "theoretical" had negative connotations such as contrary to reality, but in Germany in the nineteenth century the word found approval in the natural sciences.

The change in terminology corresponded to the increasing work of theory in physics. In an address in 1888, Kundt spoke of this change from the perspective of experimental physics. In the early nineteenth century, theories of the different classes of physical phenomena stood side-by-side, lacking "general, connecting, comprehensive ideas." Then, in the middle of the century, the great law of conservation of energy was announced, and at a stroke, separated phenomena were brought together in a "unity." On the basis of this law, mathematical physics was greatly extended and deepened. In addition, and equally important, at this time the mechanical theory of heat and electrodynamic theory began their strong development. The best physicists began to favor theory to the extent that it seemed to Kundt almost as if experimental research had lost its foundation.[40]

During our period, theoretical physicists believed that mathematics provided only an aid to physical reasoning. After the time of our book, some physicists came to see mathematics not just as an aid to physical research but as an independent guide, a revival of Leibniz's "pre-established harmony" between the physical world and mathematics and mechanics.[41] Dependent on mathematics, physicists have had different ideas about its relationship to physics over time.

1.3 Nature, Aims, and Methods of Theoretical Physics

Theories are important for providing substantiated explanations or descriptions of features of the known physical world and for suggesting experiments to learn more about it. Apart from observational facts, theories and the laws they contain are the most credible form of physical knowledge. In this section we look at major characteristics of physical theories in our period.

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[38] Hermann von Helmholtz, *Vorlesungen über die Theorie der Wärme,* ed. Franz Richarz (Leipzig: J. A. Barth, 1903), 309.

[39] *Physiker über Physiker*, ed. Christa Kirsten and Hans-Günther Körber (Berlin: Akademie-Verlag, 1975), 63.

[40] Kundt, "Antrittsrede," 680.

[41] Lewis Pyenson, *The Young Einstein: The Advent of Relativity* (Bristol and Boston: Adam Hilger, 1985), 28, 73, 139, 153.

Theories

Einstein asked: “What, then, impels us to devise theory after theory? Why do we devise theories at all?” His answer was that “we enjoy ‘comprehending.’” Helmholtz said that the aim of physical sciences is to “understand.”[42] Planck said that the “ideal aim” of the physicist is to “understand the external world of reality.”[43] Throughout our period, a favorite word of German physicists was “dark,” meaning not well understood and needing more work. Comprehending and understanding go to the heart of the desire to do work in theoretical physics, lifting the darkness. The words can apply to nature and to physicists’ representations of nature.

There are different ways of talking about understanding. Psychologically, understanding has to do with a relation between the person and the object of understanding, and both knowing and feeling enter the experience. Usually it is tied to explanation, how something works, but it is not limited to this. In the late nineteenth century, physicists disagreed over whether physics explains or describes. Kirchhoff who believed that physics describes instead of explains associated description with understanding. “It is the task of physics to investigate certain classes of phenomena, the so-called physical phenomena, to order them clearly and to present them as simply as possible. Of all physical phenomena, the simplest, i.e., those that lie closest to the understanding, are the *phenomena of motion*, which comprise the subject of *mechanics*.”[44] On occasion, physicists characterized the motivation to do theoretical research in a way that included understanding the world but went beyond it.[45]

Making Theories

The first stimulus to rebuilding or replacing a theory is commonly a new set of facts. Physicists have to decide what is wrong with the existing theory, and since a theory is constituted of a number of propositions, any one or several of which could be at fault, there usually is more than one way of solving the problem. This can lead

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[42] Helmholtz, *Einleitung … theoretische Physik*, 11. Albert Einstein, “On the Generalized Theory of Gravitation,” in *Ideas and Opinions* (New York: Dell, 1973), 332–46, on 332.

[43] Max Planck, “The Scientist’s Picture of the Physical Universe,” *Where Is Science Going?* trans. J.V. Murphy (New York: W.W. Norton, 1932), 84–106, on 85.

[44] Gustav Kirchhoff, *Vorlesungen über mathematische Physik*, vol. 4, *Vorlesungen über die Theorie der Wärme*, ed. Max Planck (Leipzig, 1894), 1.

[45] When Boltzmann talked about the construction of theory, Lorentz said, “he meant not merely making understandable this or that group of phenomena, but the achievement of an interconnected world- and life-view, with which his physical views were inextricably interwoven.” This thought would have a long history. In his eighties, the modern theoretical physicist John Wheeler wrote that the “pursuit of science is more than the pursuit of understanding. It is driven by the creative urge, the urge to construct a vision, a map, a picture of the world that gives the world a little more beauty and coherence than it had before.” To a degree, Einstein talked that way too, of wanting to make a “simplified and intelligible picture of the world.” H. A. Lorentz, “Ludwig Boltzmann,” *Verh. phys. Ges.* 9 (1907): 206–38, on 206–7. Janna Levin, *Black Hole Blues and Other Songs From Outer Space* (New York: Alfred A. Knopf, 2016), 34. Albert Einstein, “Principles of Research,” 1918, in *Ideas and Opinions* (New York: Dell), 219–22, on 219–20.

to disputes, which may be quickly resolved or may last for years.[46] Planck's law of blackbody radiation in 1900 is an example of a theoretical response to new facts; in this case, the problem was solved in a relatively short time, though the theory was subsequently revised and reinterpreted. Often the stimulus to replace a theory is not a disagreement with experiment but a change in thinking about a research field. What is found intellectually gratifying in a theory is an evolving part of physics. The wave theory of light replaced the ray theory before the latter became irrelevant, and the concept of the electron replaced the concept of charge as a state of the ether before the latter met with experimental difficulty.[47] Another stimulus is the desire to supply an empirical law governing well-known facts with physical foundations; Ohm's theoretical derivation of the law of electric currents is an example. Or the stimulus may be a desire to encompass a wider set of phenomena by generalizing a successful law with the aid of theory; Helmholtz's extension of the law of least action from mechanics to other parts of physics is an example. A law by itself is not considered a theory, but the derivation, re-derivation, or extension of a law usually is.

When a physicist sets out to make a theory, according to Boltzmann, he isolates the phenomena of interest from the rest of nature with the intent of grasping them in their full course.[48] He settles on concepts for expressing the phenomena and for reasoning about their laws, and then he develops the theory and the laws that go with it. He does this with the help of a hypothesis, an unconfirmed explanation of the phenomena, which has a probability of being correct; nineteenth-century physicists regarded the atomistic nature of matter as a hypothesis. The reason why a hypothesis is used, Helmholtz explained, is because sense experience by itself fails to provide precise enough foundations for a theory.[49] A theory may take other forms. It may begin with one or more physical postulates or principles, which are not hypothetical, but are regularities that have been experimentally established or justified by universal observation and experience; Clausius's mechanical theory of heat is based on universal laws of energy. A theory may begin with mathematical laws describing various sets of phenomena; Kirchhoff's theory of heat radiation is derived from optical and thermodynamic laws. A theory may proceed from, or be identified with, a mathematically formulated, idealized model, which obeys the known laws of physics[50]; in our period, a common theory of light made use of a

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[46] Max Planck, "New Paths of Physical Knowledge," 1913, in *A Survey of Physical Theory,* trans. R. Jones and D. H. Williams (New York: Dover Reprint, 1960), 45–55, on 46.

[47] Jed Z. Buchwald, *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century* (Chicago and London: University of Chicago Press, 1989), xxii.

[48] Boltzmann, "Stefan," 94.

[49] Helmholtz, *Einleitung . . . theoretische Physik*, 7, 18.

[50] When working with models, it was considered desirable to have more than one model for the same phenomena. Ludwig Boltzmann, "On the Fundamental Principles and Basic Equations of Mechanics," in *Philosophy of Science*, ed. and trans. J. J. Kockelmans (New York: Free Press, 1968), 246–60, on 258.

model of the ether conceived of as vibrating particles. A theory may be based on an analogy between physical systems or between mathematical expressions of laws of different sets of phenomena; Ohm's theory of the galvanic circuit is based on an analogy between electric current and heat flow. A theory often includes assumptions, which may serve as hypotheses or which may state conditions under which the theory is valid; Einstein's theory of special relativity is valid under the assumption of an inertial frame of reference. In practice, elements of theories, such as concepts, hypotheses, principles, laws, models, analogies, and assumptions, overlap and combine in various ways. The most important of these elements we discuss further below.

Physics cannot go far without the help of theory. The objection that a theory can be false is acknowledged, but even a false theory can be useful if it leads to new experiments.[51] In his text on mechanics, Voigt illustrates the need for theory in the search for natural laws with an example. To arrive at the laws governing the phenomena of frictional fluids, it is unproductive to look to experiments, owing to the "great complications" of the decisive physical relations. Instead it is proper to develop a "theory from a system of assumptions," or probable hypotheses, which are then tested by comparing the consequences of the theory with observation. The assumptions in this case are physical and mathematical: the cause of fluid friction is molecular actions, and the components of the pressure of fluid friction are linear functions of the velocities.[52]

Concepts

According to Helmholtz, concepts are defined in terms of properties such that the objects that fall under them have other properties in common; to be useful, concepts must have these connotations. Because physical sciences deal with changes, the problem is to find concepts with connotations for classifying changes; examples are the mutual acceleration of attracting bodies and the bending of light rays in transparent bodies.[53] Concepts enter physical laws and are correlated with quantitative observations in the laboratory; the "extraordinarily great" number of concepts used in physics during our period mostly fall under a small number of classes of quantities, which are, in order of complexity, scalar, vector, and tensor.[54] Theories also contain concepts that do not correspond to observables such as ether particles and hidden masses.[55] Concepts can be suggested by experience but they cannot be deduced from it.[56]

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[51] Ludwig Boltzmann, "On the Development of the Methods of Theoretical Physics in Recent Times," 1899, in *Philosophical Forum,* trans. Y. Elkana, n.s. 1 (1968–1969): 97–120, on 107.

[52] Woldemar Voigt, *Elementare Mechanik als Einleitung in das Studium der theoretischen Physik*, 2nd rev. ed. (Leipzig: Veit, 1901), 473–74.

[53] Helmholtz, *Einleitung . . . theoretische Physik*, 11.

[54] Voigt, *Elementare Mechanik,* 2–11.

[55] D'Agostino, *History of the Ideas of Theoretical Physics*, 199.

[56] Albert Einstein, "On the Method of Theoretical Physics," in *Ideas and Opinions,* 263–70, on 267.

Hypotheses

A hypothesis serves as a preliminary form of a law or a conjecture, from which conclusions can be deduced with the help of known laws of physics and mathematics. The conclusions need to agree with what is known about the phenomena, and to interest experimenters they should include predictions to test and constants to measure. An agreement with what is known is taken to strengthen the assumptions of the hypothesis, and depending on the number and severity of the experimental tests, the hypothesis may lose its provisional status and be incorporated in the theory or become the theory itself. If a hypothesis conflicts with experiment, it is abandoned as incorrect, and if a theoretical controversy cannot be settled by experiment, a hypothesis may be rejected as simply unfruitful.[57] Most often a hypothesis relates to things for which we have no sense impressions, such as molecules. In his book on the principles of mechanics, Hertz said that the "totality of things visible and tangible do not form an universe conformable to law"; for mechanics to grasp the laws of the universe, it is necessary to form a hypothesis, to imagine "behind the things which we see, other, invisible things—to imagine confederates concealed beyond the limits of our senses."[58] A hypothesis opens up a direction of research that leads to new results. When a hypothesis of Planck's was criticized because he had not shown it to be unique, he replied that this asks too much of a hypothesis: "if one could prove the hypothesis, it would no longer be a hypothesis, and one did not have to formulate it. However, one could then not derive anything new from it."[59] Boltzmann conceded that critics were correct in thinking that poor hypotheses can lead physics in a wrong direction, but this cannot be helped, for theories are made out of "many arbitrary pictures of the connections between phenomena, of so called hypotheses."[60] To propose a good hypothesis that covers a large field of phenomena requires experience, calling on intuition and often conviction. Paul Volkmann thought that most great advances in science leading to new laws were made by scientists who believed in their hypotheses.[61] With the aid of hypotheses, theory assumes a leading role in physical research, though that does not rule out experimental discoveries for which there is no theoretical anticipation.

Laws

A law is something that is always the same. Physicists look for a point of rest among the flux of phenomena, and they abstract from the phenomena the

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[57] Helmholtz, *Einleitung … theoretische Physik*, 18. Max Planck, "The Place of Modern Physics in the Mechanical View of Nature," 1910, in *A Survey of Physical Theory,* 27–44, on 33. An example in our period was the controversy between Franz Neumann and A. J. Fresnel over the relation between the plane of polarization and the vibration of light.

[58] In previous versions of mechanics, force and energy are concealed things; in Hertz's mechanics, concealed things are invisible masses and motions. Hertz, *Principles of Mechanics*, 25.

[59] Max Planck, "On the Theory of the Energy Distribution Law of the Normal Spectrum," 1900, in *The Old Quantum Theory*, ed. D. ter Haar (Oxford: Pergamum Press, 1967), 82–87, on 86.

[60] Boltzmann, "Relations of Applied Mathematics," 593.

[61] Volkmann, *Erkenntnistheoretische Grundzüge*, 62, 114.

unchanging something and isolate it as a summary of phenomena, or law.[62] Helmholtz said that we recognize a law as "lasting," ready at any time to act, as something "powerful" in nature, and that the task of physics is to seek the laws of natural phenomena.[63] He put it more generally in a letter to Hertz: it is the aim of science to "grasp the transitory as the phenomenal manifestation of the intransitory—that is, of Law."[64] Physics cannot be separated from its laws, he said, because for physics to qualify as a science, it must provide an "intellectual grasp of the connection of ideas" and an anticipation of new results, both of which follow from the "*law* of the phenomena."[65] He associated natural laws with the a priori law of causality, which says that all natural phenomena must be comprehensible or, equivalently, that all phenomena must proceed lawfully. If a law is considered to be the cause of the phenomena, it means that the law shows itself "in each case where the conditions for the phenomena are given."[66] Voigt said that the task of the physicist is "to derive the laws of phenomena from elementary facts of experience or assumptions," and if in the process he discovers connections to other areas of phenomena, the whole question is raised to a higher level. It is then a matter for him to find and apply principles that govern different areas of phenomena simultaneously, which once obtained are capable of unifying and simplifying the conception of physical processes.[67] In addition to calling on experiments and hypotheses, physicists derive laws with the aid of other laws, and they rely on still further laws in making predictions from them. Neumann derived his law of electromagnetic induction from several previously established laws. From the law of conservation of energy, Helmholtz made several predictions with the aid of auxiliary laws. Especially useful in deriving new laws are laws that specify the dependence of forces on physical circumstances and laws that are universal, both kinds of which are considered "fundamental laws."[68] Laws have benefits in addition to uniting groups of phenomena. To Weber, laws are a method for determining

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[62] Ibid., 88–89.

[63] Helmholtz, *Einleitung … theoretische Physik*, 14.

[64] The letter was in 1886. Königsberger, *Helmholtz*, 369.

[65] Hermann von Helmholtz, "The Aim and Progress of Physical Science," 1869, in *Popular Lectures on Scientific Subjects*, 363–97, on 370.

[66] Helmholtz, *Einleitung … theoretische Physik*, 14–17.

[67] Woldemar Voigt, "Phänomenologische und atomistiche Betrachtungsweise," in *Physik*, ed. Emil Warburg (Berlin: Weidner, 1915), 714–31, on 715.

[68] Wihelm Weber, "Elektrodynamische Maassbestimmungen. Ueber ein allgemeines Grundgesetz der elektrischen Wirken," *Abh. sachs. Ges. Wiss.* (1846), 211–378; in *Werke*, vol. 3, *Galvanismus und Elektrodynamik, erster Theil*, ed. Heinrich Weber (Berlin, 1893), 25–214, on 113. Gustav Kirchhoff, *Vorlesungen über mathematische Physik*, vol. 4, *Vorlesungen über die Theorie der Wärme,* ed. Max Planck (Leipzig, 1894), 60.

forces quantitatively.[69] To Planck, laws define concepts or refine their definitions.[70] Helmholtz saw laws as the reason we work in science at all; to find a law is to "*understand* phenomena."[71] The nature of the laws of physics can change. This happened during our period: early on, physicists looked for elementary laws of phenomena; later, in some areas they renounced the search for elementary laws and looked for statistical regularities in their place. But whether the laws are "actual natural laws" or "statistical laws," they are subject to the same criterion of choice: the reduction of natural processes to the simplest possible laws.[72] As in the case of hypotheses, there is no method for arriving at laws; they are found by intuition backed by experience.[73]

In addition to deriving laws and developing their consequences, theoretical physicists work with laws in other ways. One way is to compare different laws covering the same phenomena, evaluating them according to criteria such as their agreement or disagreement with general laws. Helmholtz developed a general law of electromagnetic induction for the purpose of comparing and deciding between several competing laws for the phenomena. Another way is to show that independently founded empirical laws are theoretically dependent. Clausius brought together a law for the pressure of vapors and another law for the latent heat of vapors and showed that one law is a necessary consequence of the other.[74] Another way is to develop different forms of the same law, since in different applications one or another form is clearer. Clausius gave different formulations of the second law of thermodynamics mainly for this reason.[75] Another way is to give alternative derivations of the same law. In the belief that there was a defect in the way Ohm derived his law of galvanic circuits, Kirchhoff derived the law differently, changing

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[69] Ibid.

[70] In a letter in 1892, cited in Pyenson, *Young Einstein*, 164.

[71] Helmholtz, "Aim and Progress of Physical Science," 369.

[72] Wilhelm Wien, "Ziele und Methoden der theoretischen Physik," *Jahrbuch der Radioaktivität und Elektronik* 12 (1915): 241–59, on 258.

[73] Albert Einstein, "Principles of Research," 1918, in *Ideas and Opinions*, 219–22, on 221.

[74] Clausius assumed one of the two empirical laws to be true for the purpose of exploring it, showing that it can be generalized to contain the other law, and then he applied his theory to it. The equation he derived suggested experiments that might allow a conclusion to be drawn about the relation between the latent heats of different vapors and their tensions. Rudolph Clausius, "Ueber den theoretischen Zusammenhang zweier empirisch aufgestellter Gesetze über die Spannung und die latente Wärme verschiedener Dämpfe," *Ann.* 82 (1851): 274–79, in A*bhandlungen über die mechanische Wärmetheorie*. *Erste Abtheilung* (Braunschweig, 1864), 119–26, on 122.

[75] Eduard Riecke, "Rudolph Clausius," *Abh. Ges. Wiss. Göttingen* 35 (1888) 1–39, on 10, 14–15.

the physical assumptions. This was very common. Clausius, Boltzmann, Helmholtz, and others derived the second law of thermodynamics, each from a different viewpoint, deepening their understanding.[76]

Postulates and Principles

Einstein grouped the "various kinds of theories" into two basic types. One is a "constructive" theory, which is based on a hypothetical picture of the phenomena and uses a synthetic method. The kinetic theory of gases is an example. The other type is a "principle" theory, which is based on experience and uses an analytic method. Thermodynamics is an example. The advantages of the first type are "completeness, adaptability, and clearness"; the advantages of the second are "logical perfection and security of the foundations," Einstein's preference.[77] By "principle" or "postulate," physicists meant a law that is so comprehensive that it can be taken as a fundamental law for the "whole scientific system of physics or mechanics." For example, according to Hertz, a "principle" of mechanics is a proposition that cannot be based on other mechanical propositions, and the "principles of mechanics" are propositions from which the entire theory of mechanics can be derived by deductive reasoning.[78] "Principle" and "postulate" have a similar meaning; Einstein called his "postulate" of relativity the "principle" of relativity.[79] An alternative word is "axiom." Although a principle must agree with experiment and observation, its accuracy and validity are assumed to extend beyond the limits set by observation and experiment. The principle of energy conservation is confirmed not directly by an experiment but by its consequences throughout nature. In cases where there seems to be a departure from the energy principle, attention is directed to what is overlooked and not to the validity of the law.[80] A general method of theoretical physics is the deduction of conclusions from principles. There is no one method for finding principles; theoretical physicists must "worm" them out of nature, calling on their powers of invention.[81]

Follow-Up Work of Theories

Physicists knew that to establish a theory of physics, they eventually had to reach agreement on a range of matters, which include terminology, definitions, units,

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[76] Their starting points however all involved mechanics. A mechanical derivation is not necessarily the same as a mechanical explanation of the second law, as Helmholtz explained. "Studien zur Statik monocyklischer Systeme," *Sitzber. preuss. Akad.*, pt. 2 (1884): 755–59, on 757.

[77] Albert Einstein, "What is the Theory of Relativity?" 1919, in *Ideas and Opinions*, 222–27, on 223.

[78] Hertz, *Principles of Mechanics*, 3–4.

[79] Albert Einstein, "On the Electrodynamics of Moving Bodies," 1905, http://www.fourmilab.ch/etexts/einstein/specrel/www/, p. 1 (accessed 12 June 2015).

[80] Voigt, *Elementare Mechanik*, 179–80.

[81] Albert Einstein, "Principles of Theoretical Physics," 1914, in *Ideas and Opinions,* 216–19, on 217; "On the Method of Theoretical Physics," 267.

standards, notations, and concepts, and that as their research advanced, this follow-up work had to be renewed.[82] Theoretical physicists had to take a new theory into account in their research and in their teaching. In our period they all wrote textbooks or published their lectures. With the rapid development of physical theory from the middle years of the nineteenth century, the work of clarification and updating was never-ending.

Aims, Tasks

Paul Volkmann said that the task of the theorist is to isolate certain phenomena for study, and the experimentalist then carries through the isolation; the theorist has the further task of reducing the phenomena "in their greatest simplicity to certain principles and laws," and the experimentalist tests the theory and determines the basic constants.[83] To set up measurements in such a way that the disturbances can be ignored or calculated, physicists call on their knowledge of theoretical physics, and Voigt thought that "the task arising out of this for theory is hardly less important for the advance of physics than the deduction of general laws, which one might consider the only task of theory."[84] Helmholtz, Kirchhoff, Planck, Mach, Helm, and others held their own views on the matter, and the same physicists might describe the main task of theoretical physics differently at different times. In this book, we consider them in their proper places. The most commonly stated opinion on the main task of physics is the first one above, the setting out of laws, but theoretical physics cannot be adequately characterized by it.

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[82] As an example, together with the extensive mathematical development of theories of electricity and heat in the middle of the century, a variety of mathematical notations appeared. Clausius called attention to this in his book on heat theory, which contained partial derivatives. If z is a function of x and y, the differential is written $dz = dz/dx \cdot dx + dz/dy \cdot dy$. To make clear that dz/dx is a partial differential quotient, different authors had proposed various notations: (dz/dx), dxz/dx, and $\partial z/\partial x$, which became standard. The special case in which x and y are not independent variables, $dz = dz./dx \cdot dx + dz/dy \cdot dy/dx \cdot dx$, and $dz/dx = dz/dx + dz/dy \cdot dy/dx$. The term dz/dx on the left and right sides of the last equation has different meanings. Several suggestions were made for notations to distinguish the two cases: $1/dx \cdot dz$, $d(z)/dx$, and $\underline{dz}/dx$. (*Abhandlungen über die mechanische Wärmetheorie. Erste Abtheilung*, 2–4). A variety of notations were used in electrical theory. Kirchhoff, for example, used ∂ for a partial derivative, which became standard, while others still wrote d. Weber used dd for the second derivative instead of the more concise and later standard d^2. Franz Neumann used in addition to the then standard derivative and integral signs, d and $\int$, the unfamiliar signs D and S. Gustav Kirchhoff, "Ueber die Anwendbarkeit der Formeln für die Intensitäten der galvanishen Strőme ...," *Ann.* 75 (1848): 189–205, for example on 191; Wilhelm Weber, "Elektrodynamische Maassbestimmungen," *Ann.* 73 (1848): 193–240, for example on 229; and Franz Neumann, *Die mathematischen Gesetze der inducirten elektrischen Ströme*, 1845, ed. Carl Neumann, vol. 10 of Ostwald's Klassiker der exakten Wissenschaften (Leipzig, 1889), 10.

[83] Paul Volkmann, *Erkenntnistheoretische Grundzüge der Naturwissenschaften und ihre Beziehungen zum Geistesleben der Gegenwart. Allgemein wissenschaftliche Vorträge,* 2nd ed. (Leipzig and Berlin: B. G. Teubner, 1910), 91.

[84] Woldemar Voigt, "Der Kampf um die Dezimale in der Physik," *Deutsche Revue* 34 (1909): 71–85, on 73–74.

Beyond ordering phenomena under laws, theories have aims, which relate to the criteria of the satisfactoriness of theories and the coherence of physics. When physicists devise a new theory for facts they cannot explain, their motivation comes from outside the theory itself. Einstein regarded this kind of motive as "so to speak, trivial," compared with a "far subtler motive," the inward "striving toward unification and simplification,"[85] criteria we take up next.

Criterion of Simplicity

Helmholtz agreed with Kirchhoff that the task of physics is to describe phenomena in the simplest way with the qualification that the simplest description can be given only after the laws are formulated.[86] He approved of Maxwell's electromagnetic theory because of its "remarkable formal simplicity."[87] Drude said that theory starts with experience and aims to describe the sum of experience in the simplest way.[88] Physical foundations and fundamentals were expected to be simple. Boltzmann said that the work of the theorist is to construct theories that give the simplest pictures, and that since changes of place are the "simplest phenomena of all, mechanics is the foundation of the total natural science."[89] Ernst Mach said that the goal of physical science is the "*simplest* and *most economical* abstract expression of facts."[90] Not every physicist agreed that researchers set out to describe phenomena simply and economically, and Helmholtz, Boltzmann, and others had individual views on description, but there are plentiful examples that show how simplicity entered theoretical physics as a criterion of choice. Clausius described three variations of his electrodynamic potential law as the "most general form," a "simpler form," and the "simplest and therefore most probable form," choosing the latter.[91] Of three alternative formulations of mechanics, Hertz said, the one he favored had "greater simplicity" on its side.[92] The simplicity of a natural law was thought to be an indicator that the law is "fundamental"; because the potential of

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[85] Einstein, "On the Generalized Theory of Gravitation," 333.

[86] Helmholtz, *Einleitung . . . theoretische Physik*, 13.

[87] Max Planck, "Maxwell's Influence on Theoretical Physics in Germany," in *James Clerk Maxwell: A Commemorative Volume, 1831–1931*, ed. J. J. Thomson (Cambridge: Cambridge University Press, 1931), 45–65, on 59.

[88] Drude, "Theorie," 15.

[89] Ludwig Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, 2 vols. (Leipzig: J. A. Barth, 1897–1904), vol. 1, 1; Boltzmann, "Development of the Methods," 111.

[90] Ernst Mach, "Economical Nature of Physical Inquiry," 1882, in *Popular Scientific Lectures*, trans. T. J. McCormack (Chicago, 1895), 186–213, on 207; Mach, *Die Mechanik in ihrer Entwicklung Historisch-kritisch dargestellt* (Leipzig, 1883), translated from the 2nd German edition of 1889 by T. J. McCormack as *The Science of Mechanics: A Critical and Historical Exposition of Its Principles* (Chicago, 1893).

[91] Clausius, *Die mechanische Behandlung der Electricität,* in chap. 9.

[92] Hertz, *Principles of Mechanics*, 41.

Weber's law of electrodynamic force was "simple" compared to the law of force, he regarded it as more fundamental, and for that reason he chose to work with it.[93] Volkmann recognized the danger of underestimating the complexity of physical relations in theoretical physics, but the history of science showed more examples of the opposite case, in which nature proved to be simpler than was previously thought.[94] To Wien writing shortly after the turn of the twentieth century, nature looked "much more complex and entangled" than previously thought, but that did not affect his agreement with Kirchhoff's goal of the simplest description.[95] It was assumed that physicists would recognize what counted as "simple" and "simplest" in a given case as self-evident.

Criterion of Clarity

Although mechanics was the most thoroughly established theory of physics, it had different formulations, which had different empirical consequences.[96] Much thought was directed to the differences in the late nineteenth century, and the criterion of clarity was commonly uppermost. In his published lectures on mathematical physics in 1876, Kirchhoff said that previous formulations of mechanics used unclear concepts such as forces, which led to all sorts of ambiguities, and that by restricting the task of mechanics to description, he intended to remove the "unclarity."[97] Boltzmann attributed the "unclarities" of mechanics to the practice of developing the theory from experience rather than from pictures in the mind. In his presentation of mechanics, which by its nature is the "clearest natural scientific discipline," he sought the "clearest" mental picture.[98] Hertz's aim in founding mechanics on new principles was to bring "absolute clarity" to mechanics.[99]

Criterion of Wholeness

This criterion relates to the completeness of the coverage of a theory. The object of the theory of mechanics, according to Kirchhoff, is a "complete" description of the phenomena. Einstein's praised Planck's lectures on the theory of heat for bringing together results by different researchers into a "unified whole."[100]

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[93] Wilhelm Weber, "Elektrodynamische Maassbestimmungen insbesondere über das Princip der Erhaltung der Energie," *Abh. sächs. Ges. d. Wiss.* 10 (1871): 1–61; repr. in *Wilhelm Weber's Werke*, vol. 4, *Galvanismus und Elektrodynamik,* zweiter Theil, ed. Heinrich Weber (Berlin, 1894), 243–46 and 247–99, on 254–55.

[94] Volkmann, *Erkenntnistheoretische Grundzüge*, 92. An example of misleading simplicity is the simplicity that Weber's electrodynamic law acquires by assuming that the action between electric particles is along the line joining them. Clausius pointed out that this simplicity is arbitrary and probably wrong.

[95] Wien, "Ziele und Methoden," 247.

[96] Hertz, *Principles of Mechanics*, 40–41.

[97] Gustav Kirchhoff, *Vorlesungen über Mechanik*, 3rd ed. (Leipzig, 1883), v.

[98] Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, vol. 1, 1–2, 6.

[99] Heinrich Hertz to Arthur Meiner, 12 November 1893, Ms. Coll., DM, 3245.

[100] Einstein's reviews in the *Beiblätter* in 1905–1907 are discussed in Martin J. Klein and Allan Needell, "Some Unnoticed Publications by Einstein," *Isis* 68 (1977): 601–4.

Criterion of Logical Soundness

This criterion might seem obvious, but physicists were explicit on the logical requirements of a sound physical theory. Boltzmann spoke of “logical clarity,” Hertz spoke of “logical purity,” Einstein spoke of “logical perfection.”

Criterion of Inner Perfection

Hertz spelled out the criterion of “value” or inner perfection of a theory in his new representation of mechanics, which he called a “picture” of mechanics. To compare his picture with the main alternative pictures, he set out the “standpoints from which we must estimate the value of physical theories and the value of the representations of physical theories.” In addition to agreeing with experience, an admissible picture must not contradict the “laws of our thought,” by which he meant that it must be “logically permissible.” If two pictures agree with the above requirements, the more admissible of the two is the one that is “more distinct,” a criterion often paired with clarity. If two pictures agree with all of the above, the more admissible picture is the one that is “simpler.”[101] The usual pictures of mechanics contained indistinct and ambiguous points, and Hertz thought that a perfected picture of mechanics was needed to develop Maxwell’s electromagnetic theory of the ether, the anticipated next stage of physics. Einstein said that in addition to “external confirmation,” a theory is judged by its “inner perfection,” which refers to the naturalness or logical simplicity of the basic concepts and the relations between them. This criterion comes into play when there are two theories for a phenomenon that are more or less equally correct. Even though the choice between them involves the “weighing of incommensurable qualities,” physicists generally agree about which one is superior, and, to be sure, they agree about what makes a good theory before they agree about experimental facts.[102] This judgment came into play in work in electromagnetism in the second half of the nineteenth century. Boltzmann said that the weakest point of the physics he learned as a student was Wilhelm Weber’s “theory of electrodynamics,” which for “all its cleverness and all its mathematical finesse … carried the stamp of the artificial so that only a very few enthusiastic followers believed in its correctness.” An internal criterion of a good theory is that it appears “natural,” in the order of things, not arbitrary or artificial.[103]

Unities, Generalizations, Connections

In Germany in the nineteenth century, the words “unity,” “unify,” and “unification” had positive connotations, and theoretical physicists used them freely. Their desire for unity in their work was not disturbed by the multiplicity of their everyday researches and if anything was stimulated by it. A general meaning of unity is the bringing together of parts that appear to belong together, and this fits most of the

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[101] Hertz, *Principles of Mechanics*, 2–3.

[102] Albert Einstein, “Autobiographical Notes,” in *Albert Einstein: Philosopher-Scientist*, ed. Paul Arthur Schilpp (Evanston, IL: Library of Living Philosophers, 1949), 1–95, on 23.

[103] Boltzmann, “Development of the Methods,” 102.

physicists'references to it. A specific meaning of "unity" is identity. For example, Kirchhoff's law states that a body absorbs the same light as it gives off, expressing the "unified nature" of spectral lines; dark and bright lines are the same lines.[104] A related meaning is equivalence; Einstein showed that mass and energy are equivalent. Another meaning of "unity" is conceptual. Planck admired Kirchhoff's lectures for their "expressly established unity of conception," characteristic of all of his writings.[105] Another meaning is fusion, the result of bringing two independent theories together under a single theory. An example is Weber's electrodynamic theory, which brings together the theory of electricity at rest and the theory of electricity in motion. A related fusion is the bringing together of two independent laws. Clausius was clear on the importance of this: "a definite unification of the electrodynamic and electrostatic laws would belong to the greatest advances that have been made in physics in a long time."[106] Another meaning is bringing together two sciences in research: an example is the Leipzig faculty's recommendation for a chair of physical chemistry, countering the "*tendency toward splintering* [with] the opposite trend, the *tendency toward unification*."[107] Another meaning is reduction; an example is the derivation of laws throughout physics from the concepts and elementary laws of motion of mechanics. There are variants on this meaning. Helmholtz thought that all of theoretical physics can be developed from the concept of force. The energeticists thought that this could be done using the concept of energy instead. Planck thought that a single formula or universal law would be found to encompass all observations. Unity and simplicity come together in these examples. Physicists believed that by bringing formerly independent bodies of phenomena together within a single theoretical structure, they acquired a better understanding of the physical processes. There may be only one substance, with everything arising from a universal ether, as Hertz suggested. Physical laws are expressed mathematically, a formal kind of unity. A measuring system for all of physics is another kind. A meaning of another kind is a form of social cooperation; an example is the collaboration of theoretical and experimental physics, expressing a "higher unity." Physics was seen as an inherently unifying field among the sciences. Weber said that "physics forms a bond and a point of unification for mathematical and exact experimental research."[108] Another meaning is institutional; an example is the German university, the setting in which most theoretical physics in Germany was carried out. There were still more ways that German physicists spoke of "unity" and "unify," which come up in our book. In some

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[104] Boltzmann, *Kirchhoff*, 9.

[105] Planck's foreword to Gustav Kirchhoff, *Vorlesungen über mathematische Physik,* vol. 3, *Vorlesungen über Elektrictät und Magnetismus*, ed. M. Planck (Leipzig, 1891).

[106] Clausius to Carl Neumann, 1 November 1869, Wiedemann Personalakte, Leipzig UA, Nr. 1061.

[107] Leipzig University Philosophical Faculty to Saxon Ministry of Culture and Public Education, undated draft, ca. 1869, Wiedemann's Personalakte, Leipzig UA, PA 1060, Bl. 5–8.

[108] Weber to Kern, 1 January 1855.

cases, the unification or its meaningfulness may be questioned or reworded, but what the physicists had in mind is usually clear, as is the value they placed on it. In Germany, "unification remained a driving scientific ideal."[109]

German physicists also spoke of "generalization" and "connection," similar in meaning to "unification." Generalization was a common goal and method. Helmholtz said that after discovering laws for individual phenomena, the physicist must seek the "most general laws."[110] The fewer the natural laws needed to reduce all physical phenomena, the more advanced physics was considered to be.[111] In their research, physicists sought laws that apply to a generality of phenomena; after they had derived laws under certain conditions, they often found that the laws are valid for general cases.[112] In carrying through a generalization of a law or theory, they looked for the simplest generalization that agreed with the facts. Another use of the method was to develop a theory by first studying specific examples of a physical process and then generalizing from them; Helmholtz worked this way, for example. "Connection" in physics could mean a connection between phenomena or it could mean a connection between theories or both; for example, theories of electromagnetism and optics are connected through the velocity of light. In this book, we often use the word "connectedness" to cover the meanings of unity, generalization, and connection. Physicists used this word too.

Helmholtz was the most successful pursuer of connections in physics in Germany in the nineteenth century. In his first work in theoretical physics, he derived the law of conservation of energy, generalizing a long-known law of mechanics to apply to other areas of physics and beyond; Planck regarded this work as the first step in a unified system of physics, with energy joining space and time as the only concepts common to all of physics. Helmholtz spoke of a true advance of the "newer physics" in the common representation and measure of work of all forces, a consequence of the energy principle, present whether the forces are mechanical, electrical, chemical, or something else.[113] In a later research, Helmholtz generalized another law taken from mechanics, the least action principle, showing that it applies beyond the motions from which it originated to other parts of physics, pointing the way, Planck said, to a "unified conception of all natural forces." The

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[109] Jordi Cat, "The Unity of Science," *Stanford Encyclopedia of Philosophy* (16 May 2013), http://plato.stanford.edu/entries/scientific-unity (accessed 13 June 2015).

[110] Helmholtz, *Einleitung . . . theoretische Physik*, 20.

[111] This criterion for the progress of physics has a long history. Baumgartner and Ettingshausen, *Die Naturlehre nach ihrem gegenwärtigen Zustande mit Rücksicht auf mathematische Begründung*, 7. A hundred years after Baumgartner and Ettingshausen, Einstein said that the "grand aim of all science . . . is to cover the greatest possible number of empirical facts by logical deduction from the smallest number of hypotheses or axioms." ("The Problem of Space, Ether, and the Field in Physics," 1934, in *Ideas and Opinions*, 270–78, on 275).

[112] Wien, "Ziele und Methoden," 242. Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, vol. 1, 135.

[113] Hermann von Helmholtz, *Vorlesungen über theoretische Physik*, vol. 6, *Vorlesungen über die Theorie der Wärme*, ed. Franz Richarz (Leipzig: J. A. Barth, 1903), 280.

principle is more general and accomplishes more than the conservation of energy, allowing conclusions to be drawn about the particulars of a physical process over time.[114]

After Helmholtz, Planck was the most insistent advocate of this path in physics. In his varied writings, lectures, and addresses, he spoke of the "structure of theoretical physics," "the system of theoretical physics," and always the goal of "unity" of physics, making clear to his audiences that theoretical physics is about connectedness. Theoretical physics is like every science, he said, in that its main goal is to forge its great theories into one single theory in which all of its problems find their place and their solution.[115] On this subject, Einstein's thinking was similar to Planck's. He explained that there have always been two directions of research in physics. One is research in the individual sciences such as optics and electricity, which make up physics, each encompassing a restricted area of phenomena, with its corresponding laws. This direction belongs to the ever increasing specialization of the sciences, and it bestows great benefits on industries. The other is research into a "unifying theoretical basis for all these single sciences, consisting of a minimum of concepts and fundamental relationships," from which all of the concepts and laws of the individual sciences can be deduced. This is what is meant by the search for the "foundation of the whole of physics," the goal responsible for the "passionate devotion which has always animated the researcher.".[116]

The history of physics is rich with examples of partial successes in striving for the goal. Mechanics encompassed elasticity, hydrodynamics, and acoustics; electrodynamics encompassed electricity, magnetism, and optics; and thermodynamics was founded on universal laws [Weltgesetze], which applied throughout physics.[117] Theories of major branches of physics—mechanics, electrodynamics, and energetics—were each tried as a foundation for the whole of physics, with incomplete success. The complete solution to the problem of persisting disunities was regarded by Planck as the unfinished task of theoretical physics.

The connectedness of physics was at odds with its division into partially autonomous branches. In the words of a leading textbook at the end of our period, it was necessary to impose a "principle of division" in order to deal with the enormous body of knowledge. But the parts of physics had "close relations" with

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[114] Wilhelm Wien, "Helmholtz als Physiker," *Naturwiss.* 9 (1921): 694–99, on 694–95; Max Planck, "Helmholtz's Leistungen auf dem Gebiete der theoretischen Physik," *ADB* 51 (1906): 470–72; repr. in *Physikalische Abhandlungen und Vorträge* (Braunschweig: F. Vieweg, 1958), vol. 3, 321–23, on 323.

[115] Max Planck, "Verhältnis der Theorien zueinander," in *Physik,* ed. Emil Warburg (Berlin: Teubner, 1915), 732–37, on 732.

[116] Albert Einstein, "The Fundamentals of Theoretical Physics," 1940, in *Ideas and Opinions*, 315–26, on 316.

[117] Walther Nernst, "Rudolf Clausius 1822–1888," in *150 Jahre Rheinische Friedrich-Wilhelms-Universität zu Bonn 1818–1968. Bonner Gelehrte. Beiträge zur Geschichte der Wissenschaften in Bonn. Mathematik und Naturwissenschaften* (Bonn: H. Bouvier, Ludwig Röhrscheid, 1970), 101–9, on 107–8.

one another, and the retention for teaching purposes of the old division of physics into mechanics, acoustics, optics, heat, electricity, and magnetism was justified on purely "practical grounds."[118] It was not, that is, thought to be based on nature. In presentations of physics in textbooks and lectures, the older practical division persisted, but always with an awareness of the interconnections.

The increasing ties between the branches of physics nourished the belief that a common foundation for physics would one day be settled, strengthening the resolve to keep searching. The search took multiple directions, which approached one another as the goal appeared ever nearer to realization. The importance that physicists placed on connectedness is seen in its use as a criterion of methodology: phenomenology is not a final stage of physical theory, Boltzmann said, because it renounces "any unified vision of nature."[119]

Methods

In science as elsewhere, for example, in technology, business, politics, and everyday life, methods are important, the reason why there are forever arguments over which methods are best. What is evident about methods is that whoever has a good one may well achieve a goal, and whoever does not, may not achieve it.[120] There are two general kinds of methods in science, one of investigation, and one of presentation. This book is concerned with both.

In a lecture in 1933, Einstein said that "if you want to find out anything from theoretical physicists about the methods they use . . . stick closely to one principle: don't listen to their words, fix your attention on the deeds." His point was that physicists commonly say that they base their theories on experience, failing to recognize that this basis has a "fictitious character." He explained that the concepts and principles of physics do not come from experience but are freely invented, an understanding that came only with the general theory of relativity.[121] That theory appears after the period this book treats, but we take Einstein's advice to heart. We look at the published research of the physicists whose methods we discuss.

Mathematical Representation

The wide use of mathematics in physics would seem to validate Kant's dictum that science contains only so much truth as it contains mathematics. In practice, this means that physical ideas that remain qualitative do not lead to uncontested results.[122] Theories that are only qualitative might serve as stimulus to researchers, but as final results of theoretical research they are incomplete.[123]

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[118] *Müller-Pouillet's Lehrbuch der Physik und Meteorologie*, ed. Leopold Pfaundler, 10th rev. ed., vol. 1 (Braunschweig: F. Vieweg, 1905), 10–11.

[119] Boltzmann, "Development of the Methods," 115.

[120] Felix Auerbach, "Vorwort," in *Physik in graphischen Darstellungen* (Leipzig and Berlin: Teubner, 1912); "Vorwort," in *Die graphische Darstellung*, 2nd ed. (Leipzig and Berlin: Teubner, 1918).

[121] Einstein, "On the Method of Theoretical Physics," 266–67.

[122] Auerbach, *Die graphische Darstellung,* 117.

[123] Wien, "Ziele und Methoden," 242.

Physicists published theoretical papers with no mathematics to speak of,[124] but that was rare.

We begin with the presentation of quantities. From the time our study begins, physicists who worked on mathematical theory such as Ohm, Fechner, and Weber, and experimental physicists such as Poggendorff, supported, in Weber's words, the "general striving to determine all natural phenomena by number and measure."[125] There were three methods of presenting number and measure in our period: numerical tables, graphs, and formulas. Columns of numbers were sometimes included in theoretical as well as experimental papers, taken from experimental measurements and placed side-by-side with calculated values for comparison. Toward the end of our period, graphs became common in publications on experimental physics. Felix Auerbach welcomed the introduction of graphical methods for their greater visual quality, an advantage neglected during the "oppressive tyranny" of abstract thought. To show what could be done, in 1912 he published a book on physics with no text but over 200 pages of graphs, followed by a twenty-four-page verbal elaboration of the graphs.[126] This was an *anschaulich* [vivid] tour de force, but it was not the future of physics. The most complete expression of quantities was by formulas, which accounts for the appearance of most publications on theoretical physics: pages containing equations. Quantitative reasoning was valued for its exactitude and for obtaining, as Weber said, "a foundation for theory that is independent of sense perception or mere guessing."[127]

We turn to mathematical methods of investigation. One of Newton's greatest ideas was the "differential law,"[128] which enabled him to derive the paths of bodies moved by forces. Without differential laws, nineteenth century physicists could not have described the course of a physical process, a standard problem. Differential laws have the additional property of freeing natural laws from the accidents of size, form, and location in space and time, which gives them great generality, an advantage. To derive differential laws, theoretical physics pictures bodies as made up either of material points or of vanishly small differential volume elements

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[124] For example, Rudolph Clausius, "Ueber die Elektricitätsleitung in Elektrolyten," *Ann.* 101 (1857): 338–60. In a previous paper, he studied the heat generated by a current in a conductor without electrolysis, showing that the heat follows from Ohm's law and the principle of equivalence of heat and work. In this paper, he considered conduction by electrolysis, extending his previous analysis to this case. He stated the equivalence of the heat and the work done by a current against resistance, and he gave a formula for the work; this is the only mathematics in the paper.

[125] Wilhelm Weber, "Ueber ein allgemeines Grundgesetz der elektrischen Wirkung," *Abh. Sächs. Ges. Wiss.* (1846), 211–378, in *Werke*, vol. 3, *Galvanismus und Elektrodynamik, erster Theil*, ed. Heinrich Weber (Berlin, 1893), 25–214, on 34–35.

[126] Auerbach, "Vorwort,"in *Physik in graphischen Darstellungen*; "Vorwort," in *Die graphische Darstellung,* 9–10. Auerbach said that his book did not replace physics books using the common presentations.

[127] Wilhelm Weber, "Elektrodynamische Maassbestimmungen" (1846), 34–35.

[128] Albert Einstein, "The Mechanics of Newton and Their Influence on the Development of Theoretical Physics," 1931, in *Ideas and Opinions*, 247–55, on 254.

filled with matter, the choice depending on the problem. The actions between material points or volume elements are then summed, or integrated, to represent the bodies of our experience. Helmholtz says that this is the reason why "theoretical physics becomes predominantly mathematical."[129]

In theoretical physics, there are two ways of arriving at differential laws expressing physical regularities. One begins with a mathematical equation describing the outcome of a specific experiment, which is then differentiated with respect to time, resulting in a differential equation expressing a general law. In the other way, the experimental outcome is not described by an equation, and instead a differential equation is derived from an assumption or analogy. This is a hypothetical approach, which Helmholtz likens to taking "steps into the dark." The integral of the differential equation contains constants that do not appear in the differential equation, which match the tentative physical law to a specific experiment. The test is to see if the numbers from the integration agree with the measurements of the experiment.[130] Ohm, as we will see, derived differential laws both ways.

In the mathematical representation of nature, there are analytic requirements on the terms that enter it, which correspond to physical requirements. To describe the motion of a mass point, the position of the point must be defined by continuous, differentiable functions of time; for otherwise the point can be in two places at the same time, a discontinuity which violates the identity and indestructability of the mass point, a "fundamental law of experience of all natural phenomena." To derive the law for the diffusion of heat, for example, the differentials must be mathematically possible, which means that the key quantity in the equations, temperature, must vary continuously. Temperature in nature or in the laboratory may swing widely, but it does so continuously. Discontinuities do occur in the mathematics of classical physics, and Helmholtz shows how to deal with them in his lectures.[131]

Beyond agreeing that the methods of mathematical representation are indispensable to the work of theoretical physics, individual physicists vary in the ways they bring physical and mathematical ideas together. Boltzmann appreciated Kirchhoff's way of working with mathematics, which was to find particular integrals of general differential equations and then to look for their physical significance, but he preferred his own way, which was to look for the physical reasons why certain particular integrals are significant and others are not, offering advantages to the understanding.[132] Kirchhoff and Boltzmann were both doing theoretical physics mathematically, each in his own way.

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[129] The differential equations that theoretical physics normally works with are "linear." Non-linear equations are rarely encountered, and when they are, they are solved only approximately. Helmholtz, *Einleitung … theoretische Physik*, 22–24, 40.

[130] Hermann von Helmholtz, *Vorlesungen über theoretische Physik,* vol. 1, pt. 2, *Vorlesungen über die Dynamik discreter Massenpunkte*, ed. Otto Krigar-Menzel (Leipzig, 1898), 43–44.

[131] Helmholtz, *Einleitung … theoretische Physik*, 7; *Vorlesungen über die Theorie der Wärme*, 91–95.

[132] Ludwig Boltzmann, *Vorlesungen über Maxwells Theorie der Elektricität und des Lichtes*, vol. 2, *Verhältniss zur Fernwirkungstheorie; specielle Fälle der Elektrostatik, stationären Strömung und Induction* (Leipzig, 1893), 42.

Physicists and their colleagues considered mathematics to be the "main tool" of theoretical physics.[133] In lectures on theoretical physics, Planck said that the "material with which theoretical physics operates is measurements, and mathematics is the chief tool with which the material is worked."[134] Modern theoretical physicists speak this way too: mathematics is a "tool for reasoning" about nature, and it is an "extremely useful tool."[135] As one of them writes, "the mathematicians invent these tools and show us how they work and then we drill and plumb with this arsenal of math to understand cosmic phenomena."[136] A historian of science who has studied our subject writes that all physicists in the time this book covers regarded mathematics as only "useful tools for physical research."[137] A tool is an aid in accomplishing a task, which in the case of physics is an aid in comprehending nature.

Physicists occasionally used the expression "mathematical language" of physics, and with a clear understanding. When Einstein used it in 1918, he meant that the physical world picture requires precision, which only mathematics can provide.[138] The language used by theoretical physicists is partly mathematics and partly (mainly) words, which include a technical vocabulary, as a glance at their research papers shows; mathematics by itself would be all but unintelligible. Historians of science know that written language, which can include mathematics, is a "literary technology," a tool. Literary practices as well as social organization and scientific instruments are "*knowledge-producing tools*."[139]

Atomistic Matter

In his lectures on theoretical physics, Helmholtz devoted separate semesters to the two methods of mathematical representation described above, to bodies treated as aggregates of mass points and to bodies treated as aggregates of volume elements filled with matter. The mass point is a useful concept for solving problems in which the actual form and extension of the body can be neglected, as when two bodies

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[133] Lommel and Bauer's successful recommendation of Boltzmann for professor of theoretical physics at Munich. Dean von Baeyer of Section II of the Munich U. Philosophical Faculty to Munich U. Senate, 24 November 1889, Munich UA, E II-N, Boltzmann.

[134] Max Planck, *Eight Lectures on Theoretical Physics Delivered at Columbia University in 1909*, trans. A. P. Wills (New York: Columbia University Press, 1915), 3.

[135] Richard P. Feynman, *Character of Physical Laws* (New York: Modern Library, 1994), 34; Murray Gell-Mann, *The Quark and the Jaguar: Adventures in the Simple and the Complex* (New York: W. H. Freeman, 1994), 108.

[136] Janna Levin, *How the Universe Got Its Spots* (London: Weidenfeld & Nicolson, 2002), 115–16.

[137] Pyenson, *Young Einstein*, 28, 73.

[138] Einstein, "Principles of Research," 221. In addition to supplying precision, mathematics, for example, makes connections between quantities.

[139] Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton, NJ: Princeton University Press, 1985), 25, 77. A dissenting opinion, which we find unpersuasive, is that if mathematics can be seen as the "language" of physics, it cannot be seen as a "tool" of physics, and mathematics then is not a tool of physics. See Garber, *Language of Physics*, 17.

interact over a long distance. The usual mathematical form of laws of atomistic matter is ordinary differential equations. An example is the mathematical description of the motion of a mass point according to Newton's laws of motion together with supplementary assumptions about the acting forces, one of the "first and most important tasks of theoretical physics."[140]

The foundation of the atomistic hypothesis in physics was first indirectly established by the kinetic gas theory, and at the end of our period, the empirical electron and the electron theory reintroduced the atoms of electricity. The difficulties of the atomistic treatment of physical phenomena struck some physcists as too great to accept it as a *general* method of doing physics. Voigt had no objection to atomism, but he thought that the facts did not yet justify its general use, for too little was known about it, and mathematics could give only a rough picture of reality. The atomistic method worked better in some parts of physics such as thermo-elasticity than in others.[141]

Continuous Matter

Helmholtz said that when physicists treat phenomena by mathematically dividing bodies into volume elements that are large compared with molecular separations, they are working within the picture of continuously distributed masses. Volume elements are differentiated not only with respect to time, but also with respect to the three spatial coordinates. The presence of spatial coordinates as variables is the "essential characteristic of continuously distributed masses in contrast to systems of discrete mass points, in which time is the only primitive variable."[142] Because the laws of continuous matter contain several independent variables, they are expressed as partial instead of total differential equations. Helmholtz used the continuum method to develop the theory of deformations, which he applied to a variety of problems: dilation, bending, sheer, pressure, tension, and torsion. Commenting on the two methods, Boltzmann said that the mathematics of mass points "penetrates deeper into the nature of things, and that the mathematics of volume elements is freer of unprovable hypotheses." At the time Boltzmann wrote, in 1888, the two ways were seen to "struggle with one another,"[143] though physicists were accustomed to using both.

Phenomenology

Under the method of phenomenology, a theory is based exclusively on direct observations, and its development is limited to reducing complicated phenomena to

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[140] Helmholtz, *Einleitung ... theoretische Physik*, 22, 38, 41; Helmholtz, *Vorlesungen über die Dynamik discreter Massenpunkte*, ed. Otto Krigar-Menzel (Leipzig, 1898), 2, 7.

[141] Voigt, "Phänomenologische ... Betrachtungsweise," 718–19, 722–23.

[142] Helmholtz, *Vorlesungen über die Dynamik continuirlich verbreiteter Massen,* 8–9.

[143] Boltzmann, *Kirchhoff*, 24.

simple phenomena of the same kind, leading to the formulation of principles.[144] Based on two fundamental laws of experience, thermodynamics is a phenomenological theory. A variation is quasi-molecular, or "differential," phenomenology, described above: from experience with large bodies, a conclusion is drawn about processes in invisibly small elementary "volume parts," which are then summed mathematically to treat finite processes. Phenomenology enters statistical mechanics where it makes statements about the total result of molecular interactions, which can be checked by experience. Electricity and magnetism initially used the atomistic approach but following Maxwell, whose picture of electromagnetism was mechanical but not atomistic, the subjects were treated phenomenologically, and this was seen as the "greatest triumph" of the method. There then occurred "one of the strangest revolutions in physics," Voigt said: the advent of the electron theory brought about a change in fundamental principles, problems, and methods of electromagnetism, in which the totality of results from the phenomenological method were retained but were reinterpreted to admit atoms of electricity. Voigt thought that it was "very strange how an area in which the phenomenological view was first the uncontested ruler could within the span of a few years become a domain of the molecular view." Much the same thing happened in optics, which was originally phenomenological, but later electrons were introduced. Early relativity and quantum theories were "evidently phenomenological," though molecular considerations played a role in both. The phenomenological method did not follow a single path.[145]

Phenomenology had its critics. According to Boltzmann, physics has to go beyond experience, the starting point of phenomenological theory, to arrive at an "overview," leading to discoveries of hidden connections between phenomena. An "extreme" form of phenomenology, which Boltzmann called "mathematical phenomenology," considers it the task of physics to find the simplest equations that agree with experience and to look no further; Boltzmann pointed to Hertz's electrodynamics as an example of this. Boltzmann thought that phenomenologists were mistaken in believing that their method is free of pictures. Upon carefully considering the physical implications of mathematical equations, he concluded that the mathematics of most physical laws, or differential equations, cannot be separated from atomistic concepts. He thought that the best definition of a continuum, the phenomenologists' way around atomism, is the assumption of an increase in numbers of atoms and a decrease in their size.[146]

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[144] Voigt, "Phänomenologische ... Betrachtungsweise," 716. The theoretical physicist Murray Gell-Mann says that when physicists find regularities in phenomena but have no explanation for them, they speak of a "'phenomenological' theory, using fancy words to mean basically that we see what is going on but do not yet understand it." (*The Quark and the Jaguar*, 93). "Dynamical" is another term for the method that avoids mechanical explanations, atoms, and hypotheses. Purrington, *Physics in the Nineteenth Century*, 20.

[145] Voigt, "Phänomenologische . . . Betrachtungsweise," 728–30.

[146] Ludwig Boltzmann, "Über die Entwicklung der Methoden der theoretischen Physik in neuerer Zeit," 1899, in *Populäre Schriften*, 220–23; Boltzmann, "Relations of Applied Mathematics," 596; and Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, vol. 1, 3, 5, 39.

Principles

During our period, several basic principles emerged intact from theoretical disputes and experimental discoveries, gaining strength in the process: conservation of energy, conservation of momentum, principle of least action, and the laws of thermodynamics.[147] The great importance of the conservation of energy is its use as a method. To explain new or obscure phenomena, the universal energy principle is an unfailing guide: the physicist asks, How can the phenomena be explained as a transformation of energy?[148] Energetics, a program based on a version of the energy principle, is likewise a method of research.[149] Helmholtz placed a high value on the universal principle of least action as a method, a "heuristic principle and guide in the attempt to formulate laws for new classes of phenomena": the physicist asks, How can the phenomena be resolved into a distribution of energy over time?[150] Planck made the second law of thermodynamics his main method of solving problems in heat and chemical theory. Around the turn of the twentieth century, through the use of principles as methods, physicists overcame deeply rooted habits of thought to make major theoretical advances.

Analogies, Models

In his lectures, Kirchhoff developed physical analogies between the parts of physics. In mechanics, he pointed out that the physicist works either with distance forces like gravity or with pressures that act by contact. By analogy, in heat theory, he works with heat exchanges between distant bodies by radiation or with heat exchanges between bodies in contact by conduction.[151] Another form of analogy is mathematical. William Thomson and Maxwell noted that the same differential equations, or laws, appear in widely separated parts of physics. For example, the same laws govern heat conduction and the distribution of electricity in conductors; hydrodynamics and potential theory; and elastic-solid theory and electromagnetism. There is evidently a unity to the plan of nature, as scientists had long suspected. Analogies offer physicists considerable freedom, since it is not necessary that analogies fit every fact, whereas with hypotheses everything has to fit. In deriving a theory of electromagnetism, Maxwell used mechanical analogies, which he did not regard as hypotheses, since he recognized that any number of analogies could be made to fit the equations. In his text on Maxwell's theory, Boltzmann used mechanisms that have only a gross analogy with the phenomena rather than hypotheses about fundamental forces. The advantage of mechanisms is that they are clearly

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[147] Planck, "New Paths of Physical Knowledge," 47.

[148] James Clerk Maxwell's article, "Hermann Ludwig Ferdinand Helmholtz," in the "Scientific Worthies" series in *Nature* 15 (1877): 389–91.

[149] Robert J. Deltete, "Helm's History of Energetics: A Reading Guide *to* Georg Helm, in *The Historical Development of Energetics,* by Georg Helm, trans. R. J. Deltete (Dordrecht, Boston, and London: Kluwer Publishers, 2000), 4–45, on 22.

[150] Königsberger, *Helmholtz*, 356.

[151] Gustav Kirchhoff, *Vorlesungen über die Theorie der Wärme*, 7.

defined, well-understood systems, with heuristic value, as Maxwell showed.[152] Ohm at the beginning of our period recognized the value of analogies, as Helmholtz, Kirchhoff, Boltzmann, and others did later. Many German physicists appreciated Maxwell's understanding that physics works with analogies. As with all methods, there were other German physicists who preferred to work without them, regarding them as less secure than other methods.[153] The failure of an analogy can be as significant as its affirmation. Kirchhoff after developing an analogy between forces and exchanges of heat pointed out an important way in which mechanics and heat are not analogous, which he called the "pivot" on which the entire theory of heat rests: a quantity of heat always passes from a warm to a cold body.[154]

With reference to their methods, German physicists occasionally spoke of "models." Hertz said that physicists' "pictures" are "images or symbols of external objects," from which by means of "models" they develop consequences that apply to the external world. Boltzmann discussed Maxwell's "model" of electromagnetism, a theoretical construction of vortices and friction rollers from which he derived equations of great usefulness. Boltzmann called a model a special picture, a mechanical analogy, and a mechanical example.[155] The terminology was fluid at the time.

Comparison

Analogy is one method of comparison. Another is a comparison of different theories, often with the goal of forming a judgment, which could rest on experimental or theoretical grounds. On the basis of several criteria of suitability, mentioned above, Hertz compared different formulations of mechanics based, respectively, on force, energy, and matter and motion, settling on the last one. Helmholtz likewise compared different formulations of mechanics, preferring a different one than Hertz's. Clausius in electrodynamic theory, Kirchhoff in elasticity theory, and other physicists in other parts of physics carried out similar comparisons. Papers on the state of a field presented at German Association meetings had a related purpose, clarifying and ordering areas of research by making comparisons.

Other Methods

A general method was criticism, which often involved comparison. Helmholtz's most important work in electrodynamics had a critical direction. The present time was characterized by an "almost exaggerated criticism of the methods of natural scientific research," Boltzmann observed in 1892.[156] In 1912, Voigt wrote that in

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[152] Ludwig Boltzmann, *Vorlesungen über Maxwells Theorie der Elektricität und des Lichtes,* vol. 1, *Ableitung der Grundgleichungen für ruhende, homogene, isotrope Körper* (Leipzig, 1891), 13.

[153] Emil Warburg, "Antrittsrede," *Sitzungsber. preuss. Akad.*, 1896, 743–45.

[154] Gustav Kirchhoff, *Vorlesungen über mathematische Physik*, vol. 4, *Vorlesungen über die Theorie der Wärme,* ed. Max Planck (Leipzig, 1894), 6.

[155] Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, vol. 1, 1, 41–42; Boltzmann, "Über die Methoden der theoretischen Physik," 1892, in *Populäre Schriften*, 1–10, on 7–8.

[156] Boltzmann, "Über die Methoden der theoretischen Physik," 1.

recent years, the “inexhaustible criticism [of assumptions], which has been given to the previously unshaken principles [of physics], has proven itself surprisingly fruitful.”[157] A related method was interpretation. Finding Maxwell’s form of presentation in his treatise on electromagnetism “dark and *inconsequent*,” Boltzmann published his lectures on Maxwell’s theory as an “interpreter,” who showed the place of the old ideas of physics in Maxwell’s theory.[158] There were many special methods, and frequent new ones. Planck believed that the recent progress of thermodynamics owed to the method of “ideal processes.” This is a category of the more general method of “thought experiments,” which are useful for exploring the implications of a theory, hypothesis, or principle, and for confirming or refuting an existing theory or establishing a new theory. In his lectures, Boltzmann valued “thought pictures” because they express future phenomena as completely and easily as possible, and he showed their usefulness in an analysis of the law of inertia.[159] Einstein used “imaginary physical experiments” in introducing the principle of relativity.[160]

Philosophy and Methods

Philosophy entered the work of physicists, affecting their choice of methods and theories, often implicitly. At the beginning of the nineteenth century, German physicists responded positively to Kant’s epistemology and also to his theory of active powers in matter, which posed an alternative to the Laplacian method of imponderable fluids; and to some extent they continued to follow Kant, though with important disagreements.[161] The later school of Schelling and Hegel asserted a relation between philosophy and natural science, which, in Helmholtz’s view, expounded a worthless method of investigating nature, mistaking “abstractions and grammatical expressions” for reality, and bringing philosophy into “ill repute.” But the repudiation of philosophy had gone “much too far,” he said, and lately out of a need to criticize new methods of research natural scientists had frequently

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[157] Voigt, *Physikalische Forschung*, 11.

[158] Boltzmann, *Vorlesungen über Maxwell’s Theorie*, vol. 1, iii-iv.

[159] Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, vol. 2, 330.

[160] With the help of thought experiments, Einstein explained what is meant by synchronous clocks at different locations. Einstein, “On the Electrodynamics of Moving Bodies,” 3.

[161] Helmholtz and other physicists who in one way or another were followers of Kant would have been familiar with his declaration in the Analytic of his First Critique in 1785 that concepts (organizing structures) without intuitions (sensuous impressions) are empty, and that intuitions without concepts are blind; their characterizations of the relationship between experimental and theoretical physics recall Kant’s teaching. As an example of a disagreement, Wien observed that Kant required that each science be developed logically from unified principles, from which standpoint theoretical physics was “the model of a natural science,” but natural science had moved beyond Kant, insisting on the distinction between deductive and inductive methods (“Ziele und Methoden,” 246). A related example of a disagreement: Helmholtz accepted that Newton’s laws of motion were obtained and tested by experience; and that they were not, as Kant maintained, synthetic a priori judgments arrived at without external experience. *Vorlesungen über die Dynamik discreter Massenpunkte*,.ed. Otto Krigar-Menzel (Leipzig, 1898), 25.

discussed "philosophical questions." Helmholtz began his lectures on theoretical physics with the "logical and epistemological principles of the scientific method of experimental sciences."[162] At the end of our period, Volkmann observed that epistemological studies had multiplied lately, owing largely to the scientific work of Faraday's and Maxwell's, and as examples he referred to writings by Helmholtz, Boltzmann, Hertz, Mach, and Wilhelm Ostwald. He said that of the natural sciences, physics has the greatest epistemological significance for the reason that it has the most thoroughly constructed theories and is least satisfied with mere facts.[163] Boltzmann said that physicists' interest in epistemology arose from the importance of methods of thinking about problems, and that despite "its after-taste of the old, much-despised metaphysics," epistemology is of the "highest importance to science."[164] For theoretical physics as for the other natural sciences, Boltzmann said, it was time for their "union" with philosophy,[165] though the part of philosophy that he and his contemporaries considered important to their work was narrowly circumscribed.

In the epistemological introduction to his book on mechanics, Hertz discussed theories as "pictures," which "draw inferences as to the future from the past," predictions being, in his opinion, the most important activity of natural science.[166] Hertz's association of physical theories with pictures was epistemologically correct, to Boltzmann's way of thinking. There is a human drive to picture the outer world, he explained, and the physicists' "elaborations and constant perfection of this picture is then the chief task of theory." A theory is "purely an inner picture of the outer, physical world," which aims at being as "simple" as possible rather than being "absolutely correct." Hertz had completed Maxwell's direction in physics by bringing "to the physicist's consciousness what the philosophers had said a long time ago, that no theory makes any objective claim, any claim which has to do with reality in nature, but rather that each theory is only a picture of the phenomena, and the relation between the two is like that between a sign and the designated"; for example, like letters to speech or notes to tones. Pictures can be as general as the "world picture," which has the capability of accommodating new facts, a definite advantage, or they can be specialized pictures, which have another advantage, that of clarity and distinctness.[167] Boltzmann developed mechanics using the "method" of "special pictures," which he contrasted with the "general pictures" of energetics and phenomenology, but whether they are special or general, pictures are what

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[162] Helmholtz, *Einleitung … theoretische Physik*, 1–2.

[163] Volkmann, *Erkenntnistheoretische Grundzüge*, iii, ix, 33.

[164] Boltzmann, "Development of the Methods," 98–99.

[165] Boltzmann, "Relations of Applied Mathematics," 603.

[166] D'Agostino, *History of the Ideas of Theoretical Physics*, 198–99; Hertz, *Principles of Mechanics*, 1.

[167] "Signs," Boltzmann thought, is a better word than "pictures." Boltzmann, "Development of the Methods," 103, 110–11; Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, vol. 1, 2; Boltzmann in his 1890 Munich address, quoted in Pyenson, *Young Einstein*, 166.

physics works with: "all conception of lawfulness, all rules to grasp complex phenomena concisely together and by simple formula" rest on the "application of pictures."[168] The claim that the method of mathematical phenomenology does not replace nature by a picture is wrong: numbers and their mathematical relations are also pictures of natural processes, exactly as geometrical relations are pictures of mechanical processes.[169] Newton's concept of central forces acting between particles of matter is a picture, which was successfully extended from gravitation to magnetism and electricity, but in electrodynamics it resulted in elementary laws that looked too complicated to trust, the first indication that Newton's picture is not always the best. Faraday and Maxwell provided the alternative picture of contiguous action, which fits electrodynamics better.[170] To study the second law of thermodynamics, Helmholtz used a picture of cyclical motions without atoms. To study the same law, Boltzmann used the picture of atoms in motion. Although there can be different pictures for the same phenomena, pictures are not arbitrary. In his text on mechanics, Boltzmann said that the mechanical picture he developed there was supported by experiment, showing that he was not merely playing a game with thought-pictures. A justification of physicists' pictures was given by the theoretically knowledgeable experimentalist Emil Warburg. Besides satisfying the human craving for grasping the incomprehensible, he said, pictures are an important aid in acquiring scientific knowledge. They bring physicists closer to the goal of their research, the establishment of connections between the varied sorts of natural phenomena through comprehensive laws. Warburg referred to recent research on electrons, which led to new connections, but this was still only a picture, not to be confused with reality.[171] Not all physicists accepted the idea that theories are pictures or agreed as to whether or not the relation of pictures to reality should be taken into consideration, but pictures had influential followers, who took them as a serious way of looking at physical theory, of value in making connections and predictions. In the late nineteenth century, on the eve of modern physics, theoretical physicists debated a range of philosophical issues: pictures versus materialism, description versus explanation, phenomenology versus atomism, and others.

Method of Measurement

Physicists distinguished between two kinds of laboratory activities, "experiment" and "measurement," and they typed themselves accordingly: August Kundt was the most important German "experimental physicist," Friedrich Kohlrausch the most important "measuring physicist." The two activities relate to theoretical physics somewhat differently. The experimental physicist might explore unknown

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[168] Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, vol. 1, 3, 42.

[169] Boltzmann, "Development of the Methods," 116.

[170] Volkmann, *Erkenntnistheoretische Grundzüge*, 67.

[171] Emil Warburg, *Ueber die kinetische Theorie der Gase* (Berlin: Lange, 1900), 6–7. Paul Volkmann, *Einführung in das Studium der theoretischen Physik insbesondere in das der analytischen Mechanik mit einer Einleitung in die Theorie der physikalischen Erkenntniss* (Leipzig, B. G. Teubner, 1900), vi–vii, 7–8.

territory, in which case his work precedes a completed theory. The work of the measuring physicist too could precede theory and lead to discovery—the measurements that Kirchhoff and Bunsen made of spectra were recognized as an instance of this way of discovery—but usually measurement follows theory; it is no less valued for this reason, for the detailed knowledge of nature it provides makes accessible parts of the world where the unaided senses are helpless. Universal constants and functions such as the velocity of light and the energy distribution of blackbody radiation are the most important objects of measurement. Other measurements are concerned with arbitrary standards such as units of current and resistance and with constants and functions characteristic of particular substances such as the electrical conductivity of iron. Theoretical physicists such as Kirchhoff and Voigt who had studied at Königsberg carried into practice their teacher Neumann's idea of the work of theoretical physics, which includes making measurements in the laboratory.

Instruments used in measuring could affect the direction of theoretical research in physics. An example of this was Wilhelm Weber's electrodynamometer, modeled after Gauss's bifilar magnetometer, a precision instrument invented to introduce absolute measures into physics. This instrument enabled Weber to confirm Ampère's electrodynamic law, in turn allowing him to work out an electrodynamic theory containing a fundamental law. His collaborater Gauss looked to measurements to clarify and guide the progress of physical theories, and in anticipation he requested a magnetic observatory, where observations and experiments on theories of electric currents and magnetism could be carried out with precision instruments.

Patterns of Research

German physicists typically brought out their work in a given direction of research as a series of papers. An early instance of this is Weber's work appearing under the series title "Electrodynamic Measure Determinations," beginning in 1846 and continuing through most of his career. A later instance is Planck's series appearing under the titles "On the Principle of the Increase of Entropy" and "On Irreversible Radiation Processes," appearing in 1887–1900. Theory enters Weber's and Planck's series in different ways. In Weber's, it is subordinate to the main purpose of the series, which is the determination of electrodynamic measures. The series contains theoretical work, but the series would not be characterized primarily as theoretical physics. In Planck's series, theory has no purpose beyond itself and its comparison with experiment, and in this respect they are typical of series by other theoretical physicists we meet in this book. Kirchhoff did not give titles to his several series of researches, but they are easily identified: they consist of five or six papers each on subjects such as Ohm's law, elastic bodies, and hydrodynamics. Helmholtz's papers form series on subjects similar to Kirchhoff's, with additional series on monocyclic systems, least action principle, and meteorology. Sometimes papers in a given series are consecutive and compact, but often they are intermixed with papers on other subjects and spread over a longer time. Individual papers within any series may vary in method and emphasis: some may be purely theoretical with or without a discussion of experiments, and others may be partly or even wholly experimental. They may vary in their place of publication too, some appearing in the *Annalen*, others in an academy proceedings, and still others in journals for mathematics and mathematical physics. The individual papers of a series have interconnections, and the series in its entirety and in its parts are directed to an object of work in theoretical physics, the comprehension of a certain range of phenomena. Experimental researches often form series too, and they may be intermixed with theoretical researches. An example we meet in this book is Hertz's experimental confirmation of Maxwell's electromagnetic theory of light and his reformulation of the theory.[172] It was not unheard of for a theoretical physicist to publish a paper on pure mathematics—Helmholtz published one on non-Euclidean geometry and Boltzmann published one on the integration of partial differential equations—but it was rare and it was related to his other scientific work.

1.4 Methods, Laws, and the Development of Theoretical Physics

Physical theory from the past remained usable until it was rejected by experiment or supplanted by other work, perhaps having undergone reinterpretation in the meantime. Volkmann began his "introduction to the study of theoretical physics" with Newton's mechanics from the seventeenth century, which he considered unsurpassed, free of the uncertainties of other forms of mechanics, as pointed out by Kirchhoff and Hertz; and in addition it had epistemological as well as scientific advantages over the alternatives. Helmholtz likewise began his research and his lectures with Newton's mechanics, and he said of Newton's theory of gravitation that it embraced in "a single principle of great simplicity" a mass of facts with an agreement between theory and facts that "has never been accomplished in any other department of science, either before or since."[173] Writing about the methods of theoretical physics, Boltzmann placed the "beginning of theoretical physics" with Galileo and Newton. By assuming a mathematical law of attractive force between any two material points and by applying the laws of motion, Newton reduced "all phenomena of the heavenly bodies, of gravity, of the tides, to one and the same law,"[174] a model of what the right combination of observation, theory, law, and method could accomplish, bringing unity to a vast field of diverse phenomena. Like his colleagues, Wien thought that the most significant accomplishment of

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[172] Hertz brought together the papers of the series in a book, *Electric Waves,* which includes two theoretical papers he wrote after completing the experiments, clarifying Maxwell's theory. The experiments are exploratory even as they are guided by theoretical expectations. Because of their methods, the experimental and theoretical papers are usually discussed separately.

[173] Volkmann, *Einfuhrung*, iii-vi; Helmholtz, "Aim and Progress of Physical Science," 372–73.

[174] Boltzmann, "Development of the Methods," 100.

theoretical physics to the present day was Newton's theoretical mechanics and law of gravitation.[175] Einstein said that the first attempt at a "uniform theoretical foundation" was Newton's.[176] The term "Newtonian" does not apply to the concepts, principles, and world picture that entered physics in the nineteenth century—energy conservation, thermodynamics, and the electromagnetic field have no place in Newton's physics[177]—but Wien, Boltzmann, and others were looking at something narrower, methods and results, an interest appropriate to working physicists at any time. In their discussion of theoretical physics, they ignored many changes in physics over time, but in the context in which they compared the past with the present, they were right.

The century following Newton, the eighteenth century, saw commendable work in theoretical physics. Henry Cavendish, a physical scientist we have studied,[178] is an example; the term "theoretical physics" was not used then, nor in Britain was the term "physics," but that does not change what the work was. In a classification of papers appearing in the *Philosophical Transactions of the Royal Society of London*, Cavendish's mathematical theory of electricity was placed under "Electricity," which is where it belongs, not under "Mathematics." His theory makes use of the calculus, but that is not why it is theoretical physics. The reason is that from a mathematical-physical hypothesis, by strict reasoning he developed consequences that agreed with all known experiments and also new consequences that could be tested by new kinds of experiments, which he performed. Joseph Larmor, a twentieth-century theoretical physicist and one of the editors of Cavendish's papers, wrote that if Cavendish "had no other claim to renown he would be entitled to rank high among the theoretical physicists of his period."[179]

We begin our account of theoretical research with the mathematical theory of electric currents by Georg Simon Ohm in 1827 and follow it with mathematical theories by Gauss, Weber, Neumann, Helmholtz, Clausius, and Kirchhoff in the 1830s and 1840s. The theories yielded laws that were basic to the development of physics through much or all of the century. If in some ways their publications have a different look than Einstein's 60 years later, it is not because they were not doing mathematical-theoretical work in physics. It is because in the meantime new major laws and theories had entered physics, new instruments and new kinds of measurements had been invented, and lessons had been learned about how to bring theoretical and experimental work together. In his time, Ohm was nearly unique

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[175] Wien, "Ziele und Methoden," 245.

[176] Einstein, "Fundamentals of Theoretical Physics," 317.

[177] Peter Harman, *Energy, Force, and Matter: The Conceptual Development of Nineteenth-Century Physics* (Cambridge: Cambridge University Press, 1982), 10–11.

[178] Christa Jungnickel and Russell McCormmach, *Cavendish, the Experimental Life*, 2nd ed. (Edition Open Access: Max Planck Institute for the History of Science, 2016), http://www.edition-open-access.de (accessed 27 September 2016); Russell McCormmach, *Speculative Truth: Henry Cavendish, Natural Philosophy, and the Rise of Modern Theoretical Science* (Oxford, New York, and Auckland: Oxford University Press, 2004).

[179] Joseph Larmor, in Henry Cavendish, *The Scientific Papers of the Honourable Henry Cavendish*, vol. 2, ed. E. Thorpe (Cambridge: Cambridge University Press, 1921), 399.

among German physicists in the mathematical knowledge he combined with experimental skill; by the time of Einstein, there were many physicists who were well trained in mathematics and comfortable using mathematical methods in their theoretical and experimental work.

Writing at the end of the nineteenth century, Boltzmann said that physics and natural science in general owed their recent great advances to the development of the method of research, and he outlined the history of theoretical physics as an "illumination" of method. The great Paris mathematicians at the time of the French Revolution and into the nineteenth century created a "sharply defined method of theoretical physics." They thought that the physical assumptions on which they formed their theories were probably true, but because they lacked certainty they began with what they called "hypotheses." They worked with a clear physical image, which Boltzmann elsewhere called a "theoretical picture." They represented ponderable matter and imponderable fluids as a summation of mathematical mass points mutually connected through central forces, and they thought of the ether of light similarly. Boltzmann recalled that at the beginning of his studies in physics, the representation and the accompanying method were much the same, only the imponderable fluids for heat, light, magnetism, and electricity had been replaced by a smaller number, the ether of light and the electric fluid. These bodies together with ponderable matter were assumed to be made up of atoms that interact through forces, and the task of physics was to find the laws of these actions and then apply them. Because of its successes, the method encouraged the belief that the task of physics lay in "explaining" physical phenomena. Around this time, Maxwell and Clausius made use of the atomistic hypothesis to develop the kinetic theory of gases. In other areas, physicists developed theories from axioms considered as exhaustively confirmed rather than from atomistic hypotheses. This was the phenomenological method in theoretical physics, which Boltzmann called "Euclidean" for its similarity to the geometry that Euclid derived from axioms. Kirchhoff's appeal to simple description in mechanics belonged to this direction, which influenced the newer branches of physics, especially electromagnetism. Hertz's development of equations for electromagnetism from the simplest mathematical assumptions was an example of phenomenology after Kirchhoff. In the second half of the nineteenth century, German physicists followed theoretical work coming out of Britain. In contrast to the work of the French in the early nineteenth century, in which atomistic force centers were taken to be real and distance forces were taken to be mathematical aids, in Britain Faraday's forces acting between neighboring particles in space were taken to be real. To represent Faraday's view of physical processes mathematically, Maxwell introduced an incompressible fluid and an elastic solid to form analogies with electromagnetism. His success there and elsewhere persuaded Boltzmann that analogy was the new method of theoretical physics, to which the immediate future belonged. Since his beginning days in physics, Boltzmann said, there had been a "revolution" in methods.[180]

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[180] Boltzmann, "Development of the Methods," 99–102; Boltzmann, "Über die Methoden der theoretischen Physik," 4–10.

Boltzmann had a distinct idea of the way physics advances. The methods of theoretical physics, he said, develop over time discontinuously, like styles in music, poetry, and painting.[181] Physicists who have had success with one method think that it will last forever, but after a time they find that the method has exhausted its possibilities, and a new one takes its place, bringing on a contest between the followers of the old and the new. Wien said that there is no general answer to the question, What are the methods of theoretical physics? and theoretical physicists can never rest with the thought that they have arrived at the final methods of research. This is because methods correspond to the state of science, and a new state may call for the invention of new methods. There is no way of predicting what method will be next.[182]

We have dwelt on Boltzmann's review of theoretical physics for several reasons. The first is our focus, the *work* of physicists who developed theories. This was Boltzmann's focus too: he looked at the history of his field as a researcher, as one who works with methods. Second, Boltzmann gave an overview of an evolving, on the whole productive activity in physics. He viewed the methods introduced from France at the beginning of the nineteenth century, the methods developed in Germany in the middle of the century, and the methods later introduced from Britain as distinct methods of theoretical physics, each having strengths and limitations, and each a part of a single history of making mathematical-physical theories.[183] Third, because of who Boltzmann was and what he did, his perspective on the work of theoretical physics carries an authority, and we give it appropriate weight. Lest this section mislead the reader, we acknowledge that the history of physics in Germany in the nineteenth century is more than the history of its methods, important as they were. The next chapter prepares for the proper history of work in physics by presenting its setting in German universities and by introducing men who taught and did research in the early nineteenth century.

1.5 German Physicists on Their Work

Power of the Intellect

German physicists regarded their work as "intellectual." In his research, Helmholtz likened himself to a mountain climber who does not know the way, who slowly climbs, often reversing, and who by reflection and chance finds new ways forward; when he finally sees the royal way it is too late to help, but in his publications Helmholtz gives only the royal way. Climbing a mountain is a

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[181] Boltzmann, "Development of the Methods," 99–100.

[182] Wien, "Ziele und Methoden," 249.

[183] Like theoretical physics, history has coexisting methods, which complement one another and sometimes conflict with one another. Boltzmann admired Darwin's theory of evolution, and his approach to the history of physics would seem to reflect this.

conquest, and in characterizing their intellectual work, theoretical physicists used the imagery and vocabulary of conquest. Voigt used the expression "intellectual mastery of nature" in describing the extension of electrodynamic formulas to all of electricity and optics, and Helmholtz used the expression in his address at his jubilee. He elaborated: as early as his student years in the gymnasium, he recognized that nature could be "brought under the dominion of a mentally apprehended law," leading him ultimately to his "special line of study," physics. Throughout his career he had a desire to "dominate" nature, he said, to have intellectual mastery over it.[184] Boltzmann made a comparison with the mastery of nations: "If a nation achieves great success relative to its neighbors, it acquires a certain hegemony over them, indeed not infrequently subjugates and uses them. It is exactly the same with scientific disciplines. Mechanics soon acquired hegemony over all physics."[185] During the first world war, Wien said that to an outside observer, today's theoretical physics looks to be in chaos, a "field of rubble" of destroyed theories in which only an occasional stump of an old theory remains standing, but that is only a first impression. He compared theoretical physics with a country that is a colonial power going through a time of very distant frontiers, where everything is still unsettled.[186] Later he likened physics to war. There are "scientific battles" not of one man against another but of the human spirit for scientific understanding. As in a real war, the wastefulness is enormous as one idea after another is thrown into the battle and is lost. The ground is conquered, bit by bit, but not without retreats, and the battle never ends, for there is always the next attack.[187]

Material Power

Parallel to intellectual conquest, there is material conquest, equally celebrated, though it was not welcomed everywhere at the beginning. During the time of educational reform in the early nineteenth century, practical education was seen to conflict with the ideal of *Wissenschaft* and with the closely related movement of nature philosophy, and it was rejected as incompatible with the ends of the university; as a result technical education was largely removed to special institutions for the purpose, prototypes of the later *Technische Hochschulen*.[188] Most of the physicists in this study believed that the work of the university professor is basic research, and that this is the best way to make not only scientific but also material progress. Helmholtz explained in his lectures on theoretical physics that "our whole mastery over nature and natural forces, as it has developed in the last century,

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[184] Hermann von Helmholtz, "Autobiographical Sketch," in *Popular Lectures on Scientific Subjects*, 2nd ed., 1st ser. (1881), 272, 282.

[185] Ludwig Boltzmann, "Antritts-Vorlesung. Gehalten in Leipzig in November 1900," in "Zwei Antrittsreden," *Phys. Zs.* 4 (1902–1903): 247–56, on 248.

[186] Wien, "Ziele und Methoden," 248–49.

[187] Wilhelm Wien, *Vergangenheit, Gegenwart und Zukunft der Physik. Rede gehalten beim Stiftungsfest der Universität München am 19. Juni 1926* (Munich: Max Hueber, 1926), 17–18.

[188] R. Steven Turner, "The Growth of Professorial Research in Prussia, 1818 to 1848–Causes and Context," *HSPS* 3 (1971): 137–82, on 147–48.

follows from the knowledge of laws," which is the "only gift of prophecy that is given to man," and by means of this gift we induce "natural forces to work for us after our will and wishes."[189] In an address on the relations between the sciences, he said that the natural and moral sciences have different methods, but their common name, "sciences," arises from their common aim, which is "to establish the supremacy of intelligence over the world," with the understanding that "knowledge is power." For this reason, a nation is interested in both kinds of sciences, natural and moral, and in their applications, and any nation that does not support them will fail in the "race." Scientists work as an "organized army, laboring on behalf of the whole nation" and at its expense, confident that their discoveries will find practical use. But they should not be concerned with this in their work; they should seek nothing but truth, and if they do not, rest assured, they "will seek in vain."[190] Helmholtz took on large organizational tasks as head of the imperial standards bureau, the Physikalisch-Technische Reichsanstalt, which assisted the technology of physical instruments, but he did not give up his research in basic physics or his lectures on theoretical physics. Planck said that the "technology of today would be impossible without the aid of theoretical physics," and he brought up "electrotechnics" and "aerial navigation,"[191] but in his own research he kept to basic science. Volkmann said that natural scientists are not concerned with technology, for to them nature is the inexhaustible subject of research. With approval, he quoted his teacher Neumann's appeal to the minister of culture for a laboratory for mathematical physics: without its aid, Neumann said, "the instruction in physics must more and more fall into the hands of the technical institutions, and the ideal pure scientific direction of physical study, which has arisen originally out of the German universities, will seek another foreign home." In a talk on the history of physics in Germany, Voigt said that he "deeply and joyfully" admired the rise of technology in Germany, on which the position of Germany in the world depends, but he thought that it is best for universities to stick with their task of advancing basic science and to remain separate from the technical institutes. In almost all historical cases, useful scientific discovery did not arise from practical need, and as evidence Voigt gave examples of the application of Rőntgen rays and of electrical oscillations in medicine and telegraphy. First seek truth and knowledge, which demand "selflessness and devotion," he said; applications to life are sufficiently alluring that "researches on them will arise by themselves every time." To make the point, he paraphrased the bible, "First strive for the kingdom of God and his righteousness, then you will receive everything." In his inaugural address to the Prussian Academy of Sciences in 1874, the inventor and industrialist Werner Siemens presented another perspective on science brought about by the "enormous development of technology and its influence on culture." He said that it is the duty

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[189] Helmholtz, *Einleitung … theoretische Physik*, 21.

[190] Hermann von Helmholtz, "On the Relation of Natural Science to General Science," 1862, in *Popular Lectures on Scientific Subjects*, 1–33, on 26–28.

[191] Planck, *Eight Lectures on Theoretical Physics*, 2.

of science to connect its research with practical life, for "it does not exist for its own sake for the satisfaction of the scientific drive of the limited number of its followers .... Its task is to raise the knowledge and ability of the whole humanity and lead it thereby to a higher level of culture."[192] There were physicists who acted on this understanding. Although he was a theoretical physicist, Clausius taught technical physics in a technical institute, and he published on the theory of the electric dynamo machine.

Analogy to Art

Theoretical physicists were more inclined to recognize a kinship with art than with technology. Several of those we discuss in this book—Helmholtz, Voigt, Boltzmann, Planck, and Einstein, among others—took an active interest in music, some of them holding musical gatherings at their houses. Boltzmann received music lessons from Bruckner, and he held weekly chamber music sessions at home, at which he played the piano. He also wrote about the great Viennese composers, Beethoven being his favorite.[193] He compared mathematics to music: just as the musician immediately recognizes Mozart, Beethoven, and Schubert, the mathematician recognizes Cauchy, Gauss, Jacobi, and Helmholtz. He maintained that the "mathematician among all artists comes closest to the world-creator," and he said that anyone who doubts that mathematics can be beautiful should read Kirchhoff's publication on absorption and emission of radiation and the section of his mechanics lectures on hydrodynamics.[194] Brought up in a musical family, Voigt had a strong musical gift, and early in life he considered devoting himself wholly to music. As a soldier in the field, he played the organ in any church he came across. At Königsberg, he formed a choir to perform Bach church cantatas, later publishing a book on the subject, and at Göttingen he organized musical performances in his house for musical friends.[195] He thought that the exact scientist and the artist are alike in that in both, imagination plays a large role.[196] Dependent on imagination, inspiration, and opportunity, scientific advance cannot be forced, Wien said; "discoveries cannot be commanded any more than poetry, and are always the gift of happy circumstance."[197] Hertz said that only another scientist could appreciate Helmholtz's stature, and if one had the scientist's feeling, even in examining one of

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[192] Neumann and Siemens are quoted in Volkmann, *Erkenntnistheoretische Grundzüge*, 4, 284–86. Neumann's appeal to the minster was around 1876. Voigt, *Physikalische Forschung*, 17.

[193] Engelbert Broda, *Ludwig Boltzmann. Mensch, Physiker, Philosoph* (Vienna: F. Deuticke, 1955), 15; Woldemar Voigt, "Ludwig Boltzmann," *Gött. Nachr.* (1907): 69–82, on 81.

[194] Boltzmann, *Kirchhoff*, 29–30.

[195] Carl Runge, "Woldemar Voigt." *Gött. Nachr.* (1920): 46–52, on 47.

[196] W. Voigt, "Ueber Arbeitshypothesen," *Gött Nachr.* (1905): 99–115, on 102–3. He said that most of the advances in theoretical physics had been achieved with the help of hypothetical mechanisms for the phenomena, requiring extensive use of the imagination.

[197] Wilhelm Wien, *Die neuere Entwickelung unserer Universitäten und ihre Stellung im deutschen Geistesleben* (Würzburg, 1915), 12.

Helmholtz's minor papers, he "feels the same elevation and wonder as in beholding a pure work of art."[198] Einstein said that the natural scientist is like the painter and the poet and that each in his own way makes a cosmos in order to find the "peace and security which he cannot find in the narrow whirlpool of personal experience."[199] The parallelism between science and art could be carried only so far. In a talk on Goethe's scientific researches on light, Helmholtz distinguished the work of the natural scientist from that of the poet. To explain a natural phenomenon in physical science is to trace it to the forces causing it, and since the natural scientist can never know the forces in themselves, but only their effects, he must be content with an abstract conception, leaving the realm of the senses. The poet by contrast looks directly at his subject and is content to record his intuition without entering into the steps that led him to it. He succeeds according to the vividness of his intuition. The natural scientist discovers the "levers, the cords, and the pulleys which work behind the scenes," machinery fatal to the beautiful effects of the poet.[200] In a talk on the kinetic theory of gases, the Berlin experimental physics professor Emil Warburg characterized theories in physics as "one of the highest expressions of human art." He saw them as standing in a direct line with earlier tales of the creation of the world, "as they are given in great simplicity from God-forgiven natures as means for quieting the yearning" for answers. In a critical review of Warburg's talk, H. T. Simon said that most physicists would claim that their theories were more than mythologies, the latter being the inventions of uncontrolled imagination, whereas theories are inventions of conscious experience.[201]

Abstractions and Physical Pictures

Experiments usually involve measurement, and theory usually is about laws, which are abstract and are expressed in abstract concepts such as force. In much of the work of theoretical physics, the presence of imagery is sparing. The explicit physical images that theoretical physicists worked with were generally minimal and quite simple. For much of his career, Weber worked with a theory of electrodynamics based on a picture of electric particles of two signs, which move in opposite directions when a current flows. Over a series of researches, including the introduction of quantum theory, Planck worked with a picture of elementary electrical oscillators interacting with an electromagnetic field. In his extended researches on the theory of heat, Boltzmann worked with a picture of a container holding gas particles, which collide with one another and the walls. In contrast to the simplicity

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[198] Heinrich Hertz, "Hermann von Helmholtz," in supplement to *Münchener Allgemeine Zeitung*, 31 August 1891; repr. in *Miscellaneous Papers*, trans. D. E. Jones and G. A. Schott (London, 1896), 332–40, on 334.

[199] Einstein, "Principles of Research," 220.

[200] Hermann von Helmholtz, "On Goethe's Scientific Researches," 1853, in *Popular Scientific Lectures*, 1st ser. (1881), 1–21, on 15–16, 20.

[201] Warburg, *Ueber die kinetische Theorie der Gase*, 5–6. H. T. Simon, review in *Phys. Zs.* 4 (1902): 151.

of the physical image, the theoretical development often introduced great mathematical complexity. For example, the mathematical difficulties of characterizing the unique stationary state which a gas attains over time were formidable,[202] and Boltzmann's lengthy calculations gave his writings an abstract character.

Kirchhoff defined the task of mechanics as description, but this is not thought of as a description, say, like that of the Earth, a physical picture, but as mathematical relationships that answer any question about the motion of bodies.[203] Helmholtz based his study of the second law of thermodynamics on mechanical systems that depend on a single parameter, known as "monocyclic systems," to which no particular mechanism attaches, and it was the same with his work on a generalized theory of forces and a generalized Hamilton's principle of least action. With regard to the latter, Helmholtz said that a "law which is to comprise the total sum of alterations in Nature must necessarily deal with concepts of the most abstract kind, from which everything has been eliminated that refers to the particular properties of the natural bodies known to us . . . [Concepts] which, when any one hears them defined for the first time, shall evoke no previous concepts or experiences,—that is, in popular parlance, make him think of nothing."[204] Regarding Maxwell's electromagnetic theory as Maxwell's equations, Hertz said that "if we wish to lend more colour to the theory, there is nothing to prevent us from supplementing all this and aiding our powers of imagination by concrete representations of the various conceptions as to the nature of electric polarization, the electric current, etc. But scientific accuracy requires of us that we should in no wise confuse the simple and homely figure, as it is presented to us by nature, with the gay garment which we use to clothe it."[205] Planck spoke of the drabness of the abstract physical picture of nature, which he acknowledged detracts from the value of physics, but like his colleagues he was resigned to it as the cost of progress.[206] Wien observed that whereas the concepts of masses moving under forces of the old mechanics present a definite picture, the electric and magnetic field of Maxwell's theory is "incomparably more abstract," and with the theory of relativity, the "abstraction goes much further." He thought that the "establishing of functional connections is the real and exclusive task of theoretical physics."[207] At the end of our period, the mathematical elaboration of theories was extensive and the ether was a doubtful concept, and with these developments the presence of pictorial imagery receded further.

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[202] Theodore Des Coudres, "Ludwig Boltzmann," *Verh. sächs. Ges. Wiss.* 85 (1906): 615–27, on 623–24; Voigt, "Boltzmann," 72.

[203] Wien, "Ziele und Methoden," 247.

[204] Königsberger, *Helmholtz*, 350.

[205] Hertz, *Electric Waves*, 28.

[206] Max Planck, "The Unity of the Physical Universe," 1908, in *Survey of Physical Theory*, 1–26, on 20. Around this time, Planck anticipated that the coming physical world picture would be built from absolute constants and universal principles, promising nothing pictorial.

[207] Wien, "Ziele und Methoden," 246–47.

Thinking

Thinking is not ordinarily considered a method, but thinking is a part of all methods. According to a study in the psychology of science, scientific thinking requires "developing and testing mental models of how the world works," and this involves making observations, recognizing patterns, drawing analogies, making mental pictures, testing hypotheses, seeing causal connections, and making theories. Models are visual or abstract representations of the external world, which explain or describe its physical features by organizing and interpreting sense experience. The most important thinking in theoretical physics is causal and analogical, and the intellectual traits it calls for are the ability to think abstractly, ability to solve problems, good memory and capacity to learn, mental quickness, mathematical talent, imaginative thinking, flexibility of thought, originality, and ability to associate ideas across boundaries and categories.[208] How well these traits describe the thinking of physicists in the past is a matter of judgment, though from what we know about Helmholtz and Einstein, it would seem that in varying degrees they showed all of the traits. Both men left accounts of aspects of their thinking in physics, which belong to their methods of work. As a participant in a psychological study of mathematical invention, Einstein reported that the psychical elements of his thought were not words but certain signs and images, which he voluntarily summoned and combined, and there were muscular elements as well. The combinatorial work, which he considered the essence of productive thought, was followed by the work of connecting the elements to logical concepts and eventually to words and other kinds of signs, allowing his thinking to be communicated as physical results.[209] Not an intuitive genius, Helmholtz's method of thinking about physical problems was to develop generalizations from concrete examples, and by persevering he often arrived at profound solutions. Usually his good ideas came after a period of rest, often on waking in the morning after "long preliminary work." They often occurred to him without his realizing their importance or their origin, but at other times they arrived "like an inspiration." Once his mind had hold of them, he turned the problem he was working on over and over, dealing with its complications before writing anything down. When the ideas came, he counted himself lucky. When they did not come, only a severe headache could free him from gnawing at the problem.[210] Both Einstein and Helmholtz compared thinking in physics to climbing a mountain, hard work with an uncertain outcome.

German Physics

Taking pride in advances of physics in their native land, German physicists saw themselves as contributing to the general upswing of Germany in the late nineteenth century. As to whether or not they thought that their work had national

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[208] Gregory J. Feist, *The Psychology of Science and the Origin of the Scientific Mind* (New Haven, CT, and London: Yale University Press, 2006), 41, 54, 83–84, 86–87, 91–95.

[209] Albert Einstein, "Letter to Jacques Hadamard," in *The Creative Process: A Symposium*, ed. Brewster Ghiselin (New York: Mentor Books, 1952), 43–44.

[210] Königsberger, *Helmholtz*, 208; Helmholtz, "Autobiographical Sketch," 282.

characteristics, we find opinions, but no persisting theme. Helmholtz extolled the freedom of learning and teaching together with the value placed on research in German universities, a state of affairs he judged superior to what was found in the tight, exam-ridden universities in France and Britain.[211] Wiedemann, Helmholtz's contemporary, said that to the first great fundamental principle of nature, the conservation of matter, which Lavoisier had established, Helmholtz added a second fundamental principle, the conservation of energy. The first principle rested on experience; the second was more speculative, though not in the way of nature philosophy. Wiedemann thought that the two principles arose from the special talents and ways of thinking of the French and German, respectively.[212] Voigt observed that Boltzmann's work on gas theory was accepted in Britain before it was in Germany, a difference he attributed to German physicists avoiding mathematically difficult publications and German mathematicians having little interest.[213] Volkmann agreed with Pierre Duhem's national typology, which associated the deductive spirit with the French and the inductive spirit with the British. Volkmann thought that the "German spirit is perhaps especially capable of placing in the right relation both forms of thought."[214] In a comparison of national traits of physics to national traits of music, Boltzmann said that the French were elegant and the British were dramatic, and that the Germans could be represented by Kirchhoff, whom Boltzmann likened to Beethoven.[215]

Individual Physicists

Whatever generalities can be made about a distinctive German way of doing physics, in theoretical physics the differences between individual German physicists are more significant.[216] Einstein said of physicists like Planck that "most of them are somewhat odd, uncommunicative, solitary fellows, really less like each other, in spite of these common characteristics, than the hosts" of the other types of scientists.[217] This observation agrees with what Helmholtz said: if you compare the work of two contemporary investigators in closely related branches of science, you will "generally be able to convince yourself that the more distinguished the men are, the more clearly does their individuality come out, and the less qualified will

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[211] Hermann von Helmholtz, "On Academic Freedom," in *Popular Lectures on Scientific Subjects*, 2nd ser. (1881), 237–65, on 238.

[212] Gustav Wiedemann, "Hermann von Helmholtz' wissenschaftliche Abhandlungen," in Helmholtz, *Wissenschaftliche Abhandlungen*, 3 vols. (Leipzig, 1882–95), vol. 3, xi-xxxvi, on xxvi.

[213] Voigt, "Boltzmann," 76.

[214] Volkmann, *Erkenntnistheoretische Grundzüge*, 108.

[215] Boltzmann, *Kirchhoff*, 29–30.

[216] A special case is German theoretical physicists trained by Neumann at Königsberg, who had a strong sense of belonging to a school and who in their work tended to follow Neumann's example in research and teaching.

[217] Einstein, "Principles of Research," 220.

either of them be to carry on the other's researches."[218] In no other science or human activity is the personality of greater significance than it is it in theoretical physics, Helmholtz's former student Wien said. That is because the theoretical physicist uses the imaginative powers of his mind in forming concepts and hypotheses; in the initial stages of this work, he gives free reign to these powers, which are kept from arbitrariness by the need for his work to connect quantitatively with observations.[219] The strong element of personality in the physicist's research contrasts with the impersonality of physical knowledge; there has been a "real obliteration of personality," Planck said, as physics abandoned earlier concepts based on sense perception.[220]

Confident Science

With its general methods, principles, and encompassing theories, physics was a relatively confident science at the close of our period, and this was so even as its fundamentals were critically debated. Physicists thought of their science as first among the sciences, in agreement with Helmholtz, who said that it "forms the theoretical basis of all the other branches of Natural Science."[221] In addition, they could share the confidence generally of the natural sciences, which, as Volkmann put it, were the "driving force" of the intellectual life of the time.[222] In an address on the history of German physics in 1912, Voigt said that "in physics our time is a great time, and it is a joy to experience it."[223] In his inaugural address as rector of Berlin University in 1913, Planck said that despite appearances—everywhere "old ideas, firmly rooted, are being displaced, generally accepted theorems are being cast aside and new hypotheses taking their place"—on closer inspection we see not "destruction," but "perfection and extension," and the foundations of physics are as "fixed and immutable" as they have ever been.[224] In 1915, Wien spoke of the "now mighty theoretical physics."[225] Taking justified pride in their field, physicists were also conscious of the inherent limitations of their knowledge. Boltzmann said that physics would be complete if physicists found "formulas through which one could calculate and predict exactly and completely all the expected phenomena in every specific case, uniquely and unambiguously," but this is an unattainable ideal.[226] Einstein said that physicists renounce the completeness of the physical world picture, and that this is because of their own limitation, not because of any limitation of physics: the general laws of theoretical physics make it possible in

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[218] Helmholtz, "Relation of Natural Science to General Science," 11–12.

[219] Wien, "Ziele und Methoden," 243.

[220] Planck, *Eight Lectures on Theoretical Physics,* 6.

[221] Helmholtz, "On Academic Freedom in German Universities," 238.

[222] Volkmann, *Erkenntnistheoretische Grundzüge*, 13.

[223] Voigt, *Physikalische Forschung*, 22.

[224] Planck, "New Paths of Physical Knowledge," 45.

[225] Wien, "Ziele und Methoden," 259.

[226] Boltzmann, "Development of the Methods," 115.

principle to arrive at the theory of “every natural process,” except that it is “far beyond the capacity of the human intellect.”[227] Wien acknowledged that physicists’ knowledge of nature is only approximate, and their calculations are only approximations.[228] To the question of what lies at the bottom of physics, there is no complete answer, Planck said.[229] Conceding what they could not do, by the end of our period theoretical physicists had shown that what they could do, they did very well indeed.

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[227] Einstein, “Principles of Theoretical Physics,” 221.

[228] Wien, “Ziele und Methoden,” 242.

[229] Planck, “Place of Modern Physics in the Mechanical View of Nature,” 3.

Chapter 2
Establishing Physics at the Universities

The first important physical theory in Germany in the nineteenth century was Georg Simon Ohm's theory of galvanic currents, which he arrived at with the aid of a modest physics laboratory and, as he said, the "torch of mathematics."[1] The laboratory was in the secondary school where Ohm taught, and the mathematics was from the best French writings of the day. He began his research with experiments on galvanic currents, publishing his results in a journal devoted primarily to chemistry and only secondarily to physics. He followed this with a theoretically complete work in the form of a small book, which was reviewed by a mathematician for a general science journal and by a teacher of geography and meteorology for a literary journal. The only physicist to review it in print did so for polemical purposes in an ill-regarded science journal that propounded a certain philosophical direction. The reception of the theory did not discourage Ohm, who looked for an improved position where he would have access to publications in his field and, above all, to physical apparatus. He tried and failed to get a position at academies, universities, polytechnic schools, gymnasiums, and other secondary schools. Eventually he did get an improved teaching position, and he went on to become director of a polytechnic school and, toward the end of his life, professor at a university. His research, other than demonstrating the "literary" activity expected of a teacher who wanted to advance, had little to do with his early failure or with his later success in obtaining higher teaching positions. When it came to appointments as university

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[1] Georg Simon Ohm, *Aus Georg Simon Ohms handschriftlichem Nachlass. Briefe, Urkunden und Dokumente,* ed. Ludwig Hartmann (Munich: Bayerland-Verlag, 1927), 71. The expression Ohm used was familiar. For example, F. A. C. Gren said of J. T. Mayer, who stood out among German physicists at the time for using calculus in his work, that he "seeks to enlighten a dark field of physics by the torch of mathematics." (Review of Mayer's *Ueber die Gesetze und Modificationen des Wärmestoffs* [Erlangen, 1791], in Gren's *Journal der Physik* 4 [1791]: 146).

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_2

professors of physics, researchers were treated no differently than writers of works that incorporated no research of their own. Ohm's introduction of a new direction for German research in physics went almost unnoticed, since everything else about him was so usual.

But that is not how his story is traditionally told. The circumstances of the publication and reception of Ohm's early work and his long struggle for a university appointment have been portrayed as an exceptional case of neglect by German physicists and by government officials who had the power to place research scientists in German universities, where they presumably belonged. The story assumes that from the earliest decades of the nineteenth century, research in the natural sciences was supported in German universities as it later came to be, and that German physicists of an earlier period acted in accordance with the view of physical research that was only established after Ohm's publication. When we compare Ohm's career with the careers of his contemporaries, we find that his experience reflects the conditions under which scientists generally worked at the time. For this reason, Ohm makes an instructive introduction to our study.

We see Ohm's career in its proper light when we recognize that for the first decades of the nineteenth century, physics, like the other natural sciences, was principally two things: it was an elementary subject taught at universities and secondary schools as part of a general education, and it was a field of research open to persons with private means for maintaining and equipping their laboratories. As a result, physics was represented by persons in a variety of positions and presented through a variety of publications. The advent of significant German mathematical theory in physics in the early nineteenth century in the form of Ohm's work, irregular as it appears to us today, occurred under what was then perfectly normal circumstances.

In Ohm's day, Germany was not one state but many separate states. Born in 1789, Ohm spent most of his youth in his hometown of Erlangen. At that time, Erlangen belonged to a Franconian principality, but at the time when Ohm was a student at the local gymnasium, Erlangen was Prussian, and when he graduated from the local university, it was Bavarian. Ohm's early years coincided with the Napoleonic wars, when German frontiers changed frequently. The map of Germany was redrawn as the French extended their influence and rule, and it was redrawn again after the French were defeated. The settlement at the Congress of Vienna in 1815 left Europe with thirty-nine German states consisting of thirty-five monarchies and four free cities. Prussia and Austria were the largest and most powerful, the rest varying greatly in size. Together they constituted Germany or, to observe the distinction we make in this study, Germany and Austria. For purposes of security, the German states formed the German Confederation, but each state retained sovereignty over its internal affairs such as the administration of any universities it might have.

2.1 Ideal of *Bildung* and Tasks of the Philosophical Faculty

The political changes at the beginning of the nineteenth century stimulated hopes and ideas for reform in many areas of public life in Germany, including its universities. But the new purposes envisioned by reformers for the universities, such as research, did not then supersede or even equal in importance their established function, which remained the training of professionals needed by the German states: physicians, lawyers, and government officials who generally had to have a legal education, and of clerics and teachers, the two often being one and the same.[2] To meet this function, a university existed in every German state that could afford one. The larger southern German states maintained more than one, and Prussia had six universities by 1818. Altogether, there were nineteen German universities at the time. Despite their different origins and state affiliations, they were all constituted in much the same way: each had "faculties," or professional schools, for medicine, law, and theology. In addition, each had a fourth faculty, the philosophical faculty, which represented the humanities, mathematics, and some of the natural sciences, and which supported the other three faculties by providing general education.

The several faculties of German universities were constituted of "ordinary" professors, who received salaries from the state to teach their fields and fees from students in their courses. Universities had other teachers who were not members of the faculties and who generally did not receive salaries but only student fees. They were the "extraordinary" professors and, below them, the lecturers known as Privatdocenten. These were the three ranks of university teachers, which in practice were varied to suit local needs and circumstances.

The philosophical faculty allowed German universities to present themselves as institutions of "pure science," their practical function notwithstanding, and to claim superiority over "mere" professional schools or specialized institutions such as the military and engineering schools in France, a form of which was then beginning to be established in the German states. According to this ideal view of its purpose, the philosophical faculty continued on a higher level the task of the humanistic German secondary school, the gymnasium, of giving students *Bildung*. This consisted not only of basic knowledge but also of methods of inquiry, enabling the student to put all of his intellectual and emotional capacities to their best use, while ennobling his character and refining his taste. Including as it did all of the humanities and most of the natural sciences, the philosophical faculty offered the student an opportunity to devote, in the educator Wilhelm von Humboldt's words, "a number of years exclusively to scientific contemplation," enabling him to "grasp the unity of knowledge.".[3]

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[2] Rudolph Wagner, "Schriften über Universitäten. Dritte Artikel," *Gelehrte Anzeigen* 3 (1836): cols. 993–97, 1001–6, 1013–16.

[3] Gerhardt Giese, *Quellen zur deutschen Schulgeschichte seit 1800*, Quellensammlung zur Kulturgeschichte, ed. Wilhelm Treue (Göttingen: Musterschmidt-Verlag, 1961), vol. 15, 66.

Bildung, it was thought, was best acquired through studies of classical antiquity. It became so closely linked to classical philology that other interpretations of it were for a long time generally unacceptable, even though, inevitably, they were proposed. “For now the principle still rules everywhere that there is only one way to higher education [Geistesbildung], namely, through the most thorough knowledge of the two ancient languages,” Ernst Gottfried Fischer, a secondary school teacher of physics in Berlin, wrote with disapproval. “These take up by far the greatest part of the hours,” an imbalance that Fischer’s teaching and textbooks were meant to correct.[4] The natural scientists objected to a *Bildung* that looked to past cultures for the model of the present. The means by which this *Bildung* was to be achieved—despite loftier intentions, they usually amounted to incessant drilling in the classical languages—were antithetical to learning the modes of thought of the natural sciences.

Proper science instruction required learning by thinking and doing for oneself, which Ohm believed was equally a means of acquiring *Bildung*. When correctly taught, mathematics demanded the student’s own inner striving and not merely his receptivity to things presented to him, an interpretation of *Bildung* that Ohm introduced in his book on the teaching of geometry. In this text, which was scorned and then ignored, he said that such a *Bildung* would lead to a “new world . . . formed by active reason in man.” What Ohm promised to follow from his teaching methods must have seemed undesirable to the authoritarian governments from whom he sought employment, as it did to the humanists at gymnasiums, with their “exclusively aesthetic interest” in human achievement.[5] The more successful scientists appealed to the interests of their governments rather than to those of their fellow teachers, claiming usefulness for their subjects rather than the advancement of *Bildung*. The study of the natural sciences was now generally considered “extraordinarily important and useful as preparatory education,” Georg Wilhelm Muncke wrote in 1817 to promote the establishment of a state-supported cabinet of physical instruments and apparatus at Heidelberg University.[6]

The little contribution physics was allowed to make to higher *Bildung* was principally through the traditional course of lectures. Fischer recommended “for a fairly complete presentation of physics . . . at least a one-year course, of four to 6 h weekly.”[7] Another contribution was made through the systematic display of

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[4] Ernst Gottfried Fischer, *Lehrbuch der mechanischen Naturlehre*, 3rd ed., 2 vols. (Berlin and Leipzig, 1826–1827), vol. 1, xix.

[5] Heinrich von Füchtbauer, *Georg Simon Ohm; ein Forscher wächst aus seiner Väter Art*, 2nd ed. (Bonn: Ferdinand Dümmler, 1947), 115; Ohm, *Nachlass,* 37, 65–72.

[6] Muncke to Baden Ministry of the Interior, 1 June 1817, Bad. GLA, 235/3057. The usefulness of mathematics to civil servants and school teachers was a reason for proposing a mathematical seminar at Heidelberg in 1824. Heidelberg U. Curator Froehlich to Baden Ministry for the Interior, 23 February 1824, Bad. GLA, 235/3228.

[7] Fischer, *Lehrbuch*, vol. 1, xx.

physical instruments, commonly located in the "academic museum."[8] But actively engaging the student in learning physics did not fit the traditional idea of *Bildung*, as Wilhelm Weber discovered when he introduced experimental exercises to students of medicine, pharmacy, and chemistry and to students preparing to become secondary school teachers of mathematics.[9] Students tended not to avail themselves of the opportunity,[10] and their major professors, especially those in the medical faculty, did not encourage them.[11]

If the ideal task of the closely connected gymnasiums and philosophical faculties did not promote the intensive study of physics, neither did their real task of providing general education. Beginning university students as a rule took most of their lecture courses in the philosophical faculty. They were encouraged to do so, indeed required to do so in Bavaria,[12] because of the one-sided education they had received at the gymnasium, leaving them inadequately prepared for professional studies, particularly ones requiring a knowledge of mathematics or the natural sciences. Under the best circumstances, they had had only 2 h per week of instruction in the natural sciences, which included a good deal of natural history and geography, and 6 h per week in mathematics. A professor of mathematics at Leipzig University reported that he taught students who had never before had any mathematics.[13] The curator at Heidelberg University reported that otherwise well-educated young men lacked even an elementary knowledge of mathematics because of the superficiality and indifference of mathematical instruction in the secondary schools.[14] A faculty report on the state of physics at Tübingen University listed the

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[8] This was a traditional use of the physical instruments. For accounts of arrangements for the practice, see Weber to Göttingen U. Curator, 24 April 1832, Göttingen UA, 4/Vh/15; Otto Lehmann, "Geschichte des physikalischen Instituts der technischen Hochschule Karlsruhe," in *Festgabe zum Jubiläum der vierzigjährigen Regierung Seiner Königlichen Hoheit des Grossherzogs Friedrich von Baden* (Karlsruhe, 1892), 207–65, on 240; Academic Consistory to Pfaff, 25 October 1842, LA Schleswig-Holstein, Abt. 47, Nr. 1235; instructions to Heidelberg U. dated 4 August 1847, Bad. GLA, 235/352; *Handbuch der Architektur*, pt. 4, sect. 6, no. 2al, ed. H. Eggert, C. Junk, C. Körner, and E. Schmitt, 2nd ed. (Stuttgart: A. Kröner, 1905), 194–95.

[9] Weber to Göttingen U. Curator, 24 April 1832, Göttingen UA, 4/V h/15.

[10] Weber to Göttingen U. Curator, 10 May 1851, Göttingen UA, 4/V h/21.

[11] The Göttingen medical faculty prevented Weber's "Praktikum" from becoming as effective as it could have been by refusing his participation in the physics examination of medical students. Exchange of letters between Weber and Göttingen U. Curator, and Medical Faculty and Curator in 1848, Weber Personalakte, Göttingen UA, 4/V b/95a.

[12] E. K. J. von Siebold, review of A. F. Ringelmann's *Beyträge zur Geschichte der Universität Würzburg in den letzten zehn Jahren* (Würzburg, 1835), in *Göttingische gelehrte Anzeigen*, 1836, 65–73, on 67.

[13] Moritz Wilhelm Drobisch, *Philologie und Mathematik als Gegenstände des Gymnasialunterrichts betrachtet, mit besonderer Beziehung aus Sachsens Gelehrtenschulen* (Leipzig, 1832), 71.

[14] His purpose was to propose a mathematics seminar (at the request of the mathematics professor, F. Schweins). Heidelberg U. Curator Froehlich to Baden Ministry of the Interior, 23 February 1824, Bad. GLA, 235/3228.

"lack of the necessary preliminary knowledge," especially in mathematics, as one of the reasons why students did not profit from their physics lectures and why they did not attend the mathematics lectures.[15] When the physics professor at Freiburg University tried to remedy the problem by offering a course on the differential calculus or analytical geometry, no students attended. To have students, the professor of mathematics at Bonn University devoted his lectures to elementary instruction, to arithmetic, Euclidean geometry, algebra, and trigonometry.[16]

Professors of physics who wrote textbooks to accompany their lectures regularly had to exclude mathematics, or at least the calculus and more advanced mathematics, because their students were unprepared. Heinrich Wilhelm Brandes's published lectures on physics were meant for readers who, like his students at Leipzig University, had no knowledge of mathematics.[17] Johan Tobias Mayer's, C. W. G. Kastner's, and G. G. Schmidt's textbooks on experimental physics could be studied with little mathematical preparation.[18] When Kastner warned his readers that if they came to his lectures at Heidelberg University they would encounter equations and calculations, even there the mathematical demands were minimal; what he meant by mathematics as an "indispensable auxiliary study for the sciences and especially the physicist" was "at least arithmetic and geometry." Georg Friedrich Parrot, professor at Dorpat University, and Fischer both made a strong case for mathematics in their textbooks on theoretical and mechanical physics. Parrot recommended his mode of presentation for its "rigorous mathematical demonstrations," and Fischer described his subject as "almost completely mathematical,"[19] but neither author used the calculus, since they could not expect beginning students to know it. The chief task of physics professors was to give general introductory

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[15] Report by Minister of the Interior and of Church and School Affairs von Otto, 16 April 1821, Kabinettsakten, HSTA, Stuttgart, E11 Bü 52.

[16] Debate by Bonn U. Philosophical Faculty, 16 October 1827, Plücker Personalakte, Bonn UA. *Vorlesungen auf der Königlich Preussischen Rhein-Universität Bonn.* The professor was Ludwig August Seeber. Helmuth Gericke, *Zur Geschichte der Mathematik an der Universität Freiburg i. Br.* (Freiburg i. Br.: E. Albert, 1955), 51.

[17] Heinrich Wilhelm Brandes, *Vorlesungen über die Naturlehre zur Belehrung derer, denen es an mathematischen Vorkenntnissen fehlt* (Leipzig, 1830–1832), vol. 1, iii.

[18] Johann Tobias Mayer, *Anfangsgründe der Naturlehre zum Behuf der Vorlesungen über die Experimental-Physik,* 2nd rev. ed. (Göttingen, 1805), vii; C. W. G. Kastner, *Grundriss der Experimentalphysik* (Heidelberg, 1810), vol. 1, viii, 58–59; Georg Gottlieb Schmidt, *Handbuch der Naturlehre zum Gebrauche für Vorlesungen*, 2nd rev. ed. (Giesen, 1813), iv.

[19] Georg Friedrich Parrot, *Grundriss der theoretischen Physik zum Gebrauche für Vorlesungen* (Riga and Leipzig, 1809–1815), pt. 1, xv; Fischer, *Lehrbuch*, vol. 1, 3–4.

lectures, which were no more than surveys of the state of physical knowledge. For advanced courses on special areas of physics there were almost no students.[20]

Despite the nimbus of ideal learning it lent the German universities as a whole, the philosophical faculty was not thought to be as important as the three professional faculties. In some circles the philosophical faculty was considered not to be above a secondary school, and physics and mathematics teachers, like their colleagues in the other natural sciences and the humanities, moved easily between the two kinds of institutions, and not just in one direction; to support themselves and perhaps acquire some apparatus they often held positions simultaneously in both.[21] Ohm applied repeatedly for a simultaneous appointment to a position at a secondary school and at the local university.[22] How little teachers and ministry officials distinguished between the philosophical faculties of universities and secondary schools can also be seen in their attitude toward the assignment of teaching equipment. The philosophical faculties and the teachers at nearby secondary schools saw each other as having much the same task with regard to science

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[20] For example, at Göttingen University in 1810 C. L. Gerling found that although fifty to seventy students were interested in elementary lectures on the mathematical sciences, lectures on higher mechanics attracted only seven or eight. Gauss offered no advanced lectures at all unless two or three students expressed interest. Johann Friedrich Pfaff, *Sammlung von Briefen gewechselt zwischen Johann Friedrich Pfaff und Herzog Carl von Würtemberg, F. Bouterwek, A. v. Humboldt, A. G. Kästner und Anderen,* ed. Carl Pfaff (Leipzig, 1853), 275–76. Julius Plücker's ability to attract even a few students to his mathematical physics courses at Bonn University was taken as proof of his teaching ability, recommending him for promotion to extraordinary professor. Philosophical Faculty debate, and Philosophical Faculty to a Prussian government representative at Bonn U., 16 October 1827, Plücker Personalakte, Bonn UA. See also our discussion of this problem in chap. 5 in connection with Franz Neumann and other later physicists.

[21] See, for example, the application for Weber's professorship at Göttingen University by Lambert, a gymnasium teacher from Wetzlar, dated 27 January 1838, in J. B. Listing Personalakte, Göttingen UA, 4/V b/108. Another example is the list of candidates in the report "Die Wiederbesetzung der Lehrstelle der Physik zu Heidelberg betrf.," 7 December 1816, Bad. GLA, 235/3135. Altogether, during the first half of the nineteenth century, twenty-five university professors teaching physics at German universities—among them the physics chairholders Paul Erman (Berlin), K. D. M. Stahl and Thaddeus Siber (Munich), J. S. C. Schweigger (Halle), G. F. Wucherer, L. A . Seeber, and Johann Müller (Freiburg), C. L. Gerling and Rudolph Kohlrausch (Marburg), Julius Plücker (Bonn), Karl Snell (Jena), Eduard Reusch (Tübingen), and Wilhelm Hankel (Leipzig)—taught at secondary schools at some time during their careers, several of them after they had already held university positions. Other sources of income were positions at technical schools and at military schools, then of a lower rank than universities. Ten university teachers of physics—C. F. von Pfleiderer, Paul Erman, J. G. C. Nörrenberg, Georg Simon Ohm, L. A. Seeber, Heinrich Wilhelm Dove, August Seebeck, Hermann Karsten, Rudolph Clausius, and Wilhelm Beetz—simultaneously taught at military schools. Nine—G. F. Wucherer, Georg Simon Ohm, Heinrich Buff, August Seebeck, J. B. Listing, Wilhelm Beetz, Rudolph Clausius, Adolph Wüllner, and Eugen Lommel—moved from trade and technical schools to their university professorships.

[22] Ohm, *Nachlass*, 137–38, 153, 155, 158–59.

instruction, and they coveted each other's experimental apparatus. For Ohm it was not unreasonable to fear that the new university at Bonn might claim the collection of physical apparatus at the gymnasium in Cologne, just as it was not unreasonable for some gymnasiums in Baden to claim shared use of the physical apparatus of Heidelberg University.[23]

2.2 Tasks and Trials of Physics Professors

Because of their similar tasks of providing elementary or technical education in both secondary schools and philosophical faculties, German professors in the early nineteenth century were primarily teachers, not specialists in a particular subject. The list of candidates for the physics chair at Heidelberg University in 1816 shows the range of qualifications that applied: it names four university professors, an independent researcher who had the reputation of being an "extremely good" experimenter, an assistant at the Munich Academy of Sciences, two gymnasium teachers with good reputations as physicists, a private teacher, and a city clerk and magistrate. About the city clerk and magistrate, the faculty reported that he had studied both physics and law and now wanted the Heidelberg physics chair, even though he had never taught before and had "not yet distinguished himself through any writings."[24]

University professors could be called on to teach a variety of subjects, which were often unrelated to their special interest, as the history of the professorship of physics at Würzburg University shows. The first professor of experimental physics there in 1749 was a teacher of mathematics; he was succeeded in turn by a theologian, a physiologist, a teacher of philosophy, a physician, a professor of chemistry and pharmacy, and, at last, a physicist who, as luck would have it, was still not properly matched, for he was one of the first purely theoretical physicists, Rudolph Clausius.[25] Würzburg was typical in this respect. C. L. Gerling, professor

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[23] Ohm was concerned because he had just been appointed to the gymnasium in Cologne when Bonn University was being set up in 1817 (Ohm, *Nachlass*, 49). Heidelberg University at first agreed to share its apparatus with the secondary schools in Heidelberg, but when the secondary schools in other towns in Baden made similar demands, it rejected all sharing, pointing out that it needed the apparatus for its own lectures on physics and applied mathematics, and that frequent transportation would damage the apparatus (Heidelberg UA, IV 3e Nr. 52, 1804–1864).

[24] Report "Die Wiederbestezung der Lehrstelle der Physik zu Heidelberg betr.," 7 December 1816, Bad. GLA, 235/3135.

[25] Maria Reindl, *Lehre und Forschung in Mathematik und Naturwissenschaften, insbesondere Astronomie, an der Universität Würzburg von der Gründung bis zum Beginn des 20. Jahrhunderts* (Neustadt an der Aisch: Degener, 1966), 98–102. The physician F. A. Sorg was considered qualified for the "mathematical section" by his prizewinning paper on the breathing of insects. See report by Count von Thürheim to Elector of Bavaria, Bay. HSTA, Abt. I, MInn 23,590.

of physics at Marburg University from 1817 to 1864, devoted the years before his appointment preparing for his "future astronomical profession."[26] Even at new universities, appointments were made without regard for specialized preparation. The first professor of physics at Bonn University in 1818 had previously been an apothecary and a chemist; the first professor of physics at the newly founded Breslau University was a nature philosopher, who taught nature philosophy, anthropology, physiology, and mineralogy in addition to physics; as late as 1835 two professors of physics at Berlin University were by training chemists. A recommendation for a university position might look like this: "[L. F.] Kämtz is a very good calculator and loves calculating with formulas and numbers very much. I believe that he would also make himself into a good astronomer, namely a practical one; for he has already zealously studied theoretical [astronomy] with [J. F.] Pfaff.. . . Indeed, he can be recommended in good conscience for a teaching position for physics and astronomy, but as well for one for mathematics and physics, since he has very zealously studied pure (also higher) mathematics . . . [and] successfully lectured on conic sections."[27]

The tasks relating to teaching placed a greater burden on teachers of the natural sciences than on their colleagues in the philosophical faculty who taught the humanities. As their sciences were observational and experimental, they did not believe that they could limit their teaching effectively to the reading of a text. They required apparatus, collections of specimens, botanical gardens, and rooms in which the instruments could be stored and used and the collections displayed, all of which required large amounts of money. At some universities, especially in Prussia, the money was available for the then observational sciences—mineralogy, botany, and zoology—but not for the experimental sciences. At Berlin University, for example, from 1810 to 1840 the Prussian government spent close to 200,000 thaler, if not more, on mineralogy and zoology, but only 3500 thaler on physics (or, to be precise, nothing until 1833 and then 500 annually) and as little on chemistry. The uneven distribution of funds did not arise from any marked discrepancy in needs—instruments for physics and chemistry, not to mention proper physical and chemical laboratories, were expensive too—but from the scorn for the experimental sciences expressed by education officials such as Karl von Altenstein and his university advisors as well as from their desire to enhance the state through

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[26] Gerling to Gauss, 17 February 1816; see also Gerling to Gauss, 24 May 1814, 1 March 1815, 28 December 1815, and 25 September 1816, in Gauss Papers, Gőttingen UB, Ms. Dept.

[27] J. S. C. Schweigger's recommendation of L. F. Kämtz to J. W. Döbereiner, in *Neue Mittheilungen aus Johann Wolfgang von Goethe's handschriftlichem Nachlasse,* vols. 1–2, *Goethe's Naturwissenschaftliche Correspondenz* (1812–1832), ed. F. T. Bratranek (Leipzig, 1874), vol. 1, 110–11.

ostentatious displays of a natural history collection. In the poorer German states, the observational and the experimental sciences both struggled for funds.[28]

When we look at physics instruction being offered at German universities in the early nineteenth century, we find both "experimental" and "theoretical" lectures. Physics teachers who had apparatus for demonstrating the phenomena gave "experimental" lectures, which they generally preferred. When they lacked apparatus and had to explain the phenomena solely with words and drawings, they gave "theoretical" lectures.[29] According to Kastner, who was charged with teaching physics at several German universities in the course of his career, the "theoretical" physicist drew on "historically reported observations and experiments" in presenting the laws of nature, whereas the "experimental" physicist presented those laws directly through experiments.[30] The Tübingen physics curriculum followed the pattern. C. F. von Pfleiderer regularly offered "theoretical physics" in his so-called public lecture for a general audience, the traditional 1 h a week lecture every professor was obliged to give every semester free of charge; in addition, he alternately lectured on "experimental physics" and on elementary mathematics in his second so-called

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[28] Ludwig von Rönne, *Das Unterrichts-Wesen des Preussischen Staates* (Berlin, 1855), vol. 2, 430–62. The Prussian university institutes and collections for the *natural sciences* received 27,146 thaler annually; in addition, the universities had the unlimited use of some state institutes for the natural sciences with a budget totaling 16,610 thaler. Of these funds for the natural sciences, only about eight percent were allotted to the *experimental sciences*: 2156 thaler to physics (that is, physics at *all* six Prussian universities together received only slightly more than Hegel's salary of 2000 thaler) and 1372 thaler to chemistry. By contrast, botany—which the Minister of Culture Altenstein had once studied and in which he maintained an amateurish interest—received 21,732 thaler annually; in other words, botany could command ten times as much money as physics for its *annual* expenses, not including extraordinary acquisitions. Rudolf Köpke, *Die Gründung der Königlichen Friedrich-Wilhelms-Universität zu Berlin* (Berlin, 1860), 274–85, provides further evidence of the Prussian government's favoring of the observational sciences: the Prussian government spent 47,557 thaler buying zoological collections for Berlin University in these years, 1810–1840; it financed scientific expeditions to collect specimens at a cost that the zoologist Wilhelm Peters, compiler of these figures for Köpke in 1860, could no longer determine but judged "very considerable"; from 1820, it gave the zoological collections a fixed annual budget amounting to a total of 57,084 thaler for the first twenty years; and, during the same period, it enriched the mineralogical collection at a cost of 48,030 thaler and gave it an annual budget which from 1816 to 1837 grew from 1000 to 1520 thaler. The annual budgets of the observational sciences, unlike those of physics, did not have to pay for most of the scientific acquisitions of these collections or for their housing, since the collections were given space in the university. For an example of a small university in this connection, see *Die Universität Freiburg seit dem Regierungsantritt seiner Königlichen Hoheit des Grossherzogs Friedrich von Baden* (Freiburg i.Br. and Tübingen, 1881), 48–51, 91–96.

[29] Georg Wilhelm Muncke, "Physik," in *Johann Samuel Traugott Gehler's Physikalisches Wörterbuch,* ed. Heinrich Wilhelm Brandes, L. Gmelin, J. C. Horner, Georg Wilhelm Muncke, and C. H. Pfaff , vol. 7, pt. 1 (Leipzig, 1833), 493–573, on 505.

[30] Kastner, *Grundriss* (1810), vol. 1, 57. In the 1820–1821 edition of the textbook, Kastner left out the word "historically" (vol. 1, 47).

private lecture, which was the longer, more important, and better attended of the two and the professor's main source of student fees.[31]

In the early nineteenth century, it was generally accepted that classroom demonstrations were important for the proper teaching of physics. Mayer in 1805 advocated connecting "experience and conclusions with one another" and illustrating "the different theories by experiment."[32] Because physics professors usually did not get any or enough money from the university or the state to purchase and maintain apparatus and instruments, they had to provide them themselves—and in fact were expected to do so—out of their own income. They constructed their own demonstration instruments, transported them to and from lecture halls, sometimes across town, and set them up and dismantled them for every lecture because they had to share facilities with other professors.[33] The few physics professors who had a good collection of apparatus profited from their troubles, for they became desirable to universities and could set their own terms. For example, L. W. Gilbert threatened to turn down an offer from Leipzig University if he was not given quarters for his collection and his lectures, a laboratory, and living quarters. He got what he wanted and moved to Leipzig.[34] Physics was rapidly developing in the direction that made self-reliance in procuring experimental equipment in most cases merely a stopgap. The needed materials had multiplied to a point where even wealthy persons could rarely acquire them with their private means. For that reason, Muncke explained to the Baden ministry in 1817 when he was appointed physics professor at Heidelberg, government officials began equipping public educational institutions with the required apparatus out of a specially designated fund.[35] Governments, as Muncke was to learn, yielded only in small steps to entreaties, but if physicists wanted to keep current with the developments in their fields, they had no choice but to persist. Those universities that acquired instrument collections early and built them up assiduously became the important centers for physics in the middle of the century, their chairs attracting the best of the next generation of physicists. When Muncke arrived at Heidelberg, he found many "rather beautiful, rare, and sometimes valuable" instruments, but also "important gaps." To fill them, he needed in addition to the usual barometers and thermometers a usable electrical machine, Coulomb's scale, Volta's pile, Leslie's photometer, and other specific instruments and apparatus for studying and demonstrating the latest physics. He got three quarters of the annual fund he requested, which he reserved for buying apparatus and making urgent repairs, the remaining quarter going toward the salary of an institute servant. He had also requested extra funds so that he could soon complete the physics institute in a way that measured up to the "splendor and fame" of Heidelberg University; although he was promised these funds, he did not receive

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[31] HSTA, Stuttgart, E 31, Bü. 372, pp. 116, 123, 125, 737.

[32] Mayer, *Anfangsgründe* (1805), 18.

[33] Wagner, "Schriften über Universitäten, Dritter Artikel," col. 1016.

[34] Gilbert Personalakte, Leipzig UA, Nr. 503.

[35] Muncke to Baden Ministry of the Interior, 1 June 1817, Bad. GLA, 235/3057.

them. Eleven years later, he still held to his original estimate of the extra funds needed to improve the institute, reminding the government that it was then way behind what it should be. Most of the "great cabinets in Germany" had annual funds more than three times the amount of his, he told the government. At his own expense, he had supplied the institute with an astronomical clock and a good microscope, but he could do no more with his private means.[36] He took every opportunity for shaming the government into more support at Heidelberg, and in this way he slowly built up the instrument collection. At the end, he enabled his successor Philipp Jolly to begin his teaching by setting up a student laboratory instead of starting at the bottom with the task of accumulating instruments. This was an accomplishment, since even such an important university as Berlin had to begin at the bottom in the 1830s.

At Gőttingen, our second example, Mayer was the first professor of physics to be provided with physical apparatus. His predecessor Georg Christoph Lichtenberg, like so many other physicists then and later, had acquired his own nearly complete physical apparatus consisting of the best instruments, which he had used in his lectures in the "auditorium" in his home. Lichtenberg sold his apparatus to the state for use at the university, and after his death it was moved there, where Mayer set it up near the room in which he gave his lectures. In addition to instruments and space, Mayer also received 100 thaler in annual funds to maintain and enlarge the collection, an amount he found adequate even for "those instruments and needs . . . caused by recent discoveries."[37] He added "considerably" to the collection after he had taken it over and proudly spoke of the latest acquisition, "a large voltaic pile consisting of 300 five-inch pairs of plates of copper and zinc," which under favorable conditions could give fifteen-inch sparks.[38]

Through his control of this respectable instrument collection, Mayer could dominate the teaching of experimental physics at Gőttingen. Before he came there, an extraordinary professor of mathematics J. C. D. Wildt had been encouraged by Lichtenberg and the mathematics professor Abraham Gotthelf Kästner to devote himself to physics in the hope of succeeding one or the other of them, and on his own he had tried to accumulate the necessary apparatus.[39] He learned that attempts to force the ordinary professor of physics to share the physical apparatus of the university were worse than simply competing with him for students in his lectures.[40] He applied to the university curator for permission to use the apparatus, but that only resulted in hardening attitudes into rules and making enemies. Mayer

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[36] Muncke to Baden Ministry of the Interior, 1 June 1817, and to Heidelberg U. Curator, 25 December 1828, Bad. GLA, 235/3057.

[37] J. C. D. Wildt to Hannover government, 3 April 1799, Göttingen UA, 4/Vh/6. Hannover government to Christian Gottlob Heyne, 8 May 1799; Heyne to Hannover government 23 May 1799; J. T. Mayer Personalakte, Göttingen UA, 4/V b/67. J. T. Mayer, untitled article about the Göttingen physical cabinet, *Göttingische gelehrte Anzeigen*, 1812, vol. 2, 1417–22.

[38] Mayer, untitled article about the Göttingen physical cabinet, 1419–20.

[39] J. C. D. Wildt Personalakte, Göttingen UA, 4/V b/64.

[40] Wildt's correspondence with Hannover government, summer 1815, Göttingen UA, 4/Vh/6.

protested to the government that if Wildt were to use the apparatus, it—and with it Mayer's health—would be ruined. "At all universities it has now been established that the professor of physics alone has the use of the instruments" necessary for lectures on experimental physics, Mayer explained in support of his contention that he had been assured sole access to and use of the apparatus, and he got his way.[41] If the professor of physics had exclusive rights to the use of the instruments, then any colleagues who also wanted to teach experimental physics could only do so if he allowed it or if they had a considerable fortune. The same problem came up again with Mayer's successor, Weber, this time involving the instruments for research as well as for teaching. When the Gőttingen Privatdocent for physics Gustav von Quintus Icilius talked his way into Weber's institute as his first scientific assistant, Weber employed him under the condition that he "not be permitted to use the institute with the collections and the auditorium for his independent lectures as Privatdocent or for his special private research, which would lead to the most unpleasant collisions and would be absolutely impossible to reconcile with the order of the physical institute."[42] Quintus Icilius took on all the tasks of an institute assistant that were to become standard: assisting in the laboratory courses, helping in research, and taking care of the instruments, but he did not get paid for this work. His recompense was the opportunity to gain otherwise unavailable experience in handling the physical apparatus.

In the "big show" lectures on experimental physics at Gőttingen, as elsewhere, the more frequent and the more splendid the demonstration experiments, the more attractive the lectures were. Weber, who was not given to prodigious use of experiments in lectures, reminded the university curator in 1837 that it was time to improve the lecture apparatus in the interest of the "glamour" of the experimental physics lectures.[43]

2.3 Introduction to the World of Physics Through Early Textbooks

There was fair agreement on the core subjects of physics as taught at universities—mechanics, light, heat, electricity, and magnetism—though other subjects might be joined with them, a point of contention. Fischer was critical of existing textbooks for their careless definition of physics, including chapters on chemistry, for example, on the grounds that the dividing line between physics and chemistry could not be clearly defined. Their authors blamed the subject for what was their own failure

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[41] Mayer to Hannover government, 2 August 1815, and drafts of replies, Göttingen UA, 4/Vh/6. When not pushed, Mayer might even offer to let a colleague use part of his collection. Mayer to Göttingen U. Curator, 13 July 1829, Göttingen UA, 4/Vh/8.

[42] Weber to Göttingen U. Curator, 14 February 1852, Göttingen UA, 4/Vh/21.

[43] Weber to Göttingen U, Curator, 20 October 1837, Göttingen UA, 4/Vh/16.

to make distinctions between concepts, he said; although certain subjects such as electricity could be discussed in both chemistry and physics because certain aspects of electrical phenomena were chemical in nature and others were physical, if concepts were properly defined, there could be no question about which aspects of electricity belonged to which science. The title Fischer gave to his textbook *Lehrbuch der mechanischen Naturlehre* points to the conceptual distinction he had in mind: what belonged to physics proper were investigations of natural phenomena on the basis of the laws of mechanics, which included not only the observable motions of bodies but also the invisible motions within bodies responsible for heat and other phenomena.[44] In Fischer's understanding, physics rested on a conceptual unity.

A student consulting a lecture catalog in the early nineteenth century would, as a rule, find entries of the following kind: "Experimental Physics presented by Hr. Hofr. Mayer, according to the sixth edition of his textbook, at 4 o'clock." Most of the university physicists still "read" their lectures according to a textbook, usually their own, especially after they had a professorship. Textbooks tended to be their first long publications. At this time, textbooks were considered to belong to the "literary," original works of their authors, and as such they counted when physicists were considered for promotions or job offers from ("calls" to) other universities. To be given a favorable notice, the textbooks had to be distinguished in some way from the rival textbooks on the same subject produced by other university teachers—and by secondary school teachers, who could hope for university appointments—who were also working for promotions or for enhanced reputations to earn them larger salaries and more favorable working conditions. Textbooks might express an author's particular view of his field[45] or criticize methods and interpretations of physics by other authors,[46] or they might place the author's own research within his field of physics, thereby associating him with physicists of greater, even great, renown.[47] They always claimed that they made up the deficiencies of other text-

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[44] Fischer, *Lehrbuch*, vol. 1, vii–viii, 4–5.

[45] Some textbook authors announced their particular approach to the field in the titles of their works; for example, Muncke's *System der atomischen Physik nach den neuesten Erfahrungen und Versuchen* (Hannover, 1809); Fries's *Entwurf des Systems der theoretischen Physik; zum Gebrauche bei seinen Vorlesungen* (Heidelberg, 1813); or Parrot's *Grundriss der theoretischen Physik*; Fischer's *Lehrbuch der mechanischen Naturlehre*; Friedrich Hildebrandt's *Anfangsgründe der dynamischen Naturlehre* (n.p. [Erlangen], 1807); and Andreas von Baumgartner and Andreas von Ettingshausen's *Die Naturlehre nach ihrem gegenwärtigen Zustande mit Rücksicht auf mathematische Begründung*, 6th ed. (Vienna, 1839).

[46] Fischer wanted to produce a textbook "in as rigorously scientific a form . . . as the nature of the subject permits" (*Lehrbuch*, vol. 1, vi). He thought that the existing textbooks lacked clarity.

[47] Mayer discussed his experiments on heat—"done with the greatest possible precision"—alongside the work of Pictet, Aimé-Lair, Socquet, Saussure, Dalton, Gay-Lussac, Deluc, Lambert, Smeaton, and others, as well as Rumford later (*Anfangsgründe* [1805], 239, 263–64, 269). In the fourth edition (1820) of this work, he discussed his work on atmospheric pressure in gases alongside contemporary work on the problem by Gay-Lussac and Dalton.

books.[48] Because the authors expected to make money with their textbooks, when they asked for raises in salary, they sometimes explained that one thing or another had prevented them from adding to their income by writing a textbook.

The earliest textbooks we consider were written when Immanuel Kant was still alive. Almost to a man, their authors declared themselves followers of his philosophy, giving attention to the nature of objective knowledge and to the process by which experience becomes scientific knowledge.[49] Jacob Friedrich Fries, professor of physics at Heidelberg University, wrote full-length epistemological treatises in addition to his textbook on "theoretical physics," in which he discussed the "relationship of the natural scientist to nature."[50] He expanded on the subject in *Die mathematische Naturphilosophie*—the title must be understood as referring to Newtonian natural philosophy, not nature philosophy—which presents a philosophically sophisticated connection between Kant's ideas and natural science in its highest development in Newtonian mathematical physics.[51] Scientific knowledge for Fries is the product of our experience and of the mathematical laws we acquire through our a priori understanding, the faculty that Kant was first to posit and make the subject of critical investigation. The making of a physical theory proceeds as follows. With his senses, the physicist perceives properties of bodies such as heat and pressure, which require an explanation that is not found in experience. To arrive at it, the physicist has to use "pure" mechanics; that is, general concepts such as space, motion, and force, and the laws relating them to one another, all of which are a priori concepts according to Kant (and Fries). Pure mechanical theory is not itself an explanation of physical phenomena; it states which laws of motion the hypothetical explanations necessarily obey, which motions and forces are possible, and which are simplest. The assumptions of the hypothetical explanations become the basis for mathematical constructions that will, if successful, constitute the desired

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[48] Fischer, *Lehrbuch*, vol. 1, vi–xii; Kastner, *Grundriss* (1810), vol. 2, vi–vii.

[49] Kant held that natural science needs general principles, such as the principle of causality, which are known by experience, but cannot be established by experience. All change in nature is theoretically referred to the empirical concept of motion, the laws of which are known a priori. The task of natural philosophy is to reduce the qualitative to the quantitative, the method of which is mathematical. Mathematical theory supplies a universally valid and necessary perception of nature, and the science of nature is possible only insofar as it can be subjected to mathematics. Space and time are objects of perception, and have empirical reality, but nothing beyond that. We can never know the thing in itself behind appearances, and even the mathematical representation of nature is only an appearance (Wilhelm Windelbrand, *A History of Philosophy*, vol. 2, *Renaissance, Enlightenment, and Modern* [New York: Harper & Row, 1958], 541, 546–47, 597). Physics textbook writers demonstrated their acceptance of Kant's work in their teaching as well as in their publications. Erman, for example, was a determined follower of Kant, teaching his philosophy even when his preference for Kant drew the criticism of his superiors (Erman, "Paul Erman. Ein Berliner Gelehrtenleben 1764–1851," *Schriften des Vereins für die Geschichte Berlins* 53 [1927]: 46–51).

[50] Fries, *Entwurf*, 15.

[51] Jakob Friedrich Fries, *Die mathematische Naturphilosophie nach philosophischer Methode bearbeitet. Ein Versuch* (Heidelberg, 1822).

theory. The last step is for the physicist to verify the theory by comparing it with experimental results. "Pure theory" never strikes out on its own, but must remain in contact with experiment.[52] Textbooks by Mayer and Kastner offer further examples of philosophical discussions in physics. Mayer traces the path to scientific knowledge from sense perceptions to the concepts formed of things and their relationships and then to the action of the mind on these concepts. Natural science is limited by what is accessible to the senses, and any parts of it that are the product of thought have to be verified in experience. Natural science generalizes subjective, individual understanding and experience, which is possible because the sense organs of all humans are constituted alike and the nature of thought is the same for everyone. Our intellectual constitution, our mind, influences the generalization of experience. Entirely out of itself, the mind has developed the laws of motion, which have been successfully applied to the objects of experiments in so many ways that natural science is largely pure or applied theory of motion.[53] Kastner, in his textbook, makes similar points. In the belief that the importance of epistemology for natural science is not fully appreciated, he opens with a discussion of the "general condition of investigating man."[54] The philosopher and the physicist will only succeed in showing the true correspondence between mind and nature when the philosopher becomes more interested in exhibiting nature than in his systems and when the experimenter accepts the view that one must start from the idea of oneself in studying phenomena. The scientist has to realize that what he investigates in nature is always something that corresponds to his own nature; he studies heat and color, for example, because his senses produce these properties of bodies.[55] Accounts of epistemology were shortened in textbooks after Fries's, Mayer's, and Kastner's, eventually taking up no more than a paragraph or even a sentence, but they still appeared.[56]

The main purpose of physics textbooks was to present contemporary physics, not to educate future physicists. Often intended for the self-instruction of "friends of science," they gave detailed accounts of important new experiments, which readers might want to try. The more "scientific" textbooks contained in addition extensive descriptions of the latest barometers and other measuring instruments and apparatus. The new research in physics that Mayer added to his textbook from the second edition in 1805 to the last editions in the 1820s contained major physical and chemical advances; it included, for example, works by Laplace, Ampère, Malu, Berthollet, Gay-Lussac, Dalton, Oersted, and Chladni.[57] Textbook writers

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[52] Fries, *Die mathematische Naturphilosophie*, 9–11, 29–32, 610–11.

[53] Mayer, *Anfangsgründe* (1805), vol. 1, 8, 18.

[54] Kastner, *Grundriss* (1810), vol. 1, 1–29; in the 1820 edition he revised this discussion, vol. 1, 1–22. Mayer, *Anfangsgründe* (1805), vol. 1, 1–18.

[55] Kastner, *Grundriss* (1810), vol. 1, 2, 6–7.

[56] Gustav von Quintus Icilius, *Experimental-Physik. Ein Leitfaden bei Vorträgen* (Hannover, 1855), 1.

[57] Mayer, *Anfangsgründe* (1820), 22–23, 76, 81–82, 148, 277; (1823), 516, 541, 546, 551, 555.

conveyed to their readers the sense of living in a time of rapid change in physics. Decade by decade the understanding of nature was "so much expanded through the zealousness of the natural sciences," Schmidt wrote, that it was often difficult for him "to connect the new with the old in such a way that the whole need not be recast."[58] The second edition of Kastner's two-volume textbook on experimental physics illustrates the point. The first volume of this edition was published in 1820, but the second volume, which began with galvanism, was delayed until 1821. Meanwhile, news of Hans Oersted's discovery of the magnetic action of electric current, or electromagnetism, had reached Kastner, who immediately set about to include it in his already completed but not yet printed chapter on galvanism. This led him to work on electromagnetism himself as well as learn everything he could about it from reading. When he sent the hastily reworked manuscript to the publisher, he included along with the discussion of electromagnetism a proposal of a new name for the new phenomena, "Siderismus," since "electromagnetism" was awkward and "Oersted" did not go well with "ismus.".[59]

The largest part of physics textbooks was given over to the new theories of the so-called imponderables: heat, light, electricity, magnetism, and galvanism. The main German textbooks such as Mayer's, Kastner's, and Schmidt's presented phenomena of current interest together with verbal explanations of the *nature* of the phenomena. To discover the "nature" of the imponderables and even of phenomena that already had mathematical theories such as elasticity was the general problem that occupied many German experimental physicists. The task of physicists, according to Kastner, even though it involves observations and experiments, is to gain "insight into the nature of things"; "experiences seen as an end in themselves, without having conclusions derived from them about the nature of the observed object, are of no use to science."[60] Statements from other textbooks bear out this concern. Schmidt, remarking on the nature of heat, observed that physicists "are still arguing whether one is to think of the unknown cause of the . . . phenomena [of heat] as a substance of a particular kind, a *heat substance*, or a mere modification of the bodies, which according to some is to be sought in the motion of the particles of the body, according to others in the motion of a generally distributed fluid (the ether), or finally (as some recent dynamicists would have it) in the changed relations of the fundamental forces of matter." The question of the nature of heat being unsettled, Schmidt said that "theories of heat are nothing but hypotheses, whose inner value may only be judged by [their] degree of probability and by their usefulness to the experimenting natural scientist in unveiling the truth."[61] Concerning the nature of magnetism, Kastner discussed "a forerunner to a future

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[58] Schmidt, *Handbuch*, iii.

[59] Kastner, *Grundriss* (1820–1821), vol. 2, x–xi.

[60] Kastner, *Grundriss* (1810), vol. 1, 19, 126–27; (1820–1821), vol. 1, 24. Kastner was the physicist who saw the need to find the "nature" of elasticity.

[61] Schmidt, *Handbuch*, 257.

theory instead of a complete theory," which he thought was not yet possible.[62] In electricity, optics, and elsewhere in physics, Mayer simply acknowledged that the sciences were still very much in the dark.[63]

In the entry "Mathematics" in Gehler's physical dictionary, the physics professor at Leipzig University Heinrich Wilhelm Brandes wrote that if the work of French physicists such as Fourier, Ampère, and Poisson were any indication, soon the theories of their subjects, heat, electricity, and magnetism, would become "branches of applied mathematics," just as mechanics and optics already were.[64] In the entry "Physics" in the same physical dictionary, Muncke wrote that mathematical physics is insufficient to discover the laws of nature, which are always found through observations and experiments. He thought that the French overvalued mathematics in physics, and that many German physicists did too because they did not want to be accused of not knowing mathematics. Although mathematical physicists had given certain areas of physics excellent mathematical treatments that would forever stand as masterpieces in the mathematical literature such as Fourier's theory of heat, they had not explained the "nature and behavior" of these physical phenomena.[65] C. H. Pfaff, at Kiel University, in his text on electromagnetism in 1824 distinguished mathematical theories of physical phenomena from physical theories, though without giving up the one for the other. Mathematical theories ignore the nature of the phenomena, which is the proper subject of physical theories. The mathematician conceives of electromagnetic phenomena, for example, only as "various modifications of motions," which he seeks to describe by a fundamental equation. If he is a "deeply inquiring mathematician who is practiced in calculations," he may find the equation that describes the phenomena, but not their "secret source." By contrast, the physicist probes deeply with his physical theory, seeking to present the phenomena in their "large, general connection with all of nature." The "true physical explanation" is, for Pfaff, at the same time a mathematical one, but it "may not stop at showing only in general the inner nature of the phenomena and their mutual dependency, but it must derive the influence of all circumstances and their variations on the quantitative [aspect] of the phenomena from the nature of the forces themselves. In fact, from a certain point of view of physics, all of nature becomes simplified to form and motion, and from that point of view every physical explanation becomes necessarily—the further it advances—a more and more purely mathematical one."[66] The early textbook writers were wary of highly mathematical theories for a general reason, their abstractness or lack of *Anschaulichkeit*. Because they do not summon up any kind of image, their results cannot be grasped by the senses and so they cannot be empirically verified.

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[62] Kastner, *Grundriss* (1820–1821), vol. 1, 448. Kastner's opinion may have been based on his understanding that natural science does not, strictly speaking, *explain* the phenomena, that it does not reduce them to their essence. He said that since science is never complete and is always being added to, "every theory is useful only for a time" (p. 29 in 1810 ed.; p. 22 in 1820–1821 ed.).

[63] Mayer, *Anfangsgründe* (1805), 483, passim.

[64] Heinrich Wilhelm Brandes, "Mathematik," in *Johann Samuel Traugott Gehler's Physikalisches Wörterbuch*, vol. 6, pt. 2 (Leipzig, 1836), 1473–85, on 1477.

[65] Muncke, "Physik," 508–12.

[66] C. H. Pfaff, *Elektro-Magnetismus*, 199, 200–201.

2.4 Nature Philosophy

Early German physics textbooks took up another philosophical issue, Kant's and F. W. J. Schelling's dynamical theory of nature, which was opposed to the atomistic theory. According to Kant's theory, which he deduced from general principles, whatever is movable in space, or matter, is the product of two forces, attraction and repulsion, in varying degrees of equilibrium. Schelling's philosophy of nature is an organic view of the totality of nature based on the concept of duality, an opposition of forces, which negate each other in a higher unity.[67] Muncke wrote that "soon after its founding by the immortal Kant, the system of dynamical physics has almost acquired such an authority" that no one dared to defend the atomistic system anymore. In opposition to that view, he wrote his textbook to "show that the atomistic view" applies to the latest physical investigations.[68] That was an exaggeration, but textbook writers directed less attention to atomism than they did to Kant and Schelling, and to Kant they devoted more positive comment than to Schelling. The Giessen professor of physics Schmidt in his textbook discussed dynamical worldviews with reserve but also without ridicule, even though in the preface he said that he left out of his book and also out of his lectures all "philosophical speculations on physical subjects."[69] Mayer discussed the "dynamical nature philosophy" of Kant and Schelling and the "atomistic nature philosophy" of G. L. Lesage, Pierre Prévost, and Jean-Baptiste Lamarck, and he pointed to the extended discussion he intended to give in his lectures on Kant*'s* book on the metaphysical foundations of natural science and Schelling's book on the philosophy of nature.[70] Some authors even made these views the basis of their textbooks. Fries organized his textbook on theoretical physics and Kastner his on experimental physics on the basis of the dynamical view of nature.

Through the influence of Schelling in southern Germany and G. W. F. Hegel in northern Germany, supporters of "nature philosophy" (*Naturphilosophie*) held numerous university chairs. Beginning with Schelling's *Ideen zu einer Philosophie der Natur* in 1797, this branch of German Idealism held sway for about 30 years, until the death of Hegel. Later Helmholtz and other natural scientists wrote about the unfortunate effect it had had on the natural sciences, but at the time some important natural scientists were drawn to its teachings; for example, the natural historian Lorenz Oken, a founder of the Versammlung deutscher Naturforscher und Ärzte

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[67] Windelbrand, *History of Philosophy*, vol. 2, 597, 599.

[68] Muncke, *System*, v.

[69] Schmidt, *Handbuch*, iv. To give an example: Schmidt looked at gravity first from the point of view of Lesage's atomism, concluding that atomism does not explain gravity since it refers it to another inexplicable force; then he noted that the dynamical point of view of a physical attractive force is at least "as admissible, especially if one does not, as it were, carry this force into the bodies that are already assumed to be given but, like the dynamical system [of thought], seeks to derive the phenomena of matter themselves from forces, whose nature will of course always remain inexplicable for us" (76–77).

[70] Mayer, *Anfangsgründe* (1805), 9–12.

(German Association of Natural Scientists and Physicians), the young Giessen chemist Justus von Liebig, founder of the first teaching laboratory in Germany, and, if temporarily, the young Berlin physiologist Johannes Müller. In addition, a case has been made that in their electrodynamic researches, two of the central physicists in this study, Weber and Neumann, were influenced by nature philosophy.[71]

It was appropriate for a physicist to discuss philosophical views of nature provided that he did not make improper use of them in his practical work in physics and did not introduce ideas negating the objective reality of nature such as the "false nature-philosophical dreams," to quote Fries.[72] With reference to the dynamical view of nature, Mayer pointed out that whether or not matter is the product of the interaction of forces, the physicist has to distinguish force from matter if he is not to fall into useless dreaming or leave common uses of language too far behind.[73] When speculations about nature touched on a field of particular interest to Mayer, he could show impatience. "Playing with forces in the dynamical nature philosophies" and playing with atoms in the "corpuscular system" created "arbitrary fictions," and neither view sets limits where the mind has to stop in explaining natural phenomena "if it is not to lose itself in incalculable labyrinths."[74] Parrot in his textbook on theoretical physics wrote that "everything reasonable and logical that can be said about a dynamical system, Boscovich, Priestley, [and] Kant have said, and it can serve to compose for oneself at best a meaningful fiction beyond experience, but to want to deduce the laws of nature from this fiction, to tell nature a priori how it should behave—that would be to make a satire of the human mind.".[75]

As with epistemological discussions, discussions of philosophical views of nature underwent changes in physics textbooks. In their use of the dynamical view of nature, physicists dissociated themselves from some of the ideas of nature philosophy. Whereas Kastner had repeatedly used the word "attractions" in section headings in his textbook of 1810, in the 1820 edition he left out section headings and clarified his view of nature as the interaction of forces: nature, he wrote, acts out of necessity, not because of some inner striving, which could only be will, and he found explanations of natural phenomena involving such striving "reprehensible."[76] Similarly, Fries insisted on the strict separation of the laws of physical existence from considerations of the mind.[77] Brandes in his textbook of 1830 mentioned philosophical views only briefly, stating that the dynamical view when taken beyond statements about the extension and

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[71] Kenneth L. Caneva, "From Galvanism to Electrodynamics: The Transformation of German Physics and Its Social Context," *HSPS* 9 (1978): 63–159, on 67.

[72] Fries asserted that the unacceptable nature philosophy "most often" originates not in philosophy but in the improper use of the methods of the experimental physicist, [the improper use] of inductions and hypotheses, which "seduces" those who do not know the rules for using these methods into "false nature-philosophical dreams." Fries, *Entwurf*, 8.

[73] Mayer, *Anfangsgründe* (1805), 36.

[74] Mayer, *Anfangsgründe* (1805), 12. 13, 231.

[75] Parrot, *Grundriss*, vol. 1, iii–iv.

[76] Kastner, *Grundriss* (1820–1821), vol. 1, 2.

[77] Fries, *Entwurf*, 7.

density of matter to the definition of matter as nothing but the conflict of forces led to "indissoluble darkness." The physicist does not dare enter into such questions, especially since the "gain resulting from them, either with respect to complete reliability or with respect to practical fruitfulness, has not quite shown itself to be valuable.".[78]

Except for matters of emphasis, the physicists' quarrel was not with philosophy as a whole but only with part of it, which they often did not dignify with the name of philosophy. The campaign of the early physicists against nature philosophy did not stop with criticisms in their textbooks. These were faint echoes of what they said in private letters, reviews, addresses, periodicals, and elsewhere. Struggling for research support in Berlin and therefore closest to the seat of conflict with nature philosophy, Erman looked in anger on attempts to vindicate Prussia's military defeat by Napoleon through intellectual renewal by means of idealistic philosophy: "twenty lost battles do not bring us as much disgrace as this business of deceptions and lies in science," he told his students. Nature philosophy had made inroads in the Prussian Academy of Sciences, and Erman was bent on discrediting it. In 1811 he proposed as a prize question of the Academy that the nature philosophers' concept of polarity be tested by an investigation that "is to be conducted *purely empirically* and *independently of all speculating opinions* about the fundamental nature and the absolute existence of matter." Erman continued his struggle into the late 1820s, when he still had to endure Hegel's open contempt for the exact natural sciences and the consequences for Berlin physics of Hegel's influence with the Prussian ministry of culture.[79] When Pohl, one of the few physicists who espoused nature philosophy, criticized Ohm's work in 1828, Schweigger wrote to Ohm that he was loath to give Pohl the satisfaction of having his opinions acknowledged by scientists.[80] Henrik Steffens, another adherent of nature philosophy, having moved in 1832 from his position as professor of physics and nature philosophy at Breslau University to Hegel's philosophy chair in Berlin, recorded the scientists' view of his philosophical direction without bitterness: "The natural scientists [at Berlin] expected little of me, and although they began to see that I possess some scientific knowledge and

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[78] Brandes, *Vorlesungen* (1830), vol. 1, 13.

[79] Erman, "Paul Erman," 140–41, 240; italics ours.

[80] Ohm, *Nachlass*, 90. For his part, as it happened, Schweigger was criticized for his opinions. Anxious to keep the experimental sciences uncontaminated by "mysticism," natural scientists even scorned ideas that originated in historical research. Schweigger's interest in research combining philology and natural science was received with derision. He believed that the combination of the modern scientist's knowledge of nature and the philologist's research tools would facilitate the study of the ancient mysteries, the depositories, in his view, of the ancients' knowledge of nature, and thus would be helpful in discovering how much the ancients had known about nature. Schweigger proposed to lecture on the subject before the annual meeting of the German Association, where following French academic custom, he intended to request a commission made up of classical scholars and of physicists to examine the matter. "They found the idea … *arrogant even* (whereas in France one has exactly the opposite view when someone wants to submit something for examination)," he wrote to Ohm, "and Leopold von Buch (who had *heard* of it only *by chance*) was, as is his manner, terribly rude." Schweigger to Ohm, 15 December 1828, Ms. Coll., DM, 693.

received me in a friendly manner, they were nevertheless firm opponents of nature philosophy. The great discoveries in physics on the one hand, then in geology, and finally in comparative physiology had throttled every germ of speculative view, and nature philosophy was considered an arbitrary, fantastic game, which perhaps here and there could stimulate poetic but never scientific interest."[81] The full vocabulary of contempt for nature philosophy entered the obituary that the *Annalen der Physik* published for its editor L. W. Gilbert in 1824:

> Nothing embittered him [Gilbert] more than the shallow, superficial treatment of the sciences, the endless hypothesizing, the mystical point of view, and the poetry that had entered science. As little opposed as he was to the latter [poetry] in everyday life, as hostile he was to it when, leaving its domain, it wanted to carry the dreams of the imagination into science. And who could blame him? What true admirer of the ... exact sciences must not agree with him on this with all his soul? This mixing of fiction and truth, of poetry and science, this playing with empty, half-true analogies, this guessing and suggesting instead of knowing and understanding has ruined our good name for us Germans abroad, has led us away from thorough science, and has brought us to believing that we know everything, while we have fallen behind in real knowledge.[82]

Fischer spoke of "fantasies very fraught with [dangerous] consequences of the so-called nature philosophy."[83] Even Goethe, who had some sympathy for nature philosophy, thought that it had injured German physics and German science in general. Praising Chladni's *Acoustics*, Goethe wrote to Friedrich Schiller that Chladni "belongs ... among the blessed who don't have the faintest notion that there is such a thing as nature philosophy."[84]

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[81] *Idee und Wirklichkeit einer Universität. Dokumente zur Geschichte der Friedrich-Wilhelms-Universität zu Berlin,* ed. Wilhelm Weischedel (Berlin: Walter de Gruyter, 1960), 333.

[82] Obituary of L. W. Gilbert, *Ann.* 76 (1824): 468–69.

[83] Fischer, *Lehrbuch*, vol. 1, xii–xiii.

[84] Goethe to Schiller, 26. January 1803, in *Goethes Werke* (Weimar, 1894), pt. 4, vol. 16, 170.

Chapter 3
German Physicists Before and Around 1830

Textbook writers around the turn of the nineteenth century were wary of mathematical physics. In its abstract character, it seemed irrelevant to what interested them, the nature of the phenomena, which they thought of as physical reality. What they regarded as physical theory was qualitative and pictorial. We have seen that these writers were not trained in physics, that they usually taught other sciences in addition to physics, and that they often held jobs simultaneously in a university and in a secondary school. We have referred to them as "physicists," but they probably did not think of themselves that way. With the university teachers who came after them we are on surer grounds in calling them "physicists." Once these teachers became professors they were likely to lecture only on physics and to have no outside jobs. Probably well informed on the physics coming out of France, their research differed as well. They were less concerned with *Anschaulichkeit,* more accepting of the abstractions of mathematics, and committed to quantitative experiments and accurate measurements. Unlike their predecessors, they were willing to begin their theories with physical assumptions without insisting on their truth, concerned mainly with obtaining agreement with experiment.[1] As has happened countless times in the history of science, young scientific researchers departed from the example of their elders, and Georg Simon Ohm, Wilhelm Weber, and Franz Neumann were in the vanguard. In this chapter, we look at the physicists who worked in physics in the new way.

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[1] These generalizations are based on a study of twenty-one German contributors to electricity and magnetism in the early nineteenth century. Kenneth L. Caneva, "From Galvanism to Electrodynamics: The Transformation of German Physics and Its Social Context," *HSPS* 9 (1978): 63–160, on 69–70, 95, 132–34, 136–38, 157. This author suggests that the younger physicists broke with the physics of their teachers in part because of the questioning of authority occasioned by the Napoleonic wars and in part by the educational ideal of *Wissenschaft* and aspects of educational reform (124–25).

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_3

Before proceeding, we should note that among the earlier physicists there were exceptions such as the professor of mathematics and physics at Giessen, Schmidt,[2] and especially the Berlin extraordinary professor for physics, Fischer. Around the time that Ohm published his theory of the galvanic circuit, Fischer, who by then was in his seventies, wrote in his textbook that what is important in physics is not to decide between qualitative explanations of phenomena but to find the "laws of the phenomena with mathematical certainty." In connection with various explanations of light he wrote that what matters is not that any one of these is true but that "*we know the laws* that govern the phenomena." The intellect cannot know the "inner nature" of the laws of natural forces, but it can find "pictures" that connect the laws. If more than one picture works, we are free to use the one or the other without however believing that we have solved the "puzzle," for we must not confuse the pictures with the nature of the thing itself.[3] Younger German physicists who began their research in the 1820s and 1830s would have found Fischer's way of thinking about physics sympathetic, as indeed would many physicists at the end of our period.

The change in the idea of physics is evident in textbooks after Fischer's. An example is *Die Naturlehre nach ihrem gegwärtigen Zustande* (*Natural Philosophy in Its Present State*) brought out by the Austrian physicists Andreas von Baumgartner and Andreas von Ettingshausen in 1839. Ettingshausen, who wrote the mathematical part, used optics to show how a mathematical-physical theory is made. From an ignored, if not despised, hypothesis, the wave interpretation of light from France had become a "model of a physical theory," occupying "one of the highest places in science." To develop the theory, Ettingshausen did not begin with a full account of the phenomena of light, as earlier physicists would have, but with the medium in which light waves are propagated, the ether. Assuming that the ether is a system of material particles, he analyzed their vibrations, showing that they can all be reduced to simple vibrations in which the ether particles move either in straight lines or in circular or elliptical orbits. Only after his detour through the mechanics of the ether did he properly discuss the phenomena of light. He expected further progress in optics, which "depends not on new experiences but on the advances of theoretical mechanics, which in turn are determined in part by those of mathematical analysis."[4] A student of physics who had been exposed to traditional textbooks and lectures would have been struck by the shift in emphasis from the direct study of phenomena to a hypothesis about an unobservable substance, a system of laws, and mathematical-theoretical deductions. Ettingshausen's treatment of physics did not promise the fluent reading, or probably the enjoyment, the student was used to, but it made clearer than earlier treatments that at least part

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[2] Ibid., 69.

[3] Ernst Gottfried Fischer, *Lehrbuch der mechanischen Naturlehre*, 3rd ed., 2 vols. (Berlin and Leipzig, 1826–1827), vol. 2, 113, 259–60.

[4] They collaborated on the sixth edition of Baumgartner's *Die Naturlehre nach ihrem gegenwärtigen Zustande mit Rücksicht auf mathematische Begründung* (Vienna, 1839), 373, 382, 387, 410.

of the purpose of textbooks, whatever else it might be, was to instruct in the methods of building physical theories.

Early in our period, German physics was driven mostly by experimental research. The balance was somewhat different in France. Although important exploratory experimental work was done there, mathematical physics acquired a certain splendor among the sciences. From the second quarter of the nineteenth century, German physicists frequently took their starting point in works of French mathematical physics. We should have some notion of its character.

French physics developed within a setting that had parallels in Germany, educational institutions and an academy, but with significant differences. It had three main sources. One was the creation of a chair of experimental physics at the Collège de Navarre in the middle of the eighteenth century. Its holder was the abbé Nollet, whose writings were still discussed by German physicists in the early nineteenth century. Later in the century, in 1785, physics was recognized by the Royal Academy of Sciences in Paris. The second source was French mathematics, which had talented representatives in various branches of physics, from whom German physicists learned methods of mathematical physics. This source owed much to the Royal Academy of Sciences and the military schools. The third source was the circle of physical scientists centered on Pierre Simon Laplace, who developed a method that has been called Newtonian or neo-Newtonian.[5] They were regularly cited by German physicists.

We should be aware of what the Laplacians stood for. They replaced the analytical mechanics of eighteenth-century mathematicians with a mathematical-physical mechanics based on molecular forces and imponderable fluids, which they freely applied to heat, electricity, magnetism, optics, and other areas of physics with impressive results. Not less important, they joined mathematical theory with precise experimentation. Their expectation was that the method of molecular forces would advance physics in the way that Newton's distance forces had advanced astronomy, resulting in a "unified physical world view." As it turned out, their method was unsuited for some major developments in physics later in the century, but their emphases on mechanics and precise experiments persisted, as did their goal of a universal mechanical physics.[6]

Physics in France was sharply divided into individual sciences: mechanics, heat, optics, electricity, magnetism, and electrodynamics. Perhaps partly in response, J. B. Biot wrote in 1816 in his treatise on experimental and mathematical physics that physics was in disarray and uncertain: in one country "one numerical value is constantly employed, while in another place it is regarded as doubtful or inaccurate. Even the general principles are far from being universally adopted.. . . *What it wants is union*. It is the junction of the parts that makes a single body of it; it is a fixing of

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[5] Maurice Crosland and Crosbie Smith, "The Transmission of Physics from France to Britain: 1800–1840," *HSPS* 9 (1978): 1–61, on 6–7.

[6] Peter M. Harman, *Energy, Force, and Matter: The Conceptual Development of Nineteenth-Century Physics* (Cambridge: Cambridge University Press, 1982), 2–3, 15–17.

the data and the principles which gives the same direction to all efforts." Biot's call for "union" was accepted by all of the German physicists we study. Biot's own answer to the need for union was to carry through Laplace's method of unifying the physical sciences. When Laplace's influence on French physics waned in the early nineteenth century, his mathematical direction in physics was continued by J. B. J. Fourier in heat, A. M. Ampère in electromagnetism, S. D. Poisson in electricity, A. J. Fresnel in optics, and others.[7] All of the compliments he had received for his work, Fresnel wrote to a colleague in 1824, "never gave me so much pleasure as the discovery of a theoretic truth, or the confirmation of a calculation by experiment," a cry from the heart that could have been made by any of the major German physicists we discuss in this book.[8]

When the gifted young secondary school teacher Ohm examined French mathematical physics to see what it had left for him to do, he found in Fourier's theory of heat a model for developing theoretically the subject he was investigating experimentally, electrical conduction. Like Fourier, he based his theory on observable properties rather than on Laplace's molecules and forces, though later he would work extensively with molecular ideas. Weber and Neumann looked to other French masters for problems, methods, and inspiration.

3.1 Physics Journal and Physics Research

While science textbooks were sometimes used to publish original research, another form of publication had been evolving, the scientific journal. The most important journal for physics in Germany was the *Annalen der Physik und Chemie,* which came to "unite in itself the entire physical life in Germany,"[9] giving a "true and complete picture of the advances and transformations of the physical disciplines represented in it."[10] Since we refer to the contents of the *Annalen* for our portrayal of these advances and transformations and of the work of individual physicists in Germany, we introduce this important source here.

The founder of the journal was the professor of physics and chemistry at Halle University, F. A. C. Gren, who set out to make German readers familiar with recent discoveries in the "mathematical and chemical parts of natural philosophy [Naturlehre]." In the first volume in 1790, he called for serious original work rather than entertaining lectures, for which, he said, there already existed a crowd of journals. He set the tone by publishing an original work of his own, an "examination

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[7] Crosland and Smith, "Transmission of Physics," 7–8.

[8] All French mathematical physics has been characterized as mathematics not physics, but that is not how many German physicists regarded large parts of it. Ohm, Neumann, Helmholtz, Clausius, Kirchhoff, and others worked with it extensively.

[9] Karl Scheel, "Die literarischen Hilfsmittel der Physik," *Naturwiss.* 16 (1925): 45–48, on 46.

[10] Gustav Wiedemann, "Vorwort," *Ann.* 39 (1890), first four unnumbered pages.

of the new theories of fire, heat, inflammable matter, and air," which reflected the importance he gave to theories; the mind, he wrote, requires "theories of natural phenomena that bring unity and cohesion to our conceptions of them."[11] Most of the rest of this volume was taken up by Gren's abstracts from other independent periodicals and from society and academy transactions. These journals were beyond the means, if not beyond the linguistic and technical capacities, of the doctors, apothecaries, and handful of professors who made up much of the initial list of subscribers to Gren's journal.[12]

After Gren's death in 1798, his teaching assignments and his journal together with the new title he had intended for it, *Annalen der Physik,* passed to L. W. Gilbert, a young colleague of his at Halle University. Recognizing that the richest source of material for the journal would continue to be foreign journals,[13] Gilbert undertook an enormous work of translating, for which he was well prepared. Being of Huguenot descent, he spoke excellent French, so that on his obligatory scientific pilgrimage to Paris he was received in an especially friendly way by Laplace, Biot, and other scientists there. Since he spoke English, Dutch, and Italian as well, he could also read physics publications in those languages.[14] In the *Annalen*, he promised that German researches by the "best physicists" would stand honorably beside the best foreign work such as Henry Cavendish's experiment on the density of the Earth, published the year before, with its admirable "exactness."[15]

Gilbert's editorship of the *Annalen* established his name, but it left him little time for anything else. After 25 years, he said that the journal had swallowed up all of his

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[11] F. A. C. Gren's foreword in 1790 to the first volume of *Journal der Physik*, his original title for the *Annalen*. His paper was "Prüfung der neuern Theorien über Feuer, Wärme, Brennstoff und Luft," 3–44, 189–201. In it Gren disputed Adair Crawford's theory of heat and combustion, which he called a "lazy philosophy" (30). Gren was critical of theories based on too few experiments, which only revealed the experimenters' narrowness of view and ignorance of the many already established laws of nature (26–27). He used his journal to uphold the phlogiston theory of chemistry against Lavoisier's new chemistry, defending his own theory of the negative gravity of phlogiston against the criticism of J. T. Mayer, professor of mathematics and natural philosophy at Erlangen University. Mayer, whose work and criticism always hinged on the question of whether or not a theory or hypothesis was in conflict with the laws of mechanics, soon persuaded Gren on mechanical grounds to retract his theory of negative gravity (200), though he continued to defend phlogiston until his conversion to the new chemistry in 1794 (*Journal der Physik* 8 [1794]: 14). The decision between the old and the new chemistry was displaced as the central theme of Gren's journal by the subject of animal electricity after Galvani's experiments were reported in the journal in 1792. Throughout the *Journal der Physik*—in the articles, extracts, book reviews, letters to the editor, and annotations by the editor—rival theories in physics, chemistry, and physiology were compared in light of the results of experiments. Both theories and experiments were needed to achieve what Gren said was the "goal" of the natural philosopher: the establishment of the general laws of nature (*Journal der Physik* 8 [1790]: 25–25, 208).

[12] The subscription list at the beginning of the first volume shows that there were fewer than a hundred subscribers, among whom were fourteen professors.

[13] L. W. Gilbert, "Vorrede," *Ann.* 1 (1799), first three pages.

[14] Obituary of L. W. Gilbert, *Neuer Nekrolog* 2 (1824): 491, 493.

[15] Gilbert, "Vorrede."

other plans. Apart from his translations in the journal, his physical writings were limited to a very few original papers. His obituary said that he failed as an original experimenter, but that he was a good teacher, for which reason he was a good editor.[16] In effect, through his journal, he taught physics to Germany.[17] He gave shape to the science by grouping researches on like themes, by selecting from current work the most significant, by translating—and more than translating, rewriting—foreign work, by commenting on it extensively in footnotes, and in general by constantly guiding his readers through demanding material, as a good teacher does. Concerned that his readers could understand the papers, he omitted most mathematical discussion in the journal, even though he himself was mathematically proficient.[18] The mathematical electrodynamic theory that Ampère presented to the Paris Academy of Sciences in late 1820, which treated the force between current-carrying wires, Gilbert brought out in translation in the *Annalen* early the next year without a single mathematical formula.[19] Over time, he published a good deal of French mathematical theory; for example, Laplace's capillarity theory, in the form Biot gave to it, which stimulated much interest in Germany. In response to a criticism by a reader, Gilbert said that he not only had adapted the papers of foreigners himself but he had "also been at pains to bring the greatest possible clarity to the presentations and for each issue to find such a selection that the ... papers interlock to a certain degree and each promotes the understanding and interest of the other."[20] That is an accurate description of the character that Gilbert gave the *Annalen* during his long tenure as its editor. German physics was in his debt.

After Gilbert's death in 1824, a number of German physics professors wanted to take over the editorship,[21] but it went instead to a Berlin student who had published

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[16] Gilbert, *Neuer Nekrolog*, 484, 488.

[17] Gilbert's colleagues who wrote textbooks on physics sent their readers to Gilbert's accounts of recent physics. See, for example, Fischer, *Lehrbuch*, vol. 2, 81–82, 120. Mayer referred the reader to the "excellent annals of physics of Prof. Gilbert" in his announcement of the third edition of his *Anfangsgründe* in the *Göttingische geleherte Anzeigen*, 1812, vol. 2, 1273–74.

[18] Obituary of L. W. Gilbert, *Ann.* 76 (1824): 468–69.

[19] A. M. Ampère, "Ueber die gegenseitigen Wirkungen, welche auf einander ausüben zwei electrische Ströme, ein electrischer Strom und ein Magnet oder die Erdkugel, und zwei Magnete," *Ann.* 67 (1821): 113–63, 225–58.

[20] Gilbert's remarks, *Ann.* 69 (1821): 65.

[21] When Gilbert became ordinary professor of physics at Leipzig University in 1811, he moved the *Annalen* with him. He was succeeded in his chair by Brandes, who, according to Poggendorff, wanted to become editor of the *Annalen* as well, as did Kastner in Erlangen and Muncke in Heidelberg (Emil Frommel, *Johann Christian Poggendorff* [Berlin, 1877], 70). All three of these prospective editors were authors of several textbooks, some of them major. In the same year that Poggendorff took over the *Annalen*, Kastner began editing the *Archiv für gesammte Naturlehre*, and in the following year Brandes along with others began bringing out a new edition of *Gehler's Physikalisches Wörterbuch*. The editing of the *Annalen* was a type of literary activity that extended a professor's work.

only one paper several years before, Johann Christian Poggendorff. To persuade the publisher of the *Annalen* that he was the right man for the job, he described his earlier plan to launch a new "physical-chemical" journal, which he now wanted to combine with the *Annalen*, and that way avoid harming science by needlessly multiplying its journals. As he envisioned it, his journal was to cover principally physics and chemistry, "in their entire scientific compass," and mathematics, "which can never be given enough consideration"; it was also to cover fields that touched on these, by which Poggendorff meant most of the other physical sciences and their applications, which taken altogether constituted a "whole" whose borders "cannot be marked off precisely."[22]

Poggendorff's actual, more realistic journal came out as the *Annalen der Physik und Chemie*, which reflected his view that physics and chemistry in their present state could not be separated. In announcing the journal, he said that it would exclude pure mathematics, but that it would accept mathematics in any other form insofar as it made experiments more precise or linked experimental facts through the principles of mechanics.[23] Two years later, in 1826, two new journals that accepted mathematical physics apeared, *Zeitschrift für Physik und Mathematik,* edited by the Austrians Baumgartner and Ettingshausen, and *Journal für die reine und angewandte Mathematik,* edited by the Berlin mathematician A. L. Crelle. (These journals began the year before Ohm published his theory of galvanic currents, discussed below, suggesting that his mathematical physics belonged to a current of thought at the time.) From the beginning of Poggendorff's editorship, the journal contained a fair amount of mathematical physics, some of it highly mathematical. The most capable mathematical physicists in Germany, among them Ohm, Weber, Neumann, and August Seebeck, published their work in the *Annalen der Physik und Chemie*.

For Poggendorff, as for Gren and Gilbert before him, the job of editing the *Annalen* was onerous. Having knowledge of languages, as he told the publisher, he continued the practice of including extensive translations of foreign research. Occasionally he gave summaries of work, and he also annotated work, though less extensively than Gilbert. He rigorously excluded all "personal elements,"[24] restricting polemics between researchers to a minimum. In passing from Gilbert to Poggendorff, the *Annalen* became the severely scientific journal it remained, as detached from personality as can be expected of a journal in which complete control is invested in one man. Poggendorff could say on the fiftieth anniversary of his

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[22] J. C. Poggendorff to J. A. Barth, 16 March 1824 and 3 April 1824, quoted in Frommel, *Poggendorff*, 25–26, 31–25, on 32.

[23] J. C. Poggendorff's foreword to the first volume of *Annalen der Physik und Chemie* (1824), v–viii.

[24] Wiedemann, "Vorwort."

editorship that the *Annalen* had become the "only organ of physics for Germany, which, thank God, it has remained until the present day."[25]

People who knew Poggendorff were impressed by the sense of order that filled his whole being.[26] It equipped him for editing, and in time it also allowed him to become one of the most prolific authors of original research to appear in his journal. His editing always came first, and from early on he foresaw, correctly, that this was to be his greatest contribution to physics in Germany. He was elected a regular member of the Prussian Academy of Sciences, and he was also appointed an extraordinary professor at Berlin University, but unlike his predecessors he did not seek an academic career, turning down many offers from universities. He was financially able to pursue his editing of the *Annalen* and his researches undisturbed by the demanding official duties of an ordinary professor.[27]

German physicists who published their work in the *Annalen*, and sometimes elsewhere, were often employed as teachers. About half of those who published there in 1800–30 were professors at universities, and the rest were independent researchers, academicians, or physicists just beginning their teaching careers. The professors included the textbook writers we have discussed: Erman, Mayer, Pfaff, Schmidt, and Schweigger. Representing the last of an eighteenth-century tradition, the prominent independent researchers included E. F. F. Chladni, Thomas Seebeck in the early years of his career, Johan Wilhelm Ritter for a time, Peter Riess, and the universal giants J. W. Goethe and Alexander von Humboldt. Ritter and Julius Konrad Yelin worked at the Bavarian Academy of Sciences in Munich; Poggendorff and, in his last years, Seebeck worked at the Prussian Academy of Sciences in Berlin. Carl Wilhelm Boeckmann taught at the polytechnic school in Karlsruhe. The young university researchers who had not yet attained chairs before 1830 included Ohm, Weber, and Gustav Theodor Fechner.[28] We distinguish between independent and university researchers, but the dearth of research facilities and of financial support for research at the universities during these years made university physicists all but "independent" with respect to their resources too. The main benefit they derived from their positions was income, though to do significant research in addition to their teaching they had to have private means along with uncommon interest and initiative.

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[25] From Poggendorff's talk at the fiftieth anniversary of his editorship on 28 February 1874 (repr. Frommel, *Poggendorff*, 68–72, on 71).

[26] W. Baretin, "Johann Christian Poggendorff," *Ann.* 160 (1877): v–xxiv, on ix.

[27] Baretin, "Poggendorff," ix; Frommel, *Poggendorff*, 36.

[28] A contemporary, Brandes, included in his *Vorlesungen* (1830–32) an account of the researches of fellow Germans Fechner, Ohm, Thomas Seebeck, Erman, C. H. Pfaff, Bohnenberger (who was known for his instruments), Weber, Chladni, G. G. Schmidt, Muncke, Schweigger, Humboldt, Ritter, and in a negative remark Pohl. But he gave much more space to the researches of French physicists. Earlier textbooks on physics regularly mentioned Goethe's physical researches, both to criticize and to praise, but after Goethe's death the long accounts of his theory of color, sometimes extending to several pages, stopped.

Independent or not, physics researchers pursued a calling for which there was then no set course of training or regular career. Usually they started out by preparing for one of the traditional fields: Chaldni in law, Erman in theology, Pfaff, Seebeck, and Fechner in medicine, Schweigger in philology, Humboldt in mining, and Poggendorff in pharmacy. Later they took up physical research, often by the example of other scientists. Weber was drawn to it by Chladni's dedication to a "purely scientific life," Ritter and Riess by Humboldt's enthusiasm and often practical support.[29] Others were drawn by a great discovery announced while they were preparing for another field: C. H. Pfaff by Volta's discoveries, Thomas Seebeck by Volta's and E. L. Malus's discoveries.[30]

Just as there was no prescribed plan for study, there was no prescribed access to the means for doing research. Whether the young physicist pursued a university career in the hope of rising one day to the directorship of a physical cabinet or whether he derived his income from a source outside a university, to begin with he had to provide for his space, apparatus, books, periodicals, and perhaps even assistants, all of which were expensive. With a beginning university teacher's small income from student fees and an occasional extraordinary stipend of, commonly, 100 thaler, the researcher's "laboratory" was likely to be his room, and his research equipment was certain to be modest. There was always the chance that the senior physicist might invite him to work in his laboratory or lend him some instruments, but it rarely worked that way. The other main opportunity for a beginning physics researcher to earn a living and perhaps at the same time to be near instruments was to teach at one of the secondary or technical schools that had equipment; Schweigger, Erman, and Ohm tried this combination with varying success. Then there was the possibility of pooling resources. Pfaff, impressed by the great laboratories he saw in Paris on a visit there in 1801, together with five other young men rented a laboratory of their own.[31]

Materials and facilities for research remained a problem even after a university physicist had been given access to a physical cabinet, for he was not encouraged to

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[29] Carl Gustav Carus, *Lebenserrinerungen und Denkwürdigkeiten, nach der zweibändigen Originalausgabe von 1865/66*, ed. Elmar Jansen (Weimar: Kiepenheuer, 1966), vol. 1, 264; K. "Riess: Peter Theophil," *ADB* 28 (1970): 584–86; Robert J. McRae, "Ritter, Johann Wilhelm," *DSB* 11 (1975): 473–75, on 473.

[30] Kuno Fischer, *Erinnerungen an Moritz Seebeck, wirkl. Geheimerath und Curator der Universität Jena, nebst einem Anhange: Goethe und Thomas Seebeck* (Heidelberg, 1886), 9; C. H. Pfaff to his brother, the mathematician J. F. Pfaff, Stuttgart, 1792, in Johann Friedrich Pfaff, *Sammlung von Briefen gewechselt zwischen Johann Friedrich Pfaff und Herzog Carl von Würtemberg, F. Bouterwek, A. v. Humboldt, A. G. Kästner, und Anderen*, ed. Carl Pfaff (Leipzig, 1853), 78, 105.

[31] C. H. Pfaff to J. F. Pfaff, Paris, 1801, in J. F. Pfaff, *Sammlung von Briefen*, 159–60.

view the purpose of the instruments as furthering his private research.[32] He might use what instruments he had, but new acquisitions had to be justified first of all by teaching needs. Since the institute budget was small and the instrument collection was expected to have roughly equal strength in all parts of physics for lecture demonstrations, it was difficult for the director to acquire the apparatus he needed for research on a particular subject.

For the independent researcher too after he had used what private means he could spare—an inheritance or dowry as in the case of Ritter, Humboldt, Chladni, and Seebeck, or family wealth as in the case of Riess—the problem of materials and facilities remained. Seebeck spoke in his letters of years of idleness owing to lack of laboratory facilities. Weber was saddened when he reflected on what Chladni, who made frequent lecture tours to earn money, might have accomplished under better conditions of work.[33] What the independent researchers could and usually did do that distinguished them from most university physicists was to specialize in their research: Chladni in acoustics, Seebeck in optics and thermoelectricity, Ritter in electricity, and Riess in electromagnetism.

The career of Paul Erman shows the determination that was required to gain a reputation for research even by a physicist with a desirable position. In 1810 he became ordinary professor of physics at Berlin University, and he continued with his previous teaching positions at the French gymnasium and at a military school. The Prussian state paid him a salary of 500 thaler, which was 300 thaler less than his salary as a secondary school teacher. From the beginning of his teaching, he collected materials for research and carried out experiments in his home, with considerable success. For his research, the Institut National in Paris awarded him its galvanic prize of Fr.3000 in 1807. He was, according to Humboldt, regarded by Laplace and other leading French scientists as the "first physicist of Germany," but none of this recognition earned him any support from Prussia for his research, which he continued to carry out under the same conditions as before. His ideal was a "life dedicated solely to knowledge . . . alien to all drives of action, free from all ordered [by others] business, insatiable only with regard to knowledge,

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[32] In Baden, the government told Heidelberg University "that we consider *discovery* in science as the *business* of the *scholar*, but not as that of the *teacher*," which is what a university professor was. Otto Lehmann, "Geschichte des physikalischen Instituts der technischen Hochschule Karlsruhe," in *Festgabe zum Jubiläum der vierzigjährigen Regierung Seiner Königlichen Hoheit des Grossherzogs Friedrich von Baden* (Karlsruhe, 1892), 207–65, on 257. Lehmann assumed that the resistance offered by the physics professor at the Karlsruhe Polytechnic to having the state's physics cabinet, which was under his direction, be made the polytechnic's was due to his understanding that he would no longer be able to use the cabinet for research once it belonged to a teaching institution, whereas research was the declared purpose of the state cabinet.

[33] Thomas Seebeck to Goethe, 25 April 1812 and 11 December 1819, in *Goethe's Naturwissenschaftliche Correspondenz (1812–1832)*, ed. F. T. Bratranek (Leipzig, 1874), vol. 2, 317, 331; Wilhelm Bernhardt, *Dr. Ernst Chladni, der Akustiker* (Wittenberg, 1856); Eugen Lommel, "Chladni: Ernst Florens Friedrich," *ADB* 4 (1968): 124–26; Wilhelm Weber, "Lebensbild E. F. F. Chladni's," in Wilhelm Weber's *Werke*, vol. 1, *Akustik, Mechanik, Optik und Wärmelehre*, ed. Woldemar Voigt (Berlin, 1892), 168–97, on 172.

inexhaustible only in research." He advised scientists to reduce their personal needs as much as possible to gain the precious independence he did not have. When the Royal Society of London elected him fellow in 1827, he wrote to his son of his feeling of shame at being honored undeservedly, conscious of what he "*could* have achieved under favorable circumstances."[34] He saw himself as primarily a witness to a great time in physics, not a participant. He wrote to Oersted in 1835 that burdened by his various duties as "a conscientious teacher of physics," he had his hands full just following Oersted's work and the work of Faraday's and Fresnel's. He could only thank God that he was allowed to live at the same time as these heroes of physics.[35] Erman holds a minor honorary place in this book: the theoretical physicist Max Planck said that Erman was the first physicist to lecture on theory in Berlin,[36] and Erman was instrumental in freeing Ohm from teaching for a year to enable him to complete his theory of the galvanic circuit.

In accepting Erman's account of the strained circumstances under which he was forced to carry out his work, and accepting similar accounts by physicists who were contemporaries of his, we acknowledge that we trust to the words of interested parties, which may be unreliable. But because we find consistency between the accounts, and because the accounts agree with the information we have about the funding of physics and of the circumstances of teaching physics, we believe we do not misrepresent the work of early German physicists in citing their testimony.

Similar to Erman's efforts to keep up with the new work, most German physics research consisted of confirming, varying, and extending experiments reported from abroad. Mainly it dealt with subjects that were then receiving the most attention: electricity and galvanism above all, but also magnetism and heat. We get an idea of the character of their work by looking at one subject they cultivated intensively, electromagnetism.[37]

One problem German researchers took up in electromagnetism was the effects of their instruments or apparatus on the observed phenomena, an indication of a circumspect experimental practice: Schweigger, Schmidt, and Yelin studied the influence of the voltaic apparatus on the motions of the magnetic needle; Poggendorff and Boeckmann studied the influence of the composition of the solid conductors, and Pfaff and Poggendorff the influence of the composition of the fluid

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[34] Wilhelm Erman, "Paul Erman. Ein Berliner Gelehrtenleben 1764–1851," *Schriften des Vereins für die Geschichte Berlins* 53 (1927): 1–264, on 54, 114, 122–23, 181–82, 198, 251.

[35] Erman to Oersted, 1 August 1835 and 2 April 1836, in Hans Christian Oersted, *Correspondence de H. C. Oersted avec divers savants*, ed. M. C. Harding (Copenhagen: H. Aschehoug, 1920), vol. 2, 318–19.

[36] Max Planck, "Das Institut für theoretische Physik," in *Geschichte der Königlichen Friedrich-Wilhelms-Universität zu Berlin,* by Max Lenz, 4 vols in 5. (Halle a. d. S.: Buchhandlung des Waisenhauses, 1910–18), vol. 3, *Wissenschaftliche Anstalten. Spruchkollegium. Statistik* (1910), 276–78, on 276.

[37] Our main source is the account C. H. Pfaff wrote 4 years after Oersted's discovery, describing the work that followed: *Der Elektro-Magnetismus, eine historisch-kritische Darstellung der bisherigen Entdeckungen auf dem Gebiete desselben, nebst eigenthümlichen Versuchen* (Hamburg, 1824).

conductors.[38] A second, related problem was the effects of multiplying the electromagnetic action by coiling the wire, the principle of a new instrument, the multiplicator. Schweigger, an inventor of the instrument, used it to show the effect of the direction of the electric current on the motions of the magnetic needle; Poggendorff, a co-inventor, studied the effect of different numbers of coils on the strength of the electromagnetic action; Pfaff considered theoretically the action of the multiplicator, which depends on the spatial relations of the electric current and magnetic needle but not on the electrical properties of the conductor.[39]

Another problem of electromagnetism of interest to German researchers was the relationship between the chemical process in voltaic apparatus and the magnetic motions it gives rise to. This appealed to those who wanted to show a connection between "chemism" and magnetism, anticipating that the magnetic needle might prove to be a more sensitive reagent than chemical ones. It also appealed to others who, conversely, wanted to show that there is no connection between chemism and magnetism; for as Pfaff pointed out, not just galvanism, which involves a chemical process, but other electricity too produces magnetic actions.[40] Generally speaking, German researchers were interested in the connections between the parts of physical science. Independently of Ampère, Erman worked on a problem suggested by Oersted's discovery, the effect of magnetism on the motion of an electric conductor, furthering the link between electricity and magnetism.[41] Seebeck established thermoelectricity, connecting electricity with heat.[42] Erman and Seebeck, and also Pfaff and Yelin, tried to determine if galvanism and electricity are the same.[43]

3.2 Ohm's Research

A claim has been made that Ohm's celebrated theory was not physics but mathematics, that Ohm wore two hats, one for the mathematician who wrote the theoretical work *Galvanic Circuit* and the other for the experimenter who discovered the experimental relationship between voltage, current, and resistance.[44] Our detailed account of his work in this chapter will show that his galvanic theory was physics, as it was understood to be then and later. Ohm's career is intelligible when his two activities are seen as having a common objective: understanding the regularities of

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[38] Ibid., 71–77, 91, 95.

[39] Ibid., 71, 102, 108, 115.

[40] Ibid., 71, 78–79.

[41] Ibid., 135.

[42] For example, Kuno Fischer, *Seebeck*, 9.

[43] Pfaff, *Elektro-Magnetismus*, 182–83.

[44] This claim is made by Elizabeth Garber, *The Language of Physics: The Calculus and the Development of Theoretical Physics in Europe, 1750–1914* (Boston, Basel, Berlin:Birkhauser, 1999), 156–58.

nature, and indeed that is how he saw his work. He described himself as an "experimenting theorist," a single investigator of nature, not two, who brought to his work the methods of experiment and mathematical theory.[45] We see this in his educational work too, where he brought the teaching of mathematics and physics close together. In the case of *Galvanic Circuit*, the result was the derivation of a mathematical law of physics, grounded in experiment, subject to experimental confirmation or refutation, and capable of manifold applications in physics, as the future demonstrated.

Because we consider Ohm's work a contribution to what came to be called "theoretical physics," we are interested in his decision to become a physicist. His original impulse came from home. His father, a master locksmith with wide intellectual curiosity, on his own studied mathematics, education, and philosophy, particularly Kant's. He befriended the mathematicians at the local university in Erlangen, and with his sons Georg and Martin he studied Euler's works on analysis. His interest was continued by his sons: Martin became a university professor of mathematics, and Georg began his career as a mathematics teacher with strong interests in education and philosophy. After leaving the gymnasium, Ohm entered Erlangen University, where he took up the subjects that would interest him for the rest of his life: mathematics, physics, and philosophy, all appealing to him because of their "important influences on absolute *Menschenbildung*." On the recommendation of the Erlangen mathematicians, he studied Euler, Laplace, and S. F. Lacroix on his own.[46] This was important preparation for the mathematical and experimental investigations he would carry out in what free time he had during and in between his various teaching jobs.

As a researcher, Ohm began as an experimentalist, choosing a subject in which he thought he would meet with less competition than in others.[47] In 1825 and 1826, he carried out "innumerable" experiments on galvanic currents in metals, leading to his first publications, which appeared in Schweigger's and Poggendorff's journals. We follow his reasoning here. To determine the "law" by which metals conduct galvanic current, he performed three sets of experiments. The goal of the first set was to find the relation of lengths of wires to their conductivity and of the second to find the relative conductivities of wires of different metals. In the third set of experiments, he replaced the source, a wet-cell battery, with a thermocouple, which he used in determining "Ohm's law." In the first two sets, his procedure was to insert lengths of wires between the terminals of a wet cell and to measure the galvanic current in them by a Coulomb torsion balance; with this instrument, a magnetic needle suspended over the current by a fine wire is turned by the magnetic action of the current, the torsion of the suspension wire being the measure of the action. From the known fact that the magnetic effect of the current in a wire

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[45] Georg Simon Ohm, "Galvanische Einzelheiten," *Ann.* 63 (1844): 389–405; in *Ges. Abh.*, 650–64, on 650.

[46] Heinrich von Füchtbauer, *Georg Simon Ohm; ein Forscher wächst aus seiner Väter Art,* 2nd ed. (Bonn: Ferdinand Dümmler, 1947), 39–40, 82.

[47] Ibid., 142.

decreases as the conductivity of the wire decreases, Ohm correlated the "loss of force" of the current with the length and kind of wire it passed through. In the first set of experiments, he found that his measurements were matched by a formula: $v = 0.41 \log (1 + x)$, where v is the fractional "loss of force" of the current (instead of the intensity of the current, as we would write), and x is the length of a test conductor (a measure of resistance). In the familiar way of developing a physical law, he differentiated the empirical formula to obtain a "differential equation," $dv = m\, dx/(1 + x)$, to which he gave a more general form, $dv = m\, dx/(a + x)$, where m and a are constants. From this equation he arrived at the final result of his paper in 1825, $v = m \log (1 + x/a)$, which he called a "law," confirmed by experience. He would not leave his law in this form.[48] We will not go through his experiments on different metals.

In a paper in Schweigger's journal the next year, 1826, Ohm reported on his third set of experiments. Having exchanged the wet-cell for Seebeck's recently invented "thermo-element," which gave a steadier current, he expressed his results with a different formula, using a different variable, X, for the strength of the magnetic action of the current instead of the loss of force: $X = a/(b + x)$, where x is the length of the wire, a is a constant proportional to the temperature difference of the thermocouple junctions, a measure of the electromotive, or "exciting," force, and b is the "equivalent length," or internal resistance, of the rest of the circuit. Satisfied that this law agrees with all of the facts of thermoelectric circuits, he concluded that the "various actions of the galvanic circuit arrange themselves in colorful diversity to a beautiful whole." We recognize this equation as Ohm's law: current is equal to the ratio of voltage to resistance.[49]

Later the same year, he published a paper in the *Annalen* in which he stated two laws. The first is the law above, written $X = kwa/l$, where X is the current, k is the conductivity, w is the cross-section of the conductor, a is the difference in electric "tension" at the endpoints of the conductor, and l is the length of the conductor. He said that this law contains almost all of the phenomena that depend on current intensities, and "yet it is only the special expression of a far more general statement," a reference evidently to his theory the next year. Ohm's second law relates the "electroscopic force" u at any point x along the conductor to the length of the conductor and the difference in tension at the endpoints: $u - c = xa/l$, where c is a constant independent of x. Ohm said that the two laws connect all of the elements of the galvanic circuit, but he was not yet finished. He reserved "for a more thorough

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[48] Georg Simon Ohm, "Vorläufige Anzeige des Gesetzes, nach welchem Metalle die Contaktelekricität leiten," *Ann.* 4 (1825): 79–88, on 81–82, 84. Ohm's experiments are analyzed in Morton L. Schagrin, "Resistance to Ohm's Law," *Am. J. Phys.* 31 (1963): 536–47, especially 544; and in Kenneth L. Caneva, "Ohm, Georg Simon," *DSB* 10 (1981): 186–94, on 189.

[49] Georg Simon Ohm, "Bestimmung des Gesetzes, nach welchem Metalle die Contaktelekricität leiten, nebst einem Entwurf zu einer Theorie des Voltaischen Apparates und des Schweiggerschen Multiplicators," *Jahrb. d. Chem. u. Phys.* 46 (1826): 137–66, on 151, 154, 166. In other words, $b + x$ measures the total resistance of the circuit, which today we write as R; a measures the electromotive force of the battery, or E; and if the magnetic action is proportional to the current I, the formula Ohm wrote in 1826 is, in appropriate units, the formula we write today, $I = E/R$.

work" the derivation of the laws and their connection with natural phenomena. He hoped soon to be granted the necessary leisure to carry through this work, which would be his major publication the following year, *Galvanic Circuit*.[50]

In 1830 Ohm said that if he had had more success than others in explaining certain phenomena, it was because he was constantly guided by "theory."[51] He made the remark in connection with uni-polar action, but it applied as well to his other researches, including his research on the laws of electric conduction. His notebooks reveal that in the course of his work on the galvanic circuit, he made a number of attempts to give a "theoretical model" to explain the phenomena. We see that his work exhibited an "interplay of experiment and analogy-based theory": from an analogy between the electric current and fluid dynamics, he arrived at the law he tested experimentally,[52] and from an analogy between electric current and heat flow, he derived theoretically his general galvanic law the following year.

More than talent and desire are needed to produce a major work in physics. Time is needed as well, and Ohm asked the Prussian minister of culture for a year off from his teaching at a secondary school in Cologne. His application contained an element of calculation: he said that he regretted that the French had recently dominated physics, and that to help overcome this inferiority he had been studying mathematical works by Laplace, Fourier, Poisson, Fresnel, and other French leaders. He said that he had been doing experimental work recently, but that he had a mathematical theory of galvanic current needing only time to complete, and that he had a theory of light in progress as well. On the recommendation of the professor of physics in Berlin, Erman, the minister approved his request. With half salary, Ohm went off to Berlin in 1826 to live in his brother's house, where he had a small apartment with space for doing experiments. Under these improved working conditions, he developed the mathematical theory of the galvanic current, his brother Martin likely helping him with the mathematics.[53] The result was *Die galvanische Kette, mathematisch bearbeitet* [*Galvanic Circuit, Investigated Mathematically*].[54] Ohm published it separately, calling it his *Buchlein* [little book].

The expression "investigated mathematically" in the title of Ohm's book referred to his method, which was to deduce the properties of the galvanic circuit

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[50] Georg Simon Ohm, "Versuch einer Theorie der durch galvanische Kräfte hervorgebrachten elektroskopischen Erscheinungen," *Ann.* 6 (1826): 459–69, on 460, 463, and *Ann.*7 (1826): 45–54, 117–18.

[51] Georg Simon Ohm, "Versuche zu einer näheren Bestimmung der Natur unipolarer Leiter," *Journ. f. Chem. u. Phys. 59* (1830): 385–435, and 60 (1830): 32–59, reprinted in *Gesammelte Abhandlungen,* ed. Eugen Lommel (Leipzig, 1892), 344–401, on 344.

[52] John L. McKnight, "Laboratory Notebooks of G. S. Ohm: A Case Study in Experimental Method," *Am. J. Phys*. 35 (1967): 113–14.

[53] Füchtbauer, *Ohm*, 151–56; Ohm, *Nachlass*, 70–74.

[54] Georg Simon Ohm, *Die galvanische Kette, mathematisch bearbeitet* (Berlin, 1827); repr. Ohm's *Gesammelte Abhandlungen*, 61–186; "The Galvanic Circuit Investigated Mathematically," in *Taylor's Scientific Memoirs*, ed. Richard Taylor, trans. W. Francis (London, 1841), vol. 2, 401–506.

from a set of three “fundamental laws.” He began with the definition of the central concept of his theory, “electroscopic force,” as the force with which an electroscope is attracted to or repelled from any charged body that is brought into contact with it. The first law states that electricity passes only between adjacent particles of the conductor and that the quantity passed is proportional to the difference of electroscopic force at the two particles. For an analogy, Ohm referred to Fourier’s heat theory, in which the quantity of caloric passed between two adjacent particles is proportional to the difference of their temperatures. The second law, supported by Coulomb’s experiments, states that the loss of electricity in unit time from the conductor to the air is proportional to the electroscopic force, to the amount of surface exposed, and to a coefficient that depends on the air. Acknowledging that the second law has little bearing on the phenomena of galvanic currents, Ohm included it to make the theory complete, in analogy to Fourier’s theory of heat. The third law states that two bodies in contact maintain the same difference of electroscopic force at their common surface, the empirical basis of the contact theory of the battery. Upon expressing the laws in differential terms, he derived equations for electric currents analogous to Fourier’s and Poisson’s for heat.[55]

Following Fourier’s method, Ohm mathematically divided the conductor, a wire in this case, into infinitely thin discs and then calculated the quantity of electricity transferred per unit time across the parallel surfaces of the discs and outward through the edges of the discs. In this way, he derived a second-order partial differential equation analogous to Fourier’s for the conduction of heat, with electroscopic force replacing temperature. The electroscopic force u at each point x of a conductor carrying a current is:

$$\gamma \frac{du}{dt} = \chi \frac{d^2u}{dx^2} - \frac{bc}{\omega} u,$$

where the several constants refer to electrical and geometrical properties. This is the law of galvanic circuits, the more general statement he referred to the year before. Now having reformulated the physical problem, Ohm found a general solution to the differential equation, written as an infinite series of trigonometric functions with damping coefficients. Then limiting it to the practical case in which there is no loss of electricity to the air and the electroscopic force does not vary with time, Ohm derived a simple formula for the “electroscopic force,” the second law of his paper the year before. From that result together with his definition of current, he derived a simple formula for the current, the first law.[56]

The electroscopic law contains a “conceptual innovation,” indications of which were evident in Ohm’s earlier researches. He accepted the standard use of the

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[55] Ohm, *Die galvanische Kette*, 62–64.

[56] Ohm wrote the law as $S = A/L$, where A is the sum of all “tensions” in the circuit, L is the “reduced length” of the circuit, which is proportional to its resistance, and S is the “electric current.”

concept of "tension" (our voltage) as the difference in tension between terminals of the source, but he also attributed a tension, or "electroscopic force," to each point of a closed-circuit, an idea foreign to the older theory. He acknowledged this innovation in the first of the three fundamental laws on which he based his theory: this law, which expresses the spread of electricity, he characterized as "at least in part theoretical," that is, hypothetical.[57]

Ohm's theory of the galvanic circuit made use of relatively advanced mathematics,[58] but it was not the work of a mathematician. He did not contribute to the development of analysis, as Gauss and other mathematicians did. He did not question the existence of solutions to differential equations, nor did he examine the convergence of infinite Fourier series, treating them instead as finite polynomials. It remained for the mathematician Dirichlet, one of Ohm's students at the Cologne gymnasium, to construct an adequate theory of Fourier series.[59] (Ohm's approach to mathematics was standard in physics. Boltzmann said that in developing the theory of mechanics, he put aside all doubts about the strength of the mathematical proofs, referring his readers to the mathematicians' literature.)[60] Ohm's interest lay in the application of differential equations to physical problems, and for this it was enough that he knew the relevant mathematical texts by d'Alembert, Euler, Fourier, and Laplace.[61] He worked out the mathematical theory of galvanic currents with the understanding that mathematical physics must always return to experiment, as his *Galvanic Circuit* did. We can say that Ohm carried out research in mathematical physics in the physicists' way. Mathematics, he said, illuminates the "dark places" of physics.[62]

When *Galvanic Circuit* appeared, few physicists in Germany knew enough mathematics to read it easily if at all. Journal editors were afraid that their readers could not understand papers containing the simplest mathematics, as Ohm complained.[63] For reviewing, he sent a copy of the book to Schweigger at Halle University, who did not see the point of the mathematical treatment. To have it evaluated, the Prussian minister of culture sent a copy to L. F. Kämtz, Schweigger's colleague at Halle, whose cautious review suggested that he did not understand the mathematical derivation well. In Berlin, which sorely needed a "mathematical physicist,"[64] Ohm's work received its best known and, to him, irritating review.

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[57] Schagrin, "Resistance," 546.

[58] Ohm, *Die galvanische Kette*, 64.

[59] Joseph Heinrichs, "Ohm im mathematisch-naturwissenschaftlichen Gedankenkreis seiner Zeit," in *Georg Simon Ohm als Lehrer und Forscher in Köln 1817 bis 1826* (Köln: Kölnischer Geschichtsverein, 1939), 254–70, on 260–61.

[60] Ludwig Boltzmann, *Vorlesungen über die Prinzipe der Mechanik* (Leipzig: J. A. Barth, 1904), vol. 2, v.

[61] Heinrichs, "Ohm," 260–61.

[62] Ohm, *Nachlass*, 71. In *Die galvanische Kette,* Ohm cited experimental work by Erman, Ritter, and other German physicists, showing his physical perspective.

[63] Füchtbauer, *Ohm*, 157–58.

[64] According to Martin Ohm. Füchtbauer, *Ohm*, 157, 167–68.

Georg Friedrich Pohl, a gymnasium teacher and extraordinary professor at the University in Berlin, who was neither a mathematician nor a typical Berlin physicist, belonged to an association formed "especially under Hegel's auspices," which published a journal for scientific criticism.[65] As a reviewer for this journal, his task was to judge scientific work from a "higher point of view" such as a "simple and dignified view of nature," a policy he applied to *Galvanic Circuit*. More than most German physicists, he adopted the language of nature philosophy, complaining that Ohm had not paid attention to the "essence" of the electrical circuit but had merely expressed some properties of electricity in formulas. That was no achievement but only a repeat of Fourier's and Poisson's work in another part of physics. Pohl likened Ohm's mathematical theory to a report of a trip that enumerates the traveler's stops and his velocity between them but nothing more.[66] Having only recently completed a work of his own on the subject, "The Process of the Galvanic Circuit," which Hegel was eager to see reviewed in their journal, Pohl had understandable reasons for reading Ohm's work with a critical eye. To this circumstance, it should be added that Ohm's theory with its violation of the common distinction at the time between current and tension electricity was not easy for everyone to accept or understand.[67]

With the hope of receiving a knowledgeable reading, Ohm sent his book to the professor of chemistry and physics at Erlangen, K. W. G. Kastner, to be reviewed in his journal, *Archiv für die gesammte Naturlehre*. Kastner approached his colleague the professor of mathematics Wilhelm Pfaff, but he did not know what to do with it either, professing ignorance of the literature and of how Laplace and his circle would have proceeded in this instance. The review that appeared under Pfaff's name was apparently written by Ohm himself, after his brother had interceded. The review was of course favorable, but a favorable review does not necessarily make a book successful. Sales of *Galvanic Circuit* were unimpressive, and Ohm paid friends to order the book from out of town to make a better impression on his publisher. The book was in print for 8 years, then not again for 60 years, though in the meantime it had come out in several translations.[68] Ohm sent free copies to everyone who might help him, for he dearly did not want to return to his mathematics teaching in Cologne.

Ohm's experiments leading to his law of electric currents were not enough to satisfy his contemporaries. The necessary additional experiments were made by

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[65] G. F. Pohl to Franz Neumann, 1 January 1828 (the letter is dated 1827 in error), Neumann Papers, Göttingen UB, Ms. Dept.

[66] Füchtbauer, *Ohm*, 168. In response to Pohl's belittling of his mathematics, Ohm denied that he was a "blind adherent" to Fourier. Because of the special case in electricity he treated, his formulas had "entirely no significance in heat theory" (Georg Simon Ohm, "Nachträge zu Ohm's mathematischer Bearbeiten der galvanischen Kette," *Archiv für die gesammte Naturlehre* 14 [1828]: 475–93, on 488).

[67] Pohl to Neumann, 1 January 1828, Göttingen UB, Ms. Dept.; also, Schagrin, "Resistance," 545–46.

[68] Füchtbauer, *Ohm*, 156–57, 163–64.

Gustav Theodeor Fechner, an unpaid public lecturer on physics at Leipzig. Subsequently known as a psychophysicist and philosopher, Fechner is not often remembered as a physicist, but early in his career he acquired a good reputation for experimental research based largely on his response to Ohm's work on the galvanic circuit.[69] In confirming Ohm's law, Fechner selected his measuring methods with the greatest care, performing measurement after measurement, while methodically eliminating secondary influences and errors.[70] He gave his readers every last detail, so that they could reproduce his experiments and reconfirm the truth of the "fundamental law of the galvanic circuit."[71] Ohm's law supported Fechner's belief that "measuring and weighing are the two great secrets of chemistry and physics," the "foundations of all discoveries with which these two sciences have enriched themselves in recent times."[72] As a result of his experiments, Fechner acquired an unpaid extraordinary professorship, and in 1834 he became ordinary professor of physics at Leipzig.

Fechner's interest in Ohm stemmed from his plan to bring out a second edition of his translation of Biot's text on physics. He realized that it was not possible to leave the original treatment of galvanism unchanged, but the newer experiments on the subject were not collected in any one place nor shown in their connectedness. Two circumstances offered him a solution to the problem. One was a solid measuring procedure, which he had found in the method of oscillations. The other was the appearance of a mathematical theory of galvanism, which although it lacked adequate experimental evidence provided definite viewpoints for his presentation of galvanism. "The theory I speak of is Ohm's," Fechner wrote the year after the publication of *Galvanic Circuit,* and he cited this book and also mentioned that there were "several publications" by Ohm in journals. Fechner wrote to Ohm that his law of currents with its simple formula connects a "great area of phenomena, which previously remained unconnected in a chaotic and puzzling way," shedding "great light over many very general relationships of the galvanic circuit" and offering a "beautiful linking of facts." It was, he said, "only through your theory that I obtained clarity about the conditions of the circuit, which were apparently so complicated and are now so simply resolved because of it."[73] This appreciation

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[69] Wilhelm Wundt, "Zur Erinnerung an Gustav Theodor Fechner," *Philosophische Studien* 4 (1888): 471–78. Historically, Fechner's experimental work was the most important foundation, though Ohm's electromotive law continued to be tested with ever greater exactness under varying conditions. Ohm's second, or electroscopic, law, which expresses the intensity of electricity in a cross section of the galvanic wire as a function of the electric state and the dimensions of the conductor, was not experimentally founded until some twenty-odd years later, discussed below. Karl Max Bauernfeind, "Ohm: Georg Simon," *ADB* 24 (1970); 187–203, on 195.

[70] Gustav Theodor Fechner, *Massbestimmungen über die galvanische Kette* (Leipzig, 1831), viii–x, 6.

[71] Ibid., 5, 225.

[72] In Fechner's translation of Jean Baptiste Biot, *Lehrbuch der Experimental-Physik, oder Erfahrungs-Naturlehre*, 3rd ed., 4 vols. (Leipzig, 1824–1825), vol. 1, 151, vol. 2, 196.

[73] Fechner to Ohm, 14 November 1828; Fechner to Schweigger, 17 November 1828.

would have gratified Ohm, for it agreed with his own understanding of what he had accomplished, which was to have brought together diverse phenomena under a unified viewpoint.

The confirmation of Ohm's law was at the same time a confirmation of Ohm's theory of the galvanic circuit. In the work of Ohm and Fechner, we see a coming together of physical law, mathematical theory, and measuring experiments, which will characterize much of the best research in Germany through the rest of the century.

After *Galvanic Circuit*, Ohm's hopes of finding a job more in keeping with his interests than teaching mathematics were damped by repeated rejections, which he attributed to his insistence on the need for apparatus.[74] When he was offered a small salary for conducting private practice sessions at a military school, the Allgemeine Kriegsschule in Berlin, he was offended, but over his protests he was persuaded to accept it, and in 1828 he left his mathematics teaching job in Cologne for good.[75] Over the next 5 years, while he taught at the military school, and also briefly at another military school in Berlin, he regularly sent out job applications.

In his search, Ohm wrote repeatedly to the king of Bavaria, who received his own copy of *Galvanic Circuit*. His application was forwarded to the Bavarian Academy of Sciences, which in turn referred it to the physicists Thaddeus Siber and K. D. M. Stahl. Evidently not disturbed by the lack of *Anschaulichkeit* in Ohm's mathematical theory, they understood both the work and Ohm's intention for it, giving it a "very favorable" opinion. This encouragement was to no avail, however. The physicists did not want Ohm to teach in the university because of the "inevitable collisions" over the apparatus and auditorium, nor did they want him to interfere with the apparatus if he were hired by the academy. As if to personify Ohm's concern over the influence of nature philosophers in his life, the then president of the academy was Schelling, who informed him that they did not know what to do with him, and the best they could offer was an extraordinary position without salary.[76] Ohm went back to sending off applications. When Fischer died, Ohm took heart, thinking that he might replace him as an extraordinary professor in Berlin with responsibility for mathematical physics, but the Prussian ministry informed him that physics was well looked after in Berlin with two ordinary and three extraordinary professors and several Privatdocenten.[77]

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[74] Ohm, *Nachlass*, 118–19, 129, 148. His competitor Pohl may have stood in his way, too. Pohl was appointed extraordinary professor at Berlin in 1830 and ordinary professor at Breslau in 1832, realizing his protector's good intentions. Lenz, *Berlin*, vol. 2, pt. 1, *Ministerium Altenstein* (1910), 380–81.

[75] Füchtbauer, *Ohm*, 161. Also Ohm, *Nachlass*, 94–97.

[76] Stahl's and Siber's evaluations, 7 and 5 May 1829, in Ohm, *Nachlass*, 121–24; Schelling to King of Bavaria, 10 May 1829, on 125–26.

[77] Ohm to King of Bavaria, 1 September 1831, Ohm, *Nachlass*, 151–54; Ohm to Bavarian Minister of the Interior Ludwig Prince Öttingen-Wallerstein, 23 February 1833, on 157–59; Ohm to Prussian Ministry of Education, 29 January 1831, on 143–45; the ministry's reply, 10 March 1831, on 145–46.

Although he could point to the recognition of his work by a number of German physics professors, no offers appeared for several years. His opportunity came when Bavaria reorganized its technical education. Ohm was interested in the polytechnic school in Munich, where he could combine an appointment to its chair of physics, which had fallen vacant, with an extraordinary professorship at the university, a favorable situation for an eventual promotion to an ordinary university professorship.[78] His application was referred to the university, whose philosophical faculty voted to grant Ohm's request, giving as their main reason his usefulness to technical teaching. They further recommended him as "a quiet, harmless, and industrious teacher," who had acquired great literary fame through his book *Galvanic Circuit*. Ohm did not get his first wish. The king instead offered him a professorship of physics at the polytechnic school in Nuremberg, which he accepted. There he doubled the planned physics curriculum to ensure "scientific training" for future artisans and craftsmen, and he soon added higher mathematics to his teaching, coordinating it with his teaching in physics. In 1839 he became director of the school. All of this activity left him with little time for research, as he had foreseen.[79] In 1849, he was appointed curator of the instrument collection for mathematics and physics in the Bavarian Academy and also ministerial director of the telegraph bureau. In addition, he was named ordinary professor for mathematics and physics and was required to give lectures without receiving lecture fees.[80] His job as curator lasted only 2 years, after which he served as ordinary professor of physics at the university, with direction of the physical cabinet. In 1854, owing to poor health, he was relieved of the experimental physics lectures and the physical cabinet, in place of which he was given "mathematical physics as his assigned subject."[81] Ohm died that same year, and so it was only briefly, and for reasons of personal disability, that Munich University added an ordinary professor for mathematical physics. Not until 40 years later did the university establish this position on a regular basis.

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[78] Ohm to E. Dingler, n.d. [1831]; Ohm to King of Bavaria, 1 September 1831 and 29 July 1832; in Ohm, *Nachlass*, 149–51, 151–54, 155–56, respectively.

[79] Bauernfeind, "Ohm," 193.

[80] K. F. P. von Martius to Ohm, 13 November 1849; Carl Max von Bauernfeind to Ohm, 27 November 1849; General Curator F. W. Thiersch to Ohm, 28 November 1849; in Ohm, *Nachlass*, 203–4, 205–6, and 206–8, respectively. On 23 November 1849, the Bavarian king, Maximilian II, named Ohm second curator of the state's collection and ordinary professor for mathematics and physics. On 5 December 1849, he was appointed scientific-technical advisor on telegraphic matters to the ministry of trade. His income was identical to Steinheil's down to the payment in goods, which was a yearly measure of grain worth about 100 florins; as curator his salary was 1400 florins, and as a scientific-technical advisor to the state he received an additional 400. Ohm, *Nachlass*, 202.

[81] Bavarian Ministry of the Interior to Munich U. Senate, 28 June 1854, Munich UA, E II-N, Boltzmann. Wilhelm Wien, "Das physikalische Institut and das physikalische Seminar," in *Die wissenschaftlichen Anstalten der Ludwig-Maximilians-Universität zu München*, ed. Karl Alexander von Müller (Munich: R. Oldenbourg and Dr. C. Wolf, 1926), 207–11, on 208.

For a time after *Galvanic Circuit*, Ohm continued to work on the subject, and then for a good many years he published no more research. When he returned, it was not to galvanism but to acoustics and optics, an old interest. He also undertook a comprehensive theory of physics. In the year *Galvanic Circuit* was published, he began to speak of a greater work to come, which would treat the whole of molecular physics, deriving all physical phenomena from analytical mechanics and molecular hypotheses. He published the first volume of mathematical preliminaries, but his call to Munich University interfered with his plan, and the remaining three volumes did not appear.[82] The existence of the plan, however, points to the confidence of the author of the *Galvanic Circuit* in the power of mathematical physics to complete the understanding of nature that Newton had begun. Ohm is our first example of the theme running through the history of theoretical physics, that of a theory of the whole.

3.3 Ohm as a Physicist

We have chosen Ohm's mathematical theory of the galvanic circuit in 1827 to begin our account of theoretical physics in Germany. It is a fine example of early mathematical-physical theories, a direction in which certain German physicists will excel. In this section, we relate characteristics of Ohm's theory to our discussion in the first chapter of theoretical physics in Germany, and we point out continuities between Ohm's theoretical work and a well-known theoretical work at the end of our period.

Presentation

Aware of the limited mathematical knowledge of his intended audience, who were not mathematicians, Ohm made his book accessible by giving a simple geometrical presentation of the theory at the beginning. The second part of the book gives the full theory using methods of analysis.

Law

By themselves, Ohm's experimental laws of galvanic currents do not provide the full understanding that physics is capable of. He has arrived at partially confirmed empirical laws for electric current and electroscopic force, but he does not yet have a theoretical foundation for them. This he supplies in *Galvanic Circuit*, which provides the understanding and completes this part of his work on galvanism. The establishment of a natural law is regularly identified as the main task of theoretical physics by German physicists.

Method

Ohm says that fundamental laws can be taken from experiments or assumed as hypotheses, which are justified if the calculated consequences agree with the experiments.[83] The "whole basis" of his theory is three fundamental experimental

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[82] Eugen Lommel, "Vorrede und Einleitung," in Ohm, *Gesammelte Abhandlungen*, v–xviii, on xiv–xv.

[83] Ohm, *Galvanic Circuit*, 438.

laws, one of which is partly hypothetical. The method he uses is the one Fries describes in his text, discussed in the last chapter; having come to be known as hypothetico-deductive, it is recognized as a standard method of making physical theories by theoretical physicists after Ohm.

Quantity

Galvanic Circuit addresses what can be measured, the link between theory and experiment. Ohm works with the "quantity of electricity, without intending to determine anything thereby with respect to the material nature of electricity."[84] Theorists after Ohm direct their work to quantities, which can be measured.

Mathematics

Ohm's theory is mathematical, and the mathematics is not elementary. In his lectures on theoretical physics, Helmholtz says that physicists need "a certain penetration into the depths of mathematical analysis."[85]

Physical Mathematics

Ohm criticizes Laplace for dealing only with "*imaginary* elements of space, by which the physical nature of the bodies is almost entirely lost sight,"[86] a critical aside expressing the interest of a physicist rather than that of a mathematician.

Concept

Ohm uses a measuring instrument, the electroscope (electrometer), to define the leading physical concept, "electroscopic force." The electroscope is the instrument with which Ohm's electroscopic law and theory will be tested and confirmed. Physical theory incorporates measurable physical concepts.

Differential Equation

The expression Ohm derives for the leading concept is a differential equation, the common mathematical form of physical laws. The need for this type of equation is explained by Helmholtz in his lectures on theoretical physics, and it is affirmed by all theorists.

Analogy

Ohm observes that the differential equations he derives for galvanic currents are analogous to those that Fourier and Poisson derived for the propagation of heat, suggesting to him an "intimate connection" between the phenomena of heat and those of electricity.[87] Analogies like Ohm's appeal to later theorists such as Kirchhoff and Boltzmann.

Perfection

Ohm says that no theory of physics is "perfect" until it is mathematical. This is a truism in theoretical physics, though "complete" is more likely to be the word used.

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[84] Ibid., 440.

[85] Hermann von Helmholtz, *Einleitung zu den Vorlesungen über theoretische Physik,* ed. Arthur König and Carl Runge (Leipzig: J. A. Barth, 1903), 25.

[86] Ohm, *Galvanic Circuit*, 445.

[87] Ohm, *Die galvanische Kette*, 5.

Simplicity

The “degree of simplicity” in the mathematical treatment of galvanic circuits is “not surpassed in any branch of natural philosophy,” Ohm says.[88] Simplicity is a widely applicable criterion, an indication of being on the right track. Kirchhoff advocates simplicity as an objective, and his contemporaries agree.

Mathematical Analysis and Experiment

Ohm says that the “chief merit” of his mathematical theory is that it can stimulate experiments. Mathematical analysis “calls forth, by its never-vacillating expressions, a generality of ideas, which continually excites to renewed experiments, and thus leads to a more profound knowledge of nature.”[89] Having given *Galvanic Circuit* a favorable opinion for the Bavarian government, the physicist Siber says that it is useful to apply mathematics to physics as Ohm has done, since general formulas for phenomena point the way for experimenters’ future work. An example is Kirchhoff's first published research, which appears in the *Annalen* in 1845, a theoretical and experimental investigation of galvanic currents in two dimensions. Kirchhoff’s starting point is the “principles” Ohm “theoretically” derived for currents in one dimension in *Galvanic Circuit*. For convenience, he assumes a stationary current in a circular conducting sheet, connected to an electrical source by wires attached to its periphery. In developing the theory, he introduces a function stating the electrical tension at a point on the surface of the sheet. He sets the function equal to a constant, defining a curve along which all points have the same tension. He then introduces a second curve of constant electric tension lying infinitely close to the first. From Ohm’s principles, he expresses the current crossing an infinitesimal line lying normal to the two curves at a point. From this result, together with conditions on the function of electric tension expressing the assumptions of the theory, he deduces lines of constant tension for the simple case of a circular plane with one entrance and one exit point. To test his prediction, he uses two wires connected through a multiplicator; a magnetic needle tells him if any current passes through the wires, as it does if the ends of the wires are not at the same tension. While holding the end of one wire fixed on the conducting sheet, he touches the end of the other wire at many places on the plate, mapping a curve of equal tension. His experiments verify the calculated curves.[90]

Generalization

In *Galvanic Circuit*, Ohm develops a differential equation that expresses the general law of electroscopic force, and he shows that the special case of the law for a steady current in a simple closed circuit can be generalized to include to a number of conductors. Kirchhoff ends his paper of 1845 with two laws describing the distribution of current in a system of wires, taking the subject beyond where Ohm left it in *Galvanic Circuit*. Kirchhoff follows this with several more papers based on Ohm’s principles, further generalizing the law, a common activity of theoretical physics.

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[88] Ohm, *Die galvanische Kette*, 6–7.

[89] Ohm, *Galvanic Circuit*, 438.

[90] Gustav Kirchhoff, “Ueber den Durchgang eines elektrischen Strőmes durch eine Ebene, insbesondere durch eine kreisfőrmige,” *Ann.* 64 (1845): 497–514. Ohm, *Die galvanische Kette*, 121.

Prediction

Ohm says that a "theory . . . must have all of its consequences in accordance with observation and experiment."[91] It can happen that conclusions from laws or principles lie outside experience for the present and may have to wait until the experimental means for testing them are invented.[92] When *Galvanic Circuit* is published, there does not yet exist an electroscope sensitive enough to measure the very small difference in tension at two points along the length of a current-carrying wire, as required by Ohm's theory. The law is confirmed experimentally 22 years later by Rudolph Kohlrausch using an improved electrometer by F. Dellman.[93] In his lectures on electric currents, Franz Neumann derives both of Ohm's laws. Concerning the first law, he says that Ohm derived it "from exact observations, and with that has laid the secure grounds for the subsequent theoretical investigations. The results are laid down in the small publication: 'Die galvanische Kette,' 1827." With regard to Ohm's second law, Neumann describes Kohlrausch's experiment. The tensions are too small to measure with Ohm's thermocouple, so Kohlrausch uses a wet-cell battery (*Hydrokette*). In a long box filled with a copper-sulfate solution he immerses a zinc plate and a copper plate. A thick copper wire connects the zinc plate with the Earth, and a long wire with a multiplicator connects the two plates. From preliminary measurements of tensions, Kohlrausch calculates the total electromotive force and the resistances. He is then able to calculate the tensions at four positions along the connecting wire (α, β, γ, δ) and at four positions along the solution (a, b, c, d), which he compares with the directly measured tensions. Considering the great difficulties of an electroscopic measurement and the unavoidable errors owing to changes inside the circuit and the electroscope, Neumann says, the agreement between the calculated and observed values of tension, or electroscopic force, is remarkable and completely convincing. He considered this confirmation of Ohm's law important enough to give the experimental measurements in his lectures. Below is Kohlrausch's comparison[94]:

Calculated		Observed
α	0.93	0.85
β	1.86	1.85
γ	2.80	2.69
δ	3.73	3.70
a	4.80	5.03
b	5.86	5.99
c	6.91	6.93
d	7.98	7.96

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[91] Ohm, *Galvanic Circuit*, 404.

[92] Albert Einstein, "Principles of Theoretical Physics," 1914, in *Ideas and Opinions* (New York: Dell, 1973), 216–19, on 218. At the time, Einstein's general theory of relativity was in the same situation as Ohm's theory, waiting for means for testing it.

[93] Heinrichs, "Ohm," 263. Bauernfeind, "Ohm," 195.

[94] Franz Neumann, *Vorlesungen über die elektrische Strőme*, ed. K. Vondermühll (Leipzig, 1884), 3, 51–54.

The confirmation of the electroscopic law is at the same time a confirmation of Ohm's theory, from which it is derived.

Constant

Ohm's differential equation contains a constant, which Ohm says needs to be verified by experiment. The identification and measurement of a constant entering a law is a standard task of theoretical and experimental physics.

Unity

By means of mathematical deductions from a few experimental "principles," galvanic phenomena have been brought together in a "close connection," presenting a "unity of thought."[95] Establishing such connectedness is a persistent concern of theoretical physicists.

Wholeness

Ohm says that the galvanic circuit is just one part of electrical science, and that he intends to work on other parts of it to fashion a "whole."[96] This aspiration runs throughout the work of many theorists in the various parts of physics in the nineteenth century.

Intended Audience

Ohm writes *Galvanic Circuit* first of all for physicists not mathematicians. His object is to develop a theory of physics from fundamental laws of physics, yielding other tested and testable laws of physics, and he refers to his and others' experiments.[97] In Paul Drude's words, theory flows out of experiment and back into it,[98] the daily concern of theoretical and experimental physics.

Foreign Recognition

Physicists accept *Galvanic Circuit* as physical theory. In 1841, Ohm receives the Copley Medal of the Royal Society of London for his experimental researches on the laws of electric currents and as well for his *Galvanic Circuit*. He has proven "theoretically as well as experimentally that the action of a circuit is equal to the sum of the electromotive force divided by the sum of the resistances."[99]

The above statement about simplicity is incomplete. Because galvanic apparatus makes use of wires, current is conducted in one dimension, and because it is independent of time, the treatment of the current, Ohm says, is "simple" and is "entirely thereby suited to secure to mathematics the possession of a new 'field of

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[95] Ohm, *Die galvanische Kette*, 62.

[96] Ohm, *Galvanic Circuit*, 401.

[97] He cited himself, Davy, Becquerel, Erman, Ritter, Jäger , Pohl, and others for experiments. His book came out before the more exact confirmation of his experimental law by Fechner.

[98] Paul Drude, *Die Theorie in der Physik. Antrittsvorlesung gehalten am 5. Dezember 1894 an der Universität* (Leipzig, 1895), 14.

[99] The citation is translated by Johann Christian Poggendorff, "Oeffentliche Anerkennung der Ohm'schen Theorie in England," *Ann.* 55 (1842): 178–79.

physics' which previously almost entirely remained closed to it, without any contradiction."[100] The theory of the simple galvanic circuit joins the theories of mechanics and parts of optics as mathematical theories. This characterization of theories and their laws, their "possession" by mathematics, does not affect physicists, who use them routinely in their research. The association of the perfection of physical results with mathematics is an enduring expectation. Three years before Ohm's theory, as we have seen, the physicist C. H. Pfaff said that if physics has a mechanical foundation, as was generally assumed, "every physical explanation becomes necessarily—the further it advances—a more and more purely mathematical one."[101] Later in the century, Hertz said that the selection of the principles of mechanics must be such that it satisfies the "requirement that the whole of mechanics can be developed from it by purely deductive reasoning without any further appeal to experience."[102] Helmholtz said that "perhaps when science is perfected, physical and mathematical order may coincide!"[103]

Mathematicians might have taken an interest in Ohm's theory, though it seems unlikely that many would have spent much time on it. They would have wondered if *Galvanic Circuit* contains any significant mathematical interest that is not found in its sources, writings by French authors. Mathematicians, of course, could have taken an interest in the physics of *Galvanic Circuit*, in the implications of the physical assumptions of the theory, as they are brought out in the mathematical development. What is incontrovertibly new is not the mathematical method but a mathematical theory of laws for the phenomena of galvanic circuits. In developing the theory Ohm's interest remains consistent with his interest as an experimenter, which is to understand galvanic phenomena.

To German physicists who, like Ohm, welcomed the hypothetico-deductive method, the French theories provided them with models. Ohm uses Fourier's method of deriving the fundamental law of the motion of heat as a template for deriving the comparable law for the electroscopic force in *Galvanic Circuit*. It is instructive to consider what Fourier says.

In *The Analytical Theory of Heat*, Fourier tells how to make a mathematical theory of physics, both in general and with reference to his subject, the motion of heat. Physics is not about ultimate causes but about laws, he says. From simple phenomena, mathematical analysis deduces the laws of nature, the application of which to particular cases requires "a long series of exact observations." Mathematics is coextensive with nature, applying to all perceptible relations of time, space, force, and temperature. It unites and discovers "hidden analogies" between diverse

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[100] Ohm, *Die galvanische Kette*, 6–7.

[101] Pfaff, *Elektro-Magnetismus*, 199, 200–201.

[102] Heinrich Hertz, *The Principles of Mechanics Presented in a New Form,* trans. D. E. Jones and J. T. Walley (New York: Dover Reprint, 1956), 4.

[103] Leo Königsberger, *Hermann von Helmholtz,* trans. F. A. Welby (Oxford, 1906), 249. Helmholtz said this in the preface to his and Wertheim's translation of Thomson and Tait's text on natural philosophy.

phenomena. In its application to natural phenomena, it attests to the "unity and simplicity of the plan of the universe." As instruments develop and experiments are multiplied, mathematical analysis arrives at more general and productive methods, reaching a greater range of phenomena. Through their theories, Newton and his successors have shown that the most diverse phenomena are subject to a few fundamental mechanical laws. Their mechanical theories however do not apply to heat, which obeys laws of another kind, as they are known from observations. Fourier tells how he proceeds. On the basis of Newton's law of cooling and facts he himself has observed with the "most exact instruments," he derives differential equations for the propagation of heat in full generality. He finds general integrals of the differential equations, and then passes from the general case to particular solutions for the specific conditions of a given problem. Physical researches on the motion of heat are reduced in this way to problems of integral calculus "whose elements are given by experiment." For all particular substances, observations are made of specific behaviors of heat and the variation of the coefficients that express them. The object of Fourier's theory is to determine the laws of heat flow, which cannot be discovered without the help of mathematical analysis.[104] Ohm makes his theory the same way as Fourier: beginning with simple facts, which he calls fundamental laws, with the difference that one of the laws is hypothetical, he derives a differential equation for the electroscopic force, which he then integrates. Like Fourier, he believes that mathematical theory reveals the analogy, unity, and simplicity of nature. To this point, what Ohm says and does in his book are in broad agreement with Fourier. If "galvanic circuit" is substituted for "heat," Fourier's statements above describe Ohm's procedure fairly closely.

Fourier says that the study of nature is "the most fertile source of mathematical discoveries."[105] In his theory of heat, he introduces "Fourier series," according to which any function can be represented by a series of sines of multiples of the variable, and he shows how to study heat conduction through the boundary conditions of differential equations. His mathematical methods are readily applicable to problems in other branches of physics. The complement of his mathematics is his physical law, the object of his work: the differential equation for the diffusion of heat. The main difference between Fourier and Ohm is the former's greater emphasis on mathematics, which contains much that is original. He develops the mathematics for describing the propagation of heat in bodies of varied geometrical figures: solid sphere, solid cylinder, rectangular prism, solid cube, infinite line, and infinite solid. Ohm does not consider a comparable range of geometric figures for his conductors. His interest in *Galvanic Circuit* is not mathematical discoveries and exhaustive mathematical cases but mathematical laws for linear conductors, which correspond to experimental galvanic circuits.

In addition to comparing Ohm's work to his predecessor Fourier's, we compare it below to work by a theoretical physicist who came after him, the next-to-last

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[104] Jean Baptiste Joseph Fourier, *The Analytical Theory of Heat*, trans. Alexander Freeman (Cambridge: Cambridge University Press, 1878), 1–15.

[105] Ibid., 7.

theory we take up in this book, Planck's theory of blackbody radiation.[106] There are naturally great dissimilarities between the two theories. Although both theories are electrical, electrical science in Planck's time would have been almost unrecognizable to Ohm, and he would not have foreseen the universal principles with which Planck built his theory. Still there are no less obvious continuities across nearly a century. Ohm and Planck were doing work of the same kind, deriving experimental natural laws mathematically from physical foundations.

Preliminary Form of a Law

In 1900 the existing theory of blackbody radiation was contradicted by recent experiments. From general theoretical considerations, Planck proposed a formula that agreed with new blackbody experiments, but he did not yet have a proper theory, and for that reason he did not regard his work as complete. The mathematical form of the preliminary formula offered the prospect of deriving it by a theoretical route, giving it physical meaning. The parallel is with Ohm's experimental laws of the galvanic circuit, which he was guided to by general theoretical considerations, but which lacked a proper theoretical foundation. There is no need for us to point out the parallels between Planck's and Ohm's work each time from here on.

Simplicity

Apart from agreement with the "few [experimental] numbers," the promise of Planck's preliminary formula was based, he said, "mainly on the simple structure of the formula," which gave a "very simple" expression for the entropy.[107]

Theory

Planck's object was to supply a theoretical foundation for the formula expressing the law of distribution of energy by wavelength in blackbody radiation.

Hypothesis

He used hypotheses in making the theory: a new version of his hypothesis of natural radiation and the hypothesis of energy elements, the truth of which are decided by their experimental consequences.

Method

The method was hypothetico-deductive; the theory was built from the above hypotheses and several fundamental laws of electromagnetic radiation, thermodynamics, and probability.

Law

The formula he derived from his theory expressed the law of energy distribution in black-body radiation.[108]

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[106] Max Planck, "Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum," *Verh. d. D. Phys. Ges.* 2 (1900): 237–45; "On the Theory of the Energy Distribution Law of the Normal Spectrum," in *The Old Quantum Theory*, ed., D. ter Haar (Pergamum Press, 1967), 82–87.

[107] Planck, "On the Theory of the Energy Distribution Law of the Normal Spectrum," 82.

[108] Ibid., 85.

Differential Equation

He expressed the law of energy distribution as a differential law.[109]

Confirmation

He referred to recent experiments, but he did not discuss them in any detail; he did not give graphs or tables of numbers, but cited papers by experimentalists, which gave this kind of information. His paper had a single focus, the theoretical derivation of an empirical law known to agree with recent experiments and containing constants.

Constants

In Planck's derivation of his law, two constants appeared, and with the help of one he calculated several known constants such as the charge of an electron.[110]

Unity

The energy distribution law included all wavelengths of radiation, not just the short and long wavelengths, to which the earlier theories had been limited, a unification of two partial theories.

Wholeness

From his formula for the energy distribution of radiation, Planck derived results "of considerable importance for other branches of physics and also of chemistry," an installment on the whole.[111]

Presentation

Planck presented his theory in two versions, an easy version with limited mathematics and an altered fuller version. In the first, which he delivered to the German Physical Society, he did not give his derivation of the law in detail. Instead he gave an entirely elementary presentation of the theory with the object of explaining clearly the core of the theory.

Ohm's theory in *Galvanic Circuit* and its confirmation fit a pattern, which turns up repeatedly in the history of physics. Kirchhoff in his lectures on mathematical physics describes different methods of experimentally verifying predictions of a given theory, based on different formulas derived from the theory. Ohm, as we have seen, derived two laws from his electrical theory, which were confirmed independently at different times, using different experimental means.

Ohm enters the history of physics mainly for the law of electric currents that bears his name. In the history of physics in Germany his booklet *Galvanic Circuit* is important as well. A historian of electricity writes that *Galvanic Circuit* was a "great advance in electrical philosophy." It clarified that current depends only on the conductivity of the body and on a variable quantity, the electroscopic force, which bears to electricity the same relation as temperature does to heat, and it

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[109] Ibid.

[110] Ibid., 86–87.

[111] Ibid., 83.

pointed to the electroscopic force as the connection between electrostatics and galvanism. Research over the next quarter-century was a "natural development of the principles laid down by Ohm."[112] Together, Ohm's theory and his laws show him to be a physicist skilled in both of the general methods of his field, those of mathematics and experiment. This will be the combination looked for in physicists when positions for theoretical physics appear in German universities later in the century.

3.4 Weber's Research

In 1831, Wilhelm Weber became ordinary professor of physics. Only in his late twenties then, he had already given a good account of himself in measuring and mathematical physics. In this section, we discuss the researches that led to his recognition and rapid advancement.

Weber received his original impetus to work in science from home. For a time his family lived in the house of the Wittenberg professor of medicine and natural history, whose cabinet was well known in scientific circles.[113] Living in the same house was the independent researcher Chladni, who became a close friend of the Webers.[114] Most important for Wilhelm, his older brother Ernst Heinrich was drawn to science, becoming, in Wilhelm's words, "my only tutor."[115] The same year, 1821, that Ernst Heinrich became professor of anatomy at Leipzig University, he and the seventeen-year-old Wilhelm began an exhaustive experimental research into the mechanics of waves.

The Webers' immediate stimulus to study waves was Ernst Heinrich's observation that when he poured mercury between bottles, the mercury surface resembled the "sound figures" that Chladni had observed in fine sand strewn on vibrating plates of glass or bronze. The Webers' general stimulus was the new interest in waves generated by Fresnel's research from 1815 on the wave theory of light,

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[112] Edmund Whittaker, *A History of the Theories of Aether and Electricity*, vol. 1, *The Classical Theories* (New York: Harper & Brothers, 1960), 92–93.

[113] Christian August Langguth's cabinet contained instruments useful for instruction in physics and mathematics as well as in natural history and medicine. *Göttingische gelehrte Anzeigen*, 1811, vol. 2, 1240.

[114] It should be pointed out that the Webers no longer lived in the same house with Chladni and Langguth's cabinet after Wilhelm was nine. The biographical facts on Weber are taken mainly from Heinrich Weber, *Wilhelm Weber. Eine Lebensskizze* (Breslau, 1893), but also from Eduard Riecke, "Wilhelm Weber," *Abh. Ges. Wiss. Göttingen* 38 (1892): 1–44; and K. H. Wiederkehr, *Wilhelm Eduard Weber, Erforscher der Wellenbewegung und der Elektrizität 1804–1891* (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1967).

[115] Riecke, "Weber," 4–5.

which the Webers followed closely.[116] This had led to more research on the wave theory in optics, but little on wave phenomena in acoustics, hydrodynamics, and other parts of physics. Propagating and standing waves had long been studied mathematically, but the simplifying assumptions of the theories did not correspond to the complex physical facts of waves.[117] Leonhard Euler, Daniel Bernoulli, and other mathematicians had studied the simplest type of standing waves, which was hardly ever observed in nature. Existing theories of waves had little empirical foundation; acoustics had been studied almost exclusively from the mathematical side before Chladni's work, and the experimental study of waves in water had only just been touched on.[118] Nicholas T. Bremontier, for example, carried out experiments in the ocean under conditions that could not be controlled. To improve on this state of the science, the Webers set up a 190-foot trough for the study of water waves. Their intention was to give the wave theory a new direction by supplying it with proper empirical foundations.

The treatise containing the Webers' researches on the wave theory, *Wellenlehre auf Experimente gegründet* [*Wave Theory Founded on Experiments*], came out in 1825 and was immediately praised as a "classic work" by the German physicist best qualified to judge it, Chladni.[119] While a university student, Wilhelm immersed himself in the most difficult mathematical physics, and he and his brother examined wave theories from Newton and Euler through their own contemporaries Laplace, A. L. Cauchy, and Poisson. They also became practiced in exact measurement and observation. By using water from the local river, they followed the motion of the impurities in it, observing the detailed behavior of waves. They measured the velocity, width, and height of waves and the time a particle of water takes to complete its cyclical path. They produced patterns on the surface analogous to sound figures; they passed water through horizontal tubes with vertical glass tubes regularly inserted into them; they stretched ropes across the river and observed waves in them analogous to water waves; they observed vibrations in membranes

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[116] A. J. Fresnel submitted his first treatise to the Paris Academy in 1815. He submitted the completed work *Mémoire sur la diffraction de la lumière* to the academy in 1818, and he published some papers on the subject in the *Annales de chimie et de physique* around this time. He also published a popular account in 1822, which was translated in the *Annalen* in 1824. By 1825, when their treatise came out, the Webers knew these articles and cited them (Ferdinand Rosenberger, *Die Geschichte der Physik*, vol. 3, *Geschichte der Physik in den letzten hundert Jahren* [Braunschweig, 1890; repr. Hildesheim: G. Olms, 1965], 178–79; E. H. Weber and Wilhelm Weber, *Wellenlehre auf Experimente gegründet oder über die Wellen tropfbarer Flüssigkeiten mit Anwendung auf die Schall-und Lichtwellen* [Leipzig, 1825], repr. as vol. 5 of Weber, *Werke,* ed. Eduard Riecke [Berlin, 1893]).

[117] Rosenberger, *Geschichte der Physik*, vol. 3, 256.

[118] Introductory remarks by the Webers in their *Wellenlehre*, 1–18, especially 4–6, 12–13.

[119] The Webers dedicated *Wellenlehre* to Chladni, the occasion of Chladni's letter on 20 August 1825 to Wilhelm Weber. In it, Chladni said that the Webers had presented wave motion "more clearly and coherently" than anyone before them and had treated "really existing" nature rather than the "idealistic webs" of the nature philosophers. Wiederkehr, *Weber*, quotes from this letter on 25.

and other sounding bodies. They repeated others' experiments, compared others' theories, and, in general, discussed the whole gamut of problems of the forces and motions of particles responsible for waves. In this collaboration, Wilhelm acquired an admirable education in physics, which he could not have gotten by any formal course of study at that time.

In these first researches, Weber displayed a characteristic that was to reappear in his later, better-known researches on electricity. It was to work experimentally, mathematically, and critically over a large field of phenomena, guided by a theoretically unifying conception. Here the field was waves, waves in water, rope, air, and ether: waves, a universal phenomenon.

> Just as the surface of the ocean . . ., the air [and] all solid bodies are in a manifold, never completely ceasing wave motion. As a body that has fallen into the water excites waves . . ., so every, even the minutest, impulse on a solid, fluid, or gaseous body excites waves.. . . Above all, by means of the rapid propagation of waves, nature appears to have made it possible for us to receive, for example, through light and heat, sense impressions of bodies separated from us by enormous distances and to be able to be in contact with other people over shorter distances by means of sound.[120]

As a novice physical researcher, Weber addressed the wide physical world, the seat of ubiquitous wave phenomena, an orderly form of matter and motion.

The Webers did their research in Halle, where the family had located. They were encouraged by the local professor of physics and chemistry, Schweigger, who let them use apparatus from the university's physical cabinet. While working with his brother on *Wellenlehre*, Wilhelm studied physics with Schweigger at the university, graduating in 1826 with an acoustical dissertation originating in his wave studies.[121] To widen his scientific knowledge, he applied to the Prussian ministry of culture, which had come to his and his brother's aid in their researches on waves, to spend a year at Gőttingen to study mathematics and exact research with Gauss.[122] His request points to his understanding of the mathematical needs of an up-to-date physicist in the 1820s and of Göttingen University as a place to acquire the tools. The ministry saw no need for him to go there, reasoning that it would be less expensive if he corresponded with Gauss, but if he liked he could go see him on his own during the vacation. Weber also asked the ministry to send him to Paris, where the kind of physics was done that he wanted to do. With his brother he had studied much recent French work and had used Poisson's theory extensively to check their experimental results. They had written a summary of *Wellenlehre* in French to ensure that the competent audience in Paris would notice it, but the ministry saw no need to send Weber to Paris either. So he settled in as Privatdocent and then as extraordinary professor at Halle, where he continued his acoustical researches in his

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[120] Wilhelm Weber and E. H. Weber, "Allgemein fassliche Darstellung des Vorganges, durch welchen Saiten und Pfeifen dazu gebracht werden, einfache Töne und Flageolettöne hervorzubringen," *Allgemeine musikalische Zeitung* 28 (1826): 186–99, 206–13, 222–35, repr. in Weber, *Werke*, vol. 1, 134–67, on 135–36.

[121] Wiederkehr, *Weber*, 27–28.

[122] Heinrich Weber, *Weber*, 9–10.

"Habilitationsschrift," the research paper qualifying a candidate for university teaching, and also in a series of papers he published in Schweigger's and Poggendorff's journals in 1825–1831. Schweigger was impressed by his precocious student and junior colleague, whose special interest was "mathematical physics" and who at the same time was experienced with instruments.[123]

In 1828, the year after Ohm's *Galvanic Circuit*, Weber published a paper in the *Annalen* dealing with a theoretical work on elasticity by Poisson. We have seen how Ohm made use of a method of French mathematical physics, Fourier's phenomenology. Now Weber responded to another central method of French mathematical physics, Laplace's molecular mechanics. In excerpts from a memoir on elastic bodies published in the *Annalen* that year, Poisson supplemented Lagrange's "analytical mechanics" with "physical mechanics," the purpose of which was to refer phenomena to "molecular actions that transmit the action of given forces from point-to-point," establishing equilibrium without the need for special hypotheses. With this method, Poisson developed equations for the equilibrium and motion of elastic rods and discs, taking into account as much as possible the "physical circumstances that depend on the nature of the bodies." In this way, he arrived at a new understanding, Weber pointed out, a "dependence and connection between two fundamental forces of nature," the elastic force that a body exerts after experiencing a change in form and the elastic force that a body exerts after experiencing a change in volume. Until Poisson's proof, the connection "had only been assumed." In his memoir, Poisson compared his theoretical results with measurements of sounds and nodal lines taken from Savart's and Cagniard-Latour's experiments. Weber further tested Poisson's results using instruments he acquired for his acoustical researches, finding a close match with the theoretical calculations. He regarded Poisson's memoir as one of the most important advances of physics in recent times: a highly successful application of mathematics to physical problems, which revealed a theoretical connection between two fundamental forces, and which led to experiments confirming the theory.[124] What Weber admired in Poisson's work we will recognize in Weber's work in electrodynamics, soon to come.

Weber's researches on elastic bodies and sound waves directed him to the physics of exact measurements. He regarded acoustics as defined as the theory of strings, rods, and the propagation of sound as a completed subject. For some time, physicists had been developing it mathematically to apply to other departments of physics, but stopping short where new physical principles were required. A wider conception of acoustics included physiological effects, and for these it was not yet complete. This interested Weber, who believed that the "investigation of the fundamental forces of nature is not the only goal of scientific research," that it includes the laws by which the "motion produced by the fundamental forces act on

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[123] *Goethe's Naturwiss. Corr.*, vol. 2, 310; Wiederkehr, *Weber*, 32.

[124] Wilhelm Weber, "Bemerkung über ein von Hrn. Poission für die Extension elastischer Drähte auf gestelltes Theorem," *Ann.* 14 (1828): 174–76. Quotations from Poisson's memoir in this volume of the *Annalen*, 387, 389.

us ourselves, on our sense organs."[125] He anticipated that our acute sense of hearing would allow us to measure smaller distances than our sense of sight, so that with perfected acoustical measurements we could gain a more exact knowledge of physical processes within bodies "such as cohesion, compressibility, dilatability, [or] expansion through heat, which are not well suited for investigations by spatial measurements with the visual sense." As an example, he pointed to the great difficulty of determining visually the minute elongation of a metal rod upon heating and, by contrast, to the large change "in the height of the tone of a transversely vibrating metal string when it undergoes even the slightest elongation while it is fastened at its ends to two invariable points and stretched." The problem in using sound for exact measurements was the lack of a reliable standard for the height of tones. To answer it, Weber devised an arrangement of organ pipes to produce a standard tone, performing a series of experiments to learn the laws of reed pipes, musical-like instruments consisting of oscillating plates and columns of air. Through another of his acoustical inventions, the "monochord," he measured the shortest times then attainable, useful for studying impulses within bodies.[126] His early concern with precise measurements and their use in understanding physical processes would typify his entire career.

Precise measurements alone could not give Weber the understanding he asked from physics. As a measuring physicist, he could fit empirical formulas to the numbers he obtained with his instruments, and within the limits of accuracy of the measurements he could then regard these formulas as "*true* laws" of nature, but they were not the same as a "*true* theory." With a true theory, he could deduce *all* of the laws, as he showed with his theory of reed pipes.[127] He was critical of the use of empirical laws where theoretically deduced ones were to be had. The former, he said, are imprecise, arrived at from imperfect experimental arrangements, and their application requires skill. The application of deduced laws, by contrast, is "automatic," and therefore more reliable. Weber's motivation was similar to Ohm's in his theoretical deduction of the laws of electric currents in *Galvanic Circuit*.

In the physics section at the 1828 Berlin meeting of the German Association of Natural Scientists and Physicians, Weber gave a lecture on organ pipes and their use in precision measurements in physics, which impressed his audience, who included Gauss and Humboldt. Weber learned that there was "some hope" that he would be

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[125] Wilhelm Weber, "Vergeleichung der Theorie der Saiten, Stäbe und Blaseinstrumente," *Ann.* 28 (1833): 1–17; in *Werke*, vol. 1, 365–76, on 374.

[126] Wilhelm Weber, "Compensation der Orgelpfeifen,"*Ann.* 14 (1828): 397–408, on 400–401, 404; Wilhelm Weber, "Über die zweckmässige Einrichtung eines Monochords oder Tonmessers und den Gebrauch desselben, zum Nutzen der Physik und Musik," *Ann.* 15 (1829): 1–19, on 1–2, 14.

[127] Wilhelm Weber, "Versuche mit Zungenpfeifen," *Ann.* 16 (1829): 415–38, on 433; "Vergleichung der Theorie der Saiten, Stäbe und Blaseinstrumente," 367. He demonstrated that the laws can be deduced from a true theory in "Theorie der Zungenpfeifen," *Ann.* 17 (1829): 193–246.

invited to Göttingen when the physics chair there fell vacant.[128] In early 1830, Weber wrote to Gauss that he was sending him a complete work, which consisted of a series of small publications on reed-pipes. If his reasoning in them was correct, he intended to measure the increase in the expansive force of each kind of gas with a sudden increase of density, this way confirming experimentally Laplace's theory of the velocity of waves not just for atmospheric air but for all kinds of gases. He told Gauss that his findings were based on both "experiments" and "theoretical considerations."[129] By combining the two methods, he was doing physical research according to precedents set by Laplace, Poisson, Fresnel, Ampère, and other French physicists he admired, which also agreed with the research by his German contemporaries Ohm and Neumann.

Weber was working in the right field; after he had sent Gauss his papers, Gauss wrote to him that he had "always been of the opinion that acoustics belonged to those parts of mathematical physics where the most brilliant advances are still to be made."[130] In his application to the Hannover government for the Göttingen chair after Mayer's death, Weber proposed Gauss, Humboldt, Oersted, and Berzelius as scientists to ask for recommendations.[131] No other young physicist in Germany could have offered a more imposing list of references. When Weber moved to Göttingen in 1831, it was as Gauss's colleague and not, as he had proposed only 5 years earlier, as his student. He told Gauss that he came to Göttingen to make use of the "favorable conditions" there for "scientific investigations,"[132] Gauss's presence being foremost among them. In collaboration with Gauss, Weber would carry out researches on magnetic and electric phenomena, to which he would bring the wide mathematical and measuring experience he had acquired during his study of wave phenomena.

3.5 Neumann's Research

Having seen what drew Weber to physics, we now look at Neumann's path. In the gymnasium, Neumann excelled in mathematics and physics, leading him to consider a career in the subjects, but his father advised him to study for a profession that would guarantee a job.[133] When he entered Berlin University in 1817, he dutifully listed theology as his field. After a year he moved to Jena University, where he heard lectures on nature philosophy by the natural historian Lorenz Oken, but he

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[128] Wiederkehr, *Weber,* 34.

[129] Weber to Gauss, 10 February 1830, Gőtt. UB, Handschriften Abt. Gauss.

[130] Gauss to Weber, 2 April 1830, Gauss Papers, Göttingen UB, Ms. Dept.

[131] Weber to "Staats-Minister," 8 February 1831, Weber Personalakte, Göttingen UA, 4/V b/95a.

[132] Wiederkehr, *Weber*, 37.

[133] Luise Neumann, *Franz Neumann, Erinnerungsblätter von seiner Tochter,* 2nd ed. (Tübingen: J. C. B. Mohr, 1907), 67, 79–80.

was disappointed by the other natural history professors, and he returned to Berlin. Giving up the practical course his father had urged on him, he took up natural sciences, especially mineralogy, under the decisive influence then, and for some time after, of the mineralogy professor Ernst Christian Weiss. A second formative influence on Neumann during these years was Fourier's treatise on the analytical theory of heat, the work which would guide Ohm in his galvanic research. Lacking the means to buy Fourier's book, he copied it out in its entirety. With the exception of Weiss, he said, no one had taught him as much as Fourier, whom he often called his greatest teacher, ranking him almost alongside Newton.[134] His self-study of Fourier and of other French writers gave him a solid grounding in the methods of mathematical physics.

Like Ohm and Weber, Neumann began research in the 1820s. His subject was crystals, which he studied first using geometrical methods, and then using physical methods he studied their thermal, elastic, and optical properties. Like the Weber brothers, he was drawn to Fresnel's explanation of light as waves in place of particles. Although the particle theory of light could explain most optical experiments, it fell short in delivering quantitative results, which by Neumann's time was a serious deficiency. The wave theory would bring about a major change in the way light was studied, and it would introduce new methods in theoretical physics.[135]

Because of the importance of Fresnel's theory for Neumann, as well as for Ohm, Weber, and other German physicists, we look briefly at its origin and nature. In 1818, after having investigated light for several years, Fresnel submitted a memoir on the diffraction of light for a prize offered by the Paris Academy of Sciences. Proceeding from the wave theory of light, he calculated diffraction patterns of light passing a straight edge and through a slit, finding excellent agreement with experiment. Poisson, who was on the prize commission, recognized other cases, including the prediction of a bright spot at the center of a circular shadow, which was confirmed. The theory and the experimental verification persuaded the commission to reward Fresnel with the prize. He next examined the polarization of light, profiting from a suggestion by the English natural philosopher Thomas Young that polarization can be explained by identifying light with transverse vibrations. If non-polarized light is a rapid succession of light waves polarized in all directions, as Fresnel thought, then all light consists of transverse vibrations. To explain the vibrations physically, he proposed a molecular optical ether with different resistances to distortion and condensation. The first problem he treated with this

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[134] Ibid., 84–85, 110–11, 244–45. Also Woldemar Voigt, "Zur Erinnerung an F. E. Neumann, gestorben am 23. Mai 1895 zu Königsberg i/Pr.," *Gött. Nachr.*, 1895, 248–65, on 252; repr. as "Gedächtnissrede auf Franz Neumann," in *Franz Neumanns Gesammelte Werke*, ed. by his students, 3 vols. (Leipzig: B. G. Teubner, 1906–28), vol. 1, 3–19; Paul Volkmann, *Franz Neumann. 11. September 1798, 23. Mai 1895* (Leipzig, 1896), 7.

[135] In addition to the replacement of particles of light by waves, wave fronts replaced rays as a tool of analysis. Jed Z. Buchwald, *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century* (Chicago and London: University of Chicago Press, 1989), xiii–xxii.

proposal was the double refraction of light by a crystalline body. The results, which he presented in memoirs to the academy in 1821, 1822, and 1827, have been called "perhaps the most brilliant of all his efforts," though physicists found his earlier experiments and simple principles, which were responsible for the acceptance of his wave theory, more convincing than his ether. The search for an alternative theory of the ether faced three problems. One was to eliminate longitudinal waves; a second was to decide if the ether is continuous or molecular; and the third was to determine how the ether and matter interact. Through the century, a good number of solutions were proposed by physicists in Germany, Britain, and France. Eventually optical ether theories lost some of their interest with the acceptance of Maxwell's electromagnetic theory of light, but physicists were in a long habit of looking to optics as a route to understanding the ether, and until the theory of relativity in the next century they continued to be studied.[136]

Neumann initiated the study of the optical ether in Germany. He accepted the wave theory of light, but he questioned Fresnel's theory of double refraction in crystals. In a paper published in the *Annalen* in 1832, his first work in "theoretical physics" according to a biographer and former student, he modified the French physicist Claude-Louis Navier's hypothesis of the mutual action of particles of an elastic solid. By generalizing the basic equations for a non-crystalline elastic solid to apply to a crystalline one, taking into account the directions of the crystal axes, he derived new equations expressing the analogy between light vibrations and elastic vibrations, and from these he deduced Fresnel's laws, which had been confirmed by experiment. Navier's countryman Cauchy also developed an elastic-solid theory of double refraction, publishing his results, though not his theory, in 1831, slightly before Neumann, who granted him the priority. Some of their results agreed, but their emphases were different, Cauchy's being the "mathematical side of the question," Neumann's the "physical side." In this purely theoretical work, Neumann supplied a new derivation of existing laws, analogous to what Ohm did in another branch of physics, except that Ohm's laws did not have a previous theoretical base.[137]

In a major paper in 1835, Neumann derived the reflection and refraction of light at an interface of crystalline bodies. In Fresnel's treatment of the case of isotropic bodies, the displacement of the ether constituting light is discontinuous at the interface. To correct this error, which is how Neumann regarded it, without giving up the laws of reflection and refraction in the process, he assumed that ether vibrations and the plane of polarization are parallel, and in his derivation he assumed that optical phenomena depend only on the elasticity of the ether. Fresnel, by contrast, assumed that that the plane of polarization is normal to the vibration

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[136] Olivier Darrigol, *The History of Optics: From Greek Antiquity to the Nineteenth Century* (Oxford: Oxford University Press, 2012), 224, 242, 261; Whittaker, *History of the Theories of Aether and Electricity*, vol. 1, 108, 115–17.

[137] Franz Neumann, "Theorie der doppelten Strahlenbrechung, abgeleitet aus den Gleichungen der Mechanik," *Ann.* 25 (1832): 418–54; Wangerin, *Neumann*, 69–75.

and that optical properties depend on the density of the ether. Experiments were unable to decide conclusively between the assumptions, resulting in a controversy that lasted most of our period. Neumann's theory may have had a slight advantage over Fresnel's and Cauchy's theories, but his ether, like theirs, was justified by the results, not by fundamental principles. The same is true of a similar theory proposed around the same time by the Irish mathematician James McCulloch (with whom Neumann engaged in a priority dispute). These ether theories were based on solids that do not exist in nature. Around this time, an improved theory was put forward by the English mathematician George Green, who considered the conditions at the interface of a real elastic solid instead of one invented to arrive at Fresnel's laws.[138]

In papers in the late 1830s, Neumann dealt experimentally with problems of crystal reflection, which he had previously solved theoretically, finding that his observations agreed with his formulas. The culmination of his research in optics was a comprehensive theoretical and experimental paper in 1841 on the double refraction of light in compressed and unequally heated non-crystalline bodies. He based this work on Navier's theory of elasticity, as before. The paper has several parts. In the first part, he showed that double refraction in uniformly dilated or compressed non-crystalline bodies follows the same laws as Fresnel's for crystalline bodies. His explanation was that the ether particles are rearranged by the deformation of the body, exhibiting the same symmetry as the parts of the body. In the second part of the paper, he developed general formulas for the colors arising when polarized light is directed through unequally compressed non-crystalline bodies. Foregoing the molecular method in this case, he imagined a deformed isotropic body to be made up of elementary volumes, each of which behaves like a tiny deformed crystal, the optical elasticity axes of which are continuous functions of position. The third part of the paper is about colors arising when polarized light is directed through unequally heated non-crystalline bodies. By adding terms dependent on temperature to Poisson's equations for the equilibrium of elastic bodies, he obtained a relation between temperature and the displacement of the particles of the bodies, and from the formulas for double refraction in the early part of the paper he derived the colors in this case. A further significance of this paper is its introduction of a new problem area, dispersion. Neumann based his theory of dispersion on the interaction of the ether and matter, an important approach which reappeared in later work in optical theory.[139] At the time of Neumann's researches in optics, the 1830s, optical theory was largely ether mechanics. Beginning in the early 1850s and for the next 30 years, much of the work on optical theory was done without the aid of

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[138] Wangerin, *Neumann*, 81–83, 90; Volkmann, *Neumann*, 17; Whittaker, *History of the Theories of Aether and Electricity*, vol. 1, 137–39.

[139] Franz Neumann, "Die Gesetze der Doppelbrechung des Lichts in comprimirten oder ungleichfőrmig erwärmten unkrystallinischen Kőrpern," *Ann.* 53 (1841): 451, 454; Albert Wangerin, *Franz Neumann und sein Wirken als Forscher und Lehrer* (Braunschweig: F. Vieweg, 1907), 92, 100, 102–3, 106; Voigt, "Zur Erinnerung an F. E. Neumann," 260–62. Volkmann, *Neumann*, 18; and Paul Volkmann, "Franz Neumann als Experimentator," *Phys. Zs.* 11 (1910): 932–37, on 934–35.

mechanical derivations, beginning instead with confirmed mathematical principles of light. Kirchhoff and Helmholtz approached optics this way, relying not on models of the ether but on advanced mathematical methods.[140]

Spanning a decade, Neumann's optical work formed an interconnected series of researches with a focus on crystalline bodies. Some of the researches were purely theoretical, others contained observations, and all were directed to physical understanding. Like Ohm and Weber, Neumann looked to French mathematical and experimental physics for his problem and his starting point. As we have seen, he took up the elastic wave theory of light in its molecular formulation, as it was presented by Navier, Poisson, and Cauchy. His subsequent departure from the molecular method counts as one of his important contributions to theoretical physics. This was the method of viewing the smallest parts of finite bodies as bodies themselves, possessing the observable properties of the finite bodies, the method Gauss had introduced in the theory of Earth magnetism. Neumann, his students, and others would apply it to various other areas of physical theory with comparable success.

More examples of early nineteenth century physicists in Germany would not greatly change the picture we have drawn here. Their impulse toward study and research in physics came partly from within and partly from without, from the father or brother or uncle, from the friend or boarder or neighbor, from the secondary school teacher or university professor, and from self-study. If they had sufficient mathematical ability to enable them to understand mathematical theories of physics, and if they also had a talent for experimental work, and if in one way or another, from home or school or elsewhere, they gained access to recent publications on physics and instruments, acquiring the research skills that it was no purpose of regular education to provide, they might become an Ohm, a Weber, or a Neumann.

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[140] Buchwald, *Rise of the Wave Theory of Light,* 308–9; Darrigol, *History of Optics*, 286.

Chapter 4
Promoting a New Physics: Earth Magnetism at Göttingen

In the middle decades of the nineteenth century, a career in physics in Germany usually meant the career of a university physics professor. Within German universities, opportunities for doing research in physics increased, while outside them, independent physics research became nearly impossible, and industrial physics laboratories had not yet taken its place. Developments in physical research led to changes in physics instruction, raising it to the level of professional training. Physics professors could claim that it was not only desirable but necessary for their teaching that they also carry out research, and that they were therefore entitled to be furnished by the state with means for research. To reach the latter stage took most of the period covered by this book.

More than any other development in the 1830s, research in Earth magnetism set physics on a new course. Methods of mathematics and measuring physics that were introduced then were important for theoretical physics for the rest of the century. Earth magnetism was the subject of the first major research in physics that successfully commanded support at the universities from the German states. This came about because of the association of Earth magnetism with astronomy, the one physical science that had long been supported by the states. The reason they were supported was the same, the expectation of practical benefits.[1]

Astronomers were used to being called on for surveying and triangulation projects. For military purposes (at first those of the French during their occupation of parts of Germany), for the assimilation of newly acquired territories, and for

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[1] Gauss mentioned the practical uses of Earth magnetism in his "Einleitung" to the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre 1836*, 3–12, repr. in Carl Friedrich Gauss, *Werke*, ed. Königliche Gesellschaft der Wissenschaften zu Göttingen, vol. 5, (n.p., 1877), 345-51, on 350.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_4

reforms of taxation based on land ownership, the German states required new topographical maps, which meant jobs for their astronomers.[2] Gauss, who in addition to being a mathematician was the astronomer at Göttingen University in the service of the state of Hannover, spent much of his early career on work of this sort. To support practical astronomy, states refurbished existing or built new observatories; by 1830 Berlin, Munich, and Göttingen had all recently acquired new ones. Astronomical and surveying work required, in turn, optical and measuring instruments at a time when Napoleon's Continental blockade had cut off the supply from England of the best instruments then available. German technologists such as Georg Reichenbach and Joseph Fraunhofer soon advanced the art of instrument making to a new level, giving astronomers reason to request money from their states for the new German instruments even when they were already equipped with earlier English ones.[3] The availability of excellent instruments in German astronomical observatories proved helpful for research in Earth magnetism. "The most precise observations" now possible in Earth-magnetic research, Gauss pointed out, "can be expected only of those mathematicians who are familiar with the finest means of observation, namely, the practical astronomers."[4] Of these practical astronomers, it was Gauss himself, according to Humboldt, who brought about the "revolution" in research in Earth magnetism.[5]

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[2] A. Galle, "Über die geodätischen Arbeiten von Gauss," in Gauss, *Werke*, vol. 11, pt. 2, 1st treatise (Berlin and Göttingen: Springer, 1924), 16, 27, 38, 47–48, to cite only a few references to the interests of the different German states in the surveying and triangulation projects. The participating astronomers frequently referred to in this connection, aside from Gauss, were F. W. Bessel, J. G. F. Bohnenberger, Johann Franz Encke, Bernhard August von Lindenau, Wilhelm Olbers, Johann Georg Soldner, and Franz Xaver von Zach. E. Weis, "Bayerns Beitrag zur Wissenschaftsentwicklung im 19. und 20. Jahrhundert," in *Handbuch der bayerischen Geschichte*, vol. 4, pt. 2, *Das neue Bayern 1800–1970*, ed. Max Spindler (Munich: C. H. Beck, 1975), 1034–88, on 1043; Günter D. Roth, *Joseph von Fraunhofer, Handwerker–Forscher–Akademiemitglied 1787–1826* (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1976), 31–37, 65, 70.

[3] Roth, *Fraunhofer*, 31–32, 45–46, 71–73; Weis, "Bayerns Beitrag," 1042. An inventory of the old astronomical observatory in Göttingen dating from 1788 lists eight instruments from English instrument makers among a total of eleven, including a "Newtonian reflecting telescope by Herschel" and two telescopes by Dollond. Gauss used Dollond's instruments, but his main work in practical astronomy at Göttingen did not begin until after he had acquired from Reichenbach the new instruments that he would like to have bought from the English instrument maker Ramsden had it been possible, but which he was now getting from Ramsden's student. Martin Brendel, "Über die astronomischen Arbeiten von Gauss," in Gauss, *Werke*, vol. 11, pt. 2, 3rd treatise (Berlin and Göttingen: Springer, 1929), 46–49, 55–56. Reichenbach was sent to study in England at the expense of the Bavarian government. Weis, "Bayerns Beitrag," 1037–38.

[4] Gauss to Göttingen U. Curator, 29 January 1833, Göttingen UA, 4/V f/48.

[5] Alexander von Humboldt, *Briefe zwischen A. v. Humboldt und Gauss. Zum hundertjährigen Geburtstage von Gauss am 30. April 1877,* ed. Karl Bruhns (Leipzig, 1877), 24. Humboldt's letter to Gauss containing the remark is dated 17 February 1833.

4.1 Gauss's Interest in Earth-Magnetic Research

Gauss believed that the first step in developing a mathematical theory for a group of phenomena was to discover a physical principle, using if necessary experimental observations.[6] In search of a guiding physical principle in Earth-magnetic research, he first considered the magnetic distribution in a magnetic body, and like others working in the subject, he speculated on the source of the Earth's magnetism. To H. C. Schumacher, an astronomer in Copenhagen, he wrote in 1832: "I have always considered these immense changes [in Earth magnetism] as something highly remarkable. Without doubt, the magnetic force of the Earth is not the result of a pair of large magnets near the center of the Earth, which gradually move many miles from their places, but the result of all the polarized iron particles contained in the Earth, and in fact more of those that lie closer to the surface than of those that lie closer to the center."[7] The problem of the distribution of magnetism in a body led Gauss, as a first step, to consult Biot's *Traité de physique* of 1816 and to read Poisson's memoir of 1812 on the distribution of electricity on the surface of conductors. He was bothered by the discrepancy he found in Biot between the assumption that every magnetic body, even the smallest part of it, contains equal amounts of "northern" and "southern" magnetism and the observation that the poles of a magnetic body act as if they contain only the one or the other magnetic fluid. Poisson's paper contained a statement—"new, as far as I know," Gauss remarked to the astronomer F. W. Bessel in December 1831—that suggested a way out of the difficulty.[8] The principle Gauss arrived at he stated without proof in 1832: "however the distribution of free magnetism may behave within a body, one can always put in its place, as a result of a general theorem in accordance with a certain law, another

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[6] See, for example, the advice Gauss gave to Weber in 1844, in Clemens Schaefer, "Über Gauss' physikalische Arbeiten (Magnetismus, Elektrodynamik, Optik)," In Carl Friedrich Gauss's *Werke,* 2nd treatise in vol. 11, pt. 2 (Berlin and Göttingen: Springer 1929), 118–19.

[7] Gauss to H. C. Schumacher, 3 March 1832, in Schaefer, "Über Gauss' physikalische Arbeiten," 30.

[8] In S. D. Poisson, "Mémoire sur la distribution de l'électricité à la surface des corps conducteurs," *Mémoires de l'Institut, année 1811* (Paris, 1812), 1: "It is not sufficient for maintaining a constant electric state in an electrified conducting body that the internal border surface of the free electricity at the surface of the conductor be an equilibrium surface, but it is also required that this electricity does not exert any attraction or repulsion at any point in the internal space." Gauss now pointed out that the second condition, namely, that the resultant force at every point in the internal enclosed space be zero, is already contained in the first, if the attraction and repulsion take place according to the inverse-square force law. Quoted in Schaefer, "Über Gauss' physikalische Arbeiten," 99–100. Schaefer also quotes Gauss's letter to Encke, 18 August 1832 (13–14), and to Bessel, 31 December 1831 (100), on this work.

distribution only for the surface of the body, which to the outside acts entirely with the same forces as the former [that is, the distribution inside the body]; so that an element of magnetic fluid placed anywhere outside [the body] experiences exactly the same attraction and repulsion from the real distribution of magnetism within the body as from that imagined to be on the surface."[9]

In his "general theory" of Earth magnetism in 1839, Gauss likened the task of Earth-magnetic researchers to that of astronomers, which was to subject magnetic observations to "one principle" analogous to Newton's gravitational law and from it to predict other phenomena. Earlier researchers had calculated magnetic phenomena from one, two, or more infinitesimal magnets in the Earth's interior, which reminded Gauss of the accumulation of epicycles in pre-gravitational astronomy. By contrast, Gauss offered a theory independent of "particular hypotheses about the distribution of the magnetic fluids in the body of the Earth" and even of the hypothesis of magnetic fluids itself. Although he presented the theory in terms of the usual two magnetic fluids, nothing would have changed if instead he had viewed magnetism as originating in galvanic currents. The physical assumptions essential to his theory were, first, that the "Earth-magnetic force is the total action of the magnetized parts of the Earth" and, second, that the magnetic fluids act according to an inverse square force, a "proven physical truth."[10] These two assumptions allowed him to introduce the potential, although he did not call it that yet, into his magnetic theory.

To treat the subject mathematically, Gauss divided the Earth into infinitely small volume elements, each containing a quantity of magnetic fluid $d\mu$. He then wrote the potential of the magnetized elements of the whole Earth as $V = \int d\mu/\rho$, where ρ is the distance of the elements from the point where the potential is evaluated. From this expression he derived components of the magnetic force parallel and normal to the Earth's surface, which are given by infinite converging series of spherical functions, the first few terms of which Gauss retained as approximations. To determine the coefficients of the terms, he referred to empirical maps of the intensity, declination, and inclination of the Earth's magnetic force. He then calculated the magnetic elements for ninety-one magnetic stations spread over the Earth, finding satisfactory agreement with the observations. The predictive power of the theory was shown 2 years later when Charles Wilkes found the south magnetic pole close to where Gauss had calculated it should be.

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[9] Schaefer, "Über Gauss' physikalische Arbeiten," 14.

[10] Carl Friedrich Gauss. "Allgemeine Theorie des Erdmagnetismus," *Resultate … 1838*, repr. in Gauss, *Werke*, vol. 5, 119–75, on 122–26.

A year after the publication of his general theory of Earth magnetism, Gauss published a systematic presentation of the mathematical object he had used in that work, the potential. Here Gauss used the term "potential" to designate the function from which the components of forces are derived: $V = \sum \mu/\rho$, which expresses the action at any point of a collection of point "masses" or, in general, "agents" that interact by inverse-square forces. He developed a collection of theorems about the properties of this function, the "key to the theory of the attracting and repelling forces" of gravitation, electricity, and magnetism.[11] Gauss considered applying it to electrodynamic phenomena too, since Ampère's force between current elements depends on the inverse square of the distance; but because the action of this force is complicated by the directionality of current, Gauss did not treat it here and spoke only of discussing it in a later paper, which he never got around to.

Despite the striking differences of the observed phenomena of gravitation, electricity, and magnetism, their mathematical description draws on a common body of theorems. Potential theory provides a mathematical method of impressive generality: by studying the behavior of one function, abstracted from any one area of phenomena, the physicist could learn at once the mathematical structures relating the phenomena belonging to several areas. The conservation of energy principle, stated soon after Gauss's potential theory, made the potential an even more important aid in developing physical laws.

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[11] For the case in which the agents are continuously distributed, Gauss replaced the sum by an integral. He derived the standard equations for the potential in the absence of, and in the presence of, sources k:

$$\frac{ddV}{dx^2} + \frac{ddV}{dy^2} + \frac{ddV}{dz^2} = 0, \text{ and } \frac{ddV}{dx^2} + \frac{ddV}{dy^2} + \frac{ddV}{dz^2} = -4\pi k.$$

Gauss, "Allgemeine Lehrsätzte in Beziehung auf die im verkehrten des Quadrats der Entfernung wirkenden Anziehungs- und Abstossungs-Kräfte," *Resultate…1839*, repr. in Gauss, *Werke*, vol. 5, 195–242, on 199–200. Gauss read the paper on 9 March 1840. Limited uses of the potential long antedated Gauss's general, rigorous theory of the potential. In the late eighteenth century, Lagrange and Laplace wrote the components of the gravitational attraction as the partial differential quotients of a certain function. In the early nineteenth century, this way of expressing forces was extended by Poisson and others to electricity and magnetism (Morris Kline, *Mathematical Thought from Ancient to Modern Times* [New York: Oxford University Press, 1972], 681–82). Before Gauss, some of the theorems on the potential had already been derived by George Green in 1828, but Green's work remained unnoticed until after Gauss's had attracted wide interest in the subject. It was resurrected in 1846 by William Thomson. It was only made readily accessible to a German audience through its publication in Crelle's *Journal* in 1850–1854. For this reason it was largely on the basis of Gauss's work that potential theory was developed in Germany into an "independent mathematical discipline" of importance to mathematicians and physicists alike (Albert Wangerin's "Anmerkungen" to the repr. of Gauss's "Allgemeine Lehrsätze," ed. A. Wangerin as vol. 2 of Ostwald's Klassiker der exakten Wissenschaften [Leipzig, 1889], 51–60, on 52).

4.2 Development of Mathematical and Instrumental Techniques

The experimental, as opposed to the mathematical-theoretical, investigation of Earth magnetism depended on overcoming two obstacles. First, a way had to be found to obtain observations that were more useful than the "relative" observations gathered before 1832. Gauss solved that problem by introducing "absolute" measuring units into the study of magnetism. Second, instruments had to be developed that were precise enough for the use of absolute units. Gauss was drawn to the subject for two reasons: one was the inherent interest of magnetism, especially after the discoveries of its connections to electricity, and the other, "almost even more important," was because magnetic "experiments are becoming capable of a precision that far surpasses anything that went before, and its fundamental laws can have a truly mathematical precision."[12]

To establish absolute units for magnetic research, Gauss proceeded from the assumption of northern and southern magnetic fluids, any two elements of which mutually repel if they belong to the same fluid and mutually attract if they belong to the opposite fluids. Magnetic phenomena arise when the fluids, which are bound in equal amounts to the particles of ponderable bodies, are displaced relative to one another. Since the action between the magnetic fluids sets the ponderable bodies in motion, the measure of magnetic quantities and forces can be expressed in the units used to express the interaction of ponderable bodies. Force, mechanically measured, determines the unit of magnetic quantity, the key to Gauss's system of absolute units.[13] Gauss's introduction of absolute units had "extraordinary significance for physics"; the fundamental units of mechanics were now seen to apply to another branch of physics, magnetism, and Gauss suggested their extension to still another branch, electricity. Weber soon established absolute units in electrodynamics, and they were extended to other forces, promising a common measure

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[12] Gauss to Göttingen U. Curator, 29 January 1833.

[13] The unit of magnetic quantity is that which acts on another unit of magnetic quantity at unit distance with unit moving force. Poisson had expressed the intensity of Earth magnetism by the force with which a unit magnetic quantity acts on a second unit, but because his unit of magnetic quantity was arbitrary, he did not have an absolute system (Ernst Dorn's "Anmerkungen" to Gauss's "Intensitas vis magneticae terrestris," trans. and repr. as *Die Intensität der erdmagnetischen Kraft auf absolutes Maass zurückgeführt* [1832], ed. E. Dorn, Ostwald's Klassiker der exakten Wissenschaften [Leipzig, 1894], vol. 53, 50–62, on 54). Schaefer notes that although Poisson did not have an absolute system in Gauss's sense, his method produced values for Earth-magnetic force that are independent of the magnetic state of the needles used, and in that sense he too had "absolute measures." Schaefer, "Über Gauss' physikalische Arbeiten," 25.

throughout physics.[14] In his lectures on theoretical physics, Helmholtz said that all physical magnitudes can be expressed in absolute units, even heat.[15]

It was not enough to give the theory for reducing Earth-magnetic intensity to absolute units, since it was impossible to use them in practice as long as magnetic observations remained imprecise. Gauss set out to construct the necessary apparatus, certain that magnetic observations could be made with a precision nearly equal to that of the finest astronomical observations. The first magnetometer Gauss constructed in 1832 used magnetized prismatic rods, or "needles," about a foot long and weighing about a pound, suspended by a strong, untwisted silk thread, which could be rotated at its upper end. At one end of the needle he attached a plane mirror, set perpendicular to the magnetic axis of the needle. The freely suspended magnetic needle was then encased in such a way that it could be observed even as it was protected from disturbing influences such as air current. To obtain the desired precision, Gauss constructed instruments for observing the magnet as carefully as he arranged the magnet itself. Opposite the mirror on the magnetic needle, he set up a theodolite, a telescopic instrument for making precise measurements of angles, so that its vertical axis and the suspension thread were in the same magnetic meridian, separated from one another by about sixteen feet. To the stand of the theodolite, Gauss attached a four-foot-long horizontal scale divided into millimeters at right angles to the magnetic meridian. He called the point on the scale that lay in the same vertical plane as the optical axis of the telescope "point 0" and marked it with a weighted gold thread hanging from the middle of the objective lens. The scale was at such a level that an image of part of it could be seen in the mirror through the telescope. With this arrangement, Gauss could determine the direction of the needle and its variation precisely. He could also measure the variation frequently because he did not have to wait until the needle came to rest. His distance measurements were of "microscopic precision."[16]

Throughout 1832 Gauss worked to develop and test a method for reducing measures of the Earth's magnetic force to the mechanical units of length, time, and mass. In February he informed the astronomer H. W. M. Olbers that he was working on Earth magnetism, "in particular [on] an absolute determination of its

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[14] Dorn, "Anmerkungen," 50; Ferdinand Rosenberger, *Die Geschichte der Physik,* vol. 3, *Geschichte der Physik in den letzten hundert Jahren* (Braunschweig, 1890; repr. Hildesheim: G. Olms, 1965), 302. In the course of the nineteenth century, opinions on the possibility of expressing all physical laws in absolute units varied. The early promise of the universal applicability of absolute units led to their overvaluation, as physicists later recognized: chemistry, heat, radiation, and even electricity and magnetism required a fourth fundamental unit (Dorn, 52).

[15] Hermann von Helmholtz, *Vorlesungen über die Theorie der Wärme*, ed. Franz Richarz (Leipzig: J. A. Barth, 1903), 32. Dorn, "Anmerkungen," 50; Ferdinand Rosenberger, *Die Geschichte der Physik,* vol. 3, *Geschichte der Physik in den letzten hundert Jahren* (Braunschweig, 1890; repr. Hildesheim: G. Olms, 1965), 302.

[16] Gauss to Olbers, 2 August 1832, in Wilhelm Obers, *Wilhelm Obers, sein Leben und seine Werke. Briefwechsel zwischen Olbers und Gauss,* ed. C. Schilling, vol. 2, pt. 2 (Berlin: J. Springer, 1909), 588.

intensity. Friend Weber is doing experiments according to my specification."[17] From then until August, when he wrote to Olbers again, he had worked, "one might almost say, exclusively" on magnetism. The experimental results had not only met his expectations "but far surpassed" them: "At present I have completed two apparatus (completely alike) with which absolute declination and its variations, duration of oscillations, etc., can be measured with a precision that leaves nothing to be desired, except for a more suitable location where there is no iron nearby and every current of air is kept away." Gauss intended to devote a separate publication to the project, but in its own good time since he did not like to hurry immature work. For the time being, he told Olbers, he would read to the Gőttingen Society of Sciences a paper on the "most important" application, the determination of the absolute intensity of Earth magnetism.[18] The paper, which he read in December 1832, became his first publication on the subject.

Gauss's apparatus was immediately useful for measuring magnetic declination. Before he tackled the problem of making it equally useful for measuring magnetic intensity, he further refined its precision, and for that he needed a heavier magnet and an iron-free building.[19] On the day Gauss read his paper on absolute magnetic measures to the Gőttingen Society, Weber, who had been "extremely helpful"[20] in the experiments to test the new apparatus, requested materials for a large magnet from the Hannover government. All that was needed was sufficient iron and steel, which should not be too costly, Weber said, and he more or less promised that Gauss would direct the research with the magnet. With that reassurance, the Göttingen curator granted funds for buying 500 pounds of steel, noting the "interesting investigations on Earth magnetism that Hofrath Gauss has recently made."[21] Weber spent several days at the royal ironworks in Sollingen to make sure that the large steel rods, which were 4½ ft long and weighed 25 pounds each, and some of the smaller 4-pound steel rods were properly tempered "according to my instructions and under my eyes." (He cleverly pointed out to the curator that not only had he saved money by supervising the job at the ironworks, but he was also now bringing business to the state, since as a result of his work other universities had asked him to order similar steel rods for them.) The rest of the smaller steel rods Weber tempered at the physics institute, and he, Gauss, and some of their friends and students who were "especially interested in these investigations" did the remaining work.[22]

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[17] Gauss to Olbers, 18 February 1832, in *Briefwechsel zwischen Olbers und Gauss,* 584–85.

[18] Gauss to Olbers, 2 August 1832, in *Briefwechsel zwischen Olbers und Gauss,* 587.

[19] Gauss to Göttingen U. Curator, 29 January 1833. Also Gauss to Olbers, 2 August 1832, in *Briefwechsel zwischen Olbers und Gauss,* 587; Schaefer, "Über Gauss' physikalische Arbeiten," 28; and Gauss to Encke, 18 August and 25 December 1832, in Schaefer, 31.

[20] Gauss to Olbers, 2 August 1832, in *Briefwechsel zwischen Olbers und Gauss,* 588.

[21] Weber to Göttingen U. Curator, 15 December 1832, and Göttingen U. Curator to Weber, 17 January 1833, Göttingen UA, 4/V h/16.

[22] Weber to Göttingen U. Curator, 12 August 1834, Göttingen UA, 4/V h/16.

In 1833 an Earth-magnetic observatory was built on the grounds of the Göttingen astronomical observatory, where Gauss worked and lived. He had argued for it by indirectly pointing to his researches of the previous year. He would be neglecting his duty toward Göttingen University, he wrote to the curator, if he did not call the government's attention to an opportunity for Göttingen not only to join the increasing number of institutions with magnetic observatories, but also to light the way for them with its new method.[23] At the new magnetic observatory, Gauss's fellow astronomer at Göttingen, Carl Ludwig Harding, was to observe the variation of magnetic declination several times a day at fixed times, while Gauss determined the absolute magnetic intensity from time to time, and at several specified times during the year they were to make more frequent observations in coordination with observations at magnetic observatories around the world; that, at least, was Humboldt's program.

4.3 Organization of Earth-Magnetic Observations

Gauss's new apparatus forced him to change Humboldt's program. In the interest of precision, Gauss wanted to make observations more frequently, preferably once a minute, but he settled for every five minutes, and he accepted a reduction in the number of hours of observations on the fixed days for practical reasons. Observers elsewhere quickly fell in line, as they acquired apparatus modeled on Gauss's. From all over Europe and beyond, Earth-magnetic observatories joined in an informal organization, the Magnetic Union.[24] Their public forum was a journal begun by Gauss and Weber, the *Resultate aus den Beobachtungen des Magnetischen Vereins* [*Results of Observations of the Magnetic Union*]. Its six volumes covering 1836–1841 contained mainly Gauss's and Weber's own observational and theoretical results and descriptions of the magnetometers and other precision instruments in use.

At the beginning, observatories of the Magnetic Union only measured variations in magnetic declination. "Seeking the laws of natural phenomena has, for the natural scientist, its purpose and its value in itself alone," Gauss wrote in the introduction to the *Resultate*, "and a special enchantment surrounds the discovery of measure and harmony in what seems to be completely irregular. In following the wonderful play of the always changing variations of declination, the presently used apparatus leaves nothing to be desired with respect to certainty, precision, and ease of observation; but one cannot say the same of the means of observation [available] so far for the two other elements [inclination and intensity]."[25] As soon as the

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[23] Gauss to Göttingen U. Curator, 29 January 1833.

[24] Gauss, "Einleitung," 349.

[25] Gauss, "Einleitung," 350–51. Carl Friedrich Gauss, "Ein neues Hülfsmittel für die magnetischen Beobachtungen," in *Göttingische gelehrte Anzeigen*, 30 October 1837, 1721–28, repr. in *Werke*, vol. 5, 352–56, on 352.

apparatus for these elements was perfected, the Magnetic Union would study them too. Gauss did not expect that time to be far off.

Gauss's original, single-thread magnetometer gave the average value of the intensity over a period of time, but within it the intensity might change. In 1837 Gauss announced a new instrument "to fill the gap," the bifilar magnetometer, suited for making precise observations of magnetic intensity.[26] As its name suggests, this instrument is suspended from two "threads," actually a continuous loop of steel wire, instead of from a single thread as in the case of the earlier instrument. The "conflict" between the magnetic force acting on the needle and the restoring force of the apparatus when it has been displaced from equilibrium results in an "intermediate," or equilibrium, position of the apparatus.[27] Every change in the intensity of the Earth-magnetic force affects the position of the needle directly and can be easily, quickly, and precisely measured. The calculable mechanical restoring force gives an absolute measure of the magnetic force. With the aid of the bifilar magnetometer, the horizontal part of Earth magnetism could now be "as precisely observed as the stars in the sky."[28]

Gauss had reached an end of sorts. He had come to believe that the vertical part of the Earth's magnetism would never allow for similar precision, and he was not going to pursue it. The magnetometers he had invented had not required excessively fine and expensive mechanical work, but measurements of the vertical force would require it. An inclination apparatus of the type then available, which Gauss considered "very far from what was desirable," already cost considerably more than Gauss was given to spend in a year on the astronomical and magnetic observatories together.[29]

4.4 Extension of Techniques to Electricity

Electricity was for Gauss a "still almost completely new field," which like Earth magnetism he set out to explore in the same measuring way.[30] In 1832, he applied a magnetometer to galvanic as well as magnetic measurements, finding that it was a

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[26] Gauss, "Ein neues Hülfsmittel," vol. 5, 353. "Über ein neues, zunächst zur unmittelbaren Beobachtung der Veränderungen in der Intensität des horizontalen Theils des Erdmagnetismus bestimmtes Instrument," *Resultate … 1837*, 1–19, repr. in *Werke*, vol. 5, 357–73, on 358. The lecture to the Göttingen Scientific Society on which this paper is based was given on 19 September 1837. The subject is the "bifilar" magnetometer.

[27] Gauss, "Über ein neues … Instrument," 361.

[28] Gauss to Olbers, 2 September 1837, in *Briefwechsel zwischen Olbers und Gauss,* 649.

[29] Gauss to Olbers, 2 September 1837, in *Briefwechsel zwischen Olbers und Gauss,* 649–50.

[30] Gauss to Gerling, 28 October 1832, quoted in Schaefer, "Über Gauss' physikalische Arbeiten," 104.

"most precise" measurer "for the strongest as well as the weakest forces of a galvanic current," and he expected no difficulty in reducing galvanic measurements to absolute measures.[31] At about the same time, Weber brought him news of Fechner's recent measurements confirming Ohm's law, which were the "finest made so far." To Gauss they already seemed only "rough approximations."[32]

Michael Faraday's first publication on electromagnetic induction, the generation of an electric current in a conductor by a changing magnetic force, had appeared in Britain at the end of 1831, followed by a translation in the *Annalen der Physik*. This work naturally attracted Gauss's and Weber's interest. Weber justified his request for a large magnet in December of that year "especially now in following up Faraday's discoveries on producing the phenomena of the galvanic pile by strong magnets."[33] A few weeks later, Gauss justified his request for a magnetic observatory for its use in "almost innumerable other magnetic, galvano-magnetic, and electromagnetic observations and measurements," which would help in clarifying "many as yet dark parts in these theories." Over the next several months, Gauss used his apparatus "mainly for experiments on the so-called induction, which is one of the most interesting phenomena of nature and which I can make much more strongly visible."[34] Gauss's interest in Faraday's discovery of electric induction was an early step in what would become a trend: electric induction would be the subject of the most important German research on electrodynamics through the century.

Gauss and Weber perfected their instruments with one purpose in mind: to aid in the "investigation of the *mathematical laws* governing the production and action of the magneto-electric induction discovered by Faraday and its reduction to absolute measures."[35] The magnetometer was the necessary complement, Gauss said, to "Oersted's and Faraday's brilliant discoveries," which "have opened a new world to scientific research, the enchanted gardens of which fill us with admiration; we can subject these rich fields to our domination only under the guidance of the art of measuring."[36]

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[31] Gauss, "Intensitas vis magneticae terrestris," 301.

[32] Schaefer, "Über Gauss' physikalische Arbeiten," 104. Later Gauss did exacting galvanic measurements. By coiling a great length of wire around the magnetic bar and passing a current through it, he converted the bifilar magnetometer into a sensitive galvanometer; the weakest galvanic force deflected the twenty-five pound bar significantly. With the galvanometer, he measured frictional and thermoelectric currents as well as battery currents (Christoph Stähelin, "Wilhelm Weber in seiner allgemeinen Bedeutung für die Entwicklung und die Fortschritte der messenden und experimentirenden Naturforschung," in J. C. F. Zöllner, *Principien einer elektrodynamischen Theorie der Materie*, vol. 1, *Abhandlungen zur atomistischen Theorie der Elektrodynamik* [Leipzig, 1876], xcix–cxxiv, on c–ci, cxi).

[33] Weber to Göttingen U. Curator, 15 December 1832.

[34] Gauss to Göttingen U. Curator, 29 January 1833; Gauss to Schumacher, 21 March 1833, in Schaefer, "Über Gauss' physikalische Arbeiten," 127.

[35] Gauss, "Fortsetzung," 531–32.

[36] Carl Friedrich Gauss, "Erdmagnetismus und Magnetometer," *Schumacher's Jahrbuch für 1836*, 1–47, reprinted in *Werke*, vol. 5, 315–44, on 336.

Gauss and Weber's expansion of the magnetic facilities at Göttingen and their "development and perfection" of magnetic observations allowed them "to subject *galvanic* observations to analogous principles" in a number of electromagnetic investigations.[37] When Gauss gave his first magnetometer to the physical cabinet in 1834, there were then three observing locations in Göttingen: one in the magnetic observatory, used "when great precision is required," one in the astronomical observatory, and one in Weber's institute, all linked by a wire circuit.[38] With this arrangement, Gauss and Weber studied the intensity of the electric current in the circuit. They also tried to measure the velocity of electric current from the time it took to pass through a half mile of wire, but when the current was turned on the magnets in the three locations seemed to move simultaneously. In 1833, they set up the first electromagnetic telegraph between the physics institute and the observatory. In other research, they improved on Fechner's work by giving their own experimental proof of Ohm's law.

Then, rather suddenly, outside events brought their productive collaboration to a close. Weber was dismissed from his professorship for political reasons, and the Göttingen physics institute became unavailable to Gauss to help with the acquisition of apparatus. Gauss wondered whether to "bid the public farewell and announce the destruction of our association" or to continue: "The arrangement of the new intensity apparatus lets us look into a new world of wonders. But now that the way has been paved into it, the gate is to be slammed shut in our faces."[39] Gauss's frustration at being stopped in his work with Weber at a promising point referred mainly to their work on electromagnetism. Weber would be reinstated a few years later, a subject for another chapter.

Weber saw their work on Earth magnetism as the beginning of a new way of organizing research and education in physics. "It happens to be my conviction that the way in which physics has been treated so far is outdated and needs to be changed," he wrote in 1841, "and that our treatment of the magnetic problem is a first test. It goes against many deep-rooted practices and arouses in many the wish that something like this had not been started; but if it is carried out, it will soon develop further and affect beneficially all parts of science."[40] To an Irish colleague, he wrote in 1845 that magnetic observations had already become important for "*many other physical investigations*." He also described the

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[37] Weber to Edward Sabine, 20 February 1845, in Wilhelm Weber, *Werke*, vol. 2, *Magnetismus*, ed. Eduard Riecke (Berlin, 1892), 274–76.

[38] Göttingen U. Curator to Gauss (with a copy of the letter to Weber), 5 May 1834, authorizing the transfer, Göttingen UA, 4/V h/16. Carl Friedrich Gauss, "Eine Fortsetzung der am 9. August 1834 gegebenen Nachricht," *Göttingische gelehrte Anzeigen*, 7 March 1835, 345–57, repr. in *Werke*, vol. 5, 528–36, on 529–30.

[39] Müller to Hoppenstedt, 17 May 1838.

[40] Weber to Karl von Richthofen, 9 April 1841, Göttingen UB, Ms. Dept., Phil. 182.

simultaneous influence that the Earth-magnetic investigations had had on the organization and teaching of physics:

> In Germany . . . until now there existed only collections of physical instruments without permanent facilities for their use; there were no physical *laboratories* and *observatories*. Such laboratories and observatories, which have become indispensable for the advances of science, are now beginning to come into being, and the grants given for magnetic observations provide solid and reliable *support* in this, as I can attest from my own experience. . . . At our universities one finally increasingly recognizes the importance that the education of *exact observers of nature* has for science and for practical life. So far only astronomy has offered an opportunity, very one-sided, for the education of exact observers, which could be used by only a few. Experience has shown that magnetic observatories can serve as excellent *educational institutions* for observers.[41]

The slow realization of ideas like Weber's, our subject in the next chapter, came about through the efforts of physicists whose researches were done in the "Gaussian" spirit,[42] using the new mathematical and measuring techniques. It took them several decades to reach their goal of providing proper training for physicists, since it was not highly valued in government quarters. "It is of course sad to apply one's energy to things on which no value is placed as yet in the world or at least in Germany, especially in Berlin," Weber complained to a Berlin friend in 1841; but "with perseverance, one will make truth triumph in the end after all."[43]

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[41] Weber to Sabine, 20 February 1845.

[42] The term "Gaussian" was applied to Kirchhoff's work, for example.

[43] Weber to Richthofen, 9 April 1841.

Chapter 5

Reforms in Teaching University Physics: Development of the Seminar and the Laboratory in the 1830s and 1840s

Related to the idea of *Wissenschaft*, a unity of knowledge, the idea of *Bildung* was enlarged to include the natural sciences in the training of the intellect, lending them an intrinsic value standing above their utilitarian one. In addition to teaching their fields, professors of the natural sciences came to advance them through original research. In appointments and promotions, their publications and reputations in their fields became more important than their textbooks and local support, a change which was to play a part in the professionalization of physics and the other natural sciences. By the end of the period we examine in this chapter, the productive physicist was usually a professor with, if he was lucky, a seminar and a laboratory in his university.[1]

During the middle decades, the universities in Germany added another use for physics instruction: they then not only provided general education in physics for students of the traditional professions but also physics training for those—still few—who pursued a career in physics. Instead of having to depend solely on chance encouragement and self-study, university students who were attracted to physics were introduced to research methods by physics teachers who were themselves researchers. In the physics laboratory, students handled apparatus not only to learn about nature but also to learn methods of investigating nature. Likewise, in the seminar they solved mathematical problems in physics to learn methods of investigating nature mathematically. A new spirit of teaching physics—a wish to stimulate students to independent thought, as Ohm had urged—entered the universities where the better physicists taught. The new teaching found its place in new institutions within the university such as the teaching laboratory, the physics seminar, and the physics colloquium.

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[1] R. Steven Turner, "The Growth of Professorial Research in Prussia, 1818 to 1848-Causes and Context," *HSPS* 3 (1971): 137–82, on 137–41, 147, 165, 171, 180.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_5

5.1 First Teaching Laboratory for Physics

With Weber’s arrival in Göttingen in 1831, physics teaching soon underwent an important change. In his first comprehensive report to the university curator on the physical cabinet, he mentioned the apparatus that could be used “for the purpose of practical exercises and instruction in experimenting.” He thought that after a general survey of physics, it would be helpful for students who intended to become physicians, chemists, pharmacists, and gymnasium teachers of mathematics to have “practice experimenting and making reliable observations.” “Practical chemistry exercises” were already in place, held in the “academic chemical laboratory,” and taught alongside lectures on “theoretical chemistry.” Gőttingen as yet offered no physical laboratory course like the one for chemistry, the need for which Weber fully recognized. In the summer semester of 1833, his fourth semester of teaching, in addition to his lectures on experimental physics he offered “practical-physical exercises in the academic laboratory,” the first teaching laboratory in physics in Germany. From then on, the laboratory was a standard course along with the big experimental physics lectures at Gőttingen.

At Gőttingen, as a stopgap after Mayer’s death, lecture courses had been given on “theoretical physics” and “experimental physics, following Mayer’s textbook,” by a temporary teacher. When Weber took over in the winter semester 1831–1832, he gave the lecture course on “experimental physics,” and “theoretical physics” no longer appeared in the university course listing. Gauss was then giving courses mostly under the “mathematical sciences”—practical astronomy, higher geodesy, theory of planetary and comet motions, and probability calculus in applied mathematics, especially astronomy, geodesy, and crystallography—but he also gave a course on the “theory of magnetic phenomena” under “natural philosophy [Naturelehre],” which included physics. Together, then, Weber and Gauss taught the physics courses at Göttingen: Weber gave experimental lectures over the whole of physics and conducted laboratory exercises, and Gauss gave theoretical physics lectures on magnetism, paying attention to magnetic observations.[2] There was as

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[2] Weber to Göttingen U. Curator, 24 April 1832, Göttingen UA, 4/V h/15; *Gőttingische gelehrte Anzeigen* for the years 1830–1835. Weber’s financial report for 1833–1834, dated September 1834, Göttingen UA, 4/V h/8: Weber said that he was making progress in his plan to use the physics institute “not only . . . as a collection for viewing, but as a workshop in which uninterrupted scientific work will be carried on.” It is important to note that as yet and for some time to come only the professor’s own research would be done there. A comparison with Justus Liebig’s continuous use of the chemical laboratory for scientific work—Weber himself spoke of his own “workshop” as being like a chemical laboratory in the sense that it should be continuously used for scientific work—shows that physics still lacked the counterpart of simple methods of analysis that allowed chemistry professors to use students in scientific investigations. Weber soon found comparable employment for some of his students in Earth-magnetic observations, so that the development of Earth magnetism can be seen as one of the ways of introducing the practice of student research in the German physical institutes (Weber to Göttingen U. Curator, 16 October 1835, Göttingen UA, 4/V h/16).

yet no seminar for physics. We return to laboratory instruction in the next section in connection with seminars.

5.2 Seminars for Physics

Improvements in secondary school education in the early nineteenth century made greater demands on the training of teachers. Because of the emphasis on the classical languages in the gymnasiums, the demands were first met by the philologists in the philosophical faculty. In the past, it had been the practice to allow theology students or preachers waiting for an appointment to a parish to teach classical languages in the secondary schools. Now these classes were taught by philologists properly trained in university philology seminars. When it came time for mathematicians and natural scientists to train secondary school teachers, they looked to the philology seminars as a "finished model." The success of these seminars together with a reminder of recent developments in natural sciences persuaded most German ministries of education to establish seminars specifically for the teaching of natural sciences and mathematics, beginning in the 1830s.

The idea of a seminar for mathematics and the natural sciences was first referred to in official documents and in the writings of individual scientists around the middle of the 1820s. In 1824, for example, the curator of Heidelberg University, at the instigation of the professor of mathematics, proposed to the Baden ministry of the interior the establishment of a "mathematical seminarum," pointing out that such a step would reserve for their state the "fame" of having been the first in Germany to create such an institution.[3] In 1834 Franz Neumann and the mathematician C. G. J. Jacobi set up a mathematical-physical seminar at Königsberg[4] as a parallel institution to the planned seminar for the natural sciences, which included experimental physics together with its professor, Ludwig Moser. At Berlin in 1834, the mathematician Gustav Lejeune Dirichlet began a private mathematical seminar in his house. Freiburg University established a seminar for mathematics and the

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[3] Heidelberg U. Curator Froehlich to Baden Ministry of the Interior, 23 February 1824, Bad. GLA, 235/3228. The Heidelberg classical philologists were not quite ready yet to give up their monopoly of secondary school education and concede the usefulness of seminars in other fields, even mathematics. One member of the faculty suggested, sarcastically, that seminars for Arabic, Persian, or physics might be next. Their dominance did not come to an end until 1863. (Bernhard Riese, *Die Hochschule auf dem Wege zum wissenschaftlichen Grossbetrieb. Die Universität Heidelberg und das badische Hochschulwesen 1860–1914*. Industrielle Welt, Schriftenreihe des Arbeitskreises für moderne Sozialgeschichte, ed. W. Conze [Stuttgart: Ernst Klett, 1977], vol. 19, 194, 196).

[4] "Statuten des mathematisch-physikalischen Seminars an der Königsberger Universität," February 1834, Göttingen UA, 4/V h/20. Also Albert Wangerin, *Franz Neumann und sein Wirken als Forscher und Lehrer* (Braunschweig: F. Vieweg, 1907), 150.

natural sciences in 1846. Göttingen, Munich, Breslau, Heidelberg, and Tübingen all established mathematical-physical seminars in the 1850s and 1860s.[5] With reference to the Göttingen mathematical-physical seminar, the mathematician Stern said that it had a "scientific purpose," which seminars that combined several autonomous natural sciences did not. The reason for the difference was the intimate relationship of mathematics and physics, for by definition a "thorough" education in physics required mathematics, making the "separate training of mathematicians and physicists no longer thinkable."[6]

The seminars offered future gymnasium teachers of mathematics and the natural sciences the opportunity to learn how to present their subject, handle basic instruments, and conduct observations and simple experiments in the most important natural sciences. The training of these teachers was the original justification for establishing seminars, but it was an insufficient reason for them to survive and flourish. When in 1855 Wilhelm Weber and his co-director J. B. Listing of the Göttingen seminar were confronted with the charge that they were producing too many gymnasium teachers, they replied that even though the "immediate" purpose of the seminar was the training of teachers, this was not their "primary" purpose. They explained that the seminar was meant to improve university studies in the mathematical-physical sciences, to promote scientific endeavors, and to contribute to the training of researchers.[7] Neumann likewise expected the Königsberg mathematical-physical seminar to promote the serious study of physics including

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[5] Freiburg: *Statuten des Seminars für Mathematik und Naturwissenschaften an der Universität zu Freiburg im Breisgau* (Freiburg i. Br., 1846); also Freiburg U. Senate to Baden Ministry of the Interior, 7 September 1846, Bad. GLA, 235/7766; Göttingen: "Statuten des mathematisch-physikalischen Seminars zu Göttingen," 11 February 1850, Göttingen UA, 4/V h/20. Munich: "Statuten für das mathematisch-physikalische Seminar an der kgl. Universität München" and official decree establishing the seminar, 12 June 1856, Munich UA, Sen. 209, Nr. 5213. Breslau University, *Festschrift zur Feier des hundertjährigen Bestehens der Universität Breslau, Geschichte der Fächer, Institute und Ämter der Universität Breslau 1811–1911,* ed. Georg Kaufmann (Breslau: F. Hirt, 1911), pt. 2, 440. Heidelberg: Kirchhoff and Leo Königsberger to Baden Ministry of the Interior, 14 April 1869, including "Statut für das mathematisch-physikalische Seminar in Heidelberg," Bad. GLA, 235/3228. Tübingen: Minister of Church and School Affairs to King of Württemberg, "Anbringen ... betreffend das mathematisch-physikalische Seminar in Tübingen," 25 July 1887, HSTA, Stuttgart, B14, B1475; also Karl Klüpfel, *Die Universität Tübingen in ihrer Vergangenheit und Gegenwart dargestellt* (Leipzig, 1877), 128; Württemberg, Statistisches Landesamt, *Statistik der Universität Tübingen*, ed. K. - Statistisch-Topographisches Bureau (Stuttgart, 1877), 65; *Festgabe zum 25. Regierungs-Jubiläum seiner Majestät des Königs Karl von Württemberg* (Tübingen, 1889), 14.

[6] Stern to "Ministerial-Vorstand" Braun, 10 October 1849.

[7] "Bericht des Seminars," 19 November 1855, Göttingen UA, 4/V h/20. In the first annual seminar report in 1851, Weber had already said that many members of the seminar did not intend to become secondary school teachers; the students were coming to the seminar for other reasons. "Jahresbericht des Vorstandes des mathematisch physikalischen Seminars, den Zustand des Seminars von Ostern 1850 bis dahin 1851 betreffend" to Göttingen U. Curator, 31 March 1851, Göttingen UA, 4/V h/20.

independent scientific work.[8] Physicists came to use seminars in ways that only incidentally contributed to the training of secondary school teachers.

Seminars benefited professors as well as students. Physics professors often found in seminars a partial remedy for insufficient facilities and apparatus, and because formerly they lacked students for all but the general physics lectures, seminars guaranteed them at least some students with the proper preparation for their advanced lectures. That physicists planned to use their seminars in this way is clear from the statutes for Göttingen: "The mathematical-physical seminar . . . is to give students who dedicate themselves primarily to mathematics and physics the opportunity to acquaint themselves with such parts of the sciences that are treated briefly or not at all in the usual academic lectures."[9] The prerequisites of the Königsberg seminar exceeded those at Göttingen and elsewhere, admitting as ordinary members only students majoring in mathematics or physics who knew the differential calculus, the beginnings of the integral calculus, and the main subjects treated in Fischer's physics textbook.[10] Seminar directors could insist on regular attendance, an advantage that no lecturer enjoyed, and seminar rules generally demanded that members attend all of the exercises and meetings. To receive credit for the seminar, members had to attend the whole course, which sometimes extended over several years.

5.3 Running the Seminar

In this account of how physicists used seminars, we draw mainly on the experiences of the early seminar directors Neumann and Weber. Although the Königsberg mathematical-physical seminar was set up with statutes to guide its work, Neumann ran the physical section as circumstances and students' progress dictated, not by the book.[11] Instead of the prescribed two sections of advanced and beginning students, he sometimes had only advanced, sometimes only beginning, students, but often both and often working together in some fashion. A reasonable regularity in the work of his section was assured by the order of his private lectures: from 1838 until about 1860, he devoted his four hours of weekly lectures to three subjects, "theoretical physics," "theory of light," and because he was professor of physics and

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[8] Wangerin, *Neumann*, 153; Kirchhoff and Königsberger to Baden Ministry of the Interior, 14 April 1869, Bad. GLA, 235/3228.

[9] "Statuten . . . Göttingen."

[10] For example, Neumann's seminar report for 1873–1875, quoted in Luise Neumann, *Franz Neumann, Erinnerungsblätter von seiner Tochter*, 2nd ed. (Tübingen: J. C. B. Mohr [P. Siebeck], 1907), 443. For the requirements for the Königsberg seminar, "Statuten . . . Königsberger Universität."

[11] The following discussion of Neumann's seminar is based primarily on Neumann's seminar reports, the drafts of which are in the Neumann Papers in the Manuscript Department of the Göttingen University Library.

mineralogy, "mineralogy," each taught for a semester, the whole taught as a regular cycle, which he rarely varied.[12] The cycle affected his seminar teaching, since he correlated the start of seminar work with his "theoretical physics" lecture course. Beginning students were expected to take the seminar and the course together or to postpone the seminar until after they had completed his lectures. His lectures affected the seminar in another way: in seminar discussions and homework assignments, he either prepared his students for his lectures or he deepened their understanding of what he had covered in his lectures, testing their understanding by letting them apply what they had learned; in the seminar, he also augmented lectures he had been unable to complete in class or to treat thoroughly enough. Only in the case of advanced students was seminar work independent of his lectures. In the 1850s he came closest to making the seminar what he thought it should be. This was largely because he set up a laboratory in his house after having failed to receive one from the Prussian ministry, despite repeated applications and promises.[13] One reason he needed a laboratory was to teach the students how to bridge the "chasm" between "theoretical understanding and practical execution." A second reason was Neumann's own research, as he explained:

> The academic teacher who is conscious of his calling knows that he is called not only for instruction in his discipline but he is also supposed to take part in its development and advancement; he also knows that his effectiveness is fruitful and secured only to the extent to which he feels himself to be an independent researcher in that science. If he is denied the means for his own scientific work, as he is if he is not granted a laboratory, then he will take the first opportunity to leave the university.[14]

For his students and for himself, he wrote, the "ultimate purpose and the real goal" of the seminar was to make "measuring observations" for determining physical phenomena.[15]

As a result of Weber's dismissal in 1837 and his reappointment in 1849, Göttingen acquired two physics institutes, Weber's for experimental physics, and Listing's for

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[12] Neumann taught "Mineralogy" from 1827, "Theory of Light" from 1830, and "Introduction to Theoretical Physics," a course on the mechanical foundations of physics, from 1838. From 1861 on, he extended that series to five semesters by adding lecture courses on the theory of electric currents and the theory of elasticity. The five courses covered the major areas of Neumann's researches. Almost from the start, according to Voigt, Neumann's lectures on mineralogy included all areas of theoretical physics. (Woldemar Voigt, "Zur Erinnerung an F. E. Neumann, gestorben am 23. Mai 1895 zu Königsberg i/Pr." *Gött. Nachr.* [1895]: 254–55). In time, he also gave separate two-hour public lectures on the parts of theoretical physics. The complete list of the lectures Neumann actually held—as opposed to the lectures he announced—together with the numbers of students who attended is given by Paul Volkmann, *Franz Neumann. 11. September 1798, 23. Mai 1895* (Leipzig, 1896), 56–58. Neumann's lectures were published by some of his students in the 1880s.

[13] Neumann, *Franz Neumann, Erinnerungsblätter von seiner Tochter*, 373.

[14] Ibid., 455.

[15] Ibid., 372: quotation from Neumann's 1849 report to the ministry.

mathematical physics (an important development we discuss later). The two professors were free to teach subjects belonging to the other's domain, and they both participated in the mathematical-physical seminar, founded soon after Weber's return. The mathematical section of the seminar was directed by two professors of mathematics. The plan for the physics section was for meetings to be held in the physics laboratory for two to four hours each week, where the two directors, Weber and Listing, each in his own part of the laboratory, would lecture on subjects of "theoretical physics" in connection with "exercises in observations and measurements." Since Weber got the lion's share of the apparatus and rooms in the original institute along with a larger budget than Listing's, his part of the seminar offered students most of their practice in "observing and measuring physics." In addition to conducting laboratory exercises, members of the seminar reported on the contents of physics treatises and papers in physics journals relating to the exercises.[16] The subjects were chosen mainly from the directors' own researches, since their institutes were equipped with the relevant instruments and apparatus. In the second year of the seminar, for example, Weber chose as subjects "the theory of the bifilar magnetometer together with the determination of its elements" and "the theory of the electrodynamometer."[17]

The Königsberg and Gőttingen seminars were influential in German university physics education in different ways. The Göttingen seminar gave rise to an institution devoted entirely to laboratory training. Because there were usually a dozen or more students in the seminar, the setting up of laboratory equipment for the exercises took more of Weber's time than he was willing to give, and in 1866 the position of a salaried assistant at the physics institute was entered as an item in the university budget. The position was filled by Friedrich Kohlrausch, whose organization of experimental physics instruction within the mathematical-physical seminar became a model for laboratory courses at other German universities.[18] Unlike Weber, Neumann lacked adequate laboratory facilities, and as a result he regarded his teaching of "mathematical physics" at Königsberg as only a "shadow" of what it could have been.[19] Nevertheless, the theoretical emphasis in his seminar teaching came to be imitated elsewhere.[20]

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[16] "Statuten . . . Göttingen."

[17] Our discussion of the Göttingen seminar is based mainly on the annual seminar reports for 1850–1870 to the Göttingen U. Curator, Göttingen UA, 4/V h/20.

[18] Weber to Göttingen U. Curator, 18 October 1866, Göttingen UA, 4/V h/21.

[19] Paul Volkmann, "Franz Neumann als Experimentator," *Phys. Zs.* 11 (1910): 932–37, on 936.

[20] At Würzberg, for example. "Gehorsamster Bericht der philosophischen Fakultät, hier, das mathematische Seminar betr.," 9 December 1872, in Maria Reindl, *Lehre und Forschung in Mathematik und Naturwissenschaften, insbesondere Astronomie, an der Universität Würzburg von der Gründung bis zum Beginn des 20. Jahrhunderts* (Neustadt an der Aisch: Degener, 1966), 214–15.

5.4 Other New Institutions for Physics

Simultaneously with its development in Gőttingen, experimental physics instruction was reorganized by Gustav Magnus at Berlin University. He had an advantage that neither Weber nor Neumann enjoyed, wealth, which he freely drew on to support his teaching, advancing money to Prussia to set up a university laboratory for the new way of teaching physics. Although Magnus had shown early ability in mathematics, in his research he worked as an experimentalist. In 1831 he began teaching technology at Berlin University, and he soon added physics to his teaching, advancing to extraordinary professor for the two subjects in 1834. The university then had almost no physics instruments and apparatus. Magnus found a way around this by agreeing to buy the most important apparatus himself and to turn it over to the state gradually for reimbursement, building a respectable physical cabinet of over 400 pieces by 1860.[21] For the first 10 years, while Erman was still the ordinary professor of physics, Magnus supplied a place to keep and use the university's apparatus. He lectured in his house and, lacking a university laboratory like Gőttingen's, he allowed advanced students to do research in his home laboratory and to use his and the university's apparatus and his library. In 1844 he returned the university's apparatus so that two extraordinary professors of physics could use them. He acquired for his use two rooms at the university, one for apparatus and one for lectures, but because there was no space for physical investigations, he continued to conduct student research in the laboratory in his house, which in 1863 was designated as the official Berlin "physical laboratory." A good many prominent physicists got their early experience doing experimental research there, including Helmholtz, Gustav Wiedemann, August Kundt, and Emil Warburg.[22] In 1867 Magnus proposed a proper physics institute, which was approved.[23]

As he did in the laboratory, Magnus brought teaching and research together in the weekly "physics colloquium," which he began holding in his house in 1843. "One of the most important means of physical instruction," the colloquium directed young researchers to recent publications and gave them practice in the art of lecturing. Magnus encouraged open criticism of researches by welcoming polemics against his own work, and publications that were reported on at the meetings generally were "horribly dismembered and picked to pieces."[24] The physics

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[21] Heinrich Rubens, "Das physikalische Institut," in *Geschichte der Königlichen Friedrich-Wilhelms-Universität zu Berlin*, by Max Lenz (Halle a. d. S.: Buchhandlung des Waisenhauses, 1910–1918), vol. 3, 278–96, on 278–80; *Die naturwissenschaftlichen und medicinischen Staatsanstalten Berlins. Festschrift für die 59. Versammlung deutscher Naturforscher und Aerzte*, ed. Albert Guttstadt (Berlin, 1886), 140; Peter Pringsheim, "Gustav Magnus," *Naturwiss.* 13 (1925): 49–52, on 49–50.

[22] In a single volume of the *Annalen* in 1858, six of the nine university physicists publishing in it were Magnus's present or former students; another was at Berlin University.

[23] Rubens, "Das physikalische Institut," 279–82; Guttstadt, *Staatsanstalten*, 140.

[24] Observations by two participants in Magnus's colloquium: Gustav Wiedemann, *Ein Erinnerungsblatt* ([Leipzig], 1893), 7; Adolph Paalzow, "Stiftungsfeier am 4. Januar 1896," *Verh. Phys. Ges.* 15 (1896): 36–37.

colloquium was adopted by other German universities, becoming like the laboratory and the seminar a standard institution.

Through his colloquium, Magnus's teaching gave rise indirectly to another major institution of German physics. From time to time, some younger members of the colloquium met separately in one another's rooms to continue their discussions over tea, and in 1845 they formally organized as the Berlin Physical Society. Five of the six founding members worked in Magnus's laboratory, while the sixth had a laboratory of his own.[25] They were not all physicists, but they all wanted to be familiar with physical approaches in science, the physiologists among them looking to physics to free their science of the notion of a special life force with its overtones of nature philosophy.[26]

Although the established Berlin scientists including Magnus stayed away, by the end of the first year the Society had fifty-three members, who included a half dozen mechanics and as many army officers and a good many others who, like Helmholtz, wrote "Dr." in front of their names.[27] Membership fluctuated, but on the average it grew, doubling over the first 15 years. Most of the members were from Berlin, but a scattering were from Bonn, Heidelberg, and even farther away.[28] Although the Berlin Physical Society was by no means the only or the first physical society in Germany,[29] it was the most important. Toward the end of the century, its prominence was recognized by its new name, the *German* Physical Society.

Members of the Society met every 2 weeks to report on their own or on others' work. At first the reports were given mainly by the founders of the Society, but soon increasingly by new members: Kirchhoff began presenting papers in 1847, and the same year Helmholtz presented his first paper on physics, Clausius joining them not long after.[30] The work discussed in the early meetings was often important because

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[25] Gustav Karsten's foreword in *Fortschritte der Physik im Jahre 1845* (Berlin, 1847), vi; H. Ebert, "Die Gründer der Physikalischen Gesellschaft zu Berlin," *Phys. Bl.* 6 (1971): 247–54, on 248–49. The six were the physicists Wilhelm Beetz, Gustav Karsten, and Hermann Knoblauch, the physiologists Emil du Bois-Reymond and Ernst Brücke, and the chemist Wilhelm Heintz.

[26] Gustav Wiedemann, "Stiftungsfeier am 4. Januar 1896," *Verh. Phys. Ges.* 15 (1896): 32–36, on 33.

[27] Wilhelm von Bezold's address given on 4 January 1896, repr. in "Zur Vollendung des 50. Jahrganges der 'Fortschritte,'" *Fortschritte* 50, no. 2 (1896): ii–vii, on iv. Membership list in *Fortschritte . . . 1845*, vii–viii.

[28] Membership had fallen to 48 in 1848, but it recovered the following year. In 1851 it was seventy, in 1854 eighty-seven, in 1859 ninety-seven (*Fortschritte* for these years). Of the 94 members in 1857, for example, 36 lived outside Berlin and a good many of these lived outside Germany (*Fortschritte* for 1857).

[29] For example, the Physical Society of Stettin began in 1835 and had a long life. Its founder and first president was Justus Günther Grassmann, a Stettin gymnasium teacher whose physics lectures led a number of men to desire a society to follow the progress of the physical sciences. His son, the mathematician Hermann Grassmann, was later president of the society. Friedrich Engel, *Grassmanns Leben* (Leipzig: B. G. Teubner, 1911), 63. This is vol. 3, pt. 2 of *Hermann Grassmans gesammelte mathematische und physikalische Werke*, ed. F. Engel, (Leipzig: B. G. Teubner, 1894–1911).

[30] From the volumes of the *Fortschritte* for these years. Clausius joined only in 1851; Helmholtz thought that Clausius held back out of personal regard for Magnus, believing that Magnus did not welcome this offspring of his colloquium, the Physical Society (Helmholtz, "Zur Erinnerung an Rudolf Clausius," *Verh. Phys. Ges.* 8 [1889]: 1–7, on 1–2).

of the scientific ability of its members and because the Society was founded at a time of outstanding research in physics.[31]

The main work of the Society was the compilation of an annual report, *Fortschritte der Physik*, edited by one or more of the members. It included the proceedings of the Society, though its main object was to survey the past year's research, calling on members of the Society to contribute critical reports on publications in their specialties. The *Fortschritte* was a systematic response to the problem that Magnus's colloquium had confronted, and Magnus himself had considered bringing out a "yearly physical report".[32]

Despite competent help, the editors had taken on an almost overwhelming task. By the second year, 1846, the *Fortschritte* surveyed over 800 journals in several languages and reported on hundreds of papers by hundreds of authors.[33] The literature it reviewed was often not the last word on the subject by the time it came out: the 1845 volume appeared in 1847, the 1848 volume in 1852, and eventually the volumes fell behind by as many as 7 years, giving rise to other bibliographic efforts.[34] But in the middle years of the century, for German physicists the *Fortschritte* was the indispensable guide through the world's physics literature, selecting, describing, evaluating, and organizing it.[35]

Of all the physicists to teach in Berlin, the "organizer" Magnus accomplished the most.[36] With his fine collection of physical instruments, he conducted the physics program during the long transition in Berlin from the physics professor's home laboratory to the state owned university physics institute. In his will, he left his remaining instruments and his library to the university, completing the transition. Although he did not live to see the new physics institute building—his successor, Helmholtz, received it—he planned it, and his colloquium was continued by others after him.

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[31] E. Brüche, "Aus der Vergangenheit der Physikalischen Gesellschaft," *Phys. Bl.* 16 (1960): 499–505, 616–21, on 616–21.

[32] Emil Warburg, "Zur Geschichte der Physikalischen Gesellschaft," *Naturwiss.* 13 (1925): 35–39, on 35. The first editor was Gustav Karsten, who was also the first president of the Society. Bezold's 1896 address, iii; Wiedemann, "Stiftungsfeier," 35.

[33] To be exact, there were 726 articles by 365 authors in 1846. During the first fifteen years of the *Fortschritte*, the annual number of articles reported varied roughly between 500 and 900 and the number of their authors between 300 and 800.

[34] Karl Scheel, "Die literarischen Hilfsmittel der Physik," *Naturwiss.* 16 (1925): 45–48, on 47.

[35] The reports were grouped in the large divisions corresponding to the way physics was organized in the standard textbooks.

[36] Rubens, "Das physikalische Institut," 282. Magnus's organizational talent was not limited to the physical arrangements we discuss here; he applied it generally in the affairs of the university, in the Prussian Academy, and in government commissions (Helmholtz, "Gustav Magnus. In Memoriam," in *Popular Lectures on Scientific Subjects*, trans. E. Atkinson [London, 1881], 1–25).

Chapter 6
Physics Research in "Poggendorff's *Annalen*" in the 1840s

6.1 Foreign Recognition of German Physics

In an important respect, Poggendorff's *Annalen* in the middle of the nineteenth century was a different journal than the one he began editing twenty some years before. New foreign physics in translations and reports still had a prominent place in it, but the German physics appearing there was no longer in its shadow. The *Annalen* now regularly published work by German physicists that equaled, and occasionally surpassed, the best work by foreign physicists. At the same time, German work was gaining recognition abroad. This was a greater achievement than one might think, particularly in France. Humboldt had found in his many years abroad that the German language did not "flourish excessively in the great Babel," and that at the Institut de France "almost everything is lost that is sent in in German without excerpt and explanation."[1] Even a paper by Gauss might get lost, the reason why Humboldt translated his paper on absolute measures before submitting it. The *Journal de mathématiques pures et appliquées* published no German mathematical physics at all in the early years following its founding by Joseph Liouville in 1836. Through the 1830s, the *Comptes rendus* and the *Annales de chemie et physique* together published only about a dozen papers on German physics. In the 1840s these journals added nearly a dozen new German names, mainly those of experimentalists though including the mathematical physicists Neumann and his student Kirchhoff. It was not until the 1850s that German physics came to be published in the *Annales* as copiously as foreign physics had long been published in the German *Annalen*. Twenty-five German physicists and well over 100 of their papers

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[1] A. v. Humboldt to Gauss, 17 February 1833, in Alexander von Humboldt, *Briefe zwischen A. v. Humboldt und Gauss. Zum hunderjährigen Geburtstage von Gauss am 30. April 1877,* ed. Karl Bruhns (Leipzig, 1977), 23.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_6

appeared in the *Annales* between 1850 and 1863, and German mathematical physics now received almost as much attention as German experimental physics. Experimentalists such as Magnus, Plücker, and Buff were represented by more papers than before, and the mathematical physicist Clausius appeared eight times and Kirchhoff thirteen. Liouville*'s Journal*, however, continued to publish very little German mathematical physics: of the new physicists, only Clausius appeared there with a single paper on the mechanical theory of heat in 1855.[2]

In Britain in the 1830s and 1840s, German physics papers appeared very occasionally in the *Philosophical Magazine* and the *Edinburgh New Philosophical Journal*, but frequently in *Taylor's Scientific Memoirs*. The *Memoirs* published the work of experimentalists, Magnus again, H. W. Dove, and Hermann Knoblauch, and also a good many theoretical works. The volume for 1841 contained a translation of Ohm's 1827 theory, *Galvanic Circuit,* as well as of ten other, more recent German physics papers, mainly those by Gauss and Weber dealing with Earth magnetism. The volume for 1853 was devoted almost entirely to German work, including Helmholtz's memoir on the conservation of force and many papers by Clausius. This being the last volume of the *Memoirs*, from then on the *Philosophical Magazine* assumed the responsibility of frequently publishing German physics papers. By the 1850s the work of German physicists could be read in translation in Britain about as regularly as the work of British physicists could be read by German physicists in the *Annalen*.

6.2 Physicists and Physics Appearing in the *Annalen*

In the 1840s, the German physicists who had gained a good reputation abroad were, by and large, the prominent authors in the *Annalen* as well. To publish in this journal was almost a necessity for any physicist hoping for a university career. With few exceptions, all of the established physicists published there, most of them

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[2] Our account of German physics in foreign publications is based on our survey of the journals mentioned. The work by German physicists appearing in the *Comptes rendus* of the Paris Academy of Sciences constituted a minute fraction of the journal's contents. The German physicists who published there in the 1840s were Dove (2 papers), Holtzmann (1), Kirchhoff (2), G. Karsten (1), Magnus (2), J. R. Mayer (3), Moser (2), Plücker (3), Poggendorff (2), Reich (2), and Wiedemann (1). In the 1840s Liouville's *Journal* published only three papers on mathematical physics by Germans: by Gauss (1) and Neumann (2). In the same decade, the *Annales* published papers by Buff (1), Dove (2), Magnus (3), Moser (1), Poggendorff (4), and A. Seebeck (1). In the subsequent period, 1850–63, the *Annales* published work by many more German physicists: Beer (2 and 1 with Plücker) Beetz (2), Buff (9 and 1 with Wöhler), Clausius (8), Dove (2), Eisenlohr (2), Hankel (1), Helmholtz (8 including some on physiology), Holtzmann (1), Kirchhoff (13), Knoblauch (6) R. Kohlrausch (3), Magnus (13), J. R. Mayer (1), J. Müller (2), Neumann (1), J. F. Pfaff (2), Plücker (8), Poggendorff (4), Quincke (7), Reich (3), Riess (6), Weber (1 with Kohlrausch), Wiedemann (11), and Wüllner (3); Gauss appeared with a paper on mathematical physics.

regularly, as one can see by comparing the men in the physics chairs of the German universities with the authors in the *Annalen* over a few years, 1840–1845. The only exceptions were three or four professors belonging to an earlier time in physics and to earlier publication practices and a philosopher and a mathematician who happen to be charged with teaching physics.

The *Annalen* tells us a good deal about the German researchers and the nature of their interests and work in the middle of the century. In 1840–1845, as before, the *Annalen* came out in issues that were bound in three volumes per year, each containing about 600 pages. Poggendorff allocated roughly 40% of the space to physics, the rest to chemistry, his other major subject, and to related border sciences such as mineralogy, meteorology, and physiology. Of the space devoted to physics, he gave over about half, roughly 400 pages each year, to German physics and half to foreign physics, the foreign portion including a steady flow of results from the laboratories of Faraday and Victor Regnault. Most of the German physics papers in the *Annalen* were by physicists who already had or would soon have an ordinary university professorship. In the volumes for the years we consider here, twenty such physicists contributed over half of the work; another quarter came from Poggendorff and Peter Riess; and the rest was made up of the occasional, usually brief contributions by thirty or more scientists and teachers largely outside of university physics. Expressed in terms of pages, each of the nine most productive university physicists—Fechner, Moser, Pfaff, Dove, Magnus, Ohm, Seebeck, and Wilhelm Hankel—published the equivalent of two ten-page papers per year in the *Annalen*. The two Berlin Academy physicists Riess and Poggendorff published more, the equivalent of four to six such papers per year. For the rest, a five- to ten-page paper every 2 years or so was about the rule.[3]

Three of the ordinary professors of physics—Fechner, Weber, and Neumann—published theories in the *Annalen* in the mid-1840s, all on electrodynamics and all dealing with Faraday's induction. The work of greatest consequence for the future development of physics was about to appear in the first publications of several young men who had just graduated or were still students: Clausius, Helmholtz, and Kirchhoff. Ottokar von Feilitzsch, who made a strong effort at the start of his career but did not sustain it as the other three did, began at the same time. In the 1840s, all four came to be associated with the new professor of physics at Berlin, Magnus. Clausius and Feilitzsch had been two of the original ten participants in Magnus's colloquium, and through him they kept in touch with university research while working elsewhere.

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[3] The survey of the *Annalen* for these years, on which the figures and discussions of the research published there are based, is our own. In 1840–1845, the ordinary professors of physics publishing in the *Annalen* were Buff (1 paper, 13 pages), Dove (8, 151), Fechner (6, 127), Magnus (4, 77), Moser (7, 143), J. Müller (1, 10), Muncke (1, 1), Neumann (1, 28), Osann (1, 25), C. H. Pfaff (4, 93), Pohl (1, 24), and Weber (5, 90). German physicists publishing there who later became ordinary professors were: Beetz (1, 18), Feilitzsch (3, 58), Hankel (6, 121), G. Karsten (2, 33), Kirchhoff (1, 18), Knoblauch (1, 12), Ohm (5, 98), and A. Seebeck (8, 169). Physicists at the Berlin Academy were: Poggendorff (18, 355) and Riess (10, 194).

Contributions to mathematical physics also came from outside the universities. The Berlin gymnasium professor Emil Wilde published on mathematical optics, and the Stettin gymnasium professor Hermann Grassmann published on mathematical electrodynamics. While they were teachers in technical schools, Seebeck and Holmberg published on mathematical acoustics. Gotthilf Hagen, a building official and member of the Prussian Academy of Sciences, published on fluid problems, combining theoretical with experimental approaches. More names might be added, but not many. Work on mathematical theory added up to only a small share of the whole of German research in physics; entire volumes of the *Annalen* appeared without any mathematical theory. Sparse as the work was, it nonetheless testified to the presence in Germany of at least a half-dozen physicists with strong ability in mathematical physics. Special teaching positions for the subject had hardly begun to appear, but the inclination and talent for doing research in it were evident.

6.3 Common Ground of the Sciences in the *Annalen*

We add to the above young men in Berlin the Jena Privatdocent Ernst Erhard Schmid, who at this time published mathematical work on the wave theory of light, following from his dissertation. With a remarkable dedication (for this late date) to the natural sciences in their "whole compass," he pursued, in addition to physics, mathematics, meteorology, physiology, chemistry, technology, and increasingly, mineralogy and geology. His work was seen as being "very splintered," with the result that he contributed little to physics.[4] *Annalen* authors understood that research was done by persons identified with individual sciences, by physicists, chemists, and other more or less specialized scientists, an acknowledgment of the "now more than ever unavoidable division of labor." The need to specialize was accepted, but so was the ideal of shared labor in closely related fields, and Poggendorff's policy was to include works bearing on physics and chemistry originating in the other natural sciences. From the side of mineralogy, a topic such as a new mineral species belonged in the journal; for a mineral has physical attributes such as internal and external structure, hardness, specific gravity, and responses to light and heat, and it also has chemical attributes such as its chemical constitution and its behavior in acids.[5] Matter obeys physical and chemical laws whether it burns in the sun or over the chemist's flame, whether it rises in the vessels of a tree or in the physicist's capillary tube, whether it belongs to the Earth's crust or to the mineral cabinet, or whether it is the free air or the air caught in a cylinder. For these reasons, the Earth, the heavenly bodies, the atmosphere, and man-made

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[4] Wilhelm von Gümbel, "Schmid: Ernst Erhard," *ADB* 31 (1890): 659–61, on 660.

[5] Theodor Scheerer, "Polykras und Malakon, zwei neue Mineralspecies," *Ann.* 62 (1844): 429–43, illustrates the point.

objects all belonged to the circle of interest of the *Annalen*: all had their physics and chemistry.

No subject in the *Annalen* in these years engaged the overlapping interests of physics and chemistry more persistently than galvanic currents. Physicists and chemists could claim galvanism as their subject with equal right: electric current deflects the physicist's galvanometer needle and dissociates the chemist's compounds. Galvanism and electrical phenomena in general, when joined with phenomena from other branches of physics, defined a good many of the research problems for *Annalen* authors. They wrote about the connection of electricity with heat in pyroelectric crystals and on its connection with optics in Nobili's electrically deposited colored rings. They published on the tones produced by interrupting the current in a wire spiral inside an iron cylinder, connecting electricity with acoustics; on the magnetization of steel needles by an electric spark, connecting electricity with magnetism[6]; on electrical crystal axes and electrical pictures, connecting electricity with other parts of physics.[7]

From the theoretical side, *Annalen* authors made connections between electrical and other phenomena often by applying ideas of electrical motions of one kind or another. Although the older idea of magnetic fluids was still in use, Ampère's reduction of magnetism to aligned electric currents around molecules appealed to many physicists for its linking of the "area of electricity" to the "area of magnetism."[8] Electrical resistance was explained by one author as electric motion that disappears in conduction and reappears as heat, light, magnetism, chemical action, and other kinds of motion, each of which is a relative measure of electric motion. Another author explained electric sparks as electric oscillations in the ether under tension.[9]

The basic theoretical concept of physics is motion, and the prevalence of wave motion in nature drew the special attention of *Annalen* authors. Because according to the "present state" of knowledge, light and radiant heat are wave motions, the remaining question for Ernst Brücke was whether or not they are the same wave motion differing only by wavelength. As there is no "mechanical understanding" of

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[6] Peter Riess and Gustav Rose, "Ueber die Pyroelectricität der Mineralien," *Ann.* 59 (1843): 353–90. Emil du Bois-Reymond and Wilhelm Beetz, "Zur Theorie der Nobili'schen Farbenringe," *Ann.* 71 (1847): 71–91. Anon., "Tönen beim Elektromagnetisiren," *Ann.* 63 (1844): 530. Wilhelm Hankel, "Ueber die Magnetisirung von Stahlnadeln durch den elektrischen Funken und den Nebenstrom desselben," *Ann.* 65 (1845): 537–68, especially on 549.

[7] Wilhelm Hankel, "Ueber die Thermoelektricität der Krystalle," *Ann.* 50 (1840): 237–50, 471–96, 605–15, on 615. Gustav Karsten, "Ueber elektrische Abbildungen," *Ann.* 60 (1843): 1–17, on 5.

[8] Richard van Rees, "Ueber die Vertheilung des Magnetismus in Stahlmagneten und Elektromagneten," *Ann.* 70 (1847): 1–24. Hankel, "Ueber die Magnetisirung von Stahlnadeln," 549.

[9] F. C. Henrici, "Untersuchungen über einige anomale und normale galvanische Erscheinungen," *Ann.* 58 (1843): 61–76, 375–91, on 390. K. W. Knochenhauer, "Versuche über gebundene Elektricität (Zweiter Artikel)," *Ann.* 58 (1843): 391–409, on 405–6.

any difference between light and radiant heat, he concluded that there can be no difference.[10] Moser, however, viewed light and radiant heat as distinct radiations.[11] Their conclusions differed, but both addressed the question of the identity of waves. One author offered readers of the *Annalen* an account of a stroboscopic disk designed to demonstrate waves visually, which he justified by the "great importance" of the "laws of wave theory for today's physics."[12] It was a simple statement of fact in the 1840s.

Between physics and chemistry on the one hand and the life sciences on the other, *Annalen* authors recognized a wealth of connections. The same laws govern the chemical combinations of both the nonliving and the living worlds. Exact curves of the mathematicians, which describe the motions of astronomical bodies and the motions produced in the physics laboratory, also describe the forms of plants and animals: leaves obey the Archimedean spiral, mollusks the logarithmic spiral. Hydrodynamics explains the flow of water in plants, mechanics the flow of blood and lymph in animals; part of the study of nourishment and secretion falls within the bounds of "inorganic physics." The physicist studies the physical components of sense perceptions,[13] and he uses his physiological understanding in physical experiments.[14] The ear and the eye, sense organs the physicist relies heavily on, are instruments capable of precision if used intelligently, of imprecision if used carelessly, requiring him to pay close attention to them. It is useful for him to know, for example, that the ear can distinguish a difference of one oscillation in 1200, since his studies of and with sound sometimes depend on the discrimination of the sense of hearing. Likewise, in his studies of and with light, it is useful for him to know that the eye is an unreliable judge of intensities of different colors.[15] Moser, noting that the eye completely misses a "group of light rays," the recently discovered invisible light, reasoned that the time had come to move beyond the subjective sensations of light to settle on an objective action of light that affects all bodies in the same way, allowing its intensity to be measured.[16] Papers in the

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[10] Knochenhauer, "Versuche," 405. Ernst Brücke, "Ueber das Verhalten der optischen Medien des Auges gegen Licht- und Wärmestrahlen," *Ann.* 65 (1845): 593–607, on 604–6.

[11] Ludwig Moser, "Ueber die Verschiedenheit der Licht- und Wärmestrahlen," *Ann.* 58 (1843): 105–11.

[12] Johann Müller, "Anwendung der stroboskopischen Scheibe zur Versinnlichung der Grundgesetze der Wellenlehre," *Ann.* 67 (1846): 271–72, on 271.

[13] For example, Gustav Theodor Fechner, "Ueber die subjectiven Nachbilder und Nebenbilder," *Ann.* 50 (1840): 193–221, 427–70; August Seebeck, "Bemerkungen über Resonanz und über Helligkeit der Farben im Spectrum," *Ann.* 62 (1844): 571–76.

[14] Heinrich Wilhelm Dove, "Ueber inducirte Ströme, welche bei galvanometrischer Gleichheit ungleich physiologisch wirken," *Ann.* 49 (1840): 72–98.

[15] August Seebeck, "Beiträge zur Physiologie des Gehör- und Gesichtssinnes," *Ann.* 68 (1846): 449–65, on 463–64. Ludwig Moser, "Ueber die Wirkungen der farbigen Strahlen auf das Jodsilber," *Ann.* 59 (1843): 391–407, on 393.

[16] Ludwig Moser, "Ueber das Latentwerden des Lichts," *Ann.* 57 (1842): 1–34, on 2–3.

Annalen in the 1840s by Ohm and Seebeck show that physiological acoustics belongs to the more mathematical parts of physics, and the same is true of physiological optics.[17]

6.4 Experimental Research

If French mathematical theories in the early years of the nineteenth century showed Ohm and other young German physicists that mathematical methods are useful for advancing physical understanding, Faraday's discoveries abundantly confirmed the usefulness of laboratory methods. Almost every year they read in the *Annalen* another of Faraday's long papers on electrical research, the German translation appearing almost immediately after its original publication in English. By the period we are considering, Faraday's early work on electromagnetic induction, which had stimulated Gauss and Weber along with many other researchers in Germany, was cited so frequently in German publications that Faraday's name no longer needed to be mentioned along with the phenomenon.[18] In 1840–1845, Faraday was the third most copiously translated foreign physicist in the *Annalen*. Over the whole of his career, his work filled more than twice the space given to any other foreign physicist in the *Annalen*, the equivalent of nearly three volumes.

From Faraday's publications in the *Annalen*, German readers would have learned about one of the most important concepts of physics, that of the field. Faraday's understanding of the field developed in the 1830s and 1840s together with his idea of physical lines of force, which he used to guide his experimental researches. He envisioned lines of force as existing in empty space independently of their sources. Rejecting Newtonian action at a distance forces, he believed in the reality of physical lines of force, the foundation of field theory. According to his understanding of forces, electric charges and currents and magnetic bodies generate fields, which exert forces on distant charges, currents, and magnets. His ideas were developed further by Thomson and especially by Maxwell, who created the

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[17] August Seebeck, "Beobachtungen über einige Bedingungen der Entstehung von Tönen," *Ann.* 53 (1841): 417–36; "Beobachtungen über Zurückwerfung und Beugung des Schalles," *Ann.* 59 (1843): 177–203. Georg Simon Ohm, "Ueber die Definition des Tones, nebst daran geknüpfter Theorie der Sirene und ähnlicher tonbildender Vorrichtungen," *Ann.* 59 (1843): 513–65. August Seebeck, "Ueber die Sirene," *Ann.* 60 (1843): 449–81. Georg Simon Ohm, "Noch ein paar Worte über die Definition des Tones," *Ann.* 62 (1844): 1–18. August Seebeck, "Ueber die Definition des Tones," *Ann.* 63 (1844): 353–68; "Ueber die Erzeugung von Tönen durch getrennte Eindrücke, mit Beziehung auf die Definition des Tones," *Ann.* 63 (1844): 368–80.

[18] In 1840–1845, the *Annalen* gave over 361 pages to Regnault, 229 to Knochenhauer, 214 to Faraday, 153 to Lenz, 145 to Plateau, 142 to de La Rive, 136 to Melloni, 113 to M. H. Jacobi, and 103 to Wheatstone. To look ahead, by 1874 Faraday's papers filled 1617 pages of the *Annalen*, Regnault's 696 pages, and Arago's, Becquerel's, Biot's, David Brewster's, Fresnel's, Melloni's, de La Rive's, G. G. Stokes's, and John Tyndall's each nearly as many as Regnault's. W. Bn. [Wilhelm Barentin], "Ein Rückblick," *Ann.*, Jubelband (1874): ix-xiv, on xiii.

electromagnetic field theory that would captivate German physicists in the late nineteenth century.[19]

Faraday was, for now, overshadowed by Regnault and the Austrian Friedrich Knochenhauer, who also offered German physicists impressive experimental work, as did the other half dozen foreign physicists to whom Poggendorff gave most space in 1840–1845: the Swiss Auguste de La Rive, the Italian Macedonio Melloni, the Belgian J. A. F. Plateau, the Russian H. F. E. Lenz, the German M. H. Jacobi who was working in Russia, and the Englishman Charles Wheatstone.

German physicists working in electricity and magnetism responded to Faraday's work, repeating it, contending its validity, carrying it forward, and using it for other purposes. Over a 3-year period in the early 1840s, papers dealing explicitly with his work in some form or other constituted over a third of all the many German papers on electricity and magnetism published in the *Annalen*. Even though at the time, Faraday was between what might be considered high points in his research—his discovery of the magnetic rotation of the plane of polarization of light came only at the end of the period treated in this chapter, in 1845—he was important to German "electricians" at the time, especially for his views on the galvanic circuit, which entered the controversy over the origin of galvanic electricity. The chemical theory, which Faraday supported, was opposed by the contact theory, which leading German experimental physicists such as Ohm, Fechner, Poggendorff, and Pfaff supported, though with a degree of caution. The "majority of contemporary physicists," by Poggendorff's estimate, placed "great importance" on a decision between the two theories, "whether with justification or not," and naturally German physicists had to examine and re-examine Faraday's account of the galvanic process.[20]

According to advocates of the chemical theory, electricity is developed in the battery by the chemical action of the liquid on the metals. By the contact theory, first advanced by Alessandro Volta, the source of galvanic current lies solely in the contact of the two metals, the liquid serving only as a conductor indirectly making contact between them. For followers of Volta, and Volta himself, in Poggendorff's view, a force arises on contact of the metals that is analogous to capillary force, and not reducible to chemical affinities.[21] There was considerable interest in reconciling the two theories. Buff's way was to view the attraction of the two metals in contact as chemical, a proposal which F. C. Henrici, a private researcher living in Göttingen, regarded as no solution; even if one thought of chemical attraction as general molecular attraction, as he did, rather than as chemical affinity, one was still not justified in using the term "chemical" for galvanic attraction, for it resulted only

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[19] Robert D. Purrington, *Physics in the Nineteenth Century* (New Brunswick, New Jersey, and London: Rutgers University Press, 1997), 53–55.

[20] Johann Christian Poggendorff, "Ueber die galvanischen Ketten aus zwei Flüssigkeiten und zwei einander nicht berührenden Metallen," *Ann.* 49 (1840): 31–72, on 36.

[21] J. C. Poggendorff, "Zusatz vom Herausgeber," addition to W. R. Grove, "Ueber eine Volta'sche Gas-Batterie," *Ann.* 58 (1843): 202–6; Poggendorff's addition 207–10, on 209.

in mechanical effects, not chemical ones.[22] Pohl's reconciliation was more elaborate, involving the recognition that although "mathematical formulas are a priceless vehicle of physics," the mathematical form into which Volta's theory had been put could not yield a solution to the galvanic controversy, which had to be settled by "concepts of another kind." His solution was to recognize that electricity is just one of the expressions of "chemism," the source of which is polarity, the fundamental law of nature.[23]

Johann Müller, in a report on recent advances of physics, observed that the passion for controversy dies away once a theory is well grounded. This could not yet be said for the theory of galvanism, and he offered a historical parallel: just as the wave theory of light was not completed until the discovery of polarization revealed the true nature of light waves, electrical theory would not be completed until new facts revealed the true nature of electricity. Once the facts were known, the theory would then develop "easily and without being forced," and controversy would cease.[24]

Not all German physicists agreed with Müller on the need to know the true nature of electricity to bring the controversy to an end. Poggendorff, for one, redefined the physical problem in such a way that it did not matter for the time being which hypothesis about the origin of galvanism was right, as long as one could get a measure of the forces at work. "However one may imagine the origin of galvanic current," he wrote in 1841, one could not help but think "that the electromotive force or that which is measured by it must be directly proportional to the ultimate cause of these currents, and on this assumption, which is certainly most natural, exact determinations of the electromotive force ... seem to be best suited to throw a brighter light on the origins and variation of hydroelectric currents, which in some respects are still so puzzling."[25] He said that the "true theory of voltaism " no longer means the theory of the origin of electric currents but their laws,[26] and that the advantage lies with a theory that is developed with "measure and number, the true foundation of exact scientific research." Weber, who during his years of exile from Göttingen sometimes worked with Poggendorff in Berlin, sidestepped the controversy, defining the measure of Faraday's electrolytic law in

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[22] Buff's view, as described by Henrici, "Untersuchungen über einige anomale und normale galvanische Erscheinungen," 386.

[23] G. F. Pohl, "Ueber galvanische Ketten mit zwei verschiedenen Flüssigkeiten, und über einiges aus den neuesten, diesen Gegenstand betreffenden Untersuchungen," *Ann.* 54 (1841): 515–37, on 517, 519–20.

[24] Johann Müller, *Bericht über die neuesten Fortschritte der Physik. In ihrem Zusammenhange dargestellt,* 2 vols. (Braunschweig, 1849), vol. 1, 225–26.

[25] J. C. Poggendorff, "Methode zur quantitativen Bestimmung der elektromotorischen Kraft inconstanter galvanischer Ketten," *Ann.* 54 (1841): 161–91, on 161–62.

[26] J. C. Poggendorff, "Ueber die Volta'schen Ketten mit zwei einander berührenden Flüssigkeiten," *Ann.* 53 (1841): 436–46, on 443; "Ueber einen Versuch des Hrn. Daniell und die daraus gezogene Folgerung," *Ann.* 56 (1842): 150–56, on 152.

terms of the absolute units of electricity rather than in chemical terms, as Faraday had done.[27]

One of the tasks of experimental physics was to explore little-known areas of phenomena for which mathematical physics had provided few maps, perhaps not even a first survey. Looking at some work on magnetism, Ohm saw "lasting value" in experimental researches that give only "purely empirical" laws, because the results throw "light on many dark points" and provide support where the empirical foundations are insecure. The "scholarly dress" of mathematical theory, as Ohm called it, is not always an advantage. Where physics for the time being was best served by the discovery of new phenomena and the empirical determination of their laws, a physicist might well "fear," as Ohm did, "the sin of robbing this [experimental] work of its original simplicity." Magnus explored the differences in tension of steam at different temperatures using an empirical interpolation formula rather than the existing mathematical formulas, because the latter were based on hypotheses he did not consider sufficiently established: "If one knew the law of the dependence of the tension on temperature and on all other quantities that enter, one could give a theoretical formula that would certainly be preferable to all the others. But unfortunately this law is not yet known.".[28]

Typically at the beginning of an experimental paper, the researcher introduced some minimal mathematics by way of formulas expressing laws, which he usually took from other publications. Then he extracted from the formulas constants to measure with his apparatus, and if he also wanted to explain his measurements he included some mathematical work at the end. The work of the experimentalist that required the most measuring skill and the most precise instruments was the testing of mathematical laws and their consequences. Gauss and Weber's work on Earth magnetism had shown the intimate connection between mathematical theory and measurement. The connection was extended to researches on specific heats, refractive indices, coefficients of expansion, and the "*most scientific* of all recent discoveries in the area of galvanism," Ohm's law relating current intensity, electromotive force, and resistance in a conductor.[29] The confidence of German physicists in the accuracy of Ohm's law was due to countless careful measurements carried out for all situations in which the law applies. "Only measure and number can decide here," Poggendorff said about what Ohm did, and "this theory" taught German physicists

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[27] Wilhelm Weber, "Ueber das elektro-chemische Aequivalent des Wassers," *Ann.* 55 (1842): 181–89; Poggendorff's commentary on Grove, "Ueber eine Volta'sche Gas-Batterie," 209–10.

[28] Ohm's comment appears in a footnote to Paul Wolfgang Haecker, "Versuche über das Tragvermögen hufeisenförmiger Magnete und über die Schwingungsdauer geradliniger Magnetstäbe," *Ann.* 57 (1842): 321–45, on 321–22. Gustav Magnus, "Versuche über die Spannkräfte des Wasserdampfs," *Ann.* 61 (1844): 225–47, on 244–45.

[29] Wilhelm Weber, "Ueber die Elasticität fester Körper," *Ann.* 54 (1841): 1–18, for example; or Magnus, "Ueber die Ausdehnung der Gase durch die Wärme." Poggendorff referred to Ohm's law in this way in a footnote to a paper by C. F. Schönbein, "Notizen über eine Volta'sche Säule von ungewöhnlicher Kraft," *Ann.* 49 (1840): 511–14, on 514.

"to apply measure and number to the phenomena."[30] Sometimes the growing body of physical measurements forced physicists to re-examine well-established laws by going over the experimental ground once again. A discrepancy between Gay-Lussac's law and Fredrik Rudberg's work on air led Poggendorff to undertake the tedious repetition of the earlier work, since it was important to know if "one of the most general laws of physics is right or not.".[31]

Poggendorff's reception of Kirchhoff's laws of branched circuits is an example of the interest that laws deduced from mathematical theories could hold for an experimenter.[32] Laws for the branching of linear electric currents from two points were known, and Poggendorff gave a generalized "theory" of them in 1841.[33] For more intricate circuits, as in the case of four branch points, it was not clear that known principles applied. In 1844 Weber drew Poggendorff's attention to the complexity of the problem, and he gave him his formula for the total resistance of a branched circuit. The problem had a "purely theoretical" interest at the time, but by the next year it had acquired a practical interest through Wheatstone's differential galvanometer, which contained a complex branch circuit. After Poggendorff had tested the new galvanometer and found it in some ways preferable to Becquerel's galvanometer with its simple circuit, he decided that a "detailed theory" of a complex branch circuit was desirable. He asked Weber for his full calculation, which he received. At about the same time, in 1845, Kirchhoff published his account of the flow of electricity in a plane and in a system of wires, discussed in Chap. 3. Poggendorff followed up Kirchhoff's paper, which he called a "real advance in theoretical galvanometry," with one of his own the following year. He thought that Kirchhoff's method for solving the problem of linear branching was the most general and simplest way, but with permission he published Weber's less general solution for a circuit of four branch points alongside Kirchhoff's method. Even though Poggendorff and Weber regarded the principles

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[30] The quotation is from Poggendorff's footnote to an article by Auguste de La Rive, "Neue Untersuchungen über die Eigenschaften der discontinuirlichen elektrischen Ströme von abwechselnd entgegengesetzter Richtung," *Ann.* 54 (1841): 231–54, on 236. Poggendorff, "Oeffentliche Anerkennung der Ohm'schen Theorie in England," 180. Once established, Ohm's law proved so useful to experimentalists that Buff, who gladly admitted that "Ohm's theory is a highly important aid for our researches in electrical theory," complained that it was being used too often, at every "ever so insignificant opportunity." Henirich Buff, "Bemerkungen zu einem Aufsatze von Henrici, 'zur Galvanometrie' überschrieben," *Ann.* 54 (1841): 408–12, on 411–12. Poggendorff answered Buff in a footnote (412), saying that one could hardly blame the "galvanician" for frequently mentioning Ohm's law any more than one could reprove the "optician" or the "chemist" because he spoke so often of the interference formula or the atomic theory.

[31] Magnus, "Ueber die Ausdehnung der Gase durch die Wärme," 4, 24.

[32] J. C Poggendorff, "Ueber die Einrichtung und den Gebrauch einiger Werkzeuge zum Messen der Stärke elektrischer Ströme und der dieselbe bedingungen Elemente," *Ann.* 50 (1840): 504–9, on 504.

[33] Poggendorff, "Methode zur quantitativen Bestimmung der elektromotorischen Kraft inconstanter galvanischer Ketten."

on which the solutions were based as sound, they both thought to compare "theory" and "experience," finding the expected confirmation.[34]

In their reports of experiments, physicists sometimes spoke of the "theory" of the instrument and of the observations,[35] which enabled them to know the attainable precision of their work. The best examples of the design and discussion of precision instruments from around this time are in publications by Gauss and Weber, which reappeared in excerpts in the *Annalen*.[36] Where the theory of the instrument was not yet established, as in the quantitative relation of the intensity of current to the degree of deflection of the galvanometer needle, the experimenter might use and discuss the instrument at hand together with the best experimental method he could devise for it. The instrument might be a simple, cheap galvanometer; for as Poggendorff noted, there were still many investigations in which "precision of a half to a whole degree in the deviations of the magnetic needle suffices completely for all purposes."[37] Papers in the *Annalen* show that the study of instruments and of their methods of use was no less a part of the work of experimentalist than the study of the laws that were established and tested using the instruments.

When in late October 1845, Faraday announced the laws governing the rotation of the plane of polarization of light passing through a transparent body between the poles of an electromagnet, German experimentalists were immediately interested. In the *Annalen*, the first accounts of this latest great connection in nature preceded Faraday's own there. Rudolph Boettger, a teacher of physics and chemistry at the Physical Society in Frankfurt am Main, read about this experiment of the "highest importance" in popular journals, in the *Athenäum* on 8 November, only a week after Faraday's announcement, and in the *Rheinische Beobachter* on 7 December. Certain that "all physicists of Germany and Europe were filled with deep joy" by it, he realized that if they wanted to see it for themselves soon, they had to reproduce Faraday's result from "entirely unclear and confused," incomplete reports in the

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[34] Gustav Kirchhoff, "Ueber den Durchgang eines elektrischen Stromes durch eine Ebene, insbesondere durch eine kreisförmige"; J. C. Poggendorff, "Ueber ein Problem bei linearer Verzweigung elektrischer Ströme," *Ann.* 67 (1846): 273–83. Kirchhoff's work was "of interest especially for me," Poggendorff wrote to a colleague, because it was related to a problem he and Weber had already discussed: "namely, the determination of the intensities and resistances in a wire rhombus with a wire bridge." Poggendorff encountered this "not inconsiderable" problem in a Wheatstone differential galvanometer. Weber had already developed the necessary formulas, and Poggendorff had at first meant to publish them with Kirchhoff's paper, but he did not because he thought it would be better if he, Poggendorff, could first confirm them with measurements. Poggendorff to Neumann, 20 June 1845, Neumann Papers, Göttingen UB, Ms. Dept.

[35] Adolph Erman, "Bestimmung der magnetischen Inclination und Intensität für Berlin im Jahre 1846," *Ann.* 68 (1846): 519–52, on 519. J. C. Poggendorff, "Ueber Hrn. De la Rive's Hypothese vom Rückstrom in der Volta'schen Säule," *Ann.* 56 (1842): 353–69, on 356.

[36] To take an excellent example: Wilhelm Weber, "Elektrodynamische Maassbestimmungen," *Ann.* 73 (1848): 193–240.

[37] J. C. Poggendorff, "Von dem Gebrauch der Galvanometer als Messwerkzeuge," *Ann.* 56 (1842): 324–44, on 328.

"political newspapers."[38] He set to work, and by 20 December, he was ready to show Faraday's experiment to the members of the Physical Society, and the following day he described it in a paper for the *Annalen*. Next Poggendorff commented on Faraday's discovery in the *Annalen*, since by then two reports on it had appeared in foreign scientific journals, and physicists did not have to rely on journalists any longer.[39] The translation of Faraday's full paper on the magnetic rotation of light appeared in the next volume of the *Annalen*.[40] It had only seemed a long time in coming to impatient German electrical researchers, who promptly performed experiments to confirm the discovery and then offered explanations of it that differed from Faraday's own.[41]

6.5 Theoretical Research

Much of German experimental research in the *Annalen* in the 1840s explicitly invoked theory in one way or another. Purely theoretical or mathematical papers were infrequent, but in several cases they were of such quality and importance that we discuss them individually in the next chapter after our brief summary below. By dividing German physics research into experimental and mathematical parts in our discussion, we do not mean to suggest that publications in the *Annalen* were neatly separated or that physicists at the time identified themselves with one or the other direction. University positions for physicists were rarely divided this way, and physicists only occasionally referred to one another as an "experimental physicist" or a "mathematical physicist." The designation "theoretical physicist" was not unknown, but it was still uncommon, and unnecessary.[42]

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[38] Rudolph Boettger, "Ueber Faraday's neueste Entdeckung, die Polarisationsebene eines Lichtstrahls durch einen kräftigen Elektromagneten abzulenken," *Ann.* 67 (1846): 290–93. The paper is dated 21 December 1845. Also, "Ueber die durch einen kräftigen Elektromagnet bewirkte, im polarisirten Lichte sich kundgebende Molecularveränderung flüssiger und fester Körper," *Ann.* 67 (1846): 350–53.

[39] J. C. Poggendorff, "Faraday's neue Entdeckung und deren Zusammenhang mit Seebeck's Transversalmagnetismus," *Ann.* 67 (1846): 439–40.

[40] Michael Faraday, "Neunzehnte Reihe von Experimental-Untersuchungen über Elektricität," *Ann.* 68 (1846): 105–36.

[41] German physicists commonly thought that magnetism acted on the molecules of the transparent body and not directly on light, as Faraday thought. Plücker, for example, thought that Faraday's discovery revealed a connection between the crystal forces of the transparent body and magnetic forces, which might enable physicists to determine crystal forms by magnets (Julius Plücker, "Ueber die Abstossung der optischen Axen der Krystalle durch die Pole der Magnete," *Ann.* 72 [1847]: 315–43, on 341–42).

[42] For instance, the expression "theoretical physicist" was used in reference to Laplace and Poisson by Sohn C. Brunner in "Untersuchung über die Cohäsion der Flüssigkeiten," *Ann.* 70 (1847): 481–529, on 482.

German physicists working in the 1840s did not have to contend seriously with attitudes of rejection of mathematical theory. Nevertheless, it was still possible for them to come across skeptical opinions in the *Annalen*. In 1845, the elderly Parrot, after working in physics for fifty-eight years and still sending contributions to the journal from St. Petersburg, was critical of the reality of certain common mathematical assumptions, which he viewed as the doctoring of physical assumptions for the convenience of mathematical treatment. In particular, he criticized Feilitzsch for building a hydrodynamic theory on the same error that Newton and every theorist after him had made, which was to assume that liquids are inelastic, or rather that their elasticity is so insignificant that it can be ignored. If mathematical theories with that false assumption agreed with experiment, Parrot reasoned, it only showed how "easy it is to misuse noble calculation," and he presented a verbal argument for endowing liquids with elasticity.[43] Verbal theoretical arguments were not uncommon in the *Annalen*. Like Parrot, Henrici, for example, criticized the mathematically simplifying assumption of the inelasticity of fluids, this time of the imponderable electric fluids, allowing at best that the assumption was a useful fiction for orienting physicists to electrical phenomena and enabling them to introduce mathematics.[44] He discussed his theoretical ideas in the *Annalen*, rarely mathematically. When on occasion he did use mathematics, he joined physicists such as Ohm in opposing Pohl's "polarities," as he contemptuously referred to Pohl's nature-philosophical leaning.[45]

The most significant theoretical work by German physicists in the 1840s made use of mathematical analysis, much like the French physicists, whose work German physicists cited frequently, using it where they agreed with it and criticizing it where they did not. Within a criticism of Laplace's capillarity theory, Gotthilf Hagen observed that Laplace's investigations provided a "model" for the way a physical phenomenon is to be subjected to rigorous calculation.[46] If the exactness demanded of experimenters by the mathematical capillarity theories of Laplace and Poisson may have discouraged German physicists at one time, it encouraged them

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[43] Georg Friedrich Parrot, "Zur Geschichte der Endosmose," *Ann.* 66 (1845): 595–97; "Ueber den Ausfluss der tropfbaren Flüssigkeiten durch kleine Oeffnungen im Boden der Gefässe," *Ann.* 66 (1845): 389–414, on 389–90. Parrot had been reproved for the "bitter, almost mocking tone" in which he had criticized "the application of mathematical analysis in physics." He replied that he had written only against the "misuse" of mathematical analysis, "which occurs when this analysis is based on physical data that are either only hypothetical or even in contradiction with precisely executed experiments." He scornfully called this misuse of mathematical analysis "mathematical *Naturphilosophie*" (Parrot, "Nachtrag," *Ann.* 27 [1833]: 234–38, on 234–35).

[44] F. C. Henrici, "Einige die Theorie und Anwendung der Elektricität betreffende Bemerkungen," *Ann.* 64 (1845): 345–56, on 346–47.

[45] F. C. Henrici, "Zur Galvanometrie," *Ann.* 53 (1841): 277–94. Henrici showed that Pouillet's laws of the galvanic circuit follow from Ohm's theory, and he furnished a mathematical explanation of phenomena that Pohl had cited as contradicting the theory of currents. He accused Pohl of "deliberately ignoring a theory that has long been confirmed in all its assertions by the most careful experiments, which were undertaken to test it" (286).

[46] Gotthilf Hagen, "Ueber die Oberfläche der Flüssigkeiten," *Ann.* 67 (1846): 1–31, 152–72, on 9.

in the 1840s. French mathematical physics was a point of reference for their own work. Clausius, for example, in the 1840s made direct use of Poisson's and Cauchy's work in elasticity theory, and of Carnot's and Clapeyron's in the mechanical theory of heat.

While French theories were embraced and criticized by German physicists, a body of German examples appeared alongside them in the 1840s, drawing on earlier German work. Helmholtz, Weber, Clausius, Kirchhoff, and Neumann all developed theories with the aid of the mathematical concept of potential, which owed to Gauss as well as to foreign sources. Weber's electrodynamic theory of the 1840s rested on the measuring theory and instruments that he and Gauss had introduced in the 1830s. Kirchhoff's early papers extended Ohm's law and revised his derivation to agree with Weber's electrodynamics. Work by Ohm, Gauss, Weber, and Neumann all entered Helmholtz's grand survey of contemporary physics from the perspective of the principle of conservation of energy.

Chapter 7
Connecting Laws: Careers and Theories in the 1840s

In the early nineteenth century, physicists showed a keen interest in establishing connections between diverse physical subjects. Oersted, Faraday, and other experimenters led the way, while mathematical physicists formulated laws incorporating their discoveries. The suggestion has been made that nature philosophy with its idea of the unity of nature influenced the work of some German physicists, who include physicists we discuss in this chapter. As a reason for undertaking researches that connected the branches of physics, nature philosophy is unnecessary, though this does not mean it could not have exerted some influence.

As we saw in Chaps. 2 and 3, major work in mathematical theory in combination with experiment was carried out in Germany in the 1820s and 1830s. This was continued and expanded in the 1840s, especially by Weber, Helmholtz, Kirchhoff, Neumann, and Clausius, who became leading theoretical physicists in Germany in the second half of the nineteenth century. Their work remained heavily indebted to French mathematical physics, but now they also had significant German models to draw on. As an example of how they combined sources, Helmholtz and Wiedemann were incited to learn about mathematical physics by reading Gauss's magnetic researches, and because Berlin offered no lectures on the subject in the 1840s, the two young Germans studied Poisson's works together on their own.[1]

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[1] Leo Königsberger, *Hermann von Helmholtz*, trans. F. A. Welby (Oxford, 1906), 283.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_7

7.1 Electrical Research at Göttingen and Leipzig: Weber

In the course of his and Weber's collaboration on magnetic and electromagnetic research, Gauss privately began work on a fundamental law that connected varied electrical phenomena.[2] Around 1833, starting from the known connection between electric current and magnetism, Gauss developed an equation that combined the magnetic potential and the current intensity, but he did not publish it. Two years later, he developed a fundamental law for the total electrostatic and electrodynamic action between two electric quantities, but again he left it in the form of notes. He looked for a "*derivation* of the additional forces (that are added to the mutual actions of electric particles at rest when they are in relative motion) from *action that is not instantaneous* but (in a similar way as with light) propagated in time," but he could not complete the theory until he had an idea of the manner by which the propagation takes place.[3] It was a promising insight, but it was to have little if any influence on the development of electrical theory in Germany. Even his collaborator Weber did not learn of it until a decade later. Around the same time, Gauss told the astronomer J. F. Encke of his interest in working "on the fundamental laws of galvanic currents and of induction." As it turned out, his effective approach to connecting the parts of electrical physics came from the side of measurement; in 1835 he began a work he called "Reduction of the Interaction between Galvanic Currents and Magnetism to Absolute Measure," and his notes contain examples of absolute measurements of currents and of "internal" and "external" resistances of the galvanic elements.[4] Beginning his work on electrodynamics at about the same time as Gauss, Weber approached the subject from the side of measuring physics, the particular strength he brought to their collaboration. By the summer of 1837, he had the first precision instrument for carrying out electrodynamic experiments, which allowed him to overcome a crucial technical difficulty that had impeded electrodynamics since Ampère, as he explained in his first major paper on the subject nearly a decade later, in 1846.[5]

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[2] Gauss's law is concerned with electrical quantities rather than with Ampère's current elements, and it modifies the law of electrostatic action by terms depending on the relative motion of the electrical quantities. Weber, the mathematician Bernhard Riemann, and probably others knew that Gauss had developed such a law, but it was only published after Gauss's death, in his collected works in 1867. His law was the forerunner of a number of fundamental laws of electrical action proposed by German researchers in the nineteenth century (Carl Friedrich Gauss, "Zur mathematischen Theorie der electrodynamischen Wirkungen," in *Werke*, ed. Königliche Gesellschaft der Wissenschaften zu Göttingen, vol. 5 [N.p., 1877], 601–26).

[3] Gauss to Weber, 19 March 1845, in Gauss, *Werke*, vol. 5, 627–29, on 629.

[4] Clemens Schaefer, "Über Gauss' physikalische Arbeiten (Magnetismus, Elektrodynamik, Optik)," Gauss, *Werke,* vol. 11, pt. 2, 2nd treatise (Berlin and Göttingen: Springer 1929), 115–17.

[5] Wilhelm Weber, "Elektrodynamische Maassbestimmungen. Ueber ein allgemeines Grundgesetz der elektrischen Wirkung," *Abh. Sächs. Ges. Wiss.* (1846), 211–378, in *Werke*, vol. 3, *Galvanismus und Elektrodynamik, erster Theil*, ed. Heinrich Weber (Berlin, 1893), 25–214, on 35.

The main reason for the long delay was Weber's dismissal from Göttingen University in December 1837 and his subsequent unemployment until 1843. He along with six other Göttingen professors refused to take the loyalty oath to the person of the new king of Hannover, giving the reason that he had already sworn an oath of office on the state's constitution and would not dishonor it.[6] His dismissal, which was not accompanied by exile as was some of his colleagues', brought an immediate end to his teaching but not to his use of the physics institute, where he continued to do research for several months.[7] He hoped that in the future as a private person he would still be able to work in Göttingen, as he explained to Gauss in 1838: "I am not so attached to either the physical cabinet, as it is, or the kind of lectures on experimental physics that can be given to an audience consisting mostly of medical students, that I should not greatly prefer the freer scientific position that I have in Göttingen as a member of the Society [of Sciences], by your side." To realize his ideal of a life dedicated to research, he needed only the means to go on working there.[8]

Weber had some hope that an eventual restoration of the old political order in Hannover would permit him to return to Göttingen University "as a teacher of physics . . . even if not as teacher of experimental physics," with improved working conditions. He envisioned the future arrangement for physics like those for chemistry, a "far more limited" subject, which nevertheless already had two positions at several universities: one position for the "so-called theoretical chemistry" consisting of survey courses for large audiences of beginners, and the other for the "so-called analytical (measuring) chemistry" consisting of courses for students who had a serious interest in the science. Weber did not know if Göttingen could get two professorships for physics, but he thought that it would not remain a "higher institution of learning" for physics if it did not.[9]

Despite his hopes for greater freedom, Weber was handicapped by the loss of his institute. By March 1838, 3 months after his dismissal, he had finished the work on hand, and he could not start anything new because he was no longer "*master*" of the physical cabinet. Although he still had possession of it, he had "neither the right nor

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[6] Heinrich Weber, *Wilhelm Weber. Eine Lebensskizze* (Breslau, 1893), 44–47. This includes the text of the refusal submitted by the seven Göttingen professors to the Göttingen U. Curator on 18 November 1837. The explanation is the following. Since the succession of the Hannoverian dynasty to the British throne, Hannover had been united with Britain. In 1837, the union was dissolved by Salic law, which excludes females from the line of succession. Victoria succeeded to the throne of England that year, and when her uncle Ernest-Augustus became king of Hannover, he revoked the free constitution of Hannover, causing the revolt at Göttingen.

[7] Weber to Gauss, 16 March 1838, Gauss Papers, Göttingen UB, Ms. Dept.

[8] Weber to Gauss, 31 May 1838, Gauss Papers, Göttingen UB, Ms. Dept.

[9] Ibid.

the inclination and courage" to work with it.[10] To escape Göttingen for a while, for the "purpose of not letting myself be taken away from scientific activity by the present external circumstances," he made "scientific" travels through Germany and to England and France as a promoter of Earth-magnetic observations.[11] In between months of travel, he stayed with his brothers in Leipzig, with Poggendorff in Berlin, and with Gauss in Göttingen, carrying out smaller researches on his own or with friends and editing the *Resultate*. In Gauss's letters to him, Weber saw a waning interest in physics and renewed preoccupation with mathematics, which further put into question any future collaboration.[12]

Gauss and the faculty as a whole argued repeatedly with the Hannover government that Weber must remain in Göttingen, if not as a teacher then as a researcher. The faculty made the case that the Göttingen physics institute was no longer a "mere collection" of instruments but was now set up "entirely for the present state of mathematical and scientific physics," having highly perfected instruments and apparatus that only an experienced physicist could keep from being damaged. They were unsuccessful, and a new physicist was brought in to take Weber's place. He was J. B. Listing, who had published nothing but his dissertation and whose teaching experience was limited to one year at a trade school, but Gauss regarded him as a "young man of very exceptional talent," who would do excellent work in the future.[13] Listing's appointment, it turned out, prepared the way for the dual representation of physics at Göttingen that Weber had hoped for. Had he proved an equal of the young Weber, Listing might have been promoted to Weber's chair, but he soon drew the government's displeasure, having attracted too few students—Weber later thought that that was not entirely Listing's fault, since there were not many students in any case—and not producing "important literary work."[14] The task of running the institute, of gradually enlarging it, and of equipping the new laboratory facilities, together with his teaching absorbed most of Listing's energy, even exceeding it at times.[15] In his research, he did not profit as Weber had from the link between the astronomical observatory and the physical cabinet, his interest

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[10] Weber to Gauss, 16 March 1838. Gauss Papers, Göttingen UB, Ms. Dept.

[11] Ibid. Weber's letters to Gauss document his scientific activities at Berlin (18 April 1838), at Hannover (31 May 1838), at Munich (18 August and 6 September 1839), in England (18 June, 5 July, and 12 July 1838), and in Paris (13 August 1838). Gauss papers, Göttingen UB, Ms. Dept.

[12] Gauss to Weber, 21 May 1843, Gauss Papers, Göttingen UB, Ms. Dept.

[13] Gauss to "Cabinetsrath," 31 July 1838, Gauss Papers, Göttingen UB, Ms. Dept.

[14] Draft of a letter by Hoppensted to Listing, which was not sent because it was too critical of Listing and might have upset Gauss; correspondence between Hoppenstedt and Stralenheim about the draft; and the letter that was actually sent to Listing, dated 22 November 1842; Listing Personalakte, Göttingen UA, 4/V b/108.

[15] Listing to Curator, 3 March 1846, Göttingen UA, 4/V h/8. Listing to Hannover government, 21 January 1848, Göttingen UA, 4/V h/10. Listing to Curator, 2 January 1853, Listing Personalakte, Göttingen UA, 4/V b/108. These are a few of the more important reports.

lying outside the areas of physics that Gauss and Weber had cultivated. As a consequence, Listing remained an extraordinary professor throughout the decade of Weber's absence from Göttingen University; upon Weber's return, Listing's position was to become a permanent second physics position beside the then vacant first ordinary professorship.

The earliest cases of two ordinary professorships of physics at a German university usually had one thing in common, a particularly desirable physicist available at a critical moment. When Göttingen University acquired two professorships of physics in 1849, it was, as we will see, because Weber was known to want to return to his former position. Before Weber's return, and again because of his availability, Leipzig University nearly acquired two professorships of physics. When Fechner, the physics professor at Leipzig, fell ill, in 1842 the Saxon ministry appointed Weber to the Leipzig chair. The understanding was that Weber would return the directorship of the physics institute and his place in the philosophical faculty if and when Fechner could return to work. In that case Weber would get the directorship of a new laboratory to go with the directorship of the magnetic observatory, which was to be built for him immediately.[16]

A few months after Weber's arrival in Leipzig, Fechner suddenly took a turn for the better.[17] He did not return to teaching right away, and he never reclaimed the physics professorship—his interests turned elsewhere, to philosophy, aesthetics, and psychology—and it was for that reason that two professorships for physics at Leipzig, an arrangement for which Weber had laid the groundwork, did not come into effect then. In the event, the establishment of the second professorship was postponed for nearly a half-century.

In his last paper on physics, which appeared in the *Annalen der Physik* in 1845, Fechner took up the problem of connecting the laws of electromagnetic induction with those of electrodynamics.[18] His starting point was a law stated by H. F. E. Lenz in 1834, which asserts that the direction of an induced current is such as to oppose the motion of the conductor causing the induction.[19] To explain the origins of this law, which was regarded as only an empirical rule, Fechner proposed a physical connection between Ampère's and Faraday's phenomena. From "fundamental laws" that refer the interaction of current elements to the combined action of positive and negative "particles of electricity" simultaneously moving in wires in

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[16] Saxon Ministry of Culture and Education to Leipzig Philosophical Faculty, 7 May 1842, Fechner Personalakte, Leipzig UA, PA 451.

[17] Weber to Gauss, 25 October 1843, Gauss Papers, Göttingen UB, Ms. Dept.

[18] Gustav Theodor Fechner, "Ueber die Verknüpfen der Faraday'schen Inductions-Erscheinungen mit den Ampère'schen elektro-dynamischen Erscheinungn," *Ann.* 64 (1845): 337–45.

[19] The law reads: If a wire is moved in the neighborhood of a galvanic current or a magnet, a galvanic current is induced in it, the direction of which is such that it would have caused a motion in the wire exactly opposite the one given it, provided it was movable only along the line of the motion (H. F. E. Lenz, "Ueber die Bestimmung der Richtung der durch elektrodynamische Vertheilung erregten galvanischen Ströme," *Ann.* 31 [1834]: 483–94, on 485).

opposite directions, he analyzed simple cases of interaction between wires.[20] He assumed that electrical particles of the same sign attract one another when they move in the same direction and that electrical particles of the opposite sign attract one another when they move in opposite directions and, further, that the attraction is along the line connecting the particles. He then showed that if a neutral wire is moved towards a parallel wire carrying a current, a current is induced in the moving wire and, as a result, a ponderomotive force arises between the two wires. He predicted that a charged rod turning on its axis would behave like a magnet and that, conversely, an approaching magnet would cause a charged rod to turn, but he knew that these and similar consequences would be hard to test, since Gauss and Weber had shown that enormous velocities of electrical machines and powerful electrifications were required to produce measurable induced currents. In any event, Fechner did not have the necessary apparatus to test the theory himself, nor did he have the necessary mathematical skill to develop the theory further. In his analysis of the motions of electricity in wires, he could only give the directions of the forces causing them, not their strength, an accomplishment which fell short of what he and most of his colleagues understood by an adequate physical theory. In the 1840s, it was regarded as a limitation for a theoretical law to be confined to qualitative statements.

Fechner was unable to deduce Ampère's and Faraday's phenomena from the laws of the electric force, as Poisson, his model, would have done,[21] but that did not matter to him now. He knew that Weber had derived a fundamental law of electrical action, from which everything that Fechner had found and much else besides could be deduced, and Fechner offered his theory merely as a forerunner of Weber's. When Weber published his theory the following year, he acknowledged Fechner as the first to have "really succeeded" in revealing the "interconnection" between Faraday's, Ampère's, and Lenz's laws. He endorsed Fechner's "somewhat generalized Ampère's law"; his own law was only more general yet.[22]

Weber had good working conditions at Leipzig. He was separated from Gauss, a blessing in disguise according to Poggendorff, who had long thought that Weber's

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[20] Fechner regarded currents as charges in motion, and he related the forces on current elements to forces on these charges. He resolved the forces into components parallel and perpendicular to the wires; the parallel component is the induced electromotive force causing currents to arise, and the perpendicular component is the ponderomotive force driving the wires together or apart.

[21] For Fechner, the authoritative theoretical approach to electrical phenomena was Poisson's; namely, the reduction of all phenomena to laws of attraction and repulsion of electricity. But he allowed that the difficulties in applying Poisson's investigations to actual cases encountered in the laboratory still forced physicists to seek information about the phenomena from experiments rather than to seek merely confirmations of Poisson's investigations. In considering Faraday's alternative to Poisson's approach, Fechner did not think that a quantitative expression of Faraday's curved lines of force was possible. The "composition and resolution of the actions of infinitely many points," as Faraday's approach required, was "too difficult for the calculus" (Gustav Theodor Fechner, "Ueber Elektricität durch Vertheilung," *Ann.* 51 [1840]: 321–50, on 341–43).

[22] It includes induction due to changing intensity of current as well as Fechner's induction due to the motion of steady currents. W. Weber "Elektrodynamische Maassbestimmungen" (1846), 178–79.

"servant's" role in his collaboration with Gauss had hindered him, Gauss receiving recognition for their joint work while Weber was nearly ignored. Soon after Weber lost his job in Göttingen, Poggendorff expressed to him his hope that "in science, too, you may belong to yourself alone again."[23] Weber's salary at Leipzig was nearly twice his Göttingen salary, and the physics institute's budget was also nearly twice Göttingen's. Until a new magnetic observatory was completed, he could use the subterranean vaults of the university building to set up his magnetic apparatus. In the researches on galvanism that occupied him in 1843 and 1844, he had at his command several very sensitive electrometers from the university's instrument collection, which he used to perform "Volta's fundamental experiment" in various ways, "with definite success." Soon he added to the collection instruments of his own design, which were better suited for "quantitative determination."[24] After setting up the electrodynamometer that the Göttingen mechanic had built for him,[25] he submitted the foundations of electrodynamics to experimental and mathematical investigations in his Leipzig physics institute.

In 1845 Weber sent Gauss a manuscript on electrodynamics containing an equation for the interaction of two current elements, which he intended as an improvement over Ampère's version. Gauss was critical of it, adding that he had worked on a similar theory 10 years before. Weber apologized for entering Gauss's domain: his interest in electrodynamics, in which Fechner and the mathematician A. F. Möbius had encouraged him, had led him to work on a subject that, as he realized from the beginning, was above his head.[26] But he persisted, and his paper, a book-length "treatise," became the first of eight major publications between 1846 and 1878, appearing under the series title "Elektrodynamische Maassbestimmungen" ["Determinations of Electrodynamic Measures"], which expressed his understanding of the inseparability of mathematical physics from the physics of exact measurement. His main achievement in 1846 was to derive a single fundamental law that brought together the laws of electrostatics, electrodynamics, and voltaic induction. The treatise was a masterwork of theoretical and measuring physics, which together with the other publications in the series reconstructed the physics of electricity in much the same way that Gauss's work had reconstructed the physics of magnetism. For the rest of his career, Weber worked on problems related to this first publication, and it and Neumann's publication of a mathematical theory

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[23] Poggendorff to Weber, 6 April 1839, in Heinrich Weber, *Wilhelm Weber. Eine Lebensskizze* (Breslau, 1893), 60–61.

[24] Weber to Gauss, 9 April, 5 August, and 24 December 1843, Gauss Papers, Göttingen UB, Ms. Dept.

[25] Weber to Gauss, 25 April 1844, Gauss Papers, Göttingen UB, Ms. Dept.; W. Weber, "Elektrodynamische Maassbestimmungen" (1846), 36.

[26] Weber to Gauss, 18 January, 1 February, and 31 March 1845; Gauss to Weber, 19 March 1845; Gauss Papers, Göttingen UB, Ms. Dept. "Elektrodynamische Maassbestimmungen." Wilhelm Weber, "Ueber ein allgemeines Grundgesetz der elektrischen Wirken," *Abh. sachs. Ges. Wiss.* (1846), 211–378; in *Werke*, vol. 3, *Galvanismus und Elektrodynamik, zweiter Theil*, ed. Heinrich Weber (Berlin, 1893), 25–214,

of induction the year before set problems and directions for German research in electrodynamics for the next several decades.[27]

In the preamble to the treatise, Weber discussed the state of electrodynamics, which was essentially Ampère's work, since other physicists had added almost nothing to it. Ampère, Weber said, had devoted only a part of his work to the phenomena and laws of the interaction of conductors, and Ampère himself had maintained that the work was incomplete, repeatedly pointing out what remained to be done. The task of experimentally deriving a quantitative electrodynamic law Ampère had found impossible, lacking suitable, precise measuring instruments. The experiments he had managed to carry out, Weber decided, had not confirmed his law. Ampère had lacked time to continue his investigations, and others had not followed up his discussion of the theoretical possibility of a more fundamental law of interaction between pairs of electric particles. After Ampère, the interaction of conductors was repeatedly shown to be an experimental fact but under circumstances that did not allow for quantitative determinations. The instrumental means were not only difficult to use but they also did not allow as many different and precise observations in electrodynamic investigations as they did in electromagnetic ones, which physicists tended to prefer for that reason.[28] If physicists were convinced of the accuracy of Ampère's law without direct confirmation, that did not, to Weber's mind, remove the need for it. He was puzzled why, with the general understanding that all natural phenomena were to be grasped by number and measure," no one had yet attempted to make precise measurements in electrodynamics. Weber made the first attempt not only to confirm the fundamental laws of electrodynamics but also to offer them as a "source for completely new investigations."[29]

Ampère had discussed his experiments and their significance for his theory, and he had carefully described his instruments, but he had neglected to give the details: he had said nothing about how often he repeated the experiments, how he had varied them, or the results he had obtained from them. Weber doubted that Ampère's experimental means could support his claim that his law was "derived only from experience." The main difficulty was that Ampère had connected the movable conductor to the battery in such a way that friction canceled out all or most of the electrodynamic force under observation. To confirm the source and distance dependencies of Ampère's law experimentally, Weber required an instrument in which friction was an "unnoticeable fraction" in comparison with the electrodynamic force.[30]

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[27] K. H. Wiederkehr, *Wilhelm Eduard Weber. Erforscher der Wellenbewegung und der Elektrizität 1804–1891,* vol. 32 of Grosse Naturforscher (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1967), 101.

[28] W. Weber, "Elektrodynamische Maassbestimmungen" (1846), 29–30

[29] Ibid., 34–35.

[30] Ibid., 31–35.

Weber had such an instrument, built 12 years earlier: an electrodynamometer, consisting of a wire coiled around a thin wooden frame and suspended by two fine metal wires, which also conducted current through the coil. The coil, or "bifilar roll," was set in motion by a second coil of wire, or multiplicator. The use of the suspension of the bifilar roll as the connection to the battery eliminated the friction that qualified Ampère's experiments. Following Gauss, Weber used the rotational moment of the suspended part as a measure of the rotational moment delivered to the bifilar roll by the electrodynamic force.[31] Upon making a series of observations of the angular displacement of the oscillating roll, he found "complete agreement" between his measurements and his calculations of the electrodynamic force, providing a "complete proof of Ampère's fundamental law."[32] He also showed that with his electrodynamometer he could measure Faraday's voltaic induction, a part of electrical science implicitly contained in Ampère's law.[33]

Having determined in the experimental first part of his treatise the correctness of Ampère's law for the phenomena Ampère had considered, Weber directed the second part to a theoretical derivation and application of a law more general than Ampère's. The theory he worked with was the constructive type, based on an assumption about electric current. Like Fechner, he viewed electric current as electric masses in motion: any element of current contains an equal number of positive and negative electric masses moving with the same speed in opposite directions in the wire, the measure of the current being the number of positive electric masses passing through a cross-section of the wire in unit time. He did not claim that his current resembles the real current, and at the time he rather thought otherwise. The quantitative statement of his assumption is that current intensity i equals aeu, where a is a constant dependent on the units for i, e is the quantity of positive electricity per unit length of conductor measured in mechanical units, and u is its velocity along the conductor. With the help of this definition, Weber derived a "general fundamental law" from three "facts," which came partly from observation and partly from Ampère's "fundamental law." The first fact is that two current elements in a line repel or attract according as the currents move in the same or opposite directions. The second fact is that two parallel current elements which make right angles with their connecting line attract or repel according as the currents move in the same or opposite directions. The third fact is that a current element in line with an element of wire induces a current in the wire, which has the same or opposite direction according as it decreases or increases in intensity. Weber inferred from the first fact that the electrostatic force between two electric masses is weakened when there is relative motion between them; he expressed this mathematically as a dependence of the force on their relative velocity. He inferred from the third fact that the relative acceleration between the two masses makes a contribution. For the law of force, he assumed a mathematical expression

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[31] Ibid., 35.

[32] Ibid., 69–80, quotation on 79.

[33] Ibid., 92–107.

containing terms for the electrostatic force and for the "relative" velocity and acceleration of a pair of like electric masses, and he showed that it yields AmpŁre's force and Faraday's induced current. The finished law of force, which Weber described as the "general fundamental principle *for the whole theory of electricity,*" reads:

$$\frac{ee'}{r^2}\left[1-\frac{1}{c^2}\left(\frac{dr}{dt}\right)^2+\frac{2r}{c^2}\,\frac{d^2r}{dt^2}\right],$$

where the constant c replaces the constant a. According to this formula for the mutual action of two like electric masses e and e' separated by the distance r, electrodynamics and electrostatics are no longer distinct branches of electricity, as they are in Ampère's scheme of the division of the sciences. Rather electrostatic action represented by the first term in the bracket is the limiting case of electrodynamic action when the relative velocity dr/dt and the relative acceleration d^2r/dt^2 of the electric masses vanish. In effect, Weber extended and altered Coulomb's law of force between two electric charges at rest to apply to charges in relative motion. To connect his force between imponderable electric masses with Ampère's force between ponderable wires, Weber assumed that the electric force is transferred to the wires through their resistance to the motion of the electric masses within them. In the 1846 publication and in a shortened version in the *Annalen* in 1848, Weber gave a second derivation of his fundamental law. He substituted the equation for negative and positive currents above for the currents that enter Ampère's law. From the resulting formula for the force between two current elements (there are actually four forces, corresponding to the combinations of two conductors and positive and negative electricities), and after several solid pages of formulas, he arrived at the force between a pair of moving electric masses. He combined the law of electrostatic force with the force arising from moving electric masses, yielding an expression for the force between e and e' having the same mathematical form as the above. In addition to including the electrostatic contribution, Weber's law is more general than Ampère's in another way: it is valid for moving conductors and for variable currents.[34] By contrast with Ampère's law, which is an interaction between ponderable material conductors, Weber's law is an "electric law" between electric masses; Ampère had expressed just such hope that the ponderable force would be determined by the force of electric fluids.[35]

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[34] Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford, New York, and Athens: Oxford University Press, 2000), 63. The two constants are proportional: $a^2 = 4/c^2$; the law as stated above containing c is the form Weber gave it in 1852. The symbol e is the electric mass measured electrostatically; it is simply proportional to the electric mass per unit length in Weber's definition of current. The two derivations are on pp. 134–48 and 151–57 of the 1846 treatise. Wilhelm Weber, "Elektrodynamische Maassbestimmungen" (Excerpt), *Ann.* 73 (1848): 193–240; repr. in *Werke*, vol. 3, 215–54.

[35] Ampère's "fundamental principle" of electrodynamics is

Weber's law does more than connect electrostatics and electrodynamics, as he explained in his 1846 memoir:

> When one . . . occupies oneself with the connection between *electrostatic* and *electrodynamic* phenomena, one need not be guided only by the more general scientific interest of penetrating the existing relations between the different parts of physics, but one may also envision a more specific purpose that concerns *measure determinations of Voltaic induction from a more general fundamental law of the pure theory of electricity*. These measure determinations of Voltaic induction now belong to the electrodynamic measure determinations that are the main object of this treatise.

Weber thought it was "self-evident" that the "establishment of such measure determinations is most intimately linked with the establishment of the *laws* that govern the phenomena in question, so that the one cannot be separated from the other."[36] The connection between laws of force and measurements is inherent in the meaning of laws. "According to the aim of physics," Weber said, laws do not explain the cause of forces but present a "general method for the *quantitative* determination of the forces by means of the fundamental measures established in physics for space and time."[37] Weber brought coherence to electrical science by introducing a universal instrument and absolute measurements in electrodynamics, electromagnetism, and electrostatics.[38]

The action expressed by Weber's law is instantaneous and directed along the line connecting the electric masses, in agreement with the example of the gravitational force. But it disagrees with the gravitational force in its dependency on the relative motion as well as the separation of the pair of electric masses. Physicists who discussed Weber's law remarked on this novelty; because of it, the German historian of physics Ferdinand Rosenberger writing in the 1880s described Weber's law as a "revolutionary achievement of the first-order."[39] Weber was aware that his law introduces an "entirely new element" into the concept of force, and from the start he tried to forestall objections. Because physical laws give only a method for measuring forces, he said, one cannot object "from a physical point of view" if a force is represented by a function of time as well as of distance, both time and space being measurable quantities and therefore suited for exact quantitative determination. Any analogy used in the formulation of new laws is to be followed only so long as it leads to correct results; once it ceases to do so, it is to be abandoned for new

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$$\frac{-ii'}{r^2}\left(\cos\varepsilon - \frac{3}{2}\cos\theta\cos\theta'\right)ds\,ds',$$

which expresses the ponderomotive force between current elements. In this formula ds and ds' are the elementary lengths of the two currents i and i', and ε, θ, and θ' refer to the angles that the positive currents in the two elements make with one another and with the connecting line r between them. Weber, "Elektrodynamische Maassbestimmungen" (1846), 69–70.

[36] Ibid., 132–34, on 134.

[37] Ibid., 113

[38] Darrigol, *Electrodynamics*, 65.

[39] Ferdinand Rosenberger, *Die Geschichte der Physik*, vol. 3, *Geschichte der Physik in den letzten hundert Jahren* (Braunschweig, 1890; repr. Hildesheim: G. Olms, 1965), 507.

approaches. Weber thought that physicists might "well expect" other forces to show a similar dependency on motion as his electrodynamic force.[40] In the shortened version of the 1846 paper published in the *Annalen*, he followed the derivation of his fundamental law with a formula for its potential, in keeping with the example of Gauss's work on magnetism,[41] though he did not use the potential in his early papers.

The constant c has a ubiquitous presence in Weber's electrodynamics. From the way it appears in his fundamental law, it can be looked at as the limiting constant velocity with which two electric masses must move relative to one another for them to exert no mutual force. This is the physical meaning Weber gave to it in 1852.[42] The practical importance of c arises from its place in Weber's system of absolute measures for electrodynamics.

The use of absolute measures in magnetism made the establishment of absolute measures in electrodynamics an unquestioned goal. At first, because an electric current can be measured by the motion of a magnet, Weber made use of the absolute measures of magnetism in his "fundamental measures" for electrodynamics.[43] But he recognized that an electric current can also be measured by another electric current, and that electrodynamic measures can be established on the basis of Ampère's law and the law of voltaic induction. He developed fundamental measures for current intensity, electromotive force, and resistance on this system.[44] A third system is measures used in electrostatics, where the measure of force is the same as the measure of force in mechanics, and Weber set out to derive measures of current intensity and resistance from measures used in mechanics.[45]

To use the measures of mechanics in electrodynamics, Weber had to find the rule by which all of the measurements that he had made using the electrodynamic and the electromagnetic measuring systems could be converted. He understood that this rule could be found only from his fundamental law, not from any preceding law such as Ampère's, and this meant that the conversion of the measures involved the undetermined constant c. For example, to convert measurements of current intensity from electrodynamic measures to mechanical ones, Weber had to multiply the measurements by $c/2$. It was obvious to him that as long as the value of c was

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[40] W. Weber, "Elektrodynamische Maassbestimmungen" (1846), 149–50.

[41] W. Weber, "Elektrodynamische Maassbestimmungen," excerpt, (1848), 245.

[42] Wilhelm Weber, "Elektrodynamische Maassbestimmungen insbesondere Widerstandsmessungen," *Abh. sächs. Ges. Wiss.* 1 (1852): 199–381; repr. in *Werke*, vol. 3, 301–471, on 368.

[43] W. Weber, "Elektrodynamische Maassbestimmungen" (1852), 320–21, 358. Also Eduard Riecke, "Wilhelm Weber," *Abh. Ges. Wiss. Göttingen* 38 (1892): 1–44, on 20.

[44] Weber had no independent measure for resistance in 1846 but he derived one from the measures of electromotive force and current intensity. "Elektrodynamische Maassbestimmungen" (1852), 358–65. The "electromagnetic" fundamental measures are not identical with the "electrodynamic" ones but are proportional to them.

[45] W. Weber, "Elektrodynamische Maassbestimmungen" (1852), 365–68. Electric charge is an absolute unit. The relationships between the three systems of units, electrodynamic, electromagnetic, and electrostatic, are given in Darrigol, *Electrodynamics*, 65–66, 399.

unknown, the “special” systems – the electromagnetic and electrodynamic measures – would continue to be indispensable for “practical uses in electrodynamics.” Without the determination of c, moreover, Weber’s fundamental law was incomplete, capable of predicting quantities only in ratios in which c cancels out. With his friend the Marburg physicist Rudolf Kohlrausch, he measured and published the value of c in 1855.[46] This work, which brought the absolute system of measures in electrical physics to an inner conclusion, Weber delayed as long as he did because of the difficulty in carrying out the measurement, especially the part connected with static electricity, for which Kohlrausch’s expert assistance was invaluable.[47] Weber remarked on the closeness of the measured value of c to the velocity of light, and at the same time he called attention to the physical dissimilarity of their origins.[48]

Weber’s law drew the interest of physicsts in part because of its complete generality, encompassing the several branches of electrical science. Neumann made this point in his lectures on electric currents:

> the very general mechanical law of electrical distance actions, the so-called Weber’s electrical fundamental law . . . encompasses the complete distance actions of electricity, of electrical currents as well as electricity at rest. Through it, electrodynamics has first acquired a secure foundation; the law accomplishes in its sphere entirely the same as Newton’s gravitational law. With it, we certainly have not reached the last step of our knowledge; there is still need for an explanation of the gravitational law and of Weber’s fundamental law; but through it the structure founded on these laws can suffer no alteration.

The validity of Weber’s law was tested by experiment, Neumann said, and the success was “very splendid.” The law provided a theoretical foundation for Ampère’s and Faraday’s laws, and in that respect, it could be compared with Newton’s law, which provided a theoretical foundation for Kepler’s laws. But Neumann thought that Weber’s law was probably not the last or simplest law to

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[46] Weber, “ Elektrodynamische Maassbestimmungen” (1852), 366–68. Wilhelm Weber and Rudolph Kohlrausch, “Ueber die Elektricitätsmenge, welche bei galvanischen Strömen durch den Querschnitt der Kette fliesst,” *Ann.* 99 (1856): 10–25; repr. in Weber, *Werke*, vol. 3, 597–608. Weber had already announced the value of c in remarks to the Saxon Society in 1855 when he submitted his and Kohlrausch’s paper “Elektrodynamische Maassbestimmungen insbesondere Zurückführung der Stromintensitäts-Messungen auf mechanisches Maass.” Wilhelm Weber, “Vorwort,” *Verh. sächs. Ges. Wiss.* 17 (1855): 55–61; repr. in *Werke*, vol. 3, 591–96, on 594. In their paper in the *Annalen* in 1856, Weber and Kohlrausch gave $c = 439{,}450 \times 10^6$ millimeters per second. Darrigol, *Electrodynamics*, 66.

[47] Riecke, “Weber,” 20–21; Leon Rosenfeld, “The Velocity of Light and the Evolution of Electrodynamics,” *Nuovo Cimento*, supplement to vol. 4 (1957): 1630–69, on 1633. Weber, “Vorwort” (1855), 592.

[48] In his first paper, in 1846, Weber used the constant $4/c$ rather than c, and in 1850 he used c. $c/\sqrt{2}$ is the velocity of light in Maxwell’s electromagnetic theory. In 1864 Weber gave the measured value for $c/\sqrt{2}$ as $310{,}740 \times 10^6$ mm/sec. The modern value of the velocity of light is $299{,}792 \times 10^6$ (Wilhelm Weber, “Elektrodynamische Maassbestimmungen insbesondere über elektrische Schwingungen,” *Abh. sächs. Ges. Wiss.* 6 [1864]: 571–716; repr. in *Werke*, vol. 4, *Galvanismus und Elektrodynamik, zweiter Theil*, ed. Heinrich Weber [Berlin, 1894], 105–241, on 157).

which the phenomena can be reduced, for "it remains a great problem to derive the law from motion."[49]

Weber's work on the fundamental law of electrodynamics incorporates several aspects of physicists' work on theories, as discussed in the first chapter. Using an atomistic method, Weber derived a fundamental law by generalizing an existing law, Ampère's; he gave more than one derivation of the law; by means of the law, he connected the branches of electricity; he tested the law using a precision instrument; and he determined a constant appearing in the law. Further, Weber's and Gauss's introduction of absolute measures resulted in a new mathematical representation of physical laws, replacing the one used by Coulomb, Fresnel, and others, which depended on proportionality relations between magnitudes expressed in arbitrary units.[50] We continue our discussion of Weber's way as a mathematical physicist after we take up Neumann's below.

With his choice of subject, Weber sought to correct what he perceived as stagnation in an important branch of physics. He compared Ampère's law unfavorably to Newton's law of gravitation, which had led to innumerable researches and discoveries, removing "all doubt and obscurity" regarding the law, and he foresaw his own law as accomplishing for imponderable matter of electricity what Newton's law had for ponderable matter. Over the years, his law generated a good deal of interest and research, but it would be Maxwell's theory, not his, that had consequences for physics comparable to Newton's. Boltzmann thought that Weber's theory had delayed the recognition of Maxwell's theory in Germany, but until Maxwell's theory was confirmed, Weber's law and variants of it proposed by other German researchers agreed with widely accepted ideas about how fundamental physical laws are arrived at. The success of Newton's law of gravitation led his followers to believe that other forces are of the same kind, dependent on distance. Weber's law—disregarding its unconventional dependence on motion as well as distance—had the right character. With the acceptance of Maxwell's theory it became apparent that distance forces were less useful than they once were,[51] but that understanding came much later than the work we discuss in this chapter.

In the spring of 1848, the political situation in Hannover had changed sufficiently to allow preparations for Weber's return to Göttingen. He had "great interest" in this, for he wanted to reestablish the "lively scientific sphere of activity" he had been part of. The "magical attraction" of Göttingen for Weber was the promise of renewed scientific collaboration with Gauss.[52] In the negotiations, Weber again argued that it was time to give physics "double representation" at

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[49] Franz Neumann, *Vorlesungen über elektrische Ströme*, ed. K. Vondermühll (Leipzig, 1884), 1–2, 296.

[50] Absolute measures "permitted physical laws to be expressed in richer forms of mathematical equations," "as analytical equations whose symbols represented rational numbers," a "great innovation" in physics. Salvo D'Agostino, *A History of the Ideas of Theoretical Physics: Essays on the Nineteenth and Twentieth Century Physics* (Dordrecht, Boston, and London: Kluwer Academic Publishers, 2000), 36, 39.

[51] Wilhelm Wien, "Ziele und Methoden der theoretischen Physik," *Jahrbuch der Radioaktivität und Elektronik* 12 (1915): 241–59, on 245.

[52] Weber to Hannover government official, 28 (or 23) April 1848.

Göttingen, since physics had grown to such an extent that nobody was capable of equally encompassing all of its branches.[53] He wanted to be appointed professor of experimental physics because he had broad experience in it, and he wanted Listing, who had been teaching experimental physics in Weber's absence, to be appointed professor of mathematical physics, as long as Listing was agreeable. Weber laid down two firm conditions for his return: one was that Listing be promoted from extraordinary to ordinary professor, of equal rank with Weber, and the other was that each of them be given an independent budget for his work.[54] Weber's suggestion coincided with what the ministry wanted; Listing was told of the change, not given a choice.[55] Weber asked for an extraordinary grant for the physical cabinet to get started and for a larger grant later for an urgent "renewal" of the physical cabinet, which was needed "*every 25 or 50 years*" if the institute was to meet the "demands of the time" (italics added). Weber's financial requests were approved and in October 1848 he officially accepted the Göttingen position.[56]

The creation of an ordinary professorship for a second physicist at Göttingen was attended by the establishment of two physics institutes in place of one. The ministry intended for Weber to have charge of the physical cabinet, with only a very small part of it going to Listing. Listing received somewhat more than this because the two physicists wanted to accommodate one another and because of Weber's momentary position of strength.[57] Nearly two decades later, Weber was still waiting for over half of the money he had requested for the improvement of his instrument collection. He was, however, able to use part of the annual institute budget and space in the institute for his own research. When in 1851 he reported to the curator that "at least the private practical work of the professor of physics was provided for,"[58] he may have been the only German university physicist at the time

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[53] Weber to "Regierungsrath," 22 May 1848.

[54] Ibid.

[55] Göttingen U. Curator to Listing, 31 July 1848; Listing's answer, 8 August 1848; Listing Personalakte, Göttingen UA, 4/V b/108. The Göttingen curator had already made up his mind that Listing should not continue as professor of experimental physics. To gain the desired decision from the government, he misrepresented Weber's answer above: he wrote that Weber would come "if he were given the direction of the physical cabinet to the extent that it is necessary for his subject, *experimental* physics, and if the present extraordinary professor of physics Listing were appointed ordinary professor of *mathematical* physics." Report by Göttingen U. Curator to King of Hannover, 16 June 1848, Weber Personalakte, Göttingen UA, 4/V b/95a. When Weber learned that Listing had not been consulted, he asked Gauss to obtain Listing's agreement to the arrangements directly before he accepted the offer from Hannover (Weber to Gauss, 21 June 1848; Gauss to Weber, 26 June 1848; Gauss Papers, Göttingen UB, Ms. Dept.).

[56] Weber to the Göttingen U. Curator, 8 July and 5 October 1848; Curator to Weber, 28 August 1848; Weber Personalakte, Göttingen UA, 4/V b/95a.

[57] Göttingen U. Prorector Fuchs to Curator, 5 April 1849, Göttingen UA, 4/V h/19.

[58] Listing to Göttingen U. Curator, 2 January 1853, Listing Personalakte, Göttingen UA, 4/V b/108; Listing's inventory of his mathematical-physical institute as of 1849 in Listing to Göttingen U. Curator, 3 September 1855, Göttingen UA, 4/V h/8. "Bericht des Professors Wilhelm Weber, die Instrumenten-Sammlung des physikalischen Instituts betreffend" to Göttingen U. Curator, 13 August 1849; Curator to Weber, 22 August 1849; Göttingen UA, 4/V h/19. Weber to Curator, 4 April 1851 and 10 May 1851, Göttingen UA, 4/V h/10, 21.

who could have made such a claim for his “private” research. He had implanted his view of the university physicist in the official mind.

7.2 Electrical Research at Königsberg: Neumann

The culmination of Neumann’s researches in physics came in the mid-1840s, when after completing the optical researches that had occupied him for 10 years he investigated a new subject, electromagnetic induction. Like Gauss, Fechner, and Weber, he set out to relate Faraday’s induced currents to known physical laws. His approach differed from theirs in that he did not make hypotheses about the nature of electric currents, though he spoke of the motion of the electric “fluid,” or about the nature of the ultimate forces of attraction and repulsion between electrical masses. He did not need hypotheses of this sort to establish the mathematical laws of induced currents.[59]

Neumann’s first paper on electric induction in 1845 was not an easy work for his contemporaries,[60] who found its German highly convoluted and its mathematical notation idiosyncratic. Jacobi, who proofread Neumann’s paper in Berlin, wrote to him about the problems he had with it, the “most dreadful” being his peculiar way of writing certain differentials.[61] He asked Poggendorff, who of all the Berlin physicists seemed to know Ampère’s work best, to report on Neumann’s paper to the Prussian Academy. Poggendorff having prepared the report from excerpts of Neumann’s results and from some formulas furnished by Jacobi produced such a clumsy account, Jacobi thought, that none of the physicists at the meeting could have gotten a clear idea of the work. The paper might as well have been written in Chinese, since Poggendorff could not have understood the first line of it, Jacobi wrote to Neumann.[62]

For all of its difficulty, Neumann’s first paper on electrodynamics proved influential both for its content and as an example of mathematical physics. In mathematical formulas, with little physical explanation, Neumann set out nearly all that was known quantitatively about induction: Lenz’s law, the law of

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[59] Paul Volkmann, *Franz Neumann. 11. September 1798, 23. Mai 1895* (Leipzig, 1896), 18. Albert Wangerin, *Franz Neumann und sein Wirken als Forscher und Lehrer* (Braunschweig: F. Vieweg, 1907), 107.

[60] Franz Neumann, “Die mathematischen Gesetze der inducirten elektrischen Ströme,” *Abh. preuss. Akad.,* 1845, 1–87, read 27 October 1845; repr. as vol. 10 of Ostwald’s Klassiker der exakten Wissenschaften, ed. Carl Neumann (Leipzig, 1889). An excerpt appeared as “Allgemeine Gesetze der inducirten elektrischen Ströme,” *Ann.* 67 (1846): 31–44.

[61] Jacobi observed that Neumann’s use of capital “*D*” instead of the usual lowercase “*d*” made the expression “*DsE*” look “quite like a product of three factors.” Jacobi to Neumann, 6 January 1846, quoted in Leo Königsberger, *Carl Gustav Jacob Jacobi* (Leipzig: B. G. Teubner, 1904), 359. The abbreviated version of this paper appearing in the *Annalen* used the lowercase “*d*.”

[62] Jacobi to Neumann, 5 December 1845, in Königsberger, *Jacobi*, 355.

proportionality between the induced current and the speed with which the conductor is moved, the proportionality of the induced current and the action of the inducing current, the proportionality of the induced current and the induced electromotive force, or Ohm's law, and the independence of the induced electromotive force from the material of the conductor. In a few steps Neumann derived the "general law of linear induction," the fundamental equation of his theory: it is written, as Ampère's law is, for a differential element of wire, $E \cdot Ds = -\varepsilon v C \cdot Ds$. In words, it says that the induced electromotive force in an element of wire is proportional to the total inducing action on it.[63] By integrating the differential law around a closed circuit and over a finite time, he obtained the "integral current," or "finite action of the current," which is what is usually measured.

Neumann next reformulated his theory, making use of a potential, analogous to Gauss's potential in magnetic theory. From the work performed by the electrodynamic force in a virtual displacement of an element Ds, he derived what he called the "electrodynamic potential" expressing the electrodynamic action of two closed currents on one another[64]:

$$V = -\tfrac{1}{2} jj' \iint \frac{\cos(Ds, D\sigma)}{r} Ds D\sigma,$$

where j and j' are the intensities of the two currents s and σ, which are assumed to remain constant, and $(Ds, D\sigma)$ is the angle between the two current elements Ds and $D\sigma$. The potential is the mechanical work performed against the electrodynamic force in bringing the two currents from infinity along any path with any speed. The derivative of the potential is the electrodynamic force (or the negative of the force, as he stated it 2 years later). Neumann showed that the potential allows the induction law to be expressed in an especially simple way, and when no induction occurs, the potential yields Ampère's law. In his second publication on induced electrical currents, in 1847, Neumann extended the above analysis to the case where the form of the conducting circuit changes, not just its position.[65]

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[63] Using the common lower case d instead of D for the differential sign, the differential element of wire is ds, the induced electromotive force in it is Eds, and the inducing action on it is $-\varepsilon vCds$, where v is the velocity of the element of wire, C is the component of the inducing action on the element in the direction of its motion, which is determined by Ampère's law, and ε is a constant. The negative sign comes from Lenz's rule. Neumann, *Die mathematischen Gesetze*, 17.

[64] In the general case, j and j' are inside the integral signs. Neumann, "Allgemeine Gesetze," 39; "Die mathematischen Gesetze," 69. This potential law is discussed in Wangerin, *Neumann*, 115, 123; Rosenberger, *Geschichte der Physik*, vol. 3, 510–12; Edmund Hoppe, *Geschichte der Elektrizität* (Leipzig, 1884), 438–48; Arnold Sommerfeld and R. Reiff, "Standpunkt der Fernwirkung. Die Elementargesetze," in *Encyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen*, Vol. 5, *Physik*, ed. Arnold Sommerfeld, pt. 2 (Leipzig: B. G. Teubner, 1904–1922), 3–62, on 27–34. Edmund Whittaker, *A History of the Theories of Aether and Electricity*, vol. 1, *The Classical Theories* (New York: Harper & Brothers, 1960), 198–200.

[65] Franz Neumann, "Ueber ein allgemeines Princip der mathematischen Theorie inducirter elektrischer Strőme," *Abh. Preuss. Akad.* 36 (1848): 1–71; read 9 August 1847.

Referred to as a "law" and a "fundamental principle," Neumann's potential found wide applications in the development of electrodynamics. Helmholtz said that for 30 years, he had "never applied another fundamental principle than the potential law," that he had never needed any other to find his "way in fairly labyrinthine problems of electrodynamics and at times even over previously untrodden ground." Its mathematical expression is "relatively simple," and it encompasses the "whole experimentally known area of electrodynamics, ponderomotive and electromotive actions." Helmholtz called it "one of the most fortunate and most fruitful ideas that recent mathematical physics has produced."[66]

At the beginning of 1846 Jacobi wrote to Neumann from Berlin that he was "at once" going to send a reprint of Neumann's paper to Weber, "who was here recently and was very interested in the work."[67] Weber published his theory after he had received the version of Neumann's work prepared for the *Annalen der Physik*, and so he was able to discuss it together with his own. Neumann's mathematical results "can hardly be doubted," Weber wrote, either with respect to their interconnection or with respect to their connection to empirical rules. Since Neumann had presented his work in a completely different way than he had his, Weber compared the two, finding that Neumann's law agrees with his for all cases in which induction is due to magnets or closed currents. Neumann, too, compared their work in the concluding paragraphs of his second paper. From a general expression for induction derived from Weber's law, he showed that only when the inducing circuit changes form does Weber's law differ from his own, in which case the two laws attribute opposite directions to the induced electromotive force; he performed an experiment that showed he was right. (Responding to Weber's mode of presenting his work, Neumann took what was for him an unusual step by including in his paper a schematic drawing of his experimental arrangement.) He found that Weber's law was not incorrect but that the motion of electricity in the inducing circuit of variable form was incorrectly conceived. With the necessary adjustment, he concluded that his and Weber's laws agreed. Not satisfied with the way Neumann had reached the agreement Weber repeated Neumann's experiment, first in a version of his own and then more or less as Neumann had described it, determining that it bore out Neumann's theory exactly. He next examined Neumann's derivation of the induction law from his own law to see if he had taken into account all "given relative motions of electric fluid" and their changes. He found that Neumann had overlooked a particular elementary action, a change in the velocity of the electrical particles in addition to a change in form of the circuit occurring in the experiment. In the end, by carrying through a physical analysis, Weber came to the satisfactory result that his and Neumann's laws are indeed in agreement. To reach this conclusion had not been easy, and Neumann thought that the resolution of their differences was a "very brilliant success." After this, Weber's law acquired wide authority in

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[66] Herman von Helmholtz, "Kritisches zur Elektrodynamik," *Ann.* 153 (1874): 545–56; repr. in *Wissenschaftliche Abhandlungen*, 3 vols. (Leipzig, 1882–1895), vol. 1, 763–73, on 772–73.

[67] Jacobi to Neumann, 6 January 1846, quoted in Königsberger, *Jacobi*, 359.

Germany for some time, his theory entering the textbooks as the fundamental mathematical theory of electric induction.[68]

Weber's and Neumann's examination of one another's work points to a common direction in their researches and also to their different methods. Like their starting point, the French "neo-Newtonian" theories, their electrodynamic theories make use of instantaneous, action-at a-distance forces between elementary units of analysis, electrical masses in the one case and current elements in the other. But their results look quite different. Neumann's law refers to closed, linear currents as a whole; that is, to their intensity and position only at the beginning and end of an induction. It immediately yields the sum of the electromotive forces, but it says nothing about the intervening details. The obvious advantage of a simple law like Neumann's is its ease of application. By comparison, Weber's law gives a "rule" for indirectly obtaining the electromotive forces by summing over all "elementary actions," and it is necessary to take account of them all to arrive at the induction law for a particular case. Weber's law is more general than Neumann's, applying to open as well as closed currents, to electrostatics as well as electrodynamics, and, in principle, to all electrical processes. (Weber's law covers most but not all of electrical science, since additional assumptions are needed to explain, for example, electrical resistance and magnetism.) Both Weber and Neumann combined experimental work with their theoretical derivations of laws, and in both cases it was about measurements; their experiments were confirmatory, not exploratory, leading to conviction rather than to discoveries. Neumann used familiar instruments, relying on error analysis for accuracy. More inventive of instruments than Neumann, Weber relied on precision and experimental technique for accuracy. Their differences are what we would expect of two such original researchers. Their methods in making electrodynamic theory, atomism and phenomenology, could be productively joined, as Kirchhoff would demonstrate.[69]

Weber envisioned his fundamental law as potentially applying throughout physics, the basis of a total theory, while Neumann saw his work as limited to electrodynamics, a consequence of his phenomenological preference and perhaps also his native caution. Although Neumann did not direct his varied researches toward a total physical theory, he nevertheless showed an interest in the connectedness of physics in a number of ways. His law of electrodynamic potential, as Helmholtz said, covered "the whole experimentally known area of electrodynamics." The problems he chose to work on brought together branches of physics: in his early work they were optics and elasticity, in his later work, electricity and magnetism. He accepted the common understanding that mechanics is the foundation of all of physics. He valued Weber's fundamental law of electrodynamics for bringing

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[68] Wilhelm Weber, "Bemerkungen zu Neumann's Theorie inducirter Ströme," *Verh. sächs. Ges. Wiss.* (1849): 1–8; repr. in *Werke*, vol. 3, 269–75; Neumann's discussion is quoted on 270–71. Weber, "Elektrodynamische Maassbestimmungen" (1852), 405–27, on 418. Rosenberger, *Geschichte der Physik*, vol. 3, 513. Wangerin, *Neumann*, 121–22.

[69] Darrigol, *Electrodynamics*, 65, 74–75.

together the parts of electricity, and like Weber he connected the branches of physics through measurement. In a letter to his mentor Weiss during his first period in Königsberg, he spoke of his "need" for "exact quantitative determinations of the physical quantities," which he wanted to carry out "according to plan through all of inorganic nature." He made a beginning with experiments on specific heats, believing that heat phenomena provided "the mechanical foundation and the connecting link between [illegible word] relations and the chemical properties," and in the work of others, he thought that he saw traces of the connection.[70] Neumann's research in electrodynamics was narrower than Weber's, but he was not out of step with the physical thinking of the time.

To close our discussion of Neumann, we return to the characteristics of theoretical physics introduced in Chap. 1. Theoretical physicists commonly spoke of the derivation of physical laws as their first task, and we find that Neumann derived, generalized, and gave alternative formulations to a number of laws. He performed experiments in connection with them as well, in agreement with his understanding of what the work of theoretical physics is.[71] We saw his combination of mathematical and experimental approaches in his early work in optics and elasticity, and in this chapter we see it again in his later work in electrodynamics. Together with Weber, he did the follow-up work of theoretical physics, critically comparing different derivations and formulations of laws, using both mathematical and experimental methods.

Neumann could give the impression of being more interested in methods than in results. When he recommended his student Heinrich Wild for a position at the St. Petersburg Academy in 1868, he did not talk about Wild's research but about the research methods he had at his command: the ability to use mathematics ("analysis"), to create "new theoretical methods," and to find and use experimental methods.[72] In his research and in his lectures, Neumann used a variety of methods. In some of his work in optics and elasticity he followed French authors who applied a molecular method, and in other work he used a phenomenological method.[73] In electricity, he sometimes used the method of elementary laws; he began his work in 1845 with a reduction of electrodynamics to actions between elementary currents. In 1847 he replaced "elementary" laws with "integral" laws for the potential and induced currents in closed circuits, which he found simpler to work with, but he did not consider them as final. For the complete understanding of electric phenomena, he accepted that elementary laws were needed, and he encouraged Weber in his work on elementary laws in electrodynamics.[74]

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[70] Neumann to Weiss, undated draft of a letter.

[71] P. Volkmann, *Franz Neumann. 11. September 1798, 23. Mai 1895* (Leipzig, 1896), 17.

[72] Neumann to M. H Jacobi, undated draft from 1868.

[73] He used the method of phenomenology in 1834 in treating the reflection and refraction at crystal interfaces, in 1837 in deriving laws of total reflection, and in 1841 in deriving optical phenomena in compressed and heated bodies. (Wangerin, *Neumann*, 259–61).

[74] Ibid., 33–36.

Neumann's work was mathematical and abstract, but the problems he addressed mathematically held physical interest for him, and he related his mathematical solutions to observations of the phenomena.[75] His preference was recognized by other physicists. Poggendorff, who accepted mathematical physics in the *Annalen* only if it related to experimental physics, welcomed contributions by Neumann, writing to him in 1837 that he was very happy at the prospect of a new work: "I always considered your treatises true enrichments of the *Annalen*, even though the general public, to which I pay no attention, often may not be very comfortable with them."[76] Neumann could become interested in the mathematics of a physical problem, but he did not share the mathematician's concern with elegance and generality.[77] He made one contribution to mathematics having to do with the theory of spherical functions, but this had relevance to his work in physics.[78] He made observations in the laboratory to obtain "exact measurements" of the phenomena under study, which enabled him to test a theory and to guide its further development. Volkmann called Neumann a "deductive" experimenter in contrast to "inductive" experimenters such as Plücker, Kundt, and Hertz. By "deductive," he meant that Neumann's experiments originated in deductive observations and mathematical theories; for him, theory always came first, observations after. Neumann studied the construction of physical instruments and the art of measuring methods, and he regarded the method of observation as "itself a problem of theory." In his seminar in 1852, for example, he chose problems that would "awaken interest in bringing a theoretically guided investigation to experimental application," and in 1854 and 1855 he set the problem of determining the influence of the measuring instrument on the result of the measurement.[79] In agreement on the need for exact laws and exact measurements in physics, he took part in Gauss and Weber's program of expressing magnitudes of electricity and magnetism in absolute units.

Writing from Königsberg to a colleague in 1850, Helmholtz said that Neumann "is rather difficult to get at," shy, "but a thinker of the first-order."[80] Neumann holds a unique place in our history as the first professor in a German university to give comprehensive lectures on "theoretical physics," laying the groundwork for an independent field. Partly through his researches but more through his teaching, he did what Ohm, Weber, and other early German physicists we discuss in this book did not do, establish a school. He impressed on his students a specific idea of the practice of theoretical physics, and those who became physicists held him in high esteem and extended his example. It is not uncommon for students to endow their

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[75] Woldemar Voigt, "Zur Erinnerung an F. E. Neumann, gestorben am 23. Mai 1895 zu Königsberg i/Pr.," *Gött. Nachr.* (1895): 248–65, on 255.

[76] Poggendorff to Neumann, 25 October 1837, Göttingen University Library, Manuscript Department, Neumann Nachlass.

[77] Wangerin, *Neumann*, 255.

[78] Ibid., 127–28.

[79] Ibid., 53, 59, 257. Paul Volkmann, "Franz Neumann als Experimentator," *Physik. Zeitschr.* 11 (1910): 932–37, on 932–33, 935.

[80] Königsberger, *Hermann von Helmholtz*, 64.

former teachers with exceptional accomplishments, but in Neumann's case it was not just his students. The Austrian theoretical physicist Boltzmann said that Neumann "can be called now the father and Nestor [begetter and wisest] of the new theoretical physics."[81] It is to the point that the first two German theoretical physicists to become ordinary professors for theoretical physics were Neumann's students, Kirchhoff and Voigt.

7.3 Electrical Research at Berlin: Kirchhoff

The son of a Königsberg lawyer, Gustav Kirchhoff entered the local university originally to study mathematics, but he vacillated. For a time he was attracted to chemistry, and then to physics, which he settled on, overcoming an aversion to "boring observations and even more boring calculations" (referring to Earth-magnetic measurements, by then a common method of introducing physics students to observations).[82] As a member of Neumann's seminar, he soon revealed a "talent" for mathematical physics.[83] While still a member, as we saw, he published a mathematical solution to a problem of the conduction of electricity in a metal plate together with its experimental confirmation, and he also treated currents in branching circuits.[84] This research confirmed his talent and launched his career.

When Weber saw Kirchhoff's publication, he made so much "fuss" about it that Poggendorff became interested and gave a lecture on part of it, praising it highly. "R" (probably Riess) thought that anybody could do what Kirchhoff had, but Poggendorff said that he could not do it even now, and that a few years ago Weber had sat for 3 days at Poggendorff's place struggling with the easiest particular case until he had solved it. Jacobi wanted Kirchhoff to move to Berlin in 1845, but unless he wanted to study chemistry the only lectures of interest to him would be Poggendorff's on the history of physics.[85] Neumann wrote to Jacobi in early 1846 that the Consilium had granted Kirchhoff a travel stipend of 200 rt and that it was going to ask the minister to add 200 rt. Neumann asked if Kirchhoff could live in Paris for a half year on 400 rt.[86]

We get an idea of the attraction of Paris in the 1840s from a tour taken by another of Neumann's students. Emil Schinz visited Weber in Leipzig, Gauss in Göttingen,

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[81] W. Voigt, "Zum Gedächtniss von G. Kirchhoff," *Abh. Ges. Wiss. Göttingen* 35 (1888): 3–10, on 4; "Neumann," 263; Ludwig Boltzmann, *Gustav Robert Kirchhoff* (Leipzig, 1888), iv.

[82] Kirchhoff to his brother Otto, n.d., quoted in in Emil Warburg, "Zur Erinnerung an Gustav Kirchhoff," *Naturwiss.* 13 (1925): 205–12, on 205.

[83] Wangerin, *Neumann*, 177. Luise Neumann, *Neumann,* 368.

[84] Gustav Kirchhoff, "Ueber den Durchgang eines elektrischen Stromes durch eine Ebene, insbesondere durch eine kreisförmige," *Ann.* 64 (1845): 497–514.

[85] Jacobi to Neumann, 5 Dec 1845, in Königsberger, *Jacobi*, 356.

[86] Neumann to Jacobi, 20 March 1846, in Königsberger, *Jacobi*, 365.

Gerling in Marburg, Magnus in Berlin, Seebeck in Dresden, and in Paris he attended lectures by the renowned experimental physicist Victor Regnault, who was giving a 2-year course and was presently treating the theory of light. Regnault, Schinz wrote to Neumann, is “making magnificent experiments (at the College de France, which has the most beautiful physical apparatus in all of France), and is also presenting the mathematical physical part rather completely before an audience of about 200 men and a few women.”[87] Jacobi had reason to think that a stay in Paris would be helpful to a promising young mathematical physicist like Kirchhoff.

Kirchhoff’s trip to Paris was not to be. Jacobi wrote to Neumann that the Berlin physicists Poggendorff and Magnus doubted that Kirchhoff would be able to work in Paris, and they recommended that he come to Berlin instead to perfect his chemistry and to bring some experimental work with him. Taking their advice, Kirchhoff settled in Berlin, where he was befriended by Jacobi, Poggendorff, and Magnus and also by Emil du Bois-Reymond, Dirichlet, Gustav Karsten, and Knoblauch.[88] In this way, Kirchhoff was introduced to an extended scientific society of experienced researchers who appreciated his original talent and advised him on how to cultivate it.

In Berlin Kirchhoff continued his scientific connection with Königsberg and Neumann. Mathematical laws in physics generally contain constants, the measurement of which belonged to the work of mathematical physics in the 1840s. Lacking a technical name for the new constant ε that entered his induction law, Neumann referred to it in one place as a “current” and in another place as a “concept” of a magnitude. Related to the “mysterious connection” between all bodies, “still concealed in deep mysteries,” this constant was the “true physical problem of all induction phenomena.” Neumann proposed its measurement as a prize problem in 1846 at Königsberg.[89] In a paper published in the *Annalen der Physik* in 1849, Kirchhoff explained that in Neumann’s and Weber’s mathematical laws of induced currents, “a constant occurs, which must be determined by experiment once and for all,” and that he had “undertaken to determine” it. The natural way to do it, he decided, was to give the conductors containing the inducing and the induced currents a shape that allowed him readily to calculate the potential. Then with a galvanometer (magnetic needle, multiplicator, mirror, and telescope) he measured the intensity of the two currents and computed the numerical value of ε.[90] The

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[87] Emil Schinz to Neumann, 21 January 1844.

[88] Kirchhoff to Neumann, 29 February and 13 October 1848, Neumann Papers, Göttingen UB, Ms. Dept. Kirchhoff to his brother Otto, 20 September 1848, quoted in Warburg, “Kirchhoff,” 207.

[89] Neumann regarded ε as a constant when the inducing action varies slowly. Its value depends on the choice of units. The physical meaning of the constant is complicated when put into words. It relates the “integral current,” which is the time integral of the current induced in a circuit by a change of the potential, to the change in potential and the constant current. By use of a clever design of the network, Kirchhoff avoided having to use separate circuits for the inducing and the induced currents (Darrigol, *Electrodynamics*, 46, 67–68; Wangerin, *Neumann*, 177).

[90] Gustav Kirchhoff, “Bestimmung der Constanten, von welcher die Intensität inducirter elektrischer Ströme abhängt,” *Ann.* 76 (1849): 412–26; repr. in Gustav Kirchhoff, *Gesammelte Abhandlungen* (Leipzig, 1882), 118–31, quotation on 118.

measurements were strenuous, and his calculations for the potential were extensive. His determination of this constant "deserves to be set alongside the works of Gauss and Weber," Bunsen said in recommending Kirchhoff a few years later for his first chair of physics.[91] This comparison was high praise for a beginning mathematical physicist in the middle of the nineteenth century.

Weber determined the constant too. When on a visit to Leipzig in 1848 Kirchhoff learned that Weber was working on the same problem, he thought that Weber's treatment was superior to his own, and he nearly abandoned publication. Weber published his measurement in 1850 in the second of his series *Elektrodynamische Maassbestimmungen.* Although their results were the same, Kirchhoff and Weber had different purposes in determining the constant, and their experimental setups were correspondingly different. Kirchhoff's purpose was to complete Neumann's induction law quantitatively. Concerned to understand the cause of resistance, Weber's purpose was to extend the Gaussian program of absolute measurement to complete his theory of electric conduction. Individual as Kirchhoff's and Weber's methods were, according to a historian of electrodynamics, each method embodied a "reciprocal relation between theory and precision measurement," and each showed "sophistication and elegance."[92]

Beginning in 1845 and for several years, Kirchhoff worked on galvanic currents. Proceeding from Ohm's principles, Kirchhoff derived formulas for currents in circuits consisting of any linear conductors. He took the next step in a paper in the *Annalen* in 1848, extending the work to conductors that are not linear.[93] He explained the need for this: systems that consist only of linear conductors are "in reality" seldom found, and in almost all cases in which Ohm's law is applied the circuit is in part non-linear. Formulas for such systems had not yet been derived from Ohm's principles with rigor and in all generality; here Kirchhoff cited the relevant page of Ohm's *Galvanic Circuit*.[94] The method he used was a generalization of the one he had used in 1845 to extend Ohm's law to two-dimensional conductors. This was a familiar kind of work of theoretical physics, the generalization of a method and of a physical law.

On a visit to Weber in 1848, Kirchhoff agreed that it would be preferable to replace Ohm's derivation in *Galvanic Circuit* with one having closer links with the "rest of the theory of electricity."[95] This followed from his understanding that it was always desirable to base the laws of currents on those of electrostatics. Kohlrausch's recent confirmation of Ohm's electroscopic law had still not completely clarified

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[91] Bunsen to Baden Ministry of the Interior, 26 July 1854, Bad. GLA, 235/3135.

[92] Darrigol, *Electrodynamics*, 68–71. Among Weber's lengthy speculations, his preferred explanation was that resistance is caused by the interaction of electric fluid particles following his electrodynamic law.

[93] Gustav Kirchhoff, "Ueber die Anwendbarkeit der Formeln für die Intensitäten der galvanischen Strőme in einem Systeme linearer Leiter auf Systeme, die zum Theil aus nicht linearen Leitern bestehen," *Ann.* 75 (1848): 189–205.

[94] Georg Simon Ohm, *Die galvanische Kette, mathematisch bearbeitet* (Berlin, 1827); repr. in Ohm's *Gesammelte Abhandlungen*, ed. E. Lommel (Leipzig, 1892), 61–186, on 127.

[95] Kirchhoff to Neumann, 13 October 1848.

the relation between electrostatics and the electroscopic force. Kirchhoff resolved the problem in a paper in the *Annalen* in 1849, in which he derived Ohm's law in the way he and Weber had discussed: "In order to bring the different fields of electrical theory under one point of view, one must address the task of deriving the laws of currents in a closed circuit from Weber's law." To show the need for a new derivation of Ohm's law, Kirchhoff pointed out a "contradiction" in Ohm's theory. By analogy with heat conduction, Ohm identified the gradient of the electroscopic force, or electromotive force, with the density of the charge at a point in the body of the conductor. This analogy allowed Ohm to derive the laws of currents, but it implied that a charge at rest is uniformly distributed through the volume of a conductor, whereas according to electrostatics and to experiment, the charge on a conductor in equilibrium lies on the surface. Because Ohm had not known, or at least had not used, the concept of potential, he had expressed his theory in a way that could not bring the theory of moving electricity into conformity with the theory of static electricity. Kirchhoff identified the "electroscopic" force, or "tension," of Ohm's theory with the electrostatic potential, and accordingly, the gradient of the electrostatic potential at a point in the body of a conductor is proportional to the electromotive force there. There was, however, a problem with Kirchhoff's reformulation of Ohm's law in electrostatic terms. Weber's law has in addition to an electrostatic part a part that depends on the motion of electric particles and since electricity is in motion in any volume element of a conductor carrying a current, that part of Weber's law and not just the electrostatic part should contribute to the electromotive force there. Kirchhoff suggested a way around the problem. He accepted Weber's physical picture of an electric current: when an electromotive force is applied, the "neutral electric fluid" in the interior of a conductor is decomposed, the positive electricity moving in one direction and an equal quantity of negative electricity moving in the other, constituting the current. He assumed that every particle of electricity in a current arrives at a momentary place of rest on a molecule of the conductor before the electrostatic force drives it to the next molecule, in which case only the electrostatic part of Weber's law is responsible for the electromotive force.[96] This was not an offhand speculation; in order for Kirchhoff's derivation of Ohm's law to be credible, there needed to be a conceivable physical explanation for an apparent contradiction arising from Weber's law. The physical suggestion was part of the argument of the paper, an interpretation of the formulas. What Boltzmann said about the work of the theoretical physicist is relevant here: he grasps the phenomena not only quantitatively but also "qualitatively" in their full course.[97] The qualitative part is often tentative. Another interpretation of Kirchhoff's 1849 paper is that Kirchhoff used the molecular

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[96] Gustav Kirchhoff, "Ueber eine Ableitung der Ohm'schen Gesetze, welche sich an die Theorie der Elektrostatik anschliest," *Ann.* 78 (1849): 506–13, on 506, 509, 512; *Vorlesungen über mathematische Physik*, vol. 3, *Vorlesungen über Electricität und Magnetismus,* ed. Max Planck (Leipzig, 1891), 112. Whittaker, *Aether and Electricity*, vol. 1, 225–26. Darrigol, *Electrodynamics*, 70–71.

[97] Ludwig Boltzmann, "Josef Stefan," 1895, in *Populäre Schriften* (Leipzig: J. A. Barth, 1905), 92–103, on 94.

hypothesis to guide him in deriving Ohm's law, which is not explicitly molecular.[98] For the point we make here, it comes to the same thing: Kirchhoff's physical picture was part of doing theoretical physics, part of the work.

Kirchhoff's 1849 paper was mathematical but not mathematics. As in the case of Neumann's papers, discussed above, it is significant that Poggendorff – again he did not accept papers with mathematics that were unrelated to experimental work – published this paper in the *Annalen der Physik*. There was no need for Kirchhoff to bring in experiments explicitly, since the law he derived already had experimental confirmation. In this theoretical work, Kirchhoff hoped to reconcile his own ideas with Weber's ideas in a natural way, bringing the laws of currents under Weber's general law, but this proved difficult, and he succeeded only in 1857.[99]

Kirchhoff's early electrical researches were well received. Boltzmann said that his work on current branching and his electrostatic proof of Ohm's law were "epoch-making," and that his generalization of Ohm's law found constant "applications in science and technology."[100] Kirchhoff did much to complete Ohm's work on galvanic currents, correcting the assumptions of his theory, deriving his general law from new foundations, and extending its domain from linear to planar to three-dimensional conductors and to any combination of circuits. In an extensive series of investigations on galvanic currents carried out over several years, he gave Ohm's law a range of formulations, improving its accessibility and enhancing its value for researchers. He acted on Ohm's long-run intention to treat galvanic phenomena by the "usual electrical attractions and repulsions" with the goal of rendering electrical science a "whole."[101] In joining Ohm's law to Weber's law, Kirchhoff departed from the method he is better known for, which is to proceed from experimental regularities without the help of atomistic ideas. His use of Weber's currents based on distance forces between electrical particles showed that he was open to supplementing the method with atomistic ideas when that was useful. His concern with the further development of a physical law and with the unification of a branch of physics were important features of Kirchhoff's electrical researches in the 1840s and after, as they were of German work in theoretical physics generally. In his experimental work in electricity, he used Gauss and Weber's method of measuring currents, showing his concern with exactitude, a growing trend of German physics research.[102]

Kirchhoff's work in electricity reflected the new directions in teaching and research in physics in Germany at the time. As a member of the mathematical-physical seminar in Königsberg, he was instructed in mathematical and measuring methods of physics, and as a participant in the Berlin colloquium and Physical

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[98] Darrigol, *Electrodynamics*, 70–71.

[99] Ibid., 71.

[100] Boltzmann, *Kirchhoff*, 22.

[101] Ohm, *Galvanic Circuit*, 401, 434. Specifically, he intended to reduce the actions of parts of a galvanic circuit on one another to electrical attractions and repulsions.

[102] Darrigol, *Electrodynamics*, 70.

Society, he joined the company of other researchers. From the time he entered physics, he was able to draw on major German contributions to electrical theory, basing his work on Ohm's galvanic law, Neumann's law of electrical induction, and Weber's fundamental law of electric action. Like Weber before him, Kirchhoff did not make it to Paris after graduation, as he had hoped, but that was not so important any longer.

From early on, Kirchhoff had a preferred direction in physical research. When his Königsberg mathematics teacher Friedrich Julius Richelot told him that he was in line for a call to Breslau University as extraordinary professor for experimental physics, he knew that he could not turn down the offer, though he did not really want it. What would be fitting and welcome, he told his parents, was a "call for mathematical physics," but he knew of no positions for that subject. He moved to Breslau in 1850 with the thought that it might be good for him to try experimental physics and to tear himself away from his usual range of ideas.[103]

7.4 Researches on Theories of Forces and Heat at Berlin: Helmholtz and Clausius

Helmholtz's knowledge of several sciences was an advantage, as he explained: "possessing some geometrical ability, and equipped with the knowledge of physics, I had, by good fortune, been thrown among medical men, where I found in physiology a virgin soil of great fertility; while, on the other hand, I was led by the consideration of the vital processes to questions and points of view which are usually foreign to pure mathematicians and physicists."[104] His good fortune was seen in his first work on physics, which Magnus described as a "rare example of versatile knowledge."[105] It contained the principle of the conservation of force, which he came to through his concern with the convertibility of forces in his ongoing work on physiological heat and muscular metabolism.

While a student at the gymnasium in Potsdam where his father taught, Helmholtz decided that he wanted to study physics. Because his father could afford this plan only if he studied physics within a medical education, he entered the Friedrich-Wilhelms-Institut in Berlin, a state medical-surgical institution which provided army physicians with a free medical education at the University of Berlin. He wrote his dissertation under Johannes Müller at the university on the physiology of nerves, receiving his M.D. in 1842, and the next year he took up duty as army surgeon at nearby Potsdam.[106] Drawn to the circle of Müller's students, he

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[103] Kirchhoff to his parents, n.d., quoted in Warburg, "Kirchhoff," 207.

[104] Hermann von Helmholtz, "Autobiographical Sketch," in *Popular Lectures on Scientific Subjects*, trans. E. Atkinson, 2nd ed., 1st ser. (London, 1881), 266–91, on 280.

[105] Magnus to Du Bois-Reymond, 2 August 1847, quoted in Königsberger, *Helmholtz*, vol. 1, 71.

[106] Königsberger, *Helmholtz*, vol. 1, 55.

befriended Emil du Bois-Reymond and Ernst Brücke, who were eager to see how far physics and chemistry could go in explaining life processes. Helmholtz together with the other young physiologists made contact with physicists in Berlin, above all with Magnus,[107] who allowed him to work regularly in his private laboratory during the winter of 1845–1846. Du Bois-Reymond, who had participated in Magnus's physical colloquium, introduced him to the newly formed Berlin Physical Society, which would be the first audience for his memoir on the conservation of force.[108]

The subject of the memoir required a sound knowledge of mathematical physics, which Helmholtz had acquired in his early years in Berlin by studying writings by Laplace, Biot, Poisson, Jacobi, and others. He recognized that the question of whether living beings are to be understood by the action of a life force or by the action of the same forces that govern lifeless nature is closely connected with a conservation principle for forces. He also recognized that to establish a mathematically formulated conservation principle to the satisfaction of the scientists, he needed to investigate parts of physiology and physics, a task he undertook. In the summer of 1847 he read his memoir to the Berlin Physical Society, where it aroused enthusiasm among the young members. He immediately sent it to Magnus, asking him to forward it to Poggendorff for publication in the *Annalen der Physik*. Poggendorff appreciated the importance of the problem and Helmholtz's handling of it, but he rejected the work as too long to be fitted into the journal that year. Poggendorff's main reason for rejecting it had to do with its nature: "the *Annalen* is necessarily dependent above all on experimental investigations," he explained, and he would have to sacrifice some of these if he wished to "open the door to theoretical" investigations like Helmholtz's. Poggendorff also rejected papers on the same subject by Robert Mayer and Friedrich Mohr. He recommended that Helmholtz publish the work privately, as he did in 1847.[109]

The antecedents of Helmholtz's work go far back. In the eighteenth century, there was a limited form of conservation of mechanical energy, which did not include heat as a form of energy. The understanding that heat is the invisible motion of particles of bodies began to be accepted in the 1820s, though it was not until the 1840s that the convertibility of mechanical energy and heat was generally recognized, by which time there were a number of other well-confirmed examples of force conversions. Convertibility is not the same as conservation, and for the conservation law of mechanics to be generalized, mechanics had to be recognized as the foundation of natural science. The concept of energy as a property of a physical system emerged together with the law of conservation of energy; it was the latter that made energy an indispensable concept for the development of mechanics, thermodynamics, and electrodynamics in the second half of the nineteenth century.[110]

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[107] Ibid., vol. 1, 44, 50.

[108] Ibid., vol. 1, 58, 62, 64.

[109] Helmholtz to Du Bois-Reymond, February 1847; Poggendorff to Magnus, 1 August 1847; Magnus to Du Bois-Reymond, 2 August 1847; Helmholtz to G. A. Reimer, 14 August 1847; quoted in Königsberger, *Helmholtz*, vol. 1, 68–72, 78–79.

[110] Robert D. Purrington, *Physics in the Nineteenth Century* (New Brunswick, NJ, and London: Rutgers University Press, 1997), 103–4, 110.

We will see how Helmholtz arrived at the conservation law. From his study of the older treatises, he knew about the strong proof of the impossibility of perpetual motion in mechanics, and in his physiological studies he questioned the possibility of perpetual motion outside of mechanics. His solution to the problem of determining precisely which relations must obtain between natural forces to rule out perpetual motion in general was the principle of conservation of force. He based the principle on either of two maxims, which he proved equivalent. One is that from any combination of bodies, it is impossible continuously to produce moving force from nothing. The other is that all actions can be reduced to attractive and repulsive forces that depend solely on the distance between material points. Helmholtz thought that the problem of science is the reduction of all phenomena to unchanging causes, which he identified with the unchanging forces between material points, and that the "solvability of this problem" is the "condition of the complete comprehensibility of nature." The problem of "theoretical natural science" will be solved once the "reduction of natural phenomena to simple forces is completed and at the same time is proven to be the only possible reduction the phenomena allow."[111] The grand pronouncement is distinctively Helmholtz's. Works by the other authors we discuss in this chapter are more matter of fact, closer to what we are used to in scientific publications.

The impossibility of unlimited moving force had been taken as a maxim by the French authors Sadi Carnot and B. P. E. Clapyron in their theoretical studies of heat, and Helmholtz set out to extend it throughout "all branches of physics." It is equivalent in mechanics to the principle of conservation of "living force" (or "vis viva" or "kinetic energy"). Helmholtz argued that this principle requires "central" forces; that is, forces that depend on the distances between material points and act along the lines joining them. He showed that the increase in the kinetic energy of a material point due to the action of a central force is at the cost of the sum of the "tension forces" (potential energy) due to the change in the position of the point. Mathematically, the statement reads:

$$\tfrac{1}{2}mQ^2 - \tfrac{1}{2}mq^2 = -\int_r^R \varphi dr,$$

where m is the mass of the point, q and Q are the velocities of the point at the initial and final positions r and R, and φ is the intensity of the central force. Generalizing the statement to apply to any number of interacting points, Helmholtz concluded that the sum of the kinetic energies and the potential energies of the collection of points is constant. This is what he called the "principle of the conservation of force."[112] In stating the principle he used "force" in a different sense than he did in speaking of attraction or repulsion as a "force"; for clarity, from here on in our

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[111] Hermann Helmholtz, *Ueber die Erhaltung der Kraft, eine physikalische Abhandlung* (Berlin, 1847); repr. in *Wissenschaftliche Abhandlungen,* 3 vols. (1882–1895), vol. 1, 12–68, on 16–17.

[112] Helmholtz, "Erhaltung der Kraft," 17–25.

discussion we substitute the term “energy” for the former “force,” justified below. The implication of Helmholtz’s principle is that the many newly discovered relations between the forces of nature do not require a major change in the idea of forces, the model of which was Newton’s gravitational force. In addition to deriving the conservation of energy, his object was to reduce natural phenomena to central forces.

Helmholtz applied the principle to several mechanical theorems and then to other parts of physics, which provided the interesting cases: heat, electricity, magnetism, and electrodynamics, subjects offering manifold instances of force conversions. As a supporter of the mechanical theory of heat, he accounted for the apparent loss of kinetic energy of two bodies undergoing an inelastic collision by the conversion of part of their kinetic energy into potential energy and heat. He deduced the electromotive force of two metals in a cell by equating the heat developed chemically in the cell to the heat developed electrically in the wire, as required by the conservation principle. In this example, in which heat serves as a measure of the forces, he brought together most of the known quantitative laws of electric current: Ohm’s law, Lenz’s law for the heat developed in a length of wire, James Prescott Joule’s more general law for the heat developed in any circuit, the laws of complex circuits that Kirchhoff recently worked out, and Faraday’s law of electrolysis.[113] In another application of the principle, he connected the chemical, thermal, and mechanical processes entering the interaction of a fixed, closed current produced by a cell and a nearby magnet free to move in space. Here he made use of Neumann’s potential for a closed current and with its help he derived a number of Neumann’s cases of induced currents and several new results, demonstrating the power of the conservation principle to connect the parts of physics.[114] In these examples, Helmholtz did not need a detailed knowledge of the mathematical form of the acting central forces. With regard to Weber’s fundamental law of electric action, which relates the force between electric masses to their motion as well as their position, Helmholtz observed that no hypothesis had yet been established that could reduce inductive phenomena to “constant central forces.”[115] Throughout his memoir on the conservation of energy, Helmholtz referred to experimental work on the establishment of laws by Riess, Poggendorff, Weber, and others, and he pointed

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[113] Helmholtz, “Erhaltung der Kraft,” 35, 49–57. We acknowledge discussions with Stephen M. Winters on Helmholtz’s work.

[114] Helmholtz wrote the law of conservation of force for a closed current that moves a magnet as $aAJdt = aJ^2Wdt + JdV/dt \cdot dt$, where a is the mechanical equivalent of a unit of heat, A the electromotive force, J the current, W the resistance, and V the potential; this states, in mechanical units, that the electrochemical action on the left, which produces the current, is spent in part in heating the wire and in part in increasing the kinetic energy of the magnet. Solving the equation, Helmholtz obtained $J = (A - 1/a \cdot dV/dt)/W$, which differs from Ohm’s law by the appearance of $1/a \cdot dV/dt$, a new electromotive force arising from the change in the separation of the current and magnet; this electromotive force causes a change in J, which is the “induced” current (ibid., 61–63). Later Helmholtz returned to, and improved, his derivation of the interaction of a closed current and a magnet by including the effects of self-induction.

[115] Helmholtz, “Erhaltung der Kraft,” 61–65.

to his predictions as waiting to be tested. He concluded his study with the observation that the "complete confirmation" of the conservation principle would be the main task of physics in the immediate future.[116]

Helmholtz's memoir did not report original experiments of his own, as Poggendorff remarked when he rejected it, and it would seem overly speculative to older scientists, who initially were unconvinced by the conservation principle.[117] If the law of conservation of force had the significance for nature philosophy that Helmholtz's biographer Königsberger suggests, the negative reaction of the older physicists is entirely understandable from the fact alone.[118] The task of persuading physicists was not entirely Helmholtz's in any case, since his statement of the principle was only one of several on the measure of the relations between the forces of nature. With marked differences of approach and purpose, a number of natural scientists in the 1840s worked on problems arising from a widely shared belief in the unity of nature and the indestructibility and transformability of forces.[119] In the same year the law of conservation of energy was stated by Joule, whom Helmholtz considered to be the true discoverer. From the standpoint of physics, the priority of discovery was immaterial; Joule arrived at the law from the side of experiment, Helmholtz from the side of mathematical physics, and both approaches were important in gaining acceptance of the law. Helmholtz's memoir came to be regarded as the mathematical foundation of the law. Within a few years, he acknowledged that his terms "living force" and "tension force" were synonymous with W. J. M. Rankine's "actual [kinetic] energy" and "potential energy" and that Rankine's term "conservation of energy" was preferable to his own "conservation of force."[120]

In early 1847, Helmholtz wrote to Du Bois-Reymond that in his latest reworking of his essay on the conservation of force, he had "thrown overboard everything that smells of philosophy." Later he recognized that it was not strictly true. When he included the memoir in his collected papers, he acknowledged that he had been indebted to Kant's philosophy for his idea that the law of causality is essential for understanding nature and that central forces are ultimate causes. He had derived the conservation principle within a philosophical view of nature governed by

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[116] Ibid., 68.

[117] Königsberger, *Helmholtz*, vol. 1, 79–80.

[118] Ibid., vol. 1, 85.

[119] On the scientific, technical, and philosophical issues that scientists who contributed to the establishment of the conservation law responded to: P. M. Heimann, "Helmholtz and Kant: The Metaphysical Foundations of Über die Erhaltung der Kraft," *Stud. Hist. Phil. Sci.* 5 (1974): 205–38; "Mayer's Concept of 'Force': The 'Axis' of a New Science of Physics," *HSPS* 7 (1976): 277–96; Peter M. Harman, *Metaphysics and Natural Philosophy: The Problem of Substance in Classical Physics* (Brighton: Harvester Press, 1982), 105–26; Thomas S. Kuhn, "Energy Conservation as an Example of Simultaneous Discovery," in *Critical Problems in the History of Science*, ed. M. Clagett (Madison: University of Wisconsin Press, 1959), 321–56; Yehuda Elkana, "Helmholtz 'Kraft': An Illustration of Concepts in Flux," *HSPS* 2 (1970): 263–98.

[120] Heimann, "Helmholtz," 206. Purrington, *Physics in the Nineteenth Century*, 110.

mechanical concepts and laws, to which he gave a complete definition in 1847. He developed its implications in later work on thermodynamics and other parts of physics.[121]

When Helmholtz became physics professor at Berlin, he said that theoretical physics had always been his first love, and the first demonstration of it was his memoir on the conservation of energy, which must be counted as one of the most impressive first publications in the history of physics. He himself had the highest regard for the principle he announced there, later calling it the most important scientific advance of the century because it encompasses all laws of physics and chemistry.[122] In an address at the German Association meeting in 1869, he said that the significance of the law lies in the "grand connection which it establishes between the entire processes of the universe, through all distances of place and time."[123]

The first critical examination of Helmholtz's reasoning behind the principle of conservation of energy was given by Rudolph Clausius in 1853. The truth of the principle, Clausius said, does not depend on central forces, at least not mathematically. Helmholtz replied that he was speaking from a physical not a mathematical standpoint and went on to reaffirm the starting point of his original work. In notes to the reprinting of his 1847 memoir, Helmholtz said that Clausius had shown that forces dependent on velocity satisfy the energy principle if the mechanical principle of action and reaction is given up. Helmholtz noted that recent electrodynamic researches violated one or the other of the principles, but that such results should be accepted only if "all other theoretical possibilities have been exhausted," for they abandoned the prospect of the "complete solution of scientific problems."[124]

Clausius raised a number of other objections, mostly having to do with definitions of concepts, but his main response to Helmholtz's memoir of 1847 was admiration: despite its inexactness here and there, in his view it had, "through the many beautiful ideas it contains, great scientific value."[125] For several years in his own work, Clausius had made important use of one of the main supports of Helmholtz's principle, the convertibility of work and heat together with its precise measure. He had, that is, begun to develop the "mechanical theory of heat," which was to remain his central interest and the subject of his lasting accomplishment.

The son of a school official who was also a clergyman, Clausius studied at the gymnasium in Stettin in Prussia. From there he entered Berlin University in 1840,

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[121] Heimann, "Helmholtz," 208–9, 234–38. Helmholtz, "Zusätze (1881)," in *Wiss. Abh.*, vol. 1, 68–75, on 68.

[122] Helmholtz's talk, "On the Application of the Law of the Conservation of Force to Organic Nature," on 12 April 1861, discussed in Königsberger, *Helmholtz*, vol. 1, 373.

[123] Hermann von Helmholtz, "The Aim and Progress of Physical Science," in *Popular Lectures on Scientific Subjects*, trans. E. Atkinson (New York, 1873), 363–97, on 379–80.

[124] Heimann, "Helmholtz," 234, 236–37.

[125] Rudolph Clausius, "Ueber einige Stellen der Schrift von Helmholtz 'Über die Erhaltung der Kraft,'" *Ann.* 89 (1853): 568–79, on 578–79. Clausius and Helmholtz's exchange, which extended into the following year, is discussed in Heimann, "Helmholtz," 234–35.

where he was drawn to Leopold von Ranke's lectures and considered making history his field. More important for his future work, he also heard lectures by Magnus, Dirichlet, and others, in all spending three-and-a-half years studying mathematics and the natural sciences. He concluded his study with the examination qualifying him to teach at Prussian gymnasiums. For the next 6 years, he taught at a gymnasium in Berlin and at the same time he pursued his scientific interests through his connection with the university physicists. In 1846 he became a member of the "Seminar für gelehrte Schulen" at Berlin University, which had the purpose of giving younger secondary school teachers an opportunity to continue their scientific education through independent scientific work. In 1850, he was appointed physics teacher at the Berlin Artillery and Engineering School, and that same year he became Privatdocent at Berlin University, where he taught physics from a mathematical-physical standpoint. Both positions together took up 12 h a week and paid him a small salary of 550 thaler, allowing him time to prepare a series of impressive publications in theoretical physics.[126] In his researches, he kept in close touch with the Berlin experimental physicists Magnus, Dove, and Riess, making use of their work and consulting with them.[127]

In his first publication, in 1847, the same year as Helmholtz's, Clausius spoke with authority on the work of theoretical physics. He said that too little was known about daylight, his current subject, because the methods of measuring the intensity of light were incomplete, and the resulting lack of reliable measurements had led to a lessening of interest in theoretical investigations. This was unfortunate, he explained, for by making hypotheses to bridge gaps in our knowledge, we can construct general formulas, and by comparing their consequences with "reality," we can confirm or disprove the hypotheses. He described his first publication as an "attempt to determine more precisely the light-dispersing and luminous effects of the atmosphere through theoretical consideration."[128]

The year following his dissertation on light and the atmosphere, he published a study of the equations of motion of an elastic body, which he introduced by referring to a recent publication by the French experimentalist Regnault and to the "very solid mathematical investigations" by other French authors, "Navier, Poisson, Cauchy, Lamé, and Clapyron." Their theories essentially agreed with one another, and yet their results disagreed with certain facts. To discover the

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[126] Clausius's vita submitted to President of the Swiss Education Council on 17 June 1855; Georg Sidler to President, 31 March 1855, A Schweiz. Sch., Zurich. Hermann von Helmholtz, "Zur Erinnerung an Rudolf Clausius," *Verh. phys. Ges.* 8 (1889): 1–7, on 1–2.

[127] For example, in a study of the mechanical measure of electric charge, Clausius made use of Dove's and Riess's results, and he asked Riess to make certain measurements expressly for this paper. Rudolph Clausius, "Ueber das mechanische Aequivalent einer elektrischen Entladung und die dabei stattfindende Erwärmung des Leitungsdrahtes," *Ann.* 86 (1852): 337–75.

[128] Rudolph Clausius, "Ueber die Lichtzerstreuung in der Atmosphäre und über die Intensität des durch die Atmosphäre reflectirten Sonnenlichtes," *Ann.* 72 (1847): 294–314, on 294–95. This paper is a short version of two papers appearing in the *Journ f. d. reine u. angewandte Math.* 34 (1847): 122–47 and 36 (1848): 185–215.

reason for this, Clausius examined their starting assumptions, which concerned molecular actions. He concluded his critical study by urging physicists to multiply their experimental efforts to create "secure foundations for an extended theory" of elasticity, which was not a "closed" subject.[129]

In the *Annalen der Physik* in 1850, Clausius published a paper on the "moving force of heat,"[130] which belongs to researches affirming connections between the parts of physics, here heat and mechanics. To build on the latter connection, which had recently been established experimentally, he had to resolve an apparent conflict arising from two stages in the development of heat theory. In the first stage, heat was thought to be a fluid substance, or caloric, the viewpoint Carnot applied in 1824 in his study of the motive power of heat. In explaining the work done by a steam engine through the transfer of a certain quantity of caloric from a hot to a cold body, Carnot assumed that the quantity of caloric is conserved in the process: the caloric moves from the hot to the cold body in analogy with the fall of water from a height to a lower elevation in the operation of a waterwheel, and if the engine is run in reverse, the same work transfers the caloric from the cold to the hot body, completing a full cycle. Carnot derived a theorem that states that the efficiency of an engine, the maximum work that a quantity of heat can produce, depends only on the two temperatures between which it operates and not on the nature of the working substance, steam or an alternative. Taking up the theory 10 years later, Clapyron developed the mathematics of heat theory, including a graphical representation of the Carnot cycle. In 1850 Clausius worked directly with Clapyron's results.

In the second, more recent stage in the development of the theory, heat in bodies was identified with a kind of motion rather than a conserved caloric fluid. In 1849 William Thomson called attention to an apparent conflict between Carnot's principle and Joule's recent experimental findings on the generation of heat by currents and fluid friction. These findings suggested that heat is not conserved in the production or consumption of work, bringing into question Carnot's assumption of caloric and supporting Joule's view that heat is the mechanical vibration of the particles of bodies. Other facts, however, suggested that Carnot's principle is correct. As a way out of this impasse, Thomson looked to new experiments either to confirm Carnot's principle or to lay the foundation for a new theory of heat. Clausius's response was to say that what was needed was not new experiments but a new analysis.[131]

The theory of heat that Clausius developed is an example of a principle theory. In Part 1 of his 1850 paper, he introduced the first principle, the equivalence of heat and work: whenever heat produces work, a proportional quantity of heat is

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[129] Rudolph Clausius, "Ueber die Veränderungen, welche in den bisher gebräuchlichen Formeln für das Gleichgewicht und die Bewegung elastischer fester Körper durch neuere Beobachtungen notwendig geworden sind," *Ann* 76 (1849): 46–67, on 46, 51, and 66.

[130] Rudolph Clausius, "Ueber die bewegende Kraft der Wärme und die Gesetze, welche sich daraus für die Wärmelehre selbst ableiten lassen," *Ann.* 79 (1850): 368–97, 500–524.

[131] Martin J. Klein, "Gibbs on Clausius," *HSPS* 1 (1969): 127–49, on 130–31.

consumed, and conversely.[132] To make the equivalence intelligible, he assumed that heat is the measure of the "living force" (kinetic energy) of the moving particles of bodies, without assuming any particular form for the motion. By combining this principle with the known gas laws, he analyzed the relations between heat and work in the expansion and compression of a gas in a reversible engine, a Carnot cycle. From this analysis, he arrived at certain consequences, such as the constant difference between the specific heat of a gas at constant pressure and the specific heat at constant volume.

In Part 2 of the paper, Clausius enlarged the foundations of the theory of heat to include Carnot's principle alongside the first principle. He retained the part of Carnot's principle that says that a transfer of heat occurs when work is done, only modifying it to allow some of the heat to be consumed during the transfer. His proof of the principle was a variation on Carnot's reductio ad absurdum: he showed that Carnot's conclusion about the maximum work that a heat engine can produce remains valid by showing that its denial implies the transfer of heat from a cold to a hot body without a net expenditure of work, contradicting the universal experience of the unassisted passage of heat from hot to cold bodies. It seems, Clausius concluded, "*theoretically* justified" to accept the essential part of Carnot's theorem, which is that the maximum work depends only on the quantity of heat transferred and on the two temperatures.[133]

Clausius understood that the resolution of the outstanding problem of the theory of heat was not to pick the correct fundamental law, Carnot's or otherwise, but to recognize that there are two laws, independent of one another and equally fundamental.[134] When in 1876 Helmholtz together with Kirchhoff and Werner von Siemens proposed Clausius as a corresponding member of the Prussian Academy of Sciences, they singled out his paper of 1850: "Over twenty-five years ago, he [Clausius] made a discovery of the greatest importance in theoretical physics, in that he found the law that has received the name of the second fundamental law of the mechanical theory of heat."[135] Upon Clausius's death 12 years later, Helmholtz told the Berlin Physical Society that in its "high significance, general validity, and fruitfulness," the second law was given its first rigorous formulation and development by Clausius. This law, Helmholtz continued, "is not only one of the most important but also one of the most surprising and original accomplishments of the old and new physics: important because, so far as we now know, this law is one of the few [laws] that can claim an absolute general validity independent of all the

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[132] He wrote the principle, the first law of thermodynamics, as the familiar differential equation: $dQ = dU + pdV$, where Q is heat, U is internal energy, a function of volume and temperature, p is pressure, and V is volume.

[133] Clausius, "Ueber die bewegende Kraft," 503.

[134] Klein, "Gibbs on Clausius," 131.

[135] Letter by Helmholtz, Werner von Siemens, and Kirchhoff, 14 February 1876, proposing Clausius as corresponding member of the Prussian Academy of Sciences, Document 9, in *Physiker über Physiker*, ed. Christa Kirsten and Hans-Günther Körber (Berlin: Akademie-Verlag, 1975), 87.

diversity of natural bodies and because it reveals the most surprising connections between the most distant branches of physics."[136] Universal, encompassing, and interconnecting, the second law of the mechanical theory of heat was a triumph of theoretical physics.

After his initial paper, Clausius investigated a variety of heat phenomena from the standpoint of his theory, paying close attention to announcements of new experiments bearing on it. In one paper, he applied the theory to electrical discharge and thermal electricity, revealing by a "rigorous mathematical treatment" their "inner connectedness."[137] In another paper, he applied the theory to Leiden jars, the "main instrument for machine electricity"; in his analysis, he made use of simplifying mathematical assumptions, but in the discussion he returned to the actual, more complex apparatus required to make the measurement.[138]

In 1854, in a sequel to his original paper of 1850, Clausius reformulated his modified Carnot's principle, illustrating the task of the theoretical physicist of finding clearer and more useful forms for a new law. He again placed the second law of the mechanical theory of heat alongside the first as an equally fundamental law of experience, only rewording it: heat cannot pass from a colder to a warmer body without some related change occurring at the same time. With the help of this principle, he derived a mathematical theorem for what he called the "equivalence value" of a transformation between heat and work. In a reversible cycle, there are two kinds of transformations: the production of work by a transfer of heat from a hot to a cold body, and the expenditure of work in the transfer of heat from a cold to a hot body. The "equivalence value" of these two transformations is a quantity that is conserved, replacing heat, which is not conserved: the ratio of heat Q to absolute temperature T. Using a Carnot cycle, Clausius showed that whereas dQ is not a complete differential, the quantity dQ/T is, so that for a reversible cycle the "analytical expression" of the second fundamental principle of the mechanical theory of heat is:

$$\int \frac{dQ}{T} = 0.$$

He also considered the irreversible case of an imperfect engine, deriving the inequality, $\int \frac{dQ}{T} > 0$. By speaking of the second law as the law of equivalence of transformations, Clausius made formal its analogy with the first law, the equivalence of heat and work. In 1865 he replaced the term equivalence value with "entropy," from the Greek word for transformation, and he stressed its directional

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[136] Helmholtz, "Clausius," 3.

[137] Clausius, "Ueber das mechanische Aequivalent einer elektrischen Entladung und die dabei stattfindende Erwärmung des Leitungsdrahtes," 337–75, on 337.

[138] Rudolph Clausius, "Ueber die Anordnung der Elektricität auf einer einzelnen sehr dünnen Platte und auf den beiden Belegungen einer Franklin'schen Tafel," *Ann.* 86 (1852): 161–205, on, for example, 173, 198.

nature. He stated the two fundamental laws of the mechanical theory of heat in their most general form: the “energy of the universe is constant,” and the “entropy of the universe tends to a maximum.”[139] (To say that dQ/T is a “complete differential” means that it is the derivative of a function; as a consequence, when it is integrated, its value depends only on the initial and final states and is independent of the path it takes from one to the other; dQ alone is not a complete differential, requiring the factor $1/T$ to make it one. Entropy is a “state” function, which describes the equilibrium state of a thermodynamic system without regard to the way the system arrived at that state.)

Independently of Clausius, Thomson also introduced a second fundamental law of the mechanical theory of heat, and other researchers such as K. H. A. Holtzmann at the technical school in Stuttgart were groping toward one. But Clausius was first to create a distinct science of the mechanical theory of heat or, as it came to be called, thermodynamics; that was the considered judgment of one of the foremost experts in the subject, the American physicist Josiah Willard Gibbs, a champion of Clausius’s direction. Gibbs elaborated: “If we say, in the words used by Maxwell some years ago, that thermodynamics is ‘a science with secure foundations, clear definitions, and distinct boundaries’ and ask when those foundations were laid, those definitions fixed, and those boundaries traced, there can be but one answer. Certainly not before the publication of that [1850] memoir.”[140]

Helmholtz, who saw Clausius on a daily basis for a time in the 1840s, said that he and others were impressed by Clausius’s thinking and by his determination. From the beginning, his interest was directed to “mathematical physics,” a circumstance which Helmholtz related to Magnus’s belief in the separation of the work of mathematical and experimental physicists.[141] Of all the physicists we discuss in this study, Clausius is the first about whom we can say that “all of his accomplishments lay in the area of theoretical physics.” He never made experimental physics the subject of his researches, though he lectured regularly on experimental physics, and he directed an experimental physics institute. His theoretical studies made direct contact with experience, the important point.[142] Anyone wanting to learn how to practice theoretical physics profited from reading Clausius’s papers, which

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[139] Rudolph Clausius, “Ueber eine veränderte Form des zweiten Hauptsatzes der mechanischen Wärmetheorie,” *Ann.* 93 (1854): 481–506; “On Several Convenient Forms of the Fundamental Equations of the Mechanical Theory of Heat,” 1865, in Clausius, *The Mechanical Theory of Heat*, trans. T. A. Hirst (London, 1867), 327–65, on 365; Eduard Riecke, “Rudolf Clausius,” *Abh. Ges. Wiss. Göttingen* 35 (1888): appendix, 1–39.

[140] Josiah Willard Gibbs, “Rudolf Julius Emanuel Clausius,” *Proc. Am. Acad.* 16 (1889): 458–65; quoted in Klein, “Gibbs on Clausius,” 129–30, where Clausius’s work is discussed in light of Gibbs’s observation.

[141] Helmholtz, “Clausius,” 2.

[142] Walther Nernst, “Rudolf Clausius 1822–1888,” in *150 Jahre Rheinische Friedrich-Wilhelms-Universität zu Bonn 1818–1968. Bonner Gelehrte. Beiträge zur Geschichte der Wissenschaften in Bonn. Mathematik und Naturwissenschaften* (Bonn: H. Bouvier, Ludwig Röhrscheid, 1970), 101–9, on 101.

were an even better model than Boltzmann's, according to a former student of Boltzmann's, Walther Nernst.[143]

We have considered five physicists who carried out major theoretical researches in Germany in the 1840s. In the examples of the work we discuss in this chapter, they exhibited a variety of methods. Weber and Helmholtz reasoned from material points and attractive and repulsive forces, a method which was used extensively in France, with marked success, and which would continue to be one of the main methods of theoretical physics. Neumann reasoned directly from the phenomena, as expressed in mathematical empirical laws; his work would be identified with phenomenology, a widely practiced method in theoretical physics. Kirchhoff used atomistic and phenomenological methods, with a preference for the latter. Clausius's method in establishing a principle combined criticism, clarification, and reasoning from empirically founded laws. All five made use of mathematics for the precise expression of laws of nature, and all were concerned with the complementary work of experimental physics, though not all of them did work in the laboratory. Helmholtz intended his memoir expressly for "physicists," and so were the works by the others.

The theories of the five physicists referred to large bodies of phenomena. Neumann derived a law covering all inductive phenomena and a potential law covering all electrodynamic phenomena. Weber derived a law governing in principle all electrical action. Kirchhoff generalized the law of electric currents to complex circuits and two- and three-dimensional conductors. Clausius worked with laws applicable to all of the phenomena of heat. Helmholtz derived a law applicable to all physical phenomena. On the occasion of Helmholtz's hundredth birthday in 1921, Wilhelm Wien wrote that the significance of Helmholtz's law was still growing,[144] and the same could be said of Clausius's laws. The several works of the 1840s laid the theoretical foundations of several branches of physics, and they made early major contributions to the interconnection of the parts and of the whole of physics, a goal of theoretical physics. They insured that no matter what followed, Germany would hold a notable place in the history of theoretical physics.

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[143] Nernst, "Clausius," 102.

[144] Wilhelm Wien, "Helmholtz als Physiker," *Naturwiss.* 9 (1921): 694–99, on 694.

Chapter 8
Mathematicians and Physicists

Physicists knew that to do serious theoretical work they needed a sound mathematical preparation. We saw it in Ohm's work at the beginning of our period, and after him the need only grew stronger. Fritz Haber wrote to the theoretical physicist Arnold Sommerfeld of his admiration for the "playful ease with which you master the mathematical apparatus." Applying to Sommerfeld to come to Munich to work with him, Paul Ehrenfest explained that he could not carry through calculations to the end, and unless he learned how, he would be "ruined" as a theorist. "Herr Gott," Ferdinand Braun exclaimed to the theoretical physicist Leo Graetz, "if only I had your mathematics and theory."[1] In their dependence on mathematics, physicists were aided by mathematicians, who contributed to their theoretical work in various ways.

8.1 Gauss

The theoretical physics professor at Göttingen Woldemar Voigt thought that the first decisive impulse in physics in Germany came not from physicists but from Gauss, who gave rise to a particular direction in theoretical physics. This was to develop areas of physics that had been investigated by others rather than to explore new areas of theory, treating the subject like a branch of mathematics.[2] Gauss said as much. He explained to the curator at Gőttingen that in Earth magnetism the "separation between actual so-called physics and applied mathematics here, too (as in the theory of motion and optics long ago), begins to disappear, and the more

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[1] Haber to Sommerfeld, 29 December 1911; Ehrenfest to Sommerfeld, 17/30 September 1911, Sommerfeld Correspondence, Ms. Coll., DM. Braun to Graetz, 10 April 1887, Ms. Coll., DM.

[2] Woldemar Voigt, "Zur Erinnerung an F. E. Neumann, gestorben am 23. Mai 1895 zu Königsberg i/Pr.," *Gött. Nachr.* (1895): 248–65, on 250.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_8

thorough treatment begins to fall to the mathematician."[3] Yet what Voigt said is incomplete. Let us review what we know about Gauss. He saw an increasing role for mathematicians in the development of physical theory, and in his work on Earth magnetism he demonstrated it. For this area, he developed a mathematical theory, and in connection with it he invented an instrument of corresponding precision together with methods of observation. With his model of investigation a proven success, he looked to advance other parts of physics through a combination of mathematical theory and measurements made with mathematical precision. Recent "brilliant discoveries" have opened up a "new world of scientific research," he said, and he intended to take full part in it.[4] On learning of Faraday's discovery of electromagnetism, he promptly subjected it to experiment.

In parallel to his experimental response to the new discoveries, Gauss sought a new fundamental law that combines both static and dynamic actions between electrical masses. He anticipated that electrical actions are not instantaneous but finite, analogous to light, but because he could not form a clear physical idea of how the propagation takes place, he was unsuccessful in deriving the fundamental law. With regard to Voigt's characterization, we note here that Gauss's search was theoretical and exploratory, preceding Neumann's theory of electromagnetic induction and Weber's fundamental law of electric action, with which it had similar objectives.

Gauss's work on a fundamental law is indicative of his attraction to problems that furthered the connectedness of physics. His introduction of an absolute system of units in magnetism and galvanism had a similar goal. So had his plan to reduce all Earth magnetic observations to a single principle analogous to the reduction of all astronomical motions to Newton's gravitational principle. We add to these examples his work on the potential, a concept he valued for its power to formally connect the branches of physics.

Voigt's observation about Gauss is insightful, but it needs to be qualified. Rather than making theoretical physics a branch of mathematics, the direction of theoretical physics to which Gauss gave most impetus is better characterized as seeking exactitude in the comparison of physical laws with experiment. In their time, Neumann and Weber would pursue this direction, and it would be continued by their students. When Kirchhoff was considered for a university professorship, he was recomended as "one of the most talented of the younger physicists of the exact Gaussian school."[5] In this and other ways, the mathematician Gauss found common ground with the physicists.

Gauss's statement above to the Gőttingen curator about mathematicians and physics is from a letter in 1833, a time when the first German physicists who were knowledgeable in advanced mathematics, Ohm, Weber, and Neumann, were starting out. By and large, they and their successors proved capable of providing

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[3] Gauss to Göttingen U. Curator, 29 January 1833.

[4] Carl Friedrich Gauss, "Erdmagnetismus und Magnetometer," *Schumacher's Jahrbuch für 1836*, 1–47, reprinted in *Werke*, vol. 5, 315–44, on 336.

[5] Bunsen to Liebig, 4 November 1854, Liebigiana, Bay. STB, 58 (Bunsen, Robert), Nr. 15.

a satisfactory mathematical treatment of the well-developed areas of physics, making mathematicians in Gauss's sense less needed, though not unwelcome. Physicists would sometimes ask mathematicians for help, but they worked as physicists who were already fairly knowledgeable in mathematics. Although they did not do original research in pure mathematics, they were capable of adapting new mathematics to physical uses.

At Gőttingen, Gauss developed and applied mathematical methods to physical problems, and during his later years and after his death, other mathematicians took an active interest in the work of physicists. In what follows, we look at two mathematicians from the middle years of the century, both of whom ended their careers in Gauss's chair at Göttingen, Gustav Lejeune Dirichlet and Bernhard Riemann.

In deliberations over a replacement for Gauss at Göttingen, the need for mathematics in physics and related fields was carefully considered. At the time of his death, in 1855, Gauss was professor of two fields, higher mathematics and astronomy, and so the question naturally arose whether his successor should be an astronomer or a mathematician. Since specialization was an accepted fact of scientific work by then, nobody expected to find another person equally qualified in both fields. It was necessary to have someone to run the astronomical observatory, but it was an open question whether they should look for an astronomer eminent enough to command Gauss's large salary or for a mathematician instead. The question was put to Weber, to whom the answer was clear; in giving it, he expanded on the value of higher mathematics for the physical sciences, an appreciation he had acquired over his nearly 30 years of close association with the finest German mathematicians. Higher mathematics was needed more than astronomy for university instruction, Weber said. It was "of the greatest importance and indispensable not only for the education of actual mathematicians but also for the education of astronomers and physicists, as indeed in higher education in all exact sciences and their applications." Further, an appointment in higher mathematics would enhance the Göttingen Society of Sciences, guaranteeing its "brilliant" position in the scientific world, which a good astronomer could not do, even if it would be a pleasure to have one in the Society. Weber explained that in the class of the Society concerned with higher mathematics and the "theoretical" natural sciences, natural phenomena are presented in their "causal connections, linked together by mathematical laws." These "sciences are dominated by higher mathematics, which is the Queen of the sciences, as Gauss called it, because in its abstract foundation and rigorous philosophical deduction it is completely independent, and because the ends of the thread running through all researches in the other sciences come together in it." Higher mathematics was more useful than astronomy for establishing working relationships between related sciences, Weber continued: for one science to be important to another, the close relationship of the two is not decisive in itself, since they must also differ in ways that allow them to complement one another. So by this argument, if a physicist and an astronomer were to undertake joint research, neither would bring anything new to the work of the other. "The mathematician, on the other hand, can display all the riches of his science and the results of his own researches in a suitably chosen astronomical or physical investigation which he undertakes with an astronomer or with a physicist, and through it yet he in turn gains incentive and stimulation to explore new areas of mathematical problems." Weber's final argument for the need for an appointment in higher mathematics rested on the reputation of Göttingen. Gauss, whose only equals, in Weber's opinion, were Archimedes and Newton, had given Göttingen the "dominating position in the world," and to maintain that position it was necessary for Gauss's chair to go to a "creative genius of higher mathematics."[6] Weber's argument prevailed.

8.2 Dirichlet as Gauss's Successor

Like German physicists, German mathematicians went to Paris to learn from the masters. Beginning in 1822, at age seventeen, Dirichlet spent 5 years in Paris studying mathematics. There he was befriended by Fourier, who awakened his interest in mathematical physics, a field he was to pursue with distinction.[7] Upon his return to Germany, on Humboldt's advice, he took a degree at Bonn University, taught briefly at Breslau University, and then taught for many years at Berlin University and also at a Berlin military school. In Berlin he came into personal contact with leading German mathematicians and physicists. When Weber visited Berlin for several months in 1828 to further his studies, he spent much of his time in the company of Dirichlet and the mathematician Jacob Steiner. Weber attended Dirichlet's lectures on Fourier's heat theory, and later, particularly after Weber was no longer working with Gauss, Dirichlet helped Weber with mathematical problems of his electrical theory. Weber's visits with Poggendorff in Berlin were always also visits with Dirichlet and with the family of Dirichlet's wife the musical Mendelssohns, the "most distinguished meeting point of art and science in Berlin."[8]

The early important German physical theorists made their first appearance in the 1820s, as did the early important German mathematicians, Dirichlet, Jacobi, Gotthold Eisenstein, and Niels Henrik Abel. When Gauss died, of the four, only Dirichlet was still alive, and in Weber's judgment, he alone was worthy to succeed Gauss. Recognized as a cultivator of mathematical physics, Dirichlet wrote the entry on recent "mathematical physics" for the experimental physicists Dove and

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[6] Weber to Warnstedt, 5 April 1855, Dirichlet Personalakte, Göttingen UA, 4/V b/134.

[7] Leo Königsberger, *Carl Gustav Jacob Jacobi* (Leipzig: B. G. Teubner, 1904), 9; Hermann Minkowsi, "Peter Gustav Lejeune Dirichlet und seine Bedeutung für die heutige Mathematik," in Hermann Minkowski, *Gesammelte Abhandlungen*, ed. David Hilbert, vol. 2 (Leipzig and Berlin: B. G. Teubner, 1911), 447–61, on 449; E. E. Kummer, "Gedächtnissrede auf Gustav Peter Lejeune Dirichlet," in *G. Lejeune Dirichlet's Werke*, ed. L. Kronecker and L. Fuchs (Berlin, 1889–1897), vol. 2, 311–44, on 319.

[8] Minkowski, "Dirichlet," 449–50. Heinrich Weber, *Wilhelm Weber. Eine Lebensskizze* (Breslau, 1893), 11–15, 96–97; Königsberger, *Jacobi*, 34, 57, 100. Quotation from Weber to Warnstedt, 5 April 1855.

Moser's *Repertorium der Physik* in 1837. Here he discussed among other topics the representation of arbitrary functions by Fourier's series; Dove explained that this series had recently found so many applications in the mathematical treatment of physical problems that it was necessary to give a systematic account of its mathematical basis.[9] In keeping with Weber's recommendation, the official job offer went to Dirichlet, who was told that Göttingen "urgently" needed him for the continued advancement of higher mathematics and the "natural sciences that are based on mathematical laws." Contributing to Dirichlet's decision to accept the call was the prospect of a scientific collaboration with Weber such as Gauss had enjoyed.

Dirichlet was inspired by Gauss's studies of the potential, a subject shortly to be viewed as a developing branch of mathematics.[10] Carrying it further, he was one of the first to give special lectures on it, a practice which all German universities subsequently followed. His three active years at Gőttingen were "especially filled" with thoughts about physical questions, according to a biographer, but he was then, as he had always been, reluctant to begin writing out his results, and no more than a few hints became known of his work, which was brought to an end by his death in 1859. His contribution to mathematical physics during these years was mainly through his lectures, which treated mathematical problems of importance for physics. These were continued by Riemann, who succeeded him at Göttingen.[11]

8.3 Riemann's Lectures and Researches

Bernhard Riemann was nearly 20 years younger than Dirichlet. Unlike Dirichlet, he did not need to go to Paris. By the time he entered Göttingen University as a student in 1846, it was possible to get a first-class education in higher mathematics in Germany. At Göttingen, he attended Gauss's lectures on the method of least squares, but Gauss taught only a limited range of subjects, and his mathematical colleagues did not measure up to the best in Germany. During the next 2 years, Riemann acquired the larger part of his mathematical education in Berlin from Dirichlet, Jacobi, and Eisenstein. He then returned to Göttingen to round out his education with lectures on the natural sciences and philosophy. Weber's lecture course on experimental physics was of "greatest interest" to him, and after completing it, he became a member of the mathematical-physical seminar. In Listing's

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[9] Kummer, "Dirichlet," 325, 333; Heinrich Wilhelm Dove, "Vorwort," and G. L. Dirichlet, "Ueber die Darstellung ganz willkührlicher Funktionen durch Sinus- und Consinusreihen," in *Repetorium der Physik*, ed. Henrich Wilhelm Dove and Ludwig Moser, vol. 1 (Berlin, 1837), iii–vi, 152–74.

[10] Hans Salié, "Carl Neumann," in *Bedeutende Gelehrte in Leipzig*, ed. G. Harig, vol. 2 (Leipzig: Karl-Marx-Universität, 1965), 13–23, on 21.

[11] Minkowski, "Dirichlet," 459. Felix Klein, *Vorlesungen über die Entwickelung der Mathematik im 19. Jahrhundert*, pt. 1, ed. R. Courant and O. Neugebauer (New York: Chelsea, 1967), 99.

part of the seminar, he distinguished himself for his work on optical problems, and in Weber's part he was put in charge of preparing new members for laboratory exercises.[12] By this time, Gauss had recognized his "creative talent for finding new questions and points of view for mathematical research in the field of higher mathematical physics and for being able to build on them."[13]

Riemann began teaching as a Privatdocent in the winter semester of 1854–1855. Around this time a Göttingen colleague recommended him for a position at the new Zürich Polytechnic as a "pioneering genius" in research. The Göttingen mathematicians and physicists did not want to lose him; Gauss was dying, and they saw him as "indispensable" for the "school" of advanced mathematics that Gauss had established, which they insisted "must under no circumstances be allowed to collapse."[14] To ensure that he would stay, he was given a small salary, and in 1857 at Weber's request a promotion to extraordinary professor. He lectured on "pure and applied mathematics," which included the theories of mechanics, gravitation, elasticity, electricity, magnetism, and selected physical problems.[15] In 1859 Riemann succeeded Dirichlet in Gauss's chair.

Riemann's early researches included the introduction of complex variables in the theory of functions, Fourier series, foundations of geometry, and fundamental laws of the different parts of physics. With regard to his work on the laws of physics, he looked to experiments for confirmation. When Rudolph Kohlrausch came to Göttingen to collaborate with Weber on the determination of the constant c in Weber's electrodynamic law, they invited Riemann to participate in their experiments. Riemann took the occasion to discuss his explanation of Kohlrausch's precise measurements of the electrical residue in a Leiden jar, and Kohlrausch encouraged him to work out the theory for it. This was an important opportunity, Riemann explained to his brother, "because it was the first time that I could apply my work to a previously unknown phenomenon, and I hope that the publication of this work will help to give my larger work a favorable reception."[16] Kohlrausch's measurements made it a desirable test for Riemann's law for the motion of

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[12] Richard Dedekind, "Bernhard Riemann's Lebenslauf," in *Bernhard Riemann's gesammelte mathematische Werke und wissenschaftlicher Nachlass*, ed. H. Weber (Leipzig, 1876), 507–26. This biography and Riemann's Personalakte in the Göttingen UA, 4/V b/137, are our main sources for details about his career. "Jahresbericht 1850/51"; Ulrich to Göttingen U. Curator, 2 August 1851; Göttingen UA, 4/V h/20.

[13] Weber to Göttingen U. Curator, 10 March 1855, Riemann Personalakte, Göttingen UA, 4/V b/137.

[14] Sartorius von Waltershausen to Göttingen U. Curator, 11 February 1855, Riemann Personalakte, Göttingen UA, 4/V b/137.

[15] Göttingen U. Philosophical Faculty to Curator, 11 June 1854; Göttingen U. Curator to Riemann, 9 November 1857; Riemann Personalakte, Göttingen UA, 4/V b/137. "Verzeichnis der von Riemann angekündigten Vorlesungen," in *Bernhard Riemann's gesammelte mathematische Werke*. *Nachträge*, ed. M. Noether and W. Wirtinger (Leipzig: B. G. Teubener, 1902), 114–15. Also marginal note on Riemann's lectures, in a draft of the letter of his appointment to ordinary professor, 30 July 1859, Riemann Personalakte, Göttingen UA, 4/V b/137.

[16] Riemann to his brother Wilhelm, 26 June 1854, in Dedekind, "Riemann's Lebenslauf," 516–17.

electricity,[17] and he submitted a paper on it to the *Annalen* only to withdraw it because of a suggested change that he did not want to make. In its place he published a paper on the theory of Nobili's color rings, which also allowed for "very precise measurements" for testing the "laws according to which electricity moves." In the same year, 1854, he brought out a paper on the laws of distribution of electric tension in conducting bodies, in which he discussed Ohm's theory and Kirchhoff's and Weber's correction of Ohm's derivation. In this research, he consulted with Weber, who made suggestions.[18]

In his research in mathematical physics, Riemann developed, as Weber described it, "a larger piece of work by which he intends to establish an interconnection mainly between optics and electrical theory in a way that no one else has thought of before," which Weber anticipated would be "of very great importance." Riemann described his object as an "investigation of the connection between electricity, and galvanism, light, and gravity."[19] In pursuit of it, in 1858 he presented a paper on electrodynamics to the Göttingen Society, which he began by saying that it "brings into close connection the theory of electricity and magnetism with that of light and of radiant heat."[20] His leading idea was that action between electric masses does not occur instantaneously but is propagated with a constant velocity, which was the same as Gauss's idea.[21] To express this, he generalized Poisson's equation for the electrostatic potential, rewriting it as a wave equation with a source function and solving it using a retarded potential. With this result, he needed only to identify the velocity of the retarded potential with the velocity of light to arrive at Weber's potential, which he regarded as confirmed. In a letter at the time, he proudly claimed the "discovery of the connection between electricity and light." He had been told that Gauss had already

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[17] Bernhard Riemann, "Neue Theorie des Rückstandes in electrischen Bindungsapparaten" (1854), in *Werke ... Nachlass*, 345–56, on 345.

[18] Bernhard Riemann, "Ueber die Gesetze der Vertheilung von Spannungs-electricität in ponderabeln Kőrpern . . ., "*Amt. Ber 31 Vers. Deut Naturfor.* (1854), in *Werke*, 48–53, on 53.

[19] Weber to Göttingen U. Curator, 10 March 1855. Riemann to his brother Wilhelm, 28 December 1853 and 26 June 1854, cited in Dedekind, "Riemann's Lebenslauf," 515. Riemann developed these ideas in a fragment entitled "Neue mathematische Principien der Naturphilosophie," to which he added the note "found on 1 March 1853," suggesting that he thought these principles were important. (He made it clear that by "Naturphilosophie" he meant natural philosophy.) In his *Werke . . . Nachlass*, 502–6.

[20] Bernhard Riemann, "Ein Beitrag zur Elektrodynamik" (1858), posthumously published in *Ann.* 131 (1867): 237–43, reprinted in *Werke . . . Nachlass*, 270–75, on 270.

[21] Riemann ended his paper with a comparison of the constant in Weber's law and the velocity of light. The measurement by Weber and Kohlrausch, which Riemann assisted in, was 192,965 mi./s. Two values of the velocity of light were: from Busch (based on Bradley's observations on aberration) 193,172 mi./s. and from Fizeau 192,757 mi./s. Riemann let the numbers speak for themselves. He was apparently the first to use a retarded potential, but because of the delay in the publication of the work, the first published use of it was by Ludvig Lorenz in 1861 (Leon Rosenfeld, "The Velocity of Light and the Evolution of Electrodynamics," *Nuovo Cimento*, supplement to vol. 4 [1957]: 1630–69 on 1635).

discovered a connection, but he thought that his was different and correct.[22] His intention in this work was to advance physics, not to do mathematics for its own sake; in the way that physicists worked, starting from a new physical hypothesis, he derived a known natural law. The advantage his derivation had over Weber's was that it explained why the constant in the fundamental electrodynamic law is nearly equal to the velocity of light. Earlier he had considered the nature of an ether for electricity, light, and gravity, but he did not discuss it or any other physical explanation of the finite propagation of electrodynamic action, and so his theory was only a beginning. In the event, he did not publish his paper, and when it was published posthumously in 1867, it was immediately criticized by Clausius, who pointed out a mathematical error and suggested that Riemann had withheld the paper because of it.[23]

The most thoroughly developed electrodynamic theory incorporating the velocity of light was a work by the Danish mathematical physicist Ludvig Lorenz', published in the *Annalen* in 1867. Applying Kirchhoff's generalization of Ohm's law to electrical conduction in three dimensions, he derived a wave equation for electricity moving in a medium conceived of as a poor conductor, identifying the alternating currents with the vibrations of light. Unaware of Maxwell's way of joining electricity and optics, his theory came as close to a field theory as was possible within the traditional electrodynamics based on the interaction of electric masses.[24]

Riemann did not return to the idea of propagated electrodynamic action, but in his published lectures on gravity, electricity, and magnetism, he proposed a "fundamental law" of electrodynamics.[25] Another variant of Weber's law, it differed in that the total relative velocity of the pair of electric masses enters in place of the relative velocity along the line between the masses. This time Riemann derived the potential from what he called the "extended law of Lagrange," taken from mechanics. This was an original contribution to electrodynamics: by developing electrodynamics from the same first principles as mechanics, he showed how the electrodynamic force could be treated like familiar forces that do not have

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[22] Dedekind, "Riemann's Lebenslauf," 521. The letter, for which Dedekind does not give a date, is to Riemann's sister Ida.

[23] Rudolph Clausius, "Ueber die von Gauss angeregte neue Auffassung der elektrodynamischen Erscheinungen," *Ann.* 135 (1868): 606–21, on 613–18. The error was elementary, an improper permutation of integrations.

[24] Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford: Oxford University Press, 2000), 212–13.

[25] Karl Hattendorff brought out the lectures in two volumes in 1876. Bernhard Riemann, *Partielle Differentialgleichungen und deren Anwendung auf physikalische Fragen*, ed. Karl Hattendorff (Braunschweig, 1869),1–4, 107–8. The second volume, which continued Riemann's applications of partial differential equations to additional branches of physics, he entitled *Schwere, Elektricität und Magnetismus, nach den Vorlesungen von Bernhard Riemann* (Hannover 1876). Riemann's fundamental electrodynamic law is in the second volume, 313–37.

velocity-dependent potentials. Riemann's law was taken up by physicists and compared with rival laws.

At Göttingen, Riemann offered lectures on the partial differential equations of physics, modeled after the lectures he had heard Dirichlet give in Berlin. Reviewing the subject historically, he said that "scientific physics" existed only since the discovery of the differential calculus, which allows elementary laws to be expressed in terms of basic physical concepts relating to space and time *points*. To go beyond elementary laws to laws relating to space and time *intervals*, which are all that are accessible to observation, there was need for the "method" of partial differential equations. It was not until 60 years after Newton's *Principia* that d'Alembert solved the first physical problem that led to a partial differential equation. It was another 60 years before Fourier developed general methods for solving physical problems requiring partial differential equations. Since then, physical laws that could be tested by experiment were formulated as partial differential equations. After presenting the purely mathematical aspects of his subject, Riemann devoted the second half of his lectures to the most important general second-order partial differential equation in physics, applying it to the motion of heat, oscillations of elastic bodies, motion of fluids, electricity, magnetism, and gravitation.

Theories usually start from molecular assumptions, Riemann told his audience, but because the distribution of the molecules and the forces they exert on one another are unknown, partial differential equations are needed to build the foundations. Solutions of physical problems are integrals of these equations. In the case of ordinary differential equations, there are a finite number of particular integrals, but in the case of partial differential equations, there are infinitely many particular solutions, each one of which is multiplied by an arbitrary constant. Because a general solution with arbitrary constants is useless, the "most important point of the question" in partial differential equations is the determination of the constants. Riemann gave the example of oscillations of an elastic surface. The general solution of the partial differential equation for the oscillations tells us nothing about the (Chladni) sound-figures, which can be found only by specifying an infinite number of conditions relating to the boundary of the oscillating plate.[26] Riemann's lectures on partial differential equations pointed to a continuity of mathematical methods across diverse parts of physics, a formal connectedness.[27]

Following his early death in 1866, Riemann's lectures were published in book-form in 1869, just as theoretical physics was beginning to be regularly represented in German universities and before lectures covering it began to be published. Riemann's lectures served as an early text on mathematical physics as well as a

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[26] Dedekind, "Riemann's Lebenslauf," 518.

[27] In the first half of the course Riemann developed several branches of mathematics in addition to partial differential equations: definite integrals, infinite Fourier series, and ordinary differential equations.

source of mathematical aids in doing research. Expanded versions of it were brought out later by others as a compendium of mathematical methods of physics.[28] To look ahead, Riemann's non-Euclidean geometry would be his most important single contribution to theoretical physics. Its relevance to physics was often speculated about, but it had to wait six decades, until Einstein's theory of general relativity, before it became a part of physics. Einstein had this to say about it: "Only the genius of Riemann, solitary and uncomprehended, had already won its way by the middle of the last century to a new conception of space, in which space was deprived of its rigidity, and the possibility of its partaking in physical events was recognized. This intellectual achievement commands our admiration all the more for having preceded Faraday's and Maxwell's field theory of electricity."[29]

In the first half of our period, mathematicians and physicists worked in parallel, with points of contact, formal as well as informal. Gauss and Dirichlet developed potential theory, and Weber and Riemann expressed their electrodynamic laws in terms of a potential. Dirichlet and Riemann developed the mathematics of partial differential equations of physics, and before them Ohm and later other physicists worked with the mathematics in their researches on physical laws. Potential theory and partial differential equations of physics became frequent subjects of special lecture courses at German universities, taught occasionally by physicists but usually by mathematicians. The number of special courses underestimates the amount of instruction offered in these two subjects, since they were incorporated into lectures on all parts of theoretical physics.[30] The goal of a total theory of physics was shared by physicists and mathematicians: Ohm worked on a total molecular theory of physics; Weber, as we will see, generalized his electrodynamic law to encompass

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[28] We get an idea of the physicists' need for mathematics from the mathematician Heinrich Weber's new edition in 1900–1901 of Riemann's 30-year-old lectures on the partial differential equations of physics. Physics had experienced a "sweeping transformation" as a result of Maxwell's electromagnetic theory, Weber said, and along with it the mathematics of physics had changed as well, necessitating a complete reworking of the text and a doubling of its length. In addition to providing new aids for solving partial differential equations, he introduced new mathematical subjects, prominent among which was vector analysis, an invaluable aid in working with Maxwell's concept of a physical "field." Weber retained "Riemann" in the title of his book only because it remained true to Riemann's "purpose and intellectual spirit." What was said of Weber's fifth and last edition of 1910–1912 could be said of earlier editions too: it was indispensable for doing "theoretical physics." (26.159). Heinrich Weber, *Die partiellen Differential-Gleichungen der mathematischen Physik. Nach Riemann's Vorlesungen*, 4th rev. ed. (Braunschweig: F. Vieweg, 1900–1901).

[29] Albert Einstein, "The Problem of Space, Ether, and the Field in Physics," 1934, in *Ideas and Opinions* (New York: Dell, 1973), 270–79, on 274–75.

[30] During the academic year 1886–1887, for example, potential theory was announced as the subject of special courses at ten German universities and partial differential equations of physics at six of them. The number of special courses gradually fell off; for example, eight special courses in potential theory were announced in 1900–1901, two in 1913–1914 (*Deutscher Universitäts-Kalender*).

all parts of physics; and Riemann envisioned a total theory of physics based on a hypothesis about the ether.[31]

8.4 Carl Neumann

After the adoption of "theoretical physics" as the preferred term to describe the work on mathematical theory by physicists, mathematicians continued to do work in "mathematical physics." As an example, we look at Carl Neumann, whose main achievements were mathematical, but who came to the attention of physicists for his theoretical work and for his views on the work of theoretical physics. As a student at Königsberg, he studied mathematics and physics, and later as a working mathematician he dealt almost exclusively with mathematical problems arising from physics. For his inaugural lecture at Tübingen in 1865, he picked as his subject the "present standpoint of mathematical physics." He had no quarrel, he said, with the widespread claim that natural science was rapidly advancing, but he did have one with the claim that the advance applied to theory as well as to the number of discoveries. In certain parts of physics, theories seemed fairly well established, but in other parts such as electricity and magnetism the existing theories were not likely to endure for long.[32] Beginning in 1868, and for 43 years, he held Möbius's old mathematical chair at Leipzig, described by Weber as a professorship for "higher mechanics, which essentially encompasses mathematical physics."[33] Although he was hired as a mathematician who could teach higher mathematics, not as a mathematical physicist, Neumann's lectures at Leipzig covered potential theory, mechanics, and all parts of mathematical physics. Because of the emphasis of his teaching, he counted a number of physicists among his students.[34] In his research and in his teaching, Neumann used potential theory in addressing problems of electricity and magnetism, the part of mathematics in which he did his most important work.[35]

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[31] Early in his career, Riemann developed a new conception of the known laws of nature, which would make use of experimental data on the interactions between heat, light, magnetism, and electricity. It was based on a world-space continuously filled with an incompressible, homogeneous fluid without inertia. At the head of the theory, he placed a mathematical law with two parts, one part describing gravitation and electrostatic attraction and repulsion, the other part describing the propagation of light, heat, and electrodynamic and magnetic attraction and repulsion. His evident goal was a total theory of physics proceeding from a single encompassing mathematical law.

[32] Carl Neumann, "*Der gegenwärtige Standpunkt der mathematischen Physik* (Tübingen, 1865).

[33] Weber to Göttingen U. Curator, 22 May 1848, Weber Personalakte, Göttingen UA, 4/Vb/95a.

[34] Neumann Personalakte, Leipzig UA, Nr. 774 and other documents. Hans Salié, "Carl Neumann," in *Bedeutende Gelehrte in Leipzig*, edited by G. Harig (Leipzig: Karl-Marx-Universität, 1965), vol. 2, 13–23, on 15.

[35] O. Hölder, "Carl Neumann," *Verh. sächs. Ges. Wiss.* 77 (1925): 154–80, on 156.

Neumann followed developments in physics closely. In his papers on mathematical physics, he carried through extensive mathematical analyses, but he also introduced physical hypotheses and compared their consequences with experimental observations made by others. He worked mathematically on all parts of physics, giving special attention to electrodynamics. In 1868, he developed a new theory of electrodynamics in which the action of one electric mass on another is described by a potential that depends on their separation and also on their relative velocity. The important new idea of his theory was that the action is not instantaneous but is propagated. (Riemann's similar idea was only published later.) He did not introduce an intervening medium to support the propagation, and so he did not have a field theory in Faraday and Maxwell's sense. His theory was carefully examined by Helmholtz, Clausius, Weber, and other physicists. Here and in his work in electrodynamics in general, his touchstone was Weber's fundamental law,which he regarded as fully confirmed and probably unassailable. He took Weber's side in his controversy with Helmholtz and the side of action at a distance against field theory. In 1902–1904, in a series of papers he investigated the Maxwell-Hertz theory, looking to see if there is an essential difference between the new and the old ideas, but by then he was out of touch with the general thinking of physicists in this area.[36]

When Leipzig considered candidates to refill its chair for theoretical physics at the turn of the twentieth century, Neumann and his colleague the Leipzig professor of experimental physics Otto Wiener disagreed on the qualifications. Neumann wanted the position to go to Arnold Sommerfeld. Wiener objected that Sommerfeld's candidacy contradicted the principles that had guided the establishment of the theoretical physics professorship in the first place. Sommerfeld would be incapable of directing an institute and the laboratory work of students, in which case the newly created theoretical physics institute would be superfluous and worthless. Neumann and Wiener's disagreement over Sommerfeld was based on their conflicting understandings of the nature of theoretical physics. In urging Sommerfeld's candidacy, Neumann argued that any advance in physics would be slow in coming and would depend on the "careful sifting and fashioning of what already exists" which required mathematical fluency. His emphasis was on work with mathematical methods. Wiener expected physics to advance quickly through new ideas, not gradually through a mathematical refinement of old ideas. His emphasis was on theoretical prediction and experimental methods: what was wanted was "a certain *theoretical* [*metronomic*] *timekeeping* [*Tact*], which leads to *new experimental investigations*." Since Neumann thought that Wiener should have the final say on the second physics position, Wiener won the argument, in a sense. In the end the faculty commission kept the disagreement from becoming the concern of the whole philosophical faculty by voting to remove Sommerfeld's name from the list of candidates. Sommerfeld, whose strength lay in the "forceful

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[36] Carl Neumann, "Die Principien der Elektrodynamik" (1868), *Math. Ann.* (1880): 400–434. Hőlder, "Neumann," 160–70.

application of mathematics to complete theoretical problems," had never done an experimental investigation, and this defeated his chances at Leipzig. The appointment went to Theodor Des Coudres, and Sommerfeld would become professor of theoretical physics at another university.[37]

Unlike his father, Franz, Carl Neumann did not become a physicist. He remained a mathematician, whose interest in physics was primarily mathematical,[38] and that is how Wiener and other physicists saw him. Through his teaching and research he made a modest contribution to the work of theoretical physics, but perhaps less than his philosophy would anticipate. He believed in a harmony between the intellect and the external world, which made mathematics, physics, and the other physical sciences appear to him as part of a "single great whole." He viewed himself as upholding a grand tradition that was by then largely dormant. To his regret the sciences had increasingly grown apart in the course of the nineteenth century. Jacobi, Dirichlet, and Riemann had worked in mathematics and physics, he said, but after them only mathematics could be expected from mathematicians, to their loss.[39]

In an address at the 1905 meeting of the German Association, Wien said that to solve the problems of the electron theory, physicists needed everything that mathematics could offer. The following year he complained to the mathematician David Hilbert that he got little help from his present mathematical colleagues and asked for his advice on candidates for an extraordinary professorship for mathematics who might be better. He gave a talk to the German Mathematical Society that same year on partial differential equations in physics, remarking that today theoretical physicists needed the "comprehensive cooperation" of mathematicians. Planck agreed: on receiving a copy of Wien's book on hydrodynamics in 1900, Planck noted that mathematics is powerless to treat some of the simplest physical phenomena, and he hoped that mathematicians would read the book and do something

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[37] The length of the Leipzig candidates list fluctuated, as it gradually acquired order, and as it provoked a controversy over the desirable qualifications of a theoretical physicist. When the appointment commission decided to recommend, in order, Theodore Des Coudres, Emil Wiechert, Carl Runge, and Arnold Sommerfeld, Otto Wiener wrote a separate dissenting report, a copy of which he sent to Carl Neumann. Leipzig University Philosophical Faculty to Saxon Ministry of Culture and Public Education, 11 July 1902; note by Wiener, 30 October 1902; minutes of the meetings of the commission for the reassignment of the professorship for theoretical physics, 6, 20, and 29 November and 3 December 1902; Wiener to Dean of the Philosophical Faculty and Wiener's accompanying "Separatbericht," 30 November 1902; Des Coudres Personalakte, Leipzig UA, PA 410. Carl Neumann to Otto Wiener, 29 November 1902 quoted in Hans Salié, "Neumann," 14–15. Neumann to Sommerfeld, 22 May 1903, Sommerfeld Papers, Ms. Coll., DM.

[38] Heinrich Liebmann, "Zur Erinnerung an Carl Neumann," *Jahresber. d. Deutsch. Math.-Vereinigung* 36 (1927): 174–78, on 175.

[39] Carl Neumann, "Worte zum Gedächtniss an Wilhelm Hankel," *Verh. sächs Ges. Wiss.* 51 (1899): lxii–lxvi, on lxiv. Salié, "Carl Neumann," vol. 2, 13–23, on 18.

about it.[40] Theoretical physicists asked a good deal from mathematics, and the existing mathematics was not always equal to it.

Mathematicians contributed to the work of theoretical physics in ways that did not appear as papers on theoretical physics under their names. Near the end of our period, Wien thought that theoretical physics in Germany had all but died out, and that for it to recover mathematicians needed to apply themselves to physics, as they had in the past.[41]

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[40] Planck to Wien, 24 May 1900, Wien Papers, STPK, 1973.110. Wilhelm Wien, *Über Elektronen. Vortrag gehalten auf der 77 Versammlung deutscher Naturforscher und Ärtze in Meran,* 2nd ed. (Leipzig and Berlin: B. G. Teubner), 5; "Über die partiellen Differential-Gleichungen der Physik," *Jahresber. d. Deutsch. Math.-Vereinigung* 15 (1906): 42–51, on 42. Wien to Hilbert, 3 December 1906; also Wien to Hilbert, 2 May 1909, asking Hilbert's advice on how to develop integral equations in physics; Hilbert Papers, Göttingen UB, Ms. Dept.

[41] Wien to Sommerfeld, 11 June 1898, Sommerfeld Correspondence, Ms. Coll., DM.

Chapter 9
Kirchhoff, Clausius, Weber, and Connectedness

Following major discoveries in electricity and electromagnetism in the early nineteenth century, electrical research underwent a strong, combined experimental and theoretical development through the second half of the century. Nearly all of the leading physicists in Germany worked extensively in the subject. In this chapter, we take up Clausius's new fundamental law of electrodynamics and Weber's continuing research in electrodynamics, culminating in a comprehensive theory of physics. We also take up Kirchhoff"s research in elasticity theory and the mechanical theory of heat, of wide interest to German physicists. All of these examples of work illustrate the growing theoretical connectedness of physics.

9.1 Kirchhoff at Heidelberg

By the middle of the nineteenth century, university faculties commonly exploited the rivalry between the German states to gain support for scientific institutes. To that tactic, another element increasingly entered their bids: competition for outstanding scientists. Successful teaching and research by university physicists had become associated with the availability of well-equipped laboratories. Formerly the principal measure of a state's investment in physics was its showpiece collection of physical instruments, and a second measure was the salary a state could afford to entice a physicist of literary renown to one of its universities. The two measures gradually came to merge with the difference that the physics collection was no longer primarily for show but for work, and the physicist had to have scientific rather than literary distinction; the measure of a university, particularly of the main university of a state, had become whether or not it could afford to offer an institute capable of attracting the best physicist.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_9

Kirchhoff's first call was to Breslau University in 1850 as the second physicist with the usual position of extraordinary professor. The direction of the Breslau physics institute was divided between him and the ordinary professor for physics, allowing him to use the physical cabinet. But since the cabinet was for the ordinary professor's use, a familiar conflict arose between the two, especially as the ministry had not laid out clearly the rights and duties of each. It was a source of unhappiness for Kirchhoff at Breslau.[1] When 4 years later, in 1854, he was called to Heidelberg University as ordinary professor, he was ready to move.

We begin with the state of physics at Heidelberg leading up to Kirchhoff's call. The tactic of shaming the government into giving more support for the physics institute by reporting on advances elsewhere, particularly at universities less important than their own, was used by Heidelberg physicists to improve their conditions of work. When the ordinary professor of physics G. W. Muncke's failing health allowed the second physicist Philipp Jolly to speak for physics at Heidelberg in 1846, he argued (as Muncke had) that "for a number of years now one sees at most German universities, even at those that are not equal to [Heidelberg] University either in attendance or in significance, the means for . . . physical and natural scientific studies growing apace with the rapid development of the sciences." The "rise" of small Giessen University and the remnants of "splendor" that Göttingen University still had (following the unfortunate dismissal of Weber) were "largely if not solely due to the excellent aids, the rich collections, and grand institutes that are offered there to teachers and students in equal measure.".[2]

In his description of the needs of the Heidelberg physics institute, Jolly pointed out that it "lacks a laboratory, a place for expanding the teacher's work and for student practice, a facility that nowadays, if anything is to be achieved at all in these subjects, is an inescapable requirement." Because Heidelberg lacked one, its eager and more capable students "were offered nothing," and experimental physics lay "fallow."[3] Jolly, who had taught physics side-by-side with, and often in place of, Muncke for several years, had privately acquired a collection of physics instruments, "truly rich and worth seeing,"[4] but because the instruments were for teaching, they were insufficient for "independent researchers, for the purpose of forming a school, a goal that at least at universities must not be overlooked." He asked the government for annual support for setting up and running a physics laboratory.[5] When no action was taken for several months, his solution was to

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[1] Emil Warburg, "Zur Erinnerung an Gustav Kirchhoff," *Naturwiss*. 13 (1925): 205–12, on 207.

[2] Jolly to Baden Ministry of the Interior, 12 June 1846.

[3] Ibid.

[4] Heidelberg U. Curator to Baden Ministry of the Interior, 1 August 1846, Bad. GLA, 235/3135.

[5] Jolly to Baden Ministry of the Interior, 12 June 1846.

transform his apartment into a physics institute of sorts by removing a wall to obtain a lecture hall and by turning the kitchen into a laboratory. He requested money to rent a second apartment for his family, since he paid half of his salary, 400 out of 800 florins, in rent for the flat in which he had set up his physics institute. "Physics has to be taught in Heidelberg," the Heidelberg curator said when he forwarded Jolly's request to the government, and Jolly cannot "experiment in the street."[6] Jolly was promoted to ordinary professor in 1846 and given permission to use the physical cabinet of the university.[7] The gain in space he received was too small for his many students, and it was not suited for physics experiments, but he now received an annual fund with which he set up a small laboratory for students and for his own experimental researches.[8]

When Jolly left for Munich University in 1854, Heidelberg had arrived at the moment of truth for a university: it acknowledged that it could no longer attract a first-rate physicist because of the inadequacy of its physics institute. The philosophical faculty opened its report to the Baden ministry on possible replacements for Jolly with a list of physicists who were out of reach for this reason. On their list were such "notables" as Magnus and Dove in Berlin as well as Weber, Neumann, and Ettingshausen.[9] Although it was past the midpoint of the century, the philosophical faculty, whose dean and spokesman in 1854 was the chemist Robert Bunsen, still could not take for granted that the ministry would understand or even expect the strong emphasis they placed on research. Before discussing any of the candidates, Bunsen and the other faculty explained the assumptions on which their selection was based. The literature of physics, they wrote, points in two directions: it may give an account, in compendiums and annual reports, of scientific results and of the means by which they were obtained; or it may aim at enriching individual areas of science through original investigations. Since experience has shown, the faculty said, that one direction almost excludes the other, teachers of physics must be grouped in two categories. They listed three professors belonging to the first category, alongside their successful textbooks, and nine physicists belonging to the second. It was only the second that they would consider, as they explained: "The faculty must start from the conviction that *at universities* success-

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[6] Heidelberg U. Curator to Baden Ministry of the Interior, 24 September 1846, Bad. GLA, 235/3135.

[7] Jolly's appointment, 28 September 1846, Bad. GLA, 235/3135. His new salary as ordinary professor was 1000 florins. Jolly Personalakte, Heidelberg UA, III, 5b, Nr. 233.

[8] Heidelberg U. Curator to Baden Ministry of the Interior, 24 September 1846. D. R., "Jolly," 808. C. Voit, "Philipp Johann Gustav von Jolly," *Sitzungsber. bay. Akad.* 15 (1885): 119–36, on 123–24.

[9] Dean of Heidelberg U. Philosophical Faculty Bunsen to Baden Ministry of the Interior, 26 July 1854, Bad. GLA, 235/3135.

ful teaching in its true significance is unthinkable without work that is primarily directed to the expansion and development of science."[10]

The Heidelberg faculty's determination to fill its physics position with a productive researcher was regarded as the only proper course by German physicists. To provide support for researchers was now the reason for physics chairs, Weber wrote to Heidelberg in 1854. For in physics as opposed to, say, chemistry, teaching was the only kind of work by which a researcher could hope to make a living: "In chemistry there are all kinds of positions outside of the university circle in which successful scientific work is possible, but which so far are still very much lacking in physics. It is all the more important for the growth of this science to see to the filling of the few professorships in this field and to act with the intention of preserving them for the most outstanding talents in this field who otherwise will not find any base for the development of work that would essentially further the science."[11]

In 1854 Kirchhoff was thirty, and having distinguished himself over many years as a researcher, he was in the right category from Heidelberg's point of view. Fortunately for Kirchhoff, it was no disqualification for a director of a physics institute to be known principally as a mathematical physicist so long as he did experimental work too. In his recommendation, Weber spoke of Kirchhoff's talent as an exact experimenter and of his "great superiority," with the exception of Neumann, in the "fields of theory and mathematics" over all German physicists. Neumann recommended his former student for the Heidelberg job on the basis of his "scientific works," which introduced "new methods and new points of view in physics" and of his important "criticism." In his recommendation, Ettingshausen illustrated Kirchhoff's "eminent talent for physics" by singling out individual researches, which continued the works of Ohm and Poisson: electromagnetic induction, oscillations of elastic plates, and the "branching of galvanic currents in conductors of not only linear extension, whereby he opened up a new path of investigation." He expected Kirchhoff's future work to belong to the very best.[12] What Weber, Neumann, and Ettingshausen said of Kirchhoff would be repeated by others throughout his career: he was an excellent theorist with an impressive

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[10] Bunsen to Baden Ministry of the Interior, 26 July 1854. The philosophical faculty's explanation may have had an ulterior motive. The three professors who were dismissed, in respectful terms, as textbook writers were Wilhelm Eisenlohr, Johann Müller, and Heinrich Buff. The first two were employees of the state of Baden, at Karlsruhe Polytechnic and at Freiburg University, respectively, and the third was a colleague and relative of Liebig's at Giessen, to whom Jolly was beholden for a recommendation to Munich University and for whom Jolly was trying to secure a Heidelberg position. The faculty's explanation appears designed to counteract, on the one hand, the state's possible intention to fill the Heidelberg position cheaply by promoting one of the two physicists at the other Baden institutions of higher learning and, on the other hand, to prevent Jolly from bringing about Buff's appointment. Jolly's relationship to Liebig in this connection can be seen in Jolly's letter to Liebig, 19 February 1854, Liebigiana, Bay. STB, 58 (Jolly, Philipp v.) Nr. 3.

[11] Weber to Bunsen, 12 March 1854, Bad. GLA, 235/3135.

[12] Weber to Bunsen, 12 March 1854; Neumann to Bunsen, 20 March 1854; Ettingshausen to Bunsen, 14 March 1854, Bad. GLA, 235/3135. Dean of Heidelberg U. Philosophical Faculty Bunsen to Baden Ministry of the Interior, 26 July 1854. Bad. GLA, 235/3135.

command of mathematics who had in addition excellent skill as an experimentalist, and through his work he contributed methods of research, established and generalized laws, and offered critical perspectives. These traits of Kirchhoff's constituting much of what characterizes work in theoretical physics in our period were evident to leading German physicists as early as 1854.

Bunsen's efforts were rewarded by the faculty's recommendation of Kirchhoff and the subsequent offer of the Heidelberg physics chair, which he accepted. On Kirchhoff's part, there were a number of reasons why he wanted to move. He missed his close contact with Bunsen, which had been interrupted by Bunsen's move from Breslau to Heidelberg in 1852. He would receive a promotion and higher pay, but most important, at Heidelberg he would have his own physics institute where he could develop his teaching and research unhindered, as he could not in his subordinate position as the second physicist at Breslau.[13] His one worry was that his lectures would not be as successful as Jolly's "extraordinarily attractive" ones.[14] This worry was of secondary importance, at least as far as Heidelberg was concerned. Kirchhoff's two decades at Heidelberg, from 1854 to 1874, were to prove the most productive of his career.[15]

It was only near the end of Kirchhoff's stay at Heidelberg that he had an official seminar to direct. Until relatively late, the Heidelberg philosophical faculty viewed seminars as institutions for training secondary school teachers for which philological seminars were well suited. In time this understanding proved unworkable there as elsewhere. From the late 1850s, Heidelberg's natural scientists were unhappy with their position in the philosophical faculty, in which owing to a proliferation of chairs their humanist colleagues acquired a widening numerical advantage over them. The gulf between the two faculty groups was generally seen as unbridgeable, based on fundamental differences between their methods of training and research. In the new Baden testing regulations for teaching candidates in 1867, the earlier goal of producing gymnasium teachers with a rounded philological-historical and natural-scientific education was finally abandoned. The re-organized Heidelberg philological seminar dropped its claim to be the only institution qualified to train

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[13] As extraordinary professor at Breslau, Kirchhoff got a salary of 1050 florins; as ordinary professor at Heidelberg, he got 1600 florins plus another 400 florins for housing. Kirchhoff Personalakte, Heidelberg UA, III, 5b, Nr. 244. Letter from Kirchhoff to his brother Carl, 18 October 1854, quoted in Warburg, "Kirchhoff," 208. When Neumann recommended Kirchhoff for the Heidelberg professorship, he wrote to Bunsen that he was sorry to see Kirchhoff leave a Prussian university, Breslau, but he wanted the "freer scientific activity" of Heidelberg for Kirchhoff because at Breslau he worked with difficulty under Frankenheim. Neumann to Bunsen, 20 March 1854, Bad. GLA, 235/3135.

[14] Kirchhoff to his brother Carl, 18 October 1854; Bunsen to Baden Ministry of the Interior, 26 July 1854.

[15] Robert Helmholtz, "A Memoir of Gustav Robert Kirchhoff," trans. J. de Perott, *Annual Report of the . . . Smithsonian Institution . . . to July,1889*, 1890, 527–40, on 529.

teachers, opening the way for specialized seminars for physics, mathematics, and other fields.[16]

The appointment in 1869 of Kirchhoff and Bunsen's good friend Leo Königsberger as mathematics professor provided an opportunity for establishing a mathematical-physical seminar at Heidelberg. Together Kirchhoff and Königsberger proposed the familiar organization for a seminar, containing sections for pure and applied mathematics and mathematical physics.[17] In the mathematical physics section, students would be given a weekly problem, part theoretical and part experimental, and in the accompanying lecture Kirchhoff would explain the problem and the methods to be used. Students would then carry out the prescribed experiments and write up papers, which often led to dissertations. Kirchhoff's physical part of the seminar in the summer semester of 1872 had 13 members, a manageable number for the procedure he designed for it.[18]

In addition to directing work in the seminar and laboratory, Kirchhoff gave regular lectures, the traditional responsibility of the chair holder. These were on experimental physics, often with demonstration apparatus he assembled himself, and on theoretical physics, which included lectures at an advanced level.[19] To

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[16] Reinhard Riese, *Die Hochschule auf dem Wege zum wissenschaftlichen Grossbetrieb. Die Universität Heidelberg und das badische Hochschulwesen 1860–1914*, vol. 19 of Industrielle Welt, Schriftenreihe des Arbeitskreises für moderne Sozialgeschichte, ed. Werner Conze (Stuttgart: Ernst Klett, 1977), 88–90, 194–96. By Kirchhoff's time, the presumed intellectual unity of the philosophical faculty was widely disputed and debated. The *Staatswissenschaften*, in the 1860s, were the first to separate off at Heidelberg. In 1890, at the initiative of the natural scientists, and after inquiring about Tübingen, Strassburg, and Leipzig, at which a division of the philosophical faculty had already occurred, the Heidelberg natural sciences and mathematics faculty also separated off (90). In 1900, the Heidelberg mathematical-physical seminar divided; in light of the impending call of a professor for mathematical physics, the natural sciences and mathematics faculty wanted Heidelberg, like other universities, to "divide completely the mathematical and physical seminar." ("Prodecan," Pfizer to Heidelberg U. Senate, 25 January 1900, Bad. GLA, 235/3228).

[17] By the wording of the proposal for a mathematical-physical seminar—the request for "official" recognition—it sounds as if an unofficial seminar may have been in existence. Kirchhoff and Königsberger's proposal to "Excellency," 14 April 1869, Bad. GLA, 235/3228.

[18] Kirchhoff and Königsberger's annual report on the seminar for the academic year 1869–70, Bad. GLA, 235/3228. The rigid procedure followed in Kirchhoff's seminar was observed firsthand by Arthur Schuster, *The Progress of Physics During 33 Years (1875–1908)* (Cambridge: Cambridge University Press, 1911), 13–14.

[19] Kirchhoff gave the 6-h lecture course on experimental physics and supervised practical work in the laboratory. He also offered a 3-h survey course on theoretical physics, which treated mainly mechanics in the "wider sense" and concluded with the mechanical theory of heat. In addition, he gave a 1-h course on separate branches of theoretical physics such as the "mechanics of elastic and fluid bodies" and the "theory of heat and electricity." In the summer semester of 1870, for the first time, Kirchhoff's "physical seminar" was listed among the courses of the philosophical faculty. From then on, he offered in alternate semesters his seminar and his 3-h theoretical physics course. His teaching of theoretical physics was supplemented by that of the Prviatdocent Friedrich Eisenlohr, who regularly offered a course in his specialty, theoretical optics, in addition to courses on mechanics, potential theory, and other mathematical subjects. (*Anzeige der Vorlesungen … auf der Grossherzoglich Badischen Ruprecht-Carolinischen Universität zu Heidelberg*, the published list of courses offered each semester at Heidelberg; Friedrich Pockels, "Gustav Robert Kirchhoff," In *Heidelberger Professoren aus dem 19. Jahrhundert* [Heidelberg: C. Winter, 1903], vol. 2,

follow him, students needed a proper mathematical training, and for this Kirchhoff placed great importance on Königsberger's teaching, regarding it as the condition for the success of his own. As Königsberger described their close cooperation at Heidelberg in the years 1869–1874, "Kirchhoff and I worked hand-in-hand, so that sometimes in the same semester we both lectured to the same audience on mechanics, he more from the physical, I from the purely mathematical, point of view, and daily we spoke together about the subject of the coming lecture. To me it was an indescribable joy to see such an active scientific life expand."[20] From all over Germany and from abroad, young physicists came to Heidelberg because of Kirchhoff.[21]

In quantity, Kirchhoff's researches were modest by contemporary practice, needing only a single volume together with a slender supplement to contain them, but they were choice. They were the work of a physicist who had "mastered experiment as well as mathematics," the distinguishing characteristic, one of Kirchhoff biographers observed, of the "greatest physicists nowadays."[22] From early in his career, in 1848 and on and off for much of the rest of it, Kirchhoff worked on problems in elasticity, which serve to illustrate the general character of his physics. He developed the theory of elasticity and carried out corresponding experiments; he treated the relations between elasticity and other branches of physics; he formulated equations for the deformation of bodies by electric and magnetic forces; he used elastic theory as an analogy in developing electrical theory; and he applied elastic theory to optics to derive a new law for the passage of light between transparent bodies, solving a problem that Neumann and Fresnel had long struggled with.[23] His contribution to the general theory of elasticity as a branch of mechanics is the work we discuss here.

In papers published in 1850 and 1858 in a journal for mathematics and mathematical physics, Kirchhoff derived equations for the equilibrium and motion of elastic plates and rods.[24] This work was regarded as one of the first exact treatments

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243–63, on 248; Wilhelm Lorey, *Das Studium der Mathematik an den deutschen Universitäten seit Anfang des 19. Jahrhunderts* [Leipzig and Berlin: B. G. Teubner, 1916], 72–73).

[20] Leo Königsberger, *Mein Leben* (Heidelberg: Carl Winters, 1919), 101; Schuster, *Progress of Physics*, 14.

[21] Kirchhoff's laboratory and lectures attracted not only gifted German physicists such as Eilhard Wiedemann and E. Bessel-Hagen but also many gifted foreign physicists such as Boltzmann and Victor von Lang from Austria, Gabriel Lipmann from France, H. Kamerlingh Onnes from Holland, and Schuster from Britain.

[22] Robert Helmholtz, "Kirchhoff," 528. The volume Kirchhoff edited himself, *Gesammelte Abhandlungen* (Leipzig, 1882), runs to 641 pages; the posthumous volume edited by Ludwig Boltzmann, *Nachtrag* to Kirchhoff's *Gesammelte Abhandlungen* (Leipzig, 1891), runs to 137.

[23] Woldemar Voigt, "Zum Gedächtniss von G. Kirchhoff," *Abh. Ges. Wiss. Göttingen* 35 (1888): 3–10, on 6.

[24] Gustav Kirchhoff, "Ueber das Gleichgewicht und die Bewegung einer elastischen Scheibe," *Journ. f. d. reine u. angewandte Math.* 40 (1850): 51–88, in *Ges. Abh.*, 237–79; "Ueber das Gleichgewicht und die Bewegung eines unendlich dünnen elastischen Stabes," *Journ. f. d. reine u. angewandte Math.* 56 (1858): 285–313, in *Ges. Abh.*, 285–316. Kirchhoff's 1850 and 1858 papers are closely related, but they have some important differences; Kirchhoff remarked that the first paper could be developed in a stronger way by following the method of the second (*Ges. Abh.,* 311).

of the difficult subject of the elastic relations of bodies with one or two of their dimensions assumed to be infinitely small, as approximated by a plate or a wire.[25] His mathematics is complicated, but that is in the nature of the subject, required to arrive at results that can be compared numerically with observations. By contrast with the mathematics, the experiments look simple.

Earlier, in connection with the Weber brothers' researches on waves, we discussed Chladni's discovery of a means of making visible the nodal lines, or lines of rest, of an oscillating plate by strewing sand on it. This work stimulated the interest of several French physicists and mathematicians in elasticity theory. Sophie Germain made the first attempt at explaining Chladni's figures using a hypothesis about the forces that resist a change of form in an elastic plate and developing differential equations for the oscillations, which she applied to a right angle plate, finding the tones and nodal lines in agreement with observation. Kirchhoff called her agreement an accident, since the equations were self-contradictory owing to a mistake in her hypothesis. From a more secure foundation, Poisson and Navier developed a molecular theory of elastic bodies, and from it, Poisson deduced equations for a thin circular plate in good agreement with measurements, though Kirchhoff found fault with his boundary conditions.[26] A. J. C. Barré de Saint-Venant's theory of elastic rods was an improvement over Poisson's, but his assumptions were unduly limiting. Rejecting the molecular point of view in favor of the view of elastic matter as continuously filling the space of bodies, Kirchhoff derived general equations for the form of an elastic body in agreement with Cauchy's standard equations for the interior and the surface of an elastic body. He emphasized the great generality of his derivation.[27] By specialising the general equations to the cases of an infinitely thin circular plate and an infinitely thin rod, he arrived at equations of motion and their solutions that, mathematically speaking, had "considerable interest."[28] Physically, they had considerable interest as well; in papers in 1850 and 1859 in the *Annalen*, he showed that his theoretical results were supported

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[25] Isaac Todhunter, *A History of the Theory of Elasticity and of the Strength of Materials from Galilei to the Present Time*, vol. 2, *Saint-Venant to Lord Kelvin*, pt. 2 (Cambridge, 1893), 54. Kirchhoff's 1858 paper—his "most important" elasticity paper, according to Todhunter (68)—was judged by William Thomson and P. G. Tait, *Natural Philosophy*, pt. 2 §609, as the "first thoroughly general investigation of the equations of equilibrium and motion of an elastic wire."

[26] Kirchhoff, "Ueber die Schwingungen," 279–80.

[27] In his 1850 paper, Kirchhoff's variational equation reads $0 = \delta P - K\delta\Omega$, expressing the equality of the moments of the external force and of the internal elastic forces for the equilibrium state. Here P is the moment of the external forces, K is a constant, and Ω is a volume integral of a homogenous function of the main dilations. His 1858 paper proceeds from a similar variational equation. Earlier George Green had given the equation but had not expressed it in terms of principal dilations (Todhunter, *Elasticity*, 41, 56; Kirchhoff, "Ueber das Gleichgewicht und die Bewegung eines . . . Stabes," 295).

[28] This is Todhunter's comment on Kirchhoff's solution for the vibrations of the plate, which he expressed as "doubly-infinite series of functions akin to Bessel's functions" (Todhunter, *Elasticity,* 45, 43).

by his own and others' experiments.[29] His most important result in 1850 was a derivation of the "equations for the frequencies of the tones and the positions of nodal lines" of vibrating plates.[30] To test his results, he drew on Chladni's measurements of tones and more recent measurements of nodal lines on circular glass and metal plates, and after undertaking a "very great amount of laborious calculation," he concluded that his theory agreed remarkably well with experiment, all things considered.[31]

The theory contained a constant θ, to which Poisson had given the value 1/2 and more recent experiments the value 1. Nodes and notes of vibrating plates depend on this constant, but not sensitively enough for Kirchhoff to decide between the two values from the measurements at hand. In his paper in 1859 on the relation between the constriction of the cross-section and the dilation of the length of a stretched slender steel bar or wire, he used an ingenious method that combined torsion and flexure to determine the constant θ. The experimental arrangement is simple: a horizontal bar is fixed at one end, and near the other end there is a horizontal arm normal to the bar from which a weight is suspended, which both bends and twists the horizontal bar. Experimenting with narrow, round, elastic steel bars, he measured the ratio of contraction to dilation, expressed theoretically as $\theta/(1 + 2\theta)$. The average of his measurements for this ratio was 0.294, in between Poisson's theoretical ratio of 1/4 and Wertheim's experimental value of 1/3. He discussed the various sources of errors such as elastic after-working and took pains to reduce them.[32] A historian of the theory of elasticity says that Kirchhoff's contributions to the theory of thin rods and plates "give him a permanent place in the history of elasticity."[33]

We single out here a number of characteristics of theoretical physics in Kirchhoff's work on the theory of elasticity, which relate to experiment, sources, methods, mechanics, generalization, criticism, and manner of publication. His experimental work on elastic theory is discussed in the last two paragraphs above. In the middle of the nineteenth century, the important theory in elasticity still came from France, and Kirchhoff's first two papers on elasticity, dealing with the motion and equilibrium of an elastic plate, were published in the French *Comptes rendus* in 1848 and 1849. In his papers in 1850 and 1858 on the motion and equilibrium of a thin elastic rod, he departed from Poisson in both senses of the

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[29] Todhunter, *Elasticity*, 45; Gustav Kirchhoff, "Ueber die Schwingungen einer kreisförmigen elastischen Scheibe," *Ann.* 81 (1850): 258–64, in *Ges. Abh.*, 279–85; "Ueber das Verhältniss der Quercontraction zur Längendilatation bei Stäben von federhartem Stahl," *Ann.* 108 (1859): 369–92, in *Ges. Abh.*, 316–39.

[30] Todhunter, *Elasticity*, 45.

[31] Todhunter, *Elasticity,* 46–47. Measurements of the radii of the circular nodes belonging to the different tones on glass and metal plates agreed in an "excellent way" with each other and—generally accurate to two places—with the values calculated from Kirchhoff's theory (Kirchhoff, "Ueber die Schwingungen," 285).

[32] Kirchhoff, "Verhältniss," 316–17, 338.

[33] Todhunter, *Elasticity*, 39.

word,[34] and he also discussed work by Germain, Navier, Cauchy, Saint-Venant, and Lagrange. He used mathematical theory and experiment together in a broad approach to understanding elastic behavior. His preferred theoretical method was phenomenological, proceeding from bodies treated as a continuum rather than from particles and distance forces. His theory was mechanical, concerned with the motion and equilibrium of elastic solids under stress, and he made use of a principle from the mechanical theory of heat. The derivation of his theory had impressive generality. He critically compared alternative theories based on different hypotheses with experiment, using a parameter θ for the purpose. His theory proved to be an "extremely valuable" stimulus, leading to "much good work."[35] In making his work public, he did what mathematical physicists often did: he published the full mathematical theory in a journal read by other mathematical physicists and mathematicians, and he published a résumé containing less mathematics and more calculations of frequencies of notes and locations of nodal lines in the *Annalen*, the journal read by experimental physicists. By the combination of journals, Kirchhoff acknowledged at the same time the unity of physics and the duality of its methods.

Simultaneously with Kirchhoff's "great theoretical and experimental investigation" of the elastic properties of metals, as Boltzmann described it, he continued his investigation of problems in electrical theory. In this work, he gave serious attention to analogies. In 1848, as we have seen, he generalized Ohm's theory of conduction of steady currents in one dimension to conduction of steady currents in three dimensions, accomplishing this "without much difficulty by making use of the analogy with the flow of heat, which had proved so useful to Ohm."[36] In two papers in 1857, Kirchhoff developed equations for the motion of electricity from Weber's electrodynamic law, this time considering the general case of variable not just steady currents; in the first paper he dealt with linear currents, in the second with the motion of electricity in three dimensions. In the first paper, he found that Weber's law yields an expression for the electromotive force at a point on a wire containing a term proportional to the time derivative of the current intensity, and when he combined this term with the electrostatic term from Weber's law and appealed to the conservation of energy principle, he arrived at a wave equation for the motion of electricity with a damping term proportional to the resistance. Although the phenomena of elasticity and electricity are widely different, he recognized a

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[34] Todhunter says that Kirchhoff gave too much emphasis to Poisson's error. He evidently had no criticism of Poisson's method; their differences had to do with the degree of "fitness" attributed to the elastic plates. Thomson and Tait "practically reconciled Poisson and Kirchhoff" (*Elasticity*, 40).

[35] Todhunter, *Elasticity*, 40.

[36] Gustav Kirchhoff, "Ueber die Anwendbarkeit der Formeln für die Intensitäten der galvanischen Strőme in einem Systeme linearer Leiter auf Systeme, die zum Theil aus nicht linearen Leitern bestehen," *Ann.* 75 (1848): 189, in *Ges. Abh.*, 33–48; Edmund Whittaker, *A History of the Theories of Aether and Electricity,* vol. 1, *The Classical Theories* (New York: Harper & Brothers, 1960), 224.

"remarkable analogy" between the propagation of electricity in a closed wire and the propagation of a wave in a longitudinally vibrating elastic rod. In a similar vein, he found that in the limit of infinite resistance, electricity propagates analogously to heat, and in the opposite limit of vanishing resistance, electricity propagates analogously to waves in a taut string and, "to be sure, with the velocity that light has in empty space." Kirchhoff did not develop the analogy between electricity and light, which would prove to be an identity, a curious omission since he took analogies seriously. Kirchhoff's "extraordinary failure," as it has been called, to develop the electrical significance of the velocity of light has to do, in part, with his preference for developing mathematical theory narrowly from experience, which would have stood in the way of his recognition of a connection between electricity and light. It also has to do with Weber's theory, which Kirchhoff supported and used in his research. In that theory, the velocity in question is the relative velocity of two electric particles when they exert no force on one another, a physical event which suggests nothing of an optical nature. The velocity of an electric disturbance depends on the inductance and capacitance of the wire, whereas the velocity of light depends upon the elastic properties of the ether. Moreover, the assumption of vanishing resistance is unrealistic.

Weber as usual was working on the same general problem as Kirchhoff, though with experimental confirmation in mind. In this connection, he considered only the simple case of a circular wire, and finding that the velocity of the motion of electricity depends on the length of the circuit, he could not speak of a definite velocity as Kirchhoff did. Conceding that a physical reason for the coincidence of the two velocities would have "high importance," since it would establish a connection between two branches of physics, electricity and optics, he thought that the true physical meaning of Kirchhoff's velocity "is not of the kind that would allow great expectations," and he did not think that this velocity could be attained experimentally in any event.[37] Like Weber, Kirchhoff thought that the coincidence of the two velocities was without physical significance. Eventually Kirchhoff's laws of motion of electricity entered work on the electrical theory of light, but it was work by others, notably by Ludvig Lorenz and Helmholtz, who took seriously an electrical theory of light for reasons other than Kirchhoff's elastic analogy.

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[37] Ludwig Boltzmann, *Gustav Robert Kirchhoff* (Leipzig, 1888); repr. in *Populäre Schriften* (Leipzig: J. A. Barth, 1905), 22; Gustav Kirchhoff, "Ueber die Bewegung der Elektricität in Drähten," *Ann.* 100 (1857): 193–217, in *Ges. Abh.*, 131–54; "Ueber die Bewegung der Elektricität in Leitern," *Ann.* 102 (1857): 529–44, in *Ges. Abh.*, 154–68; Wilhelm Weber, "Elektrodynamische Maassbestimmungen insbesondere über elektrische Schwingungen," *Abh. sächs. Ges. Wiss.* 6 (1864): 571–716, repr. in *Werke*, vol. 4, *Galvanismus und Elektrodynamik, zweiter Theil*, ed. Heinrich Weber (Berlin, 1894), 105–241, on 157; Leon Rosenfeld, "The Velocity of Light and the Evolution of Electrodynamics," *Nuovo Cimento*, supplement to vol. 4 (1957): 1630–69, on 1635, 1640, attributes Kirchhoff's failure to develop the analogy between light and electricity to his phenomenology. Kirchhoff's avoidance of risky physical hypotheses would have made it difficult for him to anticipate a physical basis for the coincidence of the two velocities (Darrigol, *Electrodynamics*, 71–73).

While at Heidelberg, in addition to elasticity and electricity, Kirchhoff took up a third subject of research, the mechanical theory of heat, which he applied to problems such as the absorption of gases, the solution of salt, and the evaporation of mixtures of acid and water, and in the process he developed theoretical methods of significance for physical chemistry.[38] As he had with his mathematical colleague Königsberger, with his chemistry colleague Robert Bunsen he formed a working relationship, the two joining in what was to be the principal work in chemistry for them both, the development of spectrum analysis.

In the first half of the nineteenth century, efforts were made to recognize chemical substances by placing them in a flame and observing their characteristic colors through prisms or by observing the spectra of electric arcs produced by electrodes fashioned from the substances. For a time, Bunsen studied the flames of salts using colored glasses and solutions to help him distinguish the colors. The gas burner he introduced in the 1850s facilitated this general approach to chemical analysis; by giving a flame that was hot but not bright, it did not interfere with the flame of the substance under observation, which emitted line spectra that were very sharp. Bunsen discussed his work on this problem with Kirchhoff, who pointed the way to a method based on the prismatic resolution of the colors of flames. With their burner and a fine spectral apparatus, Bunsen and Kirchhoff extended the principle of characteristic spectra from gases and metals, where it was already established, to salts, systematically examining the spectra of alkalis and alkaline earths to identify the metals they contain. They found that the location of the discrete bright lines in the spectrum of a given metal is the same regardless of how the metal is combined in salts or how hot the flame is. In this way, they developed a technique for analytic chemistry of unsuspected sensitivity, as Bunsen soon demonstrated by discovering two new metals with it.[39]

In 1859, Kirchhoff made an important observation. It had long been known that the dark D lines in the solar spectrum, which Joseph von Fraunhofer had first observed, were the same as the bright yellow lines in the flame of sodium. When Kirchhoff looked at the solar spectrum through the flame of sodium salt with his prism apparatus, he found a result for which optical theory had no explanation, and

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[38] Gustav Kirchhoff, "Ueber einen Satz der mechanischen Wärmetheorie und einige Anwendungen desselben," *Ann.* 103 (1858): 177–206, in *Ges. Abh.*, 454–82; Pockels, "Kirchhoff," 250–51.

[39] According to Boltzmann, Kirchhoff's first flint glass prism, which he obtained in 1857, had been polished by Fraunhofer, an example of a connection between research by one of our physicists and excellent German instrument makers (Boltzmann, *Kirchhoff*, 4). Gustav Kirchhoff and Robert Bunsen, "Chemische Analyse durch Spectralbeobachtungen," *Ann.* 110 [1860]: 160–89; in *Ges. Abh.*, 598–625. Bunsen told Wilhelm Ostwald the history of the discovery of spectrum analysis, which Ostwald reported in the "Anmerkungen," 71–72, appended to his edition of *Chemische Analyse durch Spectralbeobachtungen von G. Kirchhoff und R. Bunsen* (1860), vol. 72 of Ostwald's Klassiker der exakten Wissenschaften (Leipzig, 1895). William McGucken, *Nineteenth-Century Spectroscopy* (Baltimore: Johns Hopkins University Press, 1969), 26–28, 34, 50. Daniel M. Siegel, "Balfour Stewart and Gustav Robert Kirchhoff: Two Independent Approaches to 'Kirchhoff's Radiation Law,'" *Isis* 67 (1976): 565–600, on 568–69. Pockels, "Kirchhoff," 252–53.

it appeared to him as "something fundamental." If the sunlight was sufficiently strong the dark lines appeared much darker when he observed them through a sodium flame. His explanation was that the dark lines are produced by sodium in the solar atmosphere, which absorbs light of the same wavelength it emits. His general explanation was that a substance that emits a certain spectral line also absorbs the line, depending on circumstances. This opened the possibility of determining the chemical composition of stellar as well as terrestrial bodies by the analysis of absorption spectra, which Bunsen described in a letter to a chemical colleague in 1859: "At the moment I and Kirchhoff are occupied with a common work that does not let us sleep. That is to say that Kirchhoff has made a wonderful, entirely unexpected discovery, in which he has found the cause of the dark lines in the spectrum of the Sun.... By this means the way is given for identifying the material constitution of the Sun and the fixed stars with the same certainty with which we determine S, Cl, and so forth through our reagents. On earth, substances can be differentiated and identified by this method with the same sharpness as in the Sun."[40]

Kirchhoff followed this discovery with a law that applies to all bodies that emit and absorb light and radiant heat, the "theoretical foundation" of his explanation of the Fraunhofer lines. He imagined a body that emits and absorbs heat rays of only one wavelength, and then with recourse to the "general fundamental laws of the mechanical theory of heat," he derived the following law: for heat rays of the same wavelength and for a given temperature, the ratio of emissive to absorptive power is the same for all bodies.[41] He soon published another, more rigorous derivation of the law.[42] He imagined a "perfectly black" enclosure, which absorbs all of the heat rays that strike it. Inside it is an imaginary body, which emits heat and receives heat from the enclosure, a "perfect mirror" that reflects all rays, and a plate that is "perfectly diathermanous" for rays of a given wavelength and polarization but completely reflecting for all other rays. Whether or not these ideal bodies are realizable in the laboratory, they are conceivable in thought, and that is enough, for a law of nature cannot depend on our artificial means.[43] By an artful arrangement of these ideal bodies, Kirchhoff analyzed the exchange of energy between radiant heat and matter in thermal equilibrium. The result is a thermodynamic law as simple as Ohm's galvanic law in its mathematical expression, $E/A = e$. This says

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[40] Henry Roscoe, "Gedenkrede auf Bunsen," in *Gesammelte Abhandlungen von Robert Bunsen*, ed. Wilhelm Ostwald and M. Bodenstein (Leipzig: W. Engelmann, 1904), vol. 1, xv–lix, on xxxiv.

[41] Gustav Kirchhoff, "Ueber den Zusammenhang zwischen Emission und Absorption von Licht und Wärme," *Monatsber. preuss. Akad.* (1859), 783–87; in *Ges. Abh.,* 566–71, on 567, 569–70. Kirchhoff's understanding of absorption and emission was reached independently and at about the same time by Balfour Stewart, resulting in a priority dispute. Siegel, "Balfour Stewart and Gustav Robert Kirchhoff." Gustav Kirchhoff, "Zur Geschichte der Spectral-Analyse und der Analyse der Sonnenatmosphäre," *Ann.* 118 [1863]: 94–111; in *Ges. Abh.*, 625–41.

[42] Gustav Kirchhoff, "Ueber das Verhältniss zwischen dem Emissionsvermögen und dem Absorptionsvermögen der Körper für Wärme und Licht," *Ann.* 109 (1860), 275–301; in *Ges. Abh.*, 571–98, on 571–73.

[43] Kirchhoff, "Ueber das Verhältniss," 573–74; Pockels, "Kirchhoff," 256–57.

that the ratio of emissive power E to absorptive power A of any body is the same as the ratio e for a blackbody; that is, the ratio is the same for all bodies.[44] The law has been called the "triumph" of Kirchhoff's phenomenological method."[45] From the mechanical theory of heat and the laws of optics, he derived a universal law applying to matter and radiation, testimony to the power of theoretical reasoning in physics from general principles. Kirchhoff was not the only one to state the law of radiation that bears his name, but he was the first to derive it rigorously from energy considerations.[46]

An experimental consequence of Kirchhoff's law is that heat rays inside a hollow, opaque body have the same properties as rays from a blackbody, offering a practical means of closely approximating a blackbody in the laboratory. The radiation that leaves a hollow body by a tiny opening has the character of blackbody radiation, the laws of which have "fundamental significance" for physics.[47] In particular, measurements of this radiation give the ratio of emissive to absorptive power of a blackbody as a function of temperature and wavelength. Kirchhoff's work was the "key to the whole thermodynamics of radiation," which in the hands of Planck, Kirchhoff's successor at Berlin, "proved to be the key to the new world of the quanta, well beyond Kirchhoff's conceptual horizon."[48]

In the establishment of spectrum analysis, Kirchhoff and Bunsen's most important contribution was observational. Spectrum analysis first interested chemists and astronomers, but it soon began also to interest physicists, who recognized its contribution to a uniform understanding of matter everywhere. By disclosing the chemical constitution of the heavenly bodies, Voigt said, the method of spectrum analysis reveals the "unity of the universe." Belonging to the "boundary of physics, chemistry, and astronomy," Boltzmann said, spectrum analysis contributes to the recognition of the "unity of natural forces." In addition, Kirchhoff's theoretical work contributed to the connectedess of physics by bringing together two of its branches, thermodynamics and optics, and by providing further support for the "essential identity" of light and radiant heat.[49]

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[44] Kirchhoff defined the "emissive power" E of a body in terms of the kinetic energy it transfers to the ether through heat rays of a given polarization: the intensity of the rays emitted by the body in the wavelength interval λ *to* $\lambda + d\lambda$ in a unit of time is $E\,d\lambda$. He defined the "absorptive power" A of the body as the ratio of the intensity of the absorbed rays to that of the incident rays. Whereas E or A individually depends not only on wavelength but also on the polarization, on the geometry of the slits used in observing the emission and absorption, and on the condition of the body, the ratio E/A is the same for all bodies for a given temperature and for rays of a given wavelength (Kirchhoff, "Ueber das Verhältniss," 574, 592).

[45] Leon Rosenfeld, "Kirchhoff, Gustav Robert," *DSB* 7 (1973): 379–83, on 382.

[46] Voigt, "Kirchhoff," 8–9.

[47] Kirchhoff, "Ueber das Verhältniss," 597–98; Pockels, "Kirchhoff," 257.

[48] Rosenfeld, "Kirchhoff," 382.

[49] McGucken, *Spectroscopy*, 35; Voigt, "Kirchhoff," 8. Boltzmann, *Kirchhoff*, 4.

9.2 Clausius at Zürich and Bonn

Switzerland enters our study of the development of physics in Germany during this period through the establishment of a physics position at the new Zürich Polytechnic. Its first occupant was Germany's outstanding young mathematical physicist Rudolph Clausius, who taught mathematical physics together with technical physics for some years while at the same time carrying out many of his most important researches. After Clausius, the position continued to be held by quickly rising young physicists from Germany such as August Kundt and Friedrich Kohlrausch. Eight years after Clausius vacated the position, it went to H. F. Weber in whose laboratory the student Albert Einstein was to work.

In 1854, the year in which Kirchhoff acquired his first ordinary professorship, at Heidelberg, the community of German scientists was stirred by the news that the Swiss federal government was establishing a technical school at Zürich. The model was not the French technical schools this time but the German polytechnics at Karlsruhe and Stuttgart. The school was a collection of *Fachabteilungen*, or specialized departments, devoted to professional education for the branches of engineering, architecture, and secondary education in science and mathematics. The new school advertised over forty positions in European daily papers and received 189 applications within 3 months, over half of them from Germany. One of the positions was for "general," or experimental, physics intended for the education department, and one was for technical physics.[50] Because the physicists at the polytechnic were to share the collection of instruments and the rooms of the physics professor at the local secondary school, the Zürich Kantonsschule, until it had its own physics institute, the Swiss education Council appointed the current physics professor there, Albrecht Mousson, as first professor for experimental physics at the polytechnic.[51] For the technical physics position they wanted Rudolph Kohlrausch, the extraordinary professor of physics at Marburg University, who had just completed a joint investigation with Weber. They consulted Weber, who wrote that physics provides a center to the two main levers of the polytechnic sciences, mathematical and exact experimental research, and "in this connection the treatment of physics from the point of view of *mathematical physics and higher mechanics* is of special importance for the higher scientific position of a polytechnic school."[52] He said that Kohlrausch would do fine teaching technical physics, but that they could not keep him from pursuing original investigations in basic physics.

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[50] Gottfried Guggenbühl, "Geschichte der Eidgenössischen Technischen Hochschule in Zürich," in *Eidgenössische Technische Hochschule 1855–1955* (Zurich: Buchverlag der Neuen Zürcher Zeitung, 1955), 3–260, on 19, 35, 68; Zurich ETH, *100 Jahre Eidgenössische Technische Hochschule. Sonderheft der Schweizerischen Hochschulzeitung,* 28 (Zürich: Verlag Lecmann, 1955), 46; Studer to Kern, 12 June 1855, A. Schweiz. Sch., Zurich.

[51] Albrecht Mousson to (presumably) Kern, 30 August 1855; Kohlrausch to Kern, 23 January 1855; "Ausszug aus dem Protokoll der 112. Sitzung der schweizerischen Bundesrathes" to "Schulrath der polytechnischen Schule, in Zürich," 24 August 1855; A. Schweiz. Sch., Zurich.

[52] Weber to Kern, 1 January 1855.

Weber was right: Kohlrausch turned down the offer from the polytechnic out of love for "physics without purpose."[53] The Zürich planners then decided to include mathematical physics in the second physics professorship. An inducement to consider a mathematical rather than a technical physicist was the availability of the mathematical physicist Clausius, whom Weber had added to the planners' list of candidates. Weber said that Clausius "next to Neumann in Königsberg and Kirchhoff in Heidelberg" had "excelled in this direction [mathematical physics], especially with his papers on heat theory." Poggendorff added his recommendation of Clausius as the "most suitable" candidate particularly in "mathematical physics." So it turned out that a mathematical instead of a technical physicist was hired as the second physicist at Zürich Polytechnic. To ensure his equality with Mousson and to prevent conflict between them, Clausius was appointed "professor of physics … preferably for mathematical and technical physics and director of the physical exercises." In spite of the wording of the appointment, the planners considered technical physics to be Clausius's main subject and mathematical physics a subsidiary one.[54]

Clausius's position at Zürich included a responsibility that was rare for a mathematical physics teacher: he was co-director with Mousson of a physics collection and, in effect, a co-creator of a new physics institute. He differed from other teachers of physics in another respect too; whereas they usually wanted access to instruments and a laboratory to further their own research, he did his research without using the experimental means that were available to him. Weber in his recommendation of Clausius for the Zürich job said that he did not know if Clausius "had made experimental physics the special object of his work."[55] Clausius had not, but he had practical experience all the same, since in Berlin he had taught experimental physics and, according to a colleague, he had even earned praise as a skillful experimenter.[56] In Berlin he had also associated closely with the experimentalists, with his teacher Magnus, with Dove, and through his work with Riess.[57] His appointment to the Zürich Polytechnic acknowledged his experience as a teacher of experimental physics.

At Zürich, Clausius had to put his practical knowledge of physics to immediate use. As he explained, when he was called there, "I, together with my special colleague, professor Mousson, had to take on the task of equipping a new physical cabinet, for which purpose we received a one-time credit of Fr.40,000 and an additional credit of first Fr.2,000 and later Fr.2,400. Naturally, under the circumstances we were forced to occupy ourselves a great deal with the question of which

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[53] Weber to Kern, 9 May 1855, A. Schweiz. Sch., Zurich. Kohlrausch to Kern, 23 January and 12 February 1855; Weber to Kern, 1 January 1855; Studer to Kern, 30 May 1855; A. Schweiz. Sch., Zurich.

[54] Weber to Kern, 1 January 1855; Poggendorff to Brunner, 18 May 1855; A. Schweiz. Sch., Zurich. "Auszug aus dem Protokoll," 24 August 1855.

[55] Weber to Kern, 9 May 1855.

[56] Georg Sidler to Kern, 31 March 1855, A. Schweiz. Sch., Zurich.

[57] Rudolph Clausius, "Ueber das mechanische Aequivalent einer elektrischen Entladung und die dabei stattfindende Erwärmung des Leitungsdrahtes," *Ann.* 86 (1852): 365, 371.

objects are necessary for a well-equipped physical cabinet and from whom they can best be obtained."[58] Among the first instruments they ordered were expensive measuring apparatus such as Weber's "electrodynamic measuring apparatus," which would serve both experimental work and laboratory exercises for students, and they needed space for working with them. Their "over-full" cabinet[59] in the Kantonschule, to which they added over 400 new pieces of apparatus before they moved to new quarters, their "much used" auditorium, and the unfurnished and unheated small chamber that was their only place for research left them with hardly room to turn around, and no room at all for students to do independent work.[60] They planned to have a new institute within about 2 years, but it took longer than that, 9 years. When they finally did move, they explained that then the most "important direction" in physics was "exact measuring work,"[61] and they accordingly ordered many more measuring instruments. They kept their collection up-to-date by acquiring the apparatus used in the most important current researches in physics, which were "electricity and galvanism." They attended international exhibitions in Paris and London to see and buy the latest equipment on the spot.[62] As a practical physicist at Zürich, Clausius served primarily as an expert on instruments and on institute organization and not as a director of experimental research. Yet, unlike most mathematical physicists then, by the time he left Zürich in 1867 he could claim considerable experience in help running a complete institute. An ambitious physicist at that time could hardly get around the need for such experience, since success meant a university chair and an institute directorship, positions which entailed the responsibility of teaching experimental physics.

The subjects that Clausius found most important for practical applications and that he emphasized in his course on technical physics were those that he treated in his research: machine electricity, by which he meant electricity producing mechanical action and electrolysis, and heat. In his 1850 paper, in which he had laid the foundations for the mechanical theory of heat, he had not discussed molecular motion constituting heat, wanting to keep the results that depend on general principles separate from those that depend on special assumptions. In a paper in 1857, he addressed the topic of molecular motion, which he called the

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[58] Clausius to Würzburg University Senate, 18 May 1867, Clausius Acte, Würzburg UA, Nt. 404.

[59] Mousson, also writing for Clausius, to the President of the Swiss Education Council, 16 November 1857, A. Schweiz. Sch., Zurich.

[60] Mousson to President of the Swiss Education Council, 16 November 1857; Clausius and Mousson, "Bericht über die physikalische Sammlung der polytechnischen Schule 1857," 20 January 1858.

[61] Mousson, writing also for Clausius, "Jahresbericht über die physikalische Sammlung des schweizerischen Polytechnikums für das Jahr 1864," 4 January 1865.

[62] Clausius and Mousson, "Bericht über die physikalische Sammlung des schweizerischen Polytechnikums 1861," 6 January 1862; Ferdinand Rosenberger, *Die Geschichte der Physik*, vol. 3, *Geschichte der Physik in den letzten hundert Jahren* (Braunschweig, 1890; repr. Hildesheim: G. Olms, 1965), 473; "Bericht über die physikalische Sammlung der polytechnischen Schule für das Jahr 1858," 11 January 1859.

"fundamental problem of thermodynamics." It had been studied as far back as the eighteenth century, though not systematically. The prevailing theory at that time held that the particles of gases are stationary, bound in position by attracting and repelling forces. This static theory came to be combined with the imponderable fluid of heat, later called caloric, in the caloric theory of gases. In the first half of the nineteenth century, John Herapath and John James Waterston proposed kinetic theories in place of the static theory, and Joule accepted Herapath's ideas about gas particles in motion, but until the time of Clausius's work, the caloric theory remained the main theory.[63]

Clausius was induced to make public his ideas on molecular motion by a paper published the year before by August Krőnig, whose ideas were similar to, if somewhat simpler than, his own, The two physicists pictured gas molecules as elastic spheres that do not vibrate about equilibrium positions as they do in the static theory but move uniformly in straight lines until they collide with other molecules or with the walls of their container. To start with, Clausius introduced three assumptions about the molecules, their forces, and their motions, which together define an "ideal gas": molecules are sufficiently small that the volume they occupy can be ignored; the duration of a collision is small compared with the time between collisions; and the attractive forces between molecules are negligible. To develop a mathematical theory from these assumptions, Clausius ascribed a single, mean velocity u to all of the molecules in a container, although he recognized that different molecules probably have different velocities. By applying the laws of mechanics to the molecules in the container, he derived the basic equation of the theory, the ideal gas law: $p = nmu^2/3v$, where p is the pressure of the gas, v is the volume, n is the total number of molecules, and m is the mass of an individual molecule. Clausius did not know either of the key molecular quantities, n and m, but he knew their product, the total mass of the gas, which enabled him to calculate the mean velocities of different gases under normal atmospheric conditions. He considered only the translational motion of the molecules in the derivation of the ideal gas law, but he also recognized that the translational kinetic energy does not account for all of the heat of a gas. It turns out that the proportion of translational kinetic energy to total kinetic energy is a function of the ratio of specific heats of the gas at constant volume and at constant pressure, and measurements of specific heats

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[63] Rudolph Clausius, "Ueber die Art der Bewegung, welche wir Wärme nennen," *Ann.* 100 (1857): 353–80. This and later writings by Clausius on the kinetic theory are discussed in Stephen G. Brush, *The Kind of Motion We Call Heat: A History of the Kinetic Theory of Gases in the 19th Century*, vol. 1, *Physics and the Atomists* (Amsterdam and New York: North-Holland, 1976), 168–82; Edward E. Daub, "Rudolf Clausius and the Nineteenth Century Theory of Heat" (Ph.D. diss., University of Wisconsin-Madison, 1966); and "Atomism and Thermodynamics," *Isis* 58 (1967): 293–303; Elizabeth Wolfe Garber, "Maxwell, Clausius and Gibbs: Aspects of the Development of Kinetic Theory and Thermodynamics" (Ph.D. diss., Case Institute of Technology, 1966); and "Clausius and Maxwell's Kinetic Theory of Gases," *HSPS* 2 (1970): 299–319; Martin J. Klein, "Gibbs on Clausius,"*HSPS* 1 (1969): 127–49; Peter M. Harman, *Energy, Force, and Matter: The Conceptual Development of Nineteenth-Century Physics* (Cambridge: Cambridge University Press, 1982),128.

showed that in the more complex gases, part of the total kinetic energy must be accounted for by internal molecular motions. In his analysis of the problem, Clausius made use of the equipartition theorem from Waterston and Maxwell, which says that the energy of a gas is divided equally among its degrees of freedom, or the number of independent ways a physical system can move. For the case of translational, or simple rectilinear, motion, the number of degrees of freedom is three, corresponding to the three dimensions of space. Clausius showed that for the theory to agree with the specific heat measurements, the number of degrees of freedom needed to be five, evidence of a more complex motion including rotation and vibration. His study in 1857 contributed to what would become a distinct branch of physics, the kinetic theory of gases, extending the mechanical connectedness of the parts of physics. Over the next 15 years, Clausius, Maxwell, and Boltzmann would further develop the theory and its outgrowth, statistical mechanics. The agreement of the kinetic theory with experiments on the pressure, viscosity, and other properties of gases was taken as evidence for the probability of the molecular hypothesis. Clausius regarded molecules as real physical bodies. However, direct evidence of the molecular motions identified with heat had to wait for Einstein's analysis of Brownian motion in 1905.

In a sequel publication the following year, Clausius responded to recent criticisms of molecular theories, his own theory as well as Joule's and Krőnig's. He took seriously a question that the Dutch meteorologist and chemist C. H. D. Buys-Ballot posed: if molecules travel in straight lines at great velocities, as Clausius maintained, then different gases in contact with one another ought to mix quickly, and if so, why does tobacco smoke hang in layers, and why does it take minutes to detect the odor of chlorine when it is released across the room? In his answer, Clausius considered intermolecular forces, and he introduced probabilistic reasoning, which he had used previously in treating the reflection of light from particles in the atmosphere. To begin with, he determined the probability that a molecule will move a distance x through a gas, say, the air in a room, without entering the sphere of action of another molecule, as determined by intermolecular forces. By assuming that the other molecules of the gas are at rest and are distributed with uniform density through the room, he showed that the probability is $e^{-\alpha x}$, where α is a constant to be determined. If a number of free molecules are projected at the gas, a certain fraction will collide with the first layer of fixed molecules, another fraction with the second layer, and so on. If each of these fractions is multiplied by the corresponding path length, and if the sum of their products is divided by the total number of free molecules, the result is the "mean free path" between collisions. Clausius showed that this path is $l' = 1/\alpha = \lambda^3/\pi\rho^2$, where λ Is the mean separation between neighboring molecules, and ρ is the radius of the sphere of repulsive action of molecules. If the fixed molecules in this example are allowed to move with a common velocity, the path, denoted by l, is reduced by a factor of 3/4. Because neither λ nor ρ was known, Clausius supposed as a best guess that the volume of the gas is 1000 times greater than the volume of all the spheres of action, in which case, $l = 61\lambda$. All physical and chemical evidence pointed to an extremely small value for λ, so that the mean free paths of molecules must be small too, and the probability that a molecule will travel far beyond its mean free path is small. This result shows why, Clausius concluded, that a cloud of smoke in a room retains its shape for a long time and why the molecular theory of gases is credible.[64]

Clausius's most important contribution to the molecular theory of gases was the concept of "mean free path," which, Maxwell said, opened a "new field of mathematical physics."[65] Stimulated by Clausius's work, Maxwell improved Clausius's model of a gas, applying a statistical analysis to the distribution of velocities rather than considering only Clausius's average values. The two physicists entered into a critical exchange, in the course of which Clausius used the mean free path to give an early rigorous treatment of the conduction of heat in gases, and Maxwell used it to derive equations for transport phenomena of gases and other phenomena. In response to Clausius's criticism of his assumptions in deriving the velocity distribution law, Maxwell provided a new derivation in which he considered the effects of collisions. In 1873, Maxwell distinguished the statistical method of treating large aggregates of molecules from the dynamical method of treating individual molecular motions, associating the theory of gases with the former.[66] Only once, in 1874, did Clausius use Maxwell's distribution law, and he seems to have rejected it in the end. He accepted the idea of the disorder of molecular motions, and he recognized the error in likening molecular motions to ordered motions, such as those resulting from central collisions in a line of elastic spheres, but unlike Maxwell he never fully valued the statistical approach. He relied on analytical mechanics together with molecular assumptions in his continuing work on the mechanical theory of heat. He preferred to study order where it seemed possible rather than disorder.

From the standpoint of the mechanical view of nature, if bodies consist of molecules acting on one another by distance forces, the two fundamental laws of the mechanical theory of heat should appear as a consequence of the "general principles of mechanics in their application to those molecular systems." Helmholtz had shown how molecules interacting by forces lead straightaway to the first law of thermodynamics, but the connection of the second law with molecular-mechanical principles was not as evident. Clausius, who thought that it was necessary to discover the "hidden connection" to "prove the possibility of the mechanical conception for the whole area of physical phenomena,"[67] gave a mechanical explanation of the second law in 1870. The next year he derived a mechanical analog of entropy, the term he had introduced in 1865 for the quantity entering the second law of thermodynamics. Maxwell regarded this work as misconceived, for he saw the second law as essentially statistical.[68]

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[64] Rudolph Clausius, "Ueber die mittlere Länge der Wege, welche bei der Molecularbewegung gasförmiger Körper von den einzelnen Molecülen zurückgelegt werden; nebst einigen anderen Bemerkungen über die mechanische Wärmetheorie," *Ann.* 105 (1858): 239–58.

[65] Quoted in Daub, "Rudolf Clausius," 127.

[66] Harman, *Energy, Force, and Matter*, 131, 133.

[67] Riecke, "Clausius," 17, 20–22.

[68] Klein, "Gibbs on Clausius," 148; Daub, "Rudolf Clausius," 142–43; Garber, "Clausius and Maxwell's Kinetic Theory," 307–9, 317; Brush, *Motion We Call Heat*, vol. 1, 181–82; Harman, *Energy, Force, and Matter*, 138–41.

In 1864, Clausius published a collection of his papers on the mechanical theory of heat, which served as an early introduction to the subject. He did not include all of his papers, but only those in which the basic theory was developed from "simple fundamental laws." In particular, he withheld papers that made use of hypotheses about molecular motions or that dealt with electricity, reserving them for future volumes of an intended comprehensive treatise. Twelve years later he brought out a second edition, called for, he explained, because the mechanical theory of heat had become an "extensive and independent branch of science," which could not easily be learned from a collection of original papers. This time he completely reworked it to form a "connected whole," so that it was now a proper "textbook" for the subject.[69]

Soon after Clausius began his new job at Zürich, he gave an address on the nature of heat. Heat, both radiant heat and heat in bodies, is a form of the motion of particles, he explained. When the ponderable molecules of a body vibrate, they set up progressive waves in the finely particulate ether, and when these waves strike another body, they in turn set up vibrations in its ponderable molecules; the moving molecules, which all bodies are constituted of, fill the universe with ethereal vibrations proceeding in all directions. Heat is the "true moving principle" without which all bodies would be brought to an equilibrium state by mutual forces, and the Earth would become dead and unmanageable. Wherever there is motion or life, there is heat; heat drives steam engines, causes the weather, and in general runs the "great machine of nature."[70] That is the universal meaning of heat, of compelling interest to both the physicist who determines its laws and the engineer who applies them. Matter and motion underlie the conception of the physical world, and Clausius's researches at Zürich on the kinetic theory of gases were a further display of that conception.

Clausius's researches at Zürich made him an attractive candidate for jobs in Germany, and in 1867 he accepted a call to Würzburg, a small university in Bavaria. There as head of the physics institute he brought the physical cabinet up to date, building an instrument collection that could meet "all demands of modern times." In addition to teaching the customary five-hour experimental physics course, he introduced mathematical-theoretical courses, though he lacked students

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[69] Rudolph Clausius, *Abhandlungen über die mechanische Wärmetheorie. Erste Abtheilung* (Braunschweig, 1864). Quotations from "Vorrede," v–x. The second revised edition is entitled *Die mechanische Wärmetheorie*, vol. 1, *Entwickelung der Theorie*, *soweit sie sich aus den beiden Hauptsätzen ableiten lässt, nebst Anwendungen* (Braunschweig, 1876). Quotations from the preface to the English translation of the second edition by W. R. Browne, *The Mechanical Theory of Heat* (London, 1879), vii–viii.

[70] Rudolph Clausius, *Ueber das Wesen der Wärme, verglichen mit Licht und Schall* (Zurich, 1857). Quotations on 29, 31.

with sufficient mathematical preparation for his advanced courses.[71] He left after 2 years for a better opportunity at Bonn University.

At Bonn, Julius Plücker had long represented both physics and mathematics, and when he died in 1868 the Bonn science faculty wanted to use the physics half of his position to acquire an experimental physicist who could secure for their university an ample physics institute. To meet this "long felt need," they looked for a "skilled and comprehensively trained director of the physical laboratory," who could organize a physics practice course for elementary students and direct the laboratory research of advanced students. They wanted him also to have a command of theoretical methods of "fathoming the laws of nature." The candidates were Adolph Wüllner, the local physicist who had given Bonn students laboratory practice at the agricultural academy where he taught, Wiedemann, and Quincke, all three of them former students of Magnus's.[72]

The Bonn mathematician Rudolf Lipchitz agreed with most of what his colleagues wanted, but he had a higher ambition for the university, a physicist who "was opening new paths to scientific knowledge."[73] The men who fit that description best, in his view, were three physicists who were not on the faculty's list, Helmholtz, Kirchhoff, and Clausius. The Bonn scientists considered his alternatives. Helmholtz, they acknowledged, was a physicist of the first rank, but he was still employed as a physiologist at Heidelberg University, and he had never run a physics institute. They noted that the physics researches on which Helmholtz's reputation was based were "especially in a mathematical direction" rather than in an experimental direction. Moreover, they thought that it was "very questionable" that Helmholtz would accept a call to Bonn, and his "world reputation" as a physiologist would in any case make him expensive, creating problems.[74] Kirchhoff, the Bonn

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[71] *Verzeichniss der Vorlesungen ... Würzburg*. Würzburg U. Senate to Bavarian Ministry of the Interior (draft), 13 January 1869, Clausius Personalakte, Würzburg UA, Nr. 404. Bavarian Ministry of the Interior to Würzburg U. Senate, 14 March and 23 March 1867; Clausius to Würzburg U. Senate, 18 May 1867; Würzburg University sent it to the Bavarian Ministry of the Interior, 5 October 1868; Würzburg U. Senate to Bavarian Ministry of the Interior (draft), 13 January 1869.

[72] Draft of the report by the mathematics and natural sciences section of the Bonn U. Philosophical faculty on a successor in physics to Plücker, July 1868, Plücker Personalakte, Bonn UA. Also there, "Separatvotum als Erwiderung auf dasjenige des Herrn Professor Lipschitz von 13ten Juli 1868," 14 July 1868.

[73] Draft of Lipschitz's report on the same question, July 1868, Plücker Personalakte, Bonn UA.

[74] Bonn U. faculty report in July 1868, "Separatvotum" of 14 July 1868, and Lipschitz's separate vote, 13 July 1868. After Plücker's death, Lipschitz had written to Helmholtz to ask him if he wanted to move to physics and to come to Bonn. On learning that Helmholtz did, Lipschitz proposed to the Bonn philosophical faculty that they place Helmholtz first on the list of candidates for the physics chair. Neumann supported Lipschitz's efforts to bring Helmholtz to Bonn, and Lipschitz reported to Neumann that the government had done "everything in its power" to get him. Lipschitz to Neumann, 14 June 1868 and 15 January 1869, Neumann Papers, Göttingen UB, Ms. Dept. Since Helmholtz had already told the Bonn philosophical faculty of his willingness to come, their doubts about his availability appear to have reflected more their disinclination than their poor chance of getting him. Helmholtz declined the call in the end because he felt that the

scientists thought, was too tied to Heidelberg and to his chemistry colleague Robert Bunsen to be lured to Bonn, so they did not consider him further.[75] Clausius, they thought, was in no manner qualified for the main objective, the establishment of a physics institute at Bonn, since he had no experience in directing a laboratory and no experimental work to his credit.[76]

The disagreement within the Bonn science faculty resulted in a compromise: on the science faculty's list of candidates, the names of Helmholtz and Clausius would precede those of the three experimentalists. The intention of the majority of the science faculty appears to have been, and Lipchitz took it to be, that Wüllner's appointment would be assured in this way. Their arguments for Helmholtz and Clausius were flattering but brief, almost in the style in which faculties often mentioned the very best but unattainable men in the field before getting down to realistic proposals.[77] They no doubt assumed that if he were asked at all, Helmholtz would certainly refuse, and that the Prussian ministry of culture would not invite Clausius because he lacked laboratory experience. Helmholtz did refuse, but contrary to expectations, the ministry went next to Clausius, who accepted the call. Even though Clausius was a theorist in his research, he gave Bonn a physics institute of sorts, which is what the science faculty wanted of their physicist.[78] With Clausius's arrival at Bonn, Eduard Ketteler, who had been Privatdocent for physics at Bonn since 1865, found himself in the position, now becoming standard, of the second physicist responsible for theory,[79] though Clausius would also teach the subject.

While he was at Bonn, Clausius received calls to other universities, which allow us to see how the theorist Clausius tried to tailor the conditions of his work to acknowledge his specialty. When he accepted the job at Bonn, he understood that one of his obligations was the "establishment and direction of a physical

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Prussian ministry of culture was trifling with him. Helmholtz to Carl Ludwig, 27 January 1869, quoted in Königsberger, *Helmholtz* 2: 118–19.

[75] Kirchhoff for that reason was never placed on the list of candidates at Bonn. Lipschitz to Neumann, 14 June 1868.

[76] Faculty reports cited above. Also Lipschitz to Neumann, 14 June 1868; in this letter, Lipschitz mentioned that he also suggested Meyer as a candidate, but the Bonn faculty rejected him, as it initially did Clausius, for not being an experimental physicist. The Bonn faculty did not count Clausius's current, brief tenure as Würzburg's physics professor or his earlier co-directorship of the physics institute at Zurich.

[77] Mathematics and natural sciences section of Bonn U. Philosophical Faculty to "Prodecan" Knoodt, 9 July 1868, and its "Separatvotum" in reply to Lipschitz, 14 July 1868, Plücker Personalakte, Bonn UA.

[78] Clausius to Bonn U. Curator Beseler, 12 March 1869, and documents giving details of Clausius's appointment, Clausius Personalakte, Bonn UA.

[79] Ketteler to Dean of the Bonn U. Philosophical Faculty H. von Sybel, 27 June 1870, Ketteler Personalakte, Bonn UA. Ketteler's next publication was "of a more mathematical and critical nature," according to Clausius, who also said that Ketteler was "longing" for a physical cabinet. Clausius's recommendation of Ketteler for a position at the Karlsruhe Polytechnic, 29 October 1870, Bad. GLA, 448/2355.

laboratory," an extension of the laboratory practices that his predecessor Plücker had introduced 2 years earlier. He "most definitely" intended to direct the Bonn seminar for the natural sciences, where he would have abler students; as he had explained to the curator of Würzburg University, the "decisive reason" he was leaving for Bonn was the "better conditions for advanced studies among the students at Bonn."[80] As for lectures, he wanted to devote roughly equal time to the two parts of physics, experimental and theoretical, but the Prussian requirement that the ordinary professor give a series of public lectures each semester upset his plan somewhat.[81] He received a large, one-time grant for instruments,[82] and he argued for a new institute building, but he succeeded only in getting the institute moved, and then not until 1885, into a space vacated by the surgical clinic in a wing of the main university building. The new quarters were cramped and uncomfortable, especially the cellar-like laboratory rooms, which were described by Clausius's successor at Bonn, Hertz: after a hard rain, water ran down the institute walls all day and one worked with an umbrella. The maximum number of students in the laboratory practice course, 14, was reduced to six because there was no more room.[83] When Clausius received a call from Strassburg University in 1871, he asked Bonn to relieve him of some of the burden of teaching, which until then had included lecturing in both experimental and mathematical physics and directing both the physical seminar and the laboratory exercises, though in the latter it seems that he received help from Ketteler. Clausius asked only for a small sum of 500 thaler to pay an extraordinary professor or Privatdocent to conduct the physics exercises. Clausius could then, besides attending to experimental physics, "devote more time to mathematical physics, which would be useful for the university."[84] He wanted to assign the supervision of the exercises to one of the younger physicists already at Bonn, one semester at a time, which would keep the assistant dependent

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[80] Clausius to Bonn U. Curator Beseler, 12 March 1869, Clausius Personalakte, Bonn UA.

[81] For his first semester at Bonn, in the summer of 1869, Clausius originally intended to teach "optics, electricity, and magnetism treated experimentally, 5 hours per week" and "heat theory treated mathematically, 4 hours per week." He changed his schedule to an elementary public 2-h lecture course on the mechanical heat theory, a 2-h lecture course on elasticity theory and the theory of elastic oscillations treated mathematically, and the 5-h experimental lecture course he had proposed before. Clausius to Bonn U. Curator Beseler, 27 and 29 January 1869, Clausius Personalakte, Bonn UA.

[82] Heinrich Konen, "Das physikalische Institut," in *Geschichte der Rheinischen Friedrich-Wilhelm-Universität zu Bonn am Rhein*, by Bonn University, ed. A. Dyroff (Bonn: F. Cohen, 1933), vol. 2, 348–49. Clausius improved the budget in 1871 and 1873; with extraordinary grants, he bought, for example, a goniometer, an air pump, a microscope, and various electric and magnetic apparatus.

[83] Barbara Jaeckel and Wolfgang Paul, "Die Entwicklung der Physik in Bonn in 1818–1968," in *150 Jahre Rheinische Friedrich-Wilhelms-Universität zu Bonn 1818–1968* (Bonn: H. Bouvier, Ludwig Röhrscheid, 1970), 91–100, on 93. Konen, " Das physikalische Institut," 349. Hertz to his parents, 5 April 1889, quoted in Heinrich Hertz, *Erinnerungen, Briefe, Tagebücher*, ed. M. Hertz and Charles Süsskind, 2nd rev. ed. (San Francisco: San Francisco Press, 1977), 288.

[84] Clausius to Bonn U. Curator Beseler, 24 December 1871, Clausius Personalakte, Bonn UA.

on him for his position in the institute. He wanted to retain complete control of the apparatus, as he did; he lent apparatus to others only sparingly, assuring that the apparatus remained in good condition, but it did not make him a particularly successful institute director.[85] In 1883, he was offered Listing's professorship for theoretical physics at Göttingen, which gave him another chance to bring his teaching at Bonn more in line with his research. He let it be known at Bonn that he would prefer a professorship that would require him to lecture on theoretical physics only, since he saw his "real scientific calling in the exclusive occupation with this subject." The second professorship would be for theoretical physics, so that in effect Clausius would receive a new call to Bonn to fill it. The Prussian government did not create the second professorship for him, nor did Clausius leave Bonn. There was a persuasive argument against his not moving, for at Göttingen he would have received far less in student fees than he now got at Bonn, where he taught the popular experimental course. For staying at Bonn, he was also given a raise.[86]

At the time of his move to Bonn, Clausius was drawn to problems of electrodynamic theory. Having long been interested in a variety of electrical topics, approaching the subject from the side of the mechanical theory of heat, he now entered a "second electrodynamic epoch" in his research, as Riecke described it. The subject was in an unsettled state. Weber's theory was being extended, modified, and challenged, and Riemann, Carl Neumann, and E. Betti proposed alternative theories of finitely propagated electric action. Clausius was very interested in their theories, but each of them had made a fatal mathematical mistake, leading Clausius to conclude that the "solution of the problem of referring electrodynamic forces to known electrostatic forces has not been attained."[87] In 1875, he introduced a "new fundamental law of electrodynamics," which did not make use of finitely propagated electric action, and for the next several years, he elaborated his theory.[88]

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[85] Clausius to Beseler, 24 December 1871. Konen, "Das physikalische Institut," 349.

[86] Lipschitz to Bonn U. Curator, 12 May 1883, Clausius Personalakte, Bonn UA. Clausius's salary was 2700 thaler, the equivalent of 8100 marks; after his call to Göttingen, his salary was raised to 9000 marks.

[87] Rudolf Clausius, "Upon the New Conception of Electrodynamic Phenomena Suggested by Gauss," *Philosophical Magazine* 37 (1869): 445–56, on 445, 456. Clausius did not consider Gauss's law because it did not conserve energy.

[88] In 1879 Clausius reworked his electrical researches to include electrodynamic phenomena in *Die mechanische Behandlung der Elektricität* (Braunschweig, 1879), which he presented as the second volume of *Die mechanische Wärmetheorie*. This work contained, for example, his theories of dielectrics, electrolytic conduction, thermoelectricity, and his fundamental theory of electrodynamics. Eduard Riecke, "Rudolf Clausius," *Abh. Ges. Wiss. Göttingen* 35 (1888): appendix, 1–39, on 24. Walter Kaufmann, "Physik," *Naturwiss.* 7 (1919): 542–48, on 546. Rudolph Clausius, "Ueber ein neues Grundgesetz der Elektrodynamik," *Sitzungsber. Niederrhein. Ges.* (1875): 306–9; translated as "On a New Fundamental Law of Electrodynamics," *Philosophical Magazine* 1 (1876): 69–71.

At the beginning of his paper on the new law, Clausius referred to Helmholtz's objection to Weber's law, and he stated reasons of his own for coming to believe that Weber's law "does not correspond to the reality." He doubted that a current consists of Weber's opposing streams of electricity instead of a single stream, and he also doubted that the force between moving electric particles acts only along the line joining them. If Newton had considered electrodynamic forces, he speculated, he would have seen that Weber was unjustified in his assumption that the force between particles is independent of the directions of their motions. Riemann assumed that the forces acting on the two particles are equal and opposite – parallel but not necessarily in a line – but even this assumption, Clausius reasoned, was too restrictive. In electrodynamics, he was prepared to proceed without Newton's law of equality of action and reaction, which Weber and Riemann retained; he observed that this law is founded on our experience with static forces only, and so the analogy with the static gravitational force is misleading in electrodynamics.

To construct his electrodynamic law, Clausius identified the static contribution with the Coulomb interaction between electric particles, and he assumed that the dynamic contribution is linear and bilinear in the absolute velocities of the particles and linear in their absolute accelerations. By adopting unitary currents rather than Weber's dualistic currents, applying the known force between closed currents, and introducing the principle of conservation of energy, he arrived at a simplified expression for the dynamic force. He then compared the potential he derived for the force with the potentials of the rival electrodynamics, which were, in his opinion, Weber's and Riemann's. He found his potential to be the simplest, an argument in its favor, and to have greater generality, its main advantage. He had derived it by assuming that only one electricity moves, but he showed that it is still valid if both electricities move and even if they move with different speeds, as they do in electrolytic conduction.[89] He showed that his law gives the right ponderomotive and electromotive forces between two linear conductors and currents. Since his law is expressed in terms of absolute motions of electric particles, it presupposes a stationary ether, which is convenient, since the ether can contain the momentum necessary to preserve Newton's third law after all; for the same reason,

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[89] The dynamic part of Clausius's potential is:

$$\mathrm{V} = \frac{kee'}{r}\left(\frac{dxdx'}{dtdt} + \frac{dydy'}{dtdt} + \frac{dzdz'}{dtdt}\right),$$

where k is is a contant depending on units, e and e' are electric masses, r their separation, and dx/dt, dy/dt, $dz./dt$ and dx'/dt, dy'/dt, dz'/dt their absolute velocities. If ε is the angle between the absolute velocities v and v', the potential acquires the simpler form: $V=(kee'/r)\, vv' \cos \varepsilon$. Upon applying his law to the interaction of two current elements, Clausius arrived at the same force that the mathematician Hermann Grassmann had published many years before, "Neue Theorie der Elektrodynamik," *Ann.* 64 (1845): 1–18; in light of their "entirely different" starting points, Clausius considered their agreement an "encouraging corroboration." Clausius, *Die mechanische Behandlung der Electricität* (Braunschweig, 1879), 227–81, on 276–77.

not even energy has to be conserved, though Clausius's law does conserve energy. Although he acknowledged the need for the ether to justify the absolute motions, he did not attribute specific properties to it or attempt to locate its momentum.[90]

Even though he worked within an atomistic framework, Clausius again showed his preference for the general formulation of laws. We will review his reasoning. He believed that a fundamental law of electrodynamics is an elementary law between electric particles, accepting the standard method of German electrodynamics. His starting point was a general mathematical expression for the force between a pair of electric particles. Calling on a limited number of experimentally confirmed laws of electricity, he simplified the formula for the force and determined that it was the "only possible one" under the assumption of only one moving electricity. He further simplified the formula by requiring the force to satisfy the principle of conservation of energy. The potential for the force contained an undetermined function of position, which he eliminated with the help of two mathematical assumptions: "on probability grounds," the coordinate for the position should enter the different terms in the potential raised to the same power, and the resulting formula should be "as simple as possible." In his derivation of expressions for the force and the potential, Clausius reduced the element of arbitrariness by appealing to experience, avoiding presuppositions, seeking generality, choosing simplicity, and, in general, following the model of good theories.[91] Like his writings on heat, his writings on electrodynamics offered close readers a lesson on how to do theoretical physics.

In his inaugural address as rector at Bonn in 1884, Clausius talked about the "inner connection" between the "natural forces," or the "great agents of nature." He dwelt at length on the connection of heat and electricity, the two agents to which he had devoted the most research. Despite frequent claims to the contrary, heat and electricity have not, to Clausius's satisfaction, been reduced to a single agent; electricity is still elusive. It is known that electric currents set atoms into motions constituting heat, and, conversely, that thermal motions of atoms give rise to electric currents, but from this familiar transformation of one kind of motion into another we cannot, Clausius cautioned, draw inferences about the nature of electricity. Insight into electricity comes from another connection, that between radiant heat and light. According to Weber's law, if two unlike electric particles move in parallel directions with uniform velocities, they mutually attract by their

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[90] Clausius, "On a New Fundamental Law," 69; "Ueber das Verhalten des elektrodynamischen Grundgesetzes zum Princip von der Erhaltung der Energie und über eine noch weitere Vereinfachung des ersteren," *Sitzungsber. Niederrhein. Ges.* (1876): 18–22, translated as "On the Bearing of the Fundamental Law of Electrodynamics toward the Principle of the Conservation of Energy, and on a Further Simplification of the Former," *Philosophical Magazine* 1 (1876): 218–21, on 218–19. Clausius applied the new law and responded to criticism of it in *Die mechanische Behandlung der Electricität*, chaps. 10 and 11.

[91] Specifically, Clausius required that the expressions agree with the laws for closed currents and the energy principle and with the internal theoretical criteria of simplicity and generality. This is a general method in theoretical physics. Forty years after Clausius, Einstein looked for the simplest equations that the principles of general relativity allowed, reducing the role of ad hoc assumptions ("What is the Theory of Relativity," 1919, in *Ideas and Opinions* [New York: Dell, 1973], 222–27, on 223).

electrostatic force and mutually repel by their electrodynamic one, and the repulsion and attraction become equal and cancel at a definite relative velocity, which according to Weber and Kohlrausch's measurement is the velocity of radiant heat and light in empty space. The agreement of a magnitude belonging to electricity with a magnitude belonging to heat and light "cannot be without an inner reason," Clausius said. This and related agreements "leave no doubt that in the propagation of light or, what is the same, in the propagation of radiant heat, electric forces must be acting." To be sure, physicists have already begun to connect the agents of light and electricity: whereas before, they derived the equations for the propagation of light from the elastic forces of the ether, Maxwell has recently derived the same equations from electric forces, founding an "*electrodynamic* or, as he calls it, an *electromagnetic* theory of light." Clausius did not know if Maxwell's assumptions are correct, observing that they have not been established by mechanical considerations, but he allowed that if it could be shown that radiant heat and light are explained by electric forces, as Maxwell claims, the ether would have to be viewed as nothing but electricity. Today, Clausius said, only two substances are assumed to exist, electricity and matter; everything else is explained by motion. This impressive internal connectedness came about through the perfection of the theoretical conception of nature. This much Clausius was confident of.[92]

In contrast to the lively and forceful lecturers around him, Clausius had a quiet, somewhat dry manner," but owing to the depth of the content of his lectures, especially in his preferred area, the theory of heat, he made a lasting impression.[93] He was a highly productive researcher and a keen critic, often engaging in polemics. He did not form a school of followers, but his writings influenced young physicists. Described at the start of his career as locked in his own thoughts, in his research he followed his own path. As the first prominent German physicist to publish only on theory, he was a specialist before the profession of theoretical physics was recognized. At the time he died, in 1888, the partial separation between experimental and theoretical physics, which was foreshadowed by his career, was accepted and in place, and German physicists could then speak of a higher unity.

9.3 Weber at Göttingen

In 1866 Weber told the Göttingen curator that it was the "purpose of the [physics] institute that scientific researches be carried out there by various persons, by teachers and students."[94] It was understood that the new assistant in the institute,

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[92] Rudolph Clausius, *Ueber den Zusammenhang zwischen den grossen Agentien der Natur*, Rectoratsantritt, 18 October 1884 (Bonn, 1885), 20–27.

[93] Grete Ronge, "Die Züricher Jahre des Physikers Rudolf Clausius," *Gesnerus* 12 (1955): 73–108, on 82.

[94] Weber to Göttingen U. Curator, 29 December 1866, Göttingen UA, 4/Vh/10.

Friedrich Kohlrausch, would not only direct laboratory practice exercises and take care of the instruments but also do research of his own, with Weber offering him space in the institute and use of its instruments. Kohlrausch was also to assist Weber in his own researches "at all times," meaning primarily carrying out assignments in the magnetic observatory. Kohlrausch had an office in the institute, where, as Weber's demanding job description specified, he was to appear at nine o'clock every morning.[95]

As expected, Kohlrausch published frequently on researches he carried out in Weber's institute, making him an attractive candidate for positions elsewhere. As he received offers and inquiries repeatedly over the next several years, Weber made efforts to keep him at Göttingen, where he was needed. The first offer came from an agricultural college after Kohlrausch had been at Göttingen for only a few months. To keep him, Weber asked the government to establish a new physics position at Göttingen. To justify it, Weber compared the arrangements there for physics with those for chemistry: whereas chemistry had four extraordinary professors, physics as yet had none, despite the corresponding need in physics for laboratory instruction, which included the chemists' need for instruction in physics. If Kohlrausch were allowed to leave Göttingen, Weber explained, physics laboratory instruction there would be seriously disturbed. He added, appealing to the state's concern for its property, that the proper use of valuable instruments could not be guaranteed if Kohlrausch were no longer there to oversee them.[96] In Weber's opinion, the laboratory practice course had "greater importance than all the lectures on physics."[97] He had an even more important reason for keeping Kohlrausch, his research. In this connection, and in keeping with his lifelong way of working, Weber stressed his and Kohlrausch's collaboration: their "common researches," he warned the curator, "would not merely be disturbed but completely frustrated." Weber hoped to carry out a great research with Kohlrausch as he had done with his father, Rudolph, which would bring wide notice to Kohlrausch's "skill and fine scientific sense." Weber's arguments were effective, and Kohlrausch was appointed extraordinary professor at Göttingen in February 1867. Certain that other job offers for Kohlrausch would soon follow, Weber even then reminded the ministry that for their research they needed to be "assured of our collaboration for a longer time."[98]

When Kohlrausch expected a call to Würzburg as Clausius's successor and again when the Zürich Polytechnic offered him Clausius's old position there (which had

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[95] Weber to Göttingen U. Curator, 18 October 1866, Göttingen UA, 4/Vh/21.

[96] Kohlrausch was offered the ordinary professorship for mathematics and physics at the agricultural academy in Hohenheim. As a counteroffer, Weber proposed that Kohlrausch be given an extraordinary professorship carrying a small salary in addition to his assistantship. Silcher to Kohlrausch, 24 January 1867; Weber to Göttingen U. Curator, 29 January 1867; Kohlrausch Personalakte, Göttingen UA, 4/V b/156.

[97] Weber to an official, 17 June 1870, STPK, Darmst. Coll. 1912.236.

[98] Weber to Göttingen U. Curator, 29 January and 15 February 1867; Prussian Minister of Culture von Mühler to Kohlrausch, 19 February 1867; Kohlrausch Personalakte, Göttingen UA, 4/V b/156.

since become Kundt's position), Weber thought that he should be promoted to ordinary professor to hold him at Göttingen, and if that proved impossible that he should be promoted from assistant to director of the physics practice course and co-director of the physics institute and given improved working conditions in the institute. It was "for Germany, for Prussia, and for Göttingen," he pleaded. But Zürich offered Kohlrausch better terms than Göttingen's, and in July 1870 Kohlrausch requested his dismissal from Göttingen.[99] Weber complained to Richard Dedekind that no one could replace Kohlrausch.[100]

Weber hoped to keep if not Kohlrausch then at least the extraordinary professorship that he had obtained for Kohlrausch, but the best he could get was an assistant to take over Kohlrausch's duties.[101] He chose as his assistant another of his students, Eduard Riecke, who had been called up as an officer in the war with France just as he was about to present a mathematical physics dissertation to the Göttingen faculty.[102] On his return to Göttingen, Riecke took up his duties as Weber's assistant. In addition, he became Privatdocent in 1871, and that year he took over some of the experimental lectures from Weber, who was unwell, and 2 years later he took over all of the experimental lectures. In 1873 he became extraordinary professor for the same reason that Kohlrausch had, a call to an agricultural academy and Weber's energetic move to counter it. In 1876 Riecke gave his assistantship to his student and part-time assistant Carl Fromme; Riecke continued to teach the advanced students in the laboratory, while Fromme taught the beginners. In this way, Riecke was gradually tested and groomed as the probable institute director when Weber should step down completely.[103]

In 1876, on the fiftieth anniversary of Weber's doctorate, sixty-eight scientists, most of them university teachers, gave Weber a present and signed themselves as his "students." In his acknowledgment Weber wrote: "Of special value for me is your recollection of my lectures and the recognition that despite their various defects, I still succeeded in achieving the main purpose, which was to show the way of doing rigorous research in natural science and to present the connectedness

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[99] Göttingen U. Curator von Warnstedt to Prussian Minister of Culture von Mühler, 20 February 1869 and 18 June and 2 July 1870; Kohlrausch to Göttingen U. Curator, 23 July 1870; Kohlrausch Personalakte, Göttingen UA, 4/V b/156. Weber to an official, 17 June 1870.

[100] Weber to Richard Dedekind, 10 August 1870, Dedekind Papers, Göttingen UB, Ms. Dept.

[101] Weber asked the government to try to hire the Berlin extraordinary professor Quincke, who, he supposed, might be looking for a job elsewhere after Helmholtz's move to Berlin. The government replied that there was no money to appoint someone to Kohlrausch's "professorship." Göttingen U. Curator to Prussian Minister of Culture von Mühler, 9 September 1870, Göttingen UA, 4/Vh/21.

[102] Weber to Göttingen U. Curator, 7 October 1870, Göttingen UA, 4/Vh/21.

[103] Göttingen U. Philosophical Faculty to Göttingen U. Curator, 29 June 1871; Weber to Curator, 6 February 1873; Riecke's appointment to extraordinary professor, 26 February 1873; Göttingen U. Curator von Warnstedt to Prussian Ministry of Culture, 25 April 1876; Ministry to Warnstedt, 9 September 1876; Riecke Personalakte, Göttingen UA, 4/V b/173. Weber to Curator, 4 December 1873; Warnstedt to Prussian Minister of Culture Falk, 5 December 1873; Weber Personalakte, Göttingen UA, 4/V b/95a.

[of natural science] that it yields."[104] Three years earlier, Weber had told the curator that he wanted to be free of lectures so that he could concentrate on a "series of large scientific questions." His wish was respected, but his retirement was granted only for a few years to begin with; they wanted time to see if Riecke worked out as Weber's substitute, as he did.[105]

The large scientific questions Weber worked on during his provisional retirement centered on his law of electric action and on Helmholtz's new criticisms of it. Twenty-five years before, Helmholtz had raised doubts about Weber's law because of its dependence on the motions of particles, which did not agree with Helmholtz's understanding of the kind of forces required by the conservation of energy; namely, attractions and repulsions that depend only on the separations of particles. Helmholtz made his doubts more precise in 1870 in his first paper on the motion of electricity and in a subsequent paper, where he derived unphysical consequences from Weber's law. He pointed out that according to this law, sometimes an electric particle acts as if its mass is negative, which allows its velocity to increase indefinitely under the action of a force opposing its motion, a serious objection. One result of Helmholtz's renewed criticism was to help motivate Weber to develop a detailed molecular physics on the basis of an energy principle, which he defined in relation to his electrodynamics.[106]

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[104] Weber to Dedekind, 20 October 1876, Dedekind Papers, Göttingen UB, Ms. Dept.

[105] Weber to Göttingen U. Curator von Warnstedt, 26 October 1873; Warnstedt to Prussian Minister of Culture Falk, 29 October and 5 December 1873; Weber Personalakte, Göttingen UA, 4/V b/95a. In productivity, Eduard Riecke lived up to Weber's example. Although his method of research was predominantly theoretical, he regularly did experiments, usually to make measurements using theory as a guide. Woldemar Voigt, "Eduard Riecke als Physiker," *Phys. Zs.* 16 (1915): 219–21, on 219; Emil Wiechert, "Eduard Riecke," *Gött. Nachr.* (1916): 45–56, on 47–48. Both Riecke's research and his handling of the physics institute were praised by Weber, Listing, and the rest of the Göttingen science professors when they recommended his promotion to ordinary professor. Göttingen U. Curator to Prussian Ministry of Culture, 6 September 1881, Riecke Personalakte, Göttingen UA, 4/V b/173. The minister warned the Göttingen curator that Prussia would not support three physics professors at Göttingen, so that if Riecke were made professor he would have to replace Weber. The curator replied that Weber, who had given up hope of getting Friedrich Kohlrausch as his successor, wanted to be replaced by Riecke and that no one could now take it away from him in any case. So in December 1881 Riecke formally succeeded Weber. Prussian Minister of Culture Gossler to Göttingen U. Curator von Warnstedt, 4 November 1881; Warnstedt to Gossler, 8 November 1881; Minister to Warnstedt, 14 December 1881; Riecke Personalakte, Göttingen UA, 4/V b/173. Riecke's salary was 3500 marks—raised by 1000 marks in 1883 and another 2000 marks in 1886—plus 540 marks for rent.

[106] Edmund Hoppe, *Geschichte der Elektrizität* (Leipzig, 1884), 511–12; Eduard Riecke, "Wilhelm Weber," *Abh. Ges. Wiss. Göttingen* 38 (1892): 1–44, on 26–27. Whittaker, *Aether and Electricity*, vol. 1, 206.

In 1869 and 1871, Weber returned to the potential for his force, which he had derived in 1848.[107] By contrast with his law of force, which had a very "complicated" character – allowing, for example, attraction between electric particles at certain separations and repulsion at other separations – the potential was "simple" and, to Weber, more fundamental for that reason, and he now chose to work with it. In deriving the motions of pairs of electric particles, he made an assumption about the behavior of the potential when the particles are at infinite separations. It was that the relative velocity of electric particles has an upper limit, which he took to be c, from the ratio of electric units that enters his law of force or potential. He explained that until there existed a well-developed molecular dynamics, such an absolute velocity in nature could not be dismissed on a priori grounds,[108] and he used the limiting velocity to answer Helmholtz's criticism. Helmholtz, Weber, and their respective supporters ably disputed one another, and even today it is not obvious which position, Weber's or Helmholtz's, was the stronger then.[109]

With the time he gained by detaching himself from official duties at the institute, Weber applied his electrodynamics to phenomena that lie outside of electricity proper. He showed that his law allows bounded motions of electric particles, enabling him to make a "first *reconnaissance*" of chemical phenomena. By supposing that an electric particle adheres to every ponderable atom, he could account for the permanence of the atomic aggregates that chemistry studies, and by supposing that around a ponderable atom an electric particle moves in a stable orbit, forming an elementary Ampèrian current, he could explain the thermal properties of electric conductors. He showed that by assuming that each ponderable gas particle is a positive and negative pair, like a double star, he could determine the laws of collision from the laws of electric interaction, eliminating the need for the untested assumptions about molecular forces of the kinetic theory of gases. Gas particles differ from electrical particles in that they are ponderable and electric ones are not, but even this difference could be removed, Weber showed, by appealing to F. O. Mossotti's explanation of gravitation by electric forces. He also showed that electric particles could form an imponderable ether, the medium of light. In outline, Weber proposed a nearly complete electrical view of physical nature. To explain the phenomena of electrodynamics, gravitation, light, heat, chemistry, and other molecular processes, he needed only positive and negative electric particles, the laws of their motion, which mechanics provides, the fundamental law of electric

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[107] Wilhelm Weber, "Ueber einen einfachen Ausspruch des allgemeinen Grundgesetzes der elektrischen Wirkung," *Ann.* 136 (1869): 485–89; "Elektrodynamische Maassbestimmungen insbesondere über das Princip der Erhaltung der Energie," *Abh. sächs Ges. d. Wiss.* 10 (1871): 1–61; repr. in *Wilhelm Weber's Werke*, vol. 4, *Galvanismus und Elektrodynamik,* zweiter Theil, ed. Heinrich Weber (Berlin, 1894), 243–46 and 247–99.

[108] Weber, "Elektrodynamische Maassbestimmungen insbesondere über das Princip der Erhaltung der Energie," 254–55, 296–99.

[109] K. H. Wiederkehr, *Wilhelm Eduard Weber. Erforscher der Wellenbewegung und der Elektrizität 1804–1891,* vol. 32 of Grosse Naturforscher (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1967), 106.

action, which is derived from the fundamental law of electrostatics, and an appropriate version of the energy principle. In his last writing on the subject, Weber paraphrased what Laplace said of the mechanical view of nature, that nature could be calculated from the fundamental law of electric action once the "position and motion of all electric molecules . . . were given at any time." Weber worked within the tradition of research in which the fluids of heat and magnetism had been eliminated, the ether of radiant heat had been reduced to the ether of light, and the fundamental laws were elementary laws of particles and their forces, and this is where he expressly placed his work. Physics had already achieved a very considerable degree of connectedness, which Weber sought to strengthen through an encompassing molecular theory and a fundamental electrodynamic law.[110]

In one respect, Weber's approach to physics was distinctively his, or one shared with at most Gauss: he regarded his work on the law of electrodynamics as inseparable from his work on exact measurement, which connected the parts of physics through a system of absolute measures and a corresponding universal instrument. In 1880 Weber published his last experimental work, which dealt with electrodynamic measures,[111] and in 1883 he published his last paper, which gave an account of an instrument,[112] fitting conclusions to a career dedicated to setting standards of accuracy and precision in physical research.

A historian of electrodynamics perceptively characterizes Weber's, Clausius's and other German theoretical work on electrodynamics as "conservative with respect to physical concepts and mathematical techniques," often degenerating

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[110] Wilhelm Weber, "Elektrodynamische Maassbestimmungen insbesondere über die Energie der Wechselwirkung," *Abh. sächs. Ges. d. Wiss.* 11 (1878): 641–96; repr. in *Werke*, vol. 4, pt. 2, 361–412, on 394–95; "Elektrodynamische Maassbestimmungen insbesondere über den Zusammenhang des elektrischen Grundgesetzes mit dem Gravitationsgesetze," handwritten manuscript, published posthumously in 1894 in *Werke*, vol. 4, pt. 2, 479–525, on 479–81. Weber's biographer writes of Weber's enduring "vision that all natural phenomena are governed by a single law: his fundamental law of electric action" (Wiederkehr, *Weber*, 181). Weber referred his law of interaction to more fundamental features of nature. He showed that the interaction between pairs of particles was completely determined by the principle of the conservation of energy, suitably formulated, and by the law of electrostatic interaction, which for him had the requisite simplicity of a fundamental law. He saw his accomplishment as reducing his general law of interaction to a "theorem," a deductive consequence of what was truly fundamental. His method here was similar to Clausius's; see note 91 above. Wilhelm Weber, "Ueber das Aequivalent lebendiger Kräfte," *Ann.*, Jubelband (1874): 199–213; "Ueber die Bewegungen der Elektricität in Körpern von molekularer Konstitution," *Ann.* 156 (1875): 1–61; repr. in *Werke*, vol. 4, pt. 2, pp. 300–311 and 312–57; "Elektrodynamische Maassbestimmungen insbesondere über die Energie der Wechselwirkung," 372.

[111] Wilhelm Weber and Friedrich Zöllner, "Ueber Einrichtungen zum Gebrauch absoluter Maasse in der Elektrodynamik mit praktischer Anwendung," *Verh. Sächs. Ges. Wiss.* 32 (1880): 77–143; repr. in *Werke*, vol. 4, pt. 2, 420–76.

[112] Wilhelm Weber, "Ueber Construction des Bohnenberger'schen Reversionspendels zur Bestimmung der Pendellänge für eine bestimmte Schwingungsdauer im Verhältnis zu einem gegebenen Längenmaass," *Verh. Sächs. Ges. Wiss.*(1883); reprinted in *Ann.* 22 (1884): 439–49.

into “sterile axiomatization,” and out of touch with experiment.[113] Parts of their work had lasting value, but in the end, their attempts to perfect electrodynamics based on distance actions could not compete with the newer Faraday-Maxwell concept of contiguous action, and by comparison they can be seen as conservative. For all of its promise, German electrodynamics failed to establish a conclusive connection between electricity and light, and until Hertz it failed to produce experiments for deciding conclusively which foundations were correct. History of physics is more interested in success than failure, but from our point of view, which is the work of physicists who developed theories, success is just one outcome of work, and probably the less common. Wilhelm Wien observed that the theoretical physicist often works for a very long time without success if he does not happen to hit upon the right ideas.[114] We allow ineffectual researches the place they held in German theoretical physics at the time.

Weber’s and Clausius’s fundamental laws describe electric forces and the motions of electric particles. Descended from the seventeenth-century example of the law of gravitation, the method of distance forces and particles had been fashioned into a generic approach by the French mathematical physicists, who were the source of the best ideas in physics at the time when Weber and Clausius started out. So successful had been the method that it could not be discarded until its possibilities were thoroughly explored. The electrodynamic researches of Weber and Clausius, or something very like them, had to be tried and pursued to the end.

9.4 Theoretical Physics, Neighboring Sciences, and “Higher” Unities

Throughout the period covered in this book, physicists often worked on subjects belonging to other sciences or on subjects that were more or less shared by physics and neighboring sciences. Examples of such sciences were mineralogy, geology, physiology, meteorology, applied physics, and, most often, chemistry. We saw that in the second half of the nineteenth century, some of the border areas of physics were organized as new sciences in their own right. We look at one of them here, physical chemistry. Despite its appearance as another specialization in the natural sciences, physical chemistry was seen by its advocates as an example of connectedness.

Weber, who in his researches collaborated with anatomists (his brothers Ernst Heinrich and Eduard) and mathematicians (Gauss and Dirichlet) in addition to other physicists, made a strong case for Kirchhoff as professor of physics at Heidelberg partly on the prospect of cooperative research between physics and chemistry:

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[113] Darrigol, *Electrodynamics*, 214.

[114] Wilhelm Wien, “Ziele und Methoden der theoretischen Physik,” *Jahrbuch der Radioaktivität und Elektronik* 12 (1915): 241–59, on 245.

"Since we in Germany have no center for mathematics and the natural sciences such as Paris was for a long time, two scientists who by working together multiply their achievements are a rare phenomenon, which lends a special radiance to the university where it is successfully achieved." Kirchhoff, with his outstanding ability in physical theory and experimental physics, promised "especially great success" in his researches if he could work with Bunsen, for then the physicist's store of experimental problems, means, and methods would be enriched by the chemist's.[115]

Weber's prediction was borne out, as we have seen in this chapter. Following their successful work on spectrum analysis, Kirchhoff and Bunsen's collaboration was used as an argument for the recognition of physical chemistry in Germany. An early instance was the Leipzig philosophical faculty's move to secure one of its two chemistry chairs for physical chemistry. Its application to the Saxon ministry of culture reads:

> Such efforts of unification are appearing at present with regard to the areas of chemistry and physics. Through the partly theoretical, partly experimental investigations of Bunsen, Kirchhoff, Clausius, Kopp, Graham, Frankland, St. Clair Deville, Berthelot a. o., in the last years there has developed at the border between chemistry and physics an increasingly growing activity that seems to be destined gradually to fill the chasm between chemistry and physics and gradually to fuse those two areas into *one* higher and more general science, which equally profits by the treasures collected in the one as in the other area.

By directing research to the "border" area of the two sciences, physical chemistry joined the two sciences in a "higher" unity, a point which took some special pleading. The application for a physical chemistry chair continues:

> It must arouse a painful and uncomfortable feeling to see how the natural sciences (as consequence of an inner necessity) in the course of time increasingly branch out and splinter, to see how scientific researchers follow their different courses, every one of them trying to reach a *single* area, every one of them driven by the need for the time being to gain a survey of the phenomena of at least a *single* area. We must appreciate it all the more when vis-à-vis this *tendency toward splintering* the opposite trend, the *tendency toward unification*, shows itself, namely, when this tendency toward unification shows up in men who belong to the most outstanding and most famous natural scientists of our time.[116]

At the time of this communication, the late 1860s, positions for second physicists in German universities were beginning to turn up as positions for theoretical physics. The separation of theoretical physics and experimental physics, Wien wrote, was a practical necessity, owing to the demands of their respective methods, but the two halves were bound in a "higher unity."[117]

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[115] Weber to Bunsen, 12 March 1854, Bad. GLA, 235/3135.

[116] Leipzig University Philosophical Faculty to Saxon Ministry of Culture and Public Education, undated draft, ca. 1869, Wiedemann's Personalakte, Leipzig UA, PA 1060, Bl. 5–8.

[117] Wien, "Ziele und Methoden," 259.

Chapter 10
Physical Research in the *Annalen* and Other Journals Around 1870

10.1 Contributors and Contents

With this discussion of the *Annalen der Physik* we approach the close of J. C. Poggendorff*'s* long editorship. By 1874, his fiftieth anniversary, he had brought out 150 volumes.[1] *Poggendorff's Annalen*, the customary form of its citation, had become synonymous with German physics. Inevitably, work by German authors came to occupy more space in the *Annalen* and work by foreign authors less. And inevitably, it would seem, work came to be increasingly physical, as work in chemistry, especially in organic chemistry, was channeled to other specialized journals.[2] Even as the work submitted to the journal was restricted, it increased in quantity, and Poggendorff took to bringing out supplementary volumes from time to time. Toward the end of his editorship, he began a regular series, the *Beiblätter*, for brief reports of recent work not appearing in the *Annalen*.

From the beginning, the *Annalen,* as we saw in earlier chapters, made available throughout Germany the record of physical research abroad. During Poggendorff's years as editor, it filled the equivalent of nearly thirty volumes. Scarcely less important, the *Annalen* informed foreign scientists of German research; the time was long past when it stood in foreign libraries, if it stood there at all, with pages uncut because it contained little but foreign research in German translation.[3]

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[1] The volumes Poggendorff brought out over these many years included nearly nine thousand papers and notices by over two thousand authors. Each year he published works by about thirty new authors. The numerical measure of his achievement was given in the *Jubelband* for Poggendorff's fiftieth anniversary by W. Baretin, "Ein Rückblick," *Ann*. (1874): ix–xiv, on xii–xiii. We again make the *Annalen* the principal source of our survey.

[2] W. Baretin, "Johann Christian Poggendorff," *Ann*. 160 (1877): v–xxiv, on xi.

[3] The physicist J. F. Benzenberg reported that on his trip to Paris in 1815, he found that L. W. Gilbert's *Annalen* was not to be had at the Institut's library and that although it was at the Royal Library, its pages were uncut. In his *Ueber die Daltonsche Theorie* (Düsseldorf, 1830), preface.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_10

Poggendorff continued to place greatest value on experimental work, but he welcomed theoretical work as well.[4] Rudolph Clausius's many researches, which were theoretical and often highly mathematical, almost without exception appeared first in Poggendorff's journal. It is indicative of the attention the *Annalen* received abroad that Clausius's papers were nearly all translated within a year in the leading British physics journal, *Philosophical Magazine*.

The *Annalen* was available to physicists in German universities and technical institutes, and it was found as well in many gymnasium libraries.[5] German physicists regarded it as an indispensable resource for their field, as shown by the Berlin Physical Society's acknowledgment of an official responsibility for the journal after Poggendorff's death in 1877; its title page stated the collaboration of the Society and, "in particular," of Helmholtz. The new editor was again an experimental physicist, Gustav Wiedemann, but now with Helmholtz as advisor on theoretical physics. Germany's major physics journal recognized that experimental and theoretical research had their respective experts.

For the years we look at in this chapter, 1869–1871, the *Annalen* issues come bound in ten bulky volumes, containing some 6500 pages. About a third of the space is given over to foreign research, in German translation where translation is called for. Fully half of the foreign work originated in Austria-Hungary, Switzerland, and the Netherlands, where physicists had close relations with their German counterparts.[6] There was as well a certain amount of movement of physicists between teaching positions in the several countries.[7] Britain and a handful of other European countries accounted for the rest of the foreign work appearing in the *Annalen*.[8]

German contributors to the *Annalen* numbered well over one hundred in 1869–1871. There were some government officials and some persons without affiliation, but most of the contributors were connected in one way or another with teaching institutions. They included nineteen ordinary professors of physics

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[4] Baretin, "Poggendorff," xii. Poggendorff's successor, Gustav Wiedemann, spoke of theory and experiment having been joined in the *Annalen*. Wiedemann, "Vorwort," *Ann.* 39 (1890): i–iv.

[5] Emil Frommel, *Johann Christian Poggendorff* (Berlin, 1877), 67.

[6] The total number of papers in the *Annalen* from each of those countries follows in that order: Austria-Hungary, Switzerland, and Holland.

[7] For part of 1869–1871, Kundt and Friedrich Kohlrausch were in Zurich, accounting for a good share of the substantial contribution from Switzerland to the *Annalen*.

[8] The other countries represented in the *Annalen* were Britain, Denmark, Sweden, (Western) Russia, France, Italy, and Belgium. Of physics published in the New World and elsewhere, Poggendorff took no notice in 1869–1871. In one of these years he published a series of brief papers sent to him from Argentina by a young, world-traveling physicist from Berlin, who was an assistant to Magnus.

and directors of physics institutes from universities and technical institutes.[9] They also included about the same number of extraordinary professors, Privatdocenten, assistants, and students, whose work took up as much space in the *Annalen* as that of the institute directors.[10] Among the most prolific contributors to the *Annalen* during

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[9] In order of the number of pages they published in the *Annalen* in 1869–1871, the nineteen institute directors were: Wilhelm von Bezold (Munich), Adolph Wüllner (Bonn and Aachen), Wilhelm Hittorf (Münster), Eugen Lommel (Erlangen), O. E. Meyer (Breslau), Gustav Magnus (Berlin), Rudolph Clausius (Würzburg and Bonn), August Kundt (Würzburg), J. B. Listing (Göttingen), Eduard Reusch (Tübingen), Wilhelm Beetz (Munich), Heinrich Buff (Giessen), Johann Müller (Freiburg), Hermann Knoblauch (Halle), Heinrich Weber (Braunschweig), Gustav Kirchhoff (Heidelberg), Franz Melde (Marburg), Heinrich Wilhelm Dove (Berlin), and Wilhelm Weber (Göttingen). Dove, who was ordinary professor of physics at Berlin and director of the Prussian state meteorological institute, is included in this list because he took charge temporarily of the Berlin University physics institute for a year after Magnus's death in 1870. University institute directors who did not publish in the *Annalen* in 1869–1871 were: Ottokar von Freilitzsch (Greifswald), Karl Snell (Jena), Gustav Karsten (Kiel), Franz Neumann (Königsberg), Wilhelm Hankel (Leipzig), and Philipp Jolly (Munich); Rostock University did not have a physics professor. With the exception of Hankel, they published either no articles in any physics journals or at most an occasional article elsewhere. Hankel published thermoelectric studies of crystals regularly in the *Abhandlungen* of the Saxon Society of Sciences; from 1876, he republished this material in the *Annalen*.

[10] The *Annalen* authors who were not institute directors were mostly recent graduates, and their theoretical competence expressed the current understanding of what a properly trained physicist should know. For physicists just beginning about 1870, theoretical physics would provide increasing opportunities, and through their teaching or research or both, most of them would be closely associated with theoretical physics in their subsequent careers. Of the three extraordinary professors of physics publishing in the *Annalen* in 1869–1871—Friedrich Kohlrausch, Georg Quincke, and Karl Zöppritz—the latter two taught mathematical physics in their academic positions at that time. Of the—at least—eight Privatdocenten who published in the *Annalen* in these years, most had close connections with theory. Wilhelm Feussner was at Marburg, where he would teach theoretical physics for 50 years, from 1880 as extraordinary professor and from 1908 as ordinary honorary professor. Friedrich Narr was at Munich, where he would teach the subject, from 1886 as extraordinary professor, to the end of his career. Eduard Ketteler was at Bonn, where as extraordinary professor he would teach the subject from 1872 to 1889, nearly the end of his career. Leonhard Sohncke at Königsberg and Emil Warburg at Berlin would both teach theoretical physics (Warburg as extraordinary professor for the subject), and although their teaching responsibility would be experimental physics over most of their careers, they were associated with theoretical physics in their research: Sohncke as a developer of Franz Neumann's direction in theoretical crystal physics, and Warburg as one of the most theoretically capable German experimental physicists. Hermann Herwig had only a brief academic career (he died in his thirties, while professor of physics at Darmstadt); his dissertation at Göttingen had been on mathematics, and his subsequent work in experimental physics showed theoretical interest. Richard Rühlmann did not continue in an academic career but became a gymnasium teacher of physics and mathematics. Emil Budde did not continue either, but he managed to do research all the same, much of which, like his first paper in the *Annalen* in 1870, was purely theoretical. Of the—at least—six students or assistants who published in the *Annalen* in 1869–1871, only two had academic careers. Paul Glan, a Berlin graduate in 1870 and after that an assistant to Helmholtz, went on to teach theoretical physics at Berlin as Privatdocent from 1875 to the end of his life. Eduard Riecke, a Göttingen graduate in 1871, became assistant and Privatdocent the same year; then and during appointments as extraordinary professor in 1873 and ordinary professor of experimental physics in 1881, he

these years were two physicists at the Prussian Academy of Sciences, who were at the same time ordinary professors of physics without institutes at the university, Poggendorff and Riess.[11] The rest were teachers at secondary and lower technical schools, who accounted for approximately a third of all teachers publishing in the *Annalen*.

Although the subjects of German papers in the *Annalen* in 1869–1871 were primarily physical, and although the proportion of chemical to physical papers, where the two could be distinguished, had declined over the years, chemistry remained a substantial interest of the *Annalen*, as did crystallography, physiology, and other so-called border areas of physics.[12] Only pure mathematics remained outside its circle of interests.[13]

10.2 Work by Institute Directors

Institute directors had the means and the independence to develop their own directions of research in 1869–1871. As at other times, their directions seldom coincided, and as a result their researches, when taken together, touched on most parts of physics. Wilhelm Hittorf published solely on electric conduction in gases, J. B. Listing on optical instruments, Eduard Reusch—but for a description of an instrument and of what happens when one picks up shot with tweezers—on crystals, O. E. Meyer—but for a polemic against British physicists—on the friction of air, Adolph Wüllner—but for critical remarks in support of the work of a student of his—on the spectra of gases, and Eugen Lommel—but for brief descriptions of instruments—on light absorption and fluorescence. August Kundt, who became an institute director in Germany in 1870, continued the acoustical researches he had begun abroad, though he soon took up a new topic, anomalous dispersion. Wilhelm

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regularly gave lectures on theoretical physics until Woldemar Voigt was brought to Göttingen. His research was primarily theoretical throughout his career.

[11] Poggendorff's and Riess's publications in the *Annalen* compare in volume with those of the two most prolific institute directors, Bezold and Wüllner.

[12] Papers by chemistry teachers took up one fifth as much space in the *Annalen* as those by physics teachers, and papers by teachers of crystallography, mineralogy, and geology together took up one third as much space. Physiologists were well represented; less well represented were astrophysicists, mathematicians, physicians, and pharmacists. All told, the contributions to the *Annalen* from teachers outside of physics took up two thirds as much space as contributions from teachers within physics.

[13] Despite the recognized connections of mathematics and physics, only two academic mathematicians published in the *Annalen* in 1869–1871, and they published only one article each on mechanics, a topic that belonged as much to mathematics as to physics. Mathematicians had their own journals and did not need the *Annalen*, the journal that physicists increasingly regarded as their own.

von Bezold, the most versatile as well as the most prolific researcher, worked on many topics within electricity and on several outside. As directors of institutes, they usually did not have to concede, as a less favored contributor to the *Annalen* did, that they could not decide which of the possible causes of a phenomenon was “reality” because of lack of “opportunity and instruments.”[14] Although from their point of view, they rarely had enough instruments of the right kind, they had control of a physical cabinet, which they made good use of in experimental research.

The work of the institute directors in 1869–1871 would not have suggested to readers of the *Annalen* that research was separated into theoretical and experimental departments. Only Clausius, Kirchhoff, and Weber appeared there with papers that were not experimental or observational. Other institute directors—Meyer, Bezold, Kundt, and Lommel—presented experimental work together with some mathematical theory, and most of the rest revealed some concern with theoretical issues. In the work of the directors of institutes in general, experimental and theoretical discussion occurred together, the balance varying from physicist to physicist and from problem to problem.

Through an experimental study of electric conduction in gases, for example, Hittorf hoped to come nearer to a mechanical explanation of electrical processes and to rid physics of its last imponderables, the two electric fluids.[15] Lommel wished to complete what he regarded as the most developed branch of physics, optics. Inflection, double refraction, circular polarization, and other complex phenomena of light had been explained by mechanical principles, but not fluorescence, which he now proceeded to explain mechanically too, as he did the closely related phenomena arising from the action of light on chlorophyll, carrying out the necessary experiments for his theory.[16] Typically, in the *Annalen*, institute directors reported experiments containing measurements of some physical “constant” and its variation with other physical quantities; if the constant entered an equation belonging to a theory, its behavior could complete a theoretical explanation.[17] Some experiments produced geometrical “figures,” the lawfulness of which invited theoretical discussion.[18] Experiments on absorption and emission of radiant heat

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[14] Overzier, vol. 139, 651–60, on 660. Because we refer to a large number of papers published in the *Annalen*, in the remainder of this chapter we use this highly abbreviated form of citation: author’s last name, volume number, page numbers. The journal, unless otherwise specified, is always the *Annalen*, and the volumes all fall within the 3 years, 1869–1871.

[15] Hittorf, vol. 136, 1–31, 197–234, on 223.

[16] Lommel, vol. 143, 26–51, 568–85, on 30–34.

[17] For example, Kundt on the variation of the index of refraction with wavelength (vol. 142,163–71), and O. E. Meyer on the variation of the constant of the internal friction of air with temperature and, possibly, with pressure (vol. 143, 14–26). Kirchhoff studied theoretically the variation of the constant of the magnetization of iron with the intensity of the magnetizing force (supplementary vol. 5, 1–15).

[18] For example, Bezold on “electric dust figures” (vol. 140, 145–59) and on “Lichtenberg figures” (vol. 144, 337–63, 526–50), and Melde and Kundt on “sound figures” (Melde, vol. 139, 485–93; Kundt, vol. 137, 456–70, vol. 140, 297–305).

and light were derived from, and suggested new, theoretical ideas on the relations of matter and ether.[19] A small proportion of the work submitted to the *Annalen* by institute directors contained reflections on particular phenomena[20] or on features of particular laws rather than reports of new experimental results.[21] Of this work, Clausius's investigations of heat were the most significant for the foundations of physics.[22] Ludwig Boltzmann, who had worked on the same fundamental problem of heat theory as Clausius, entered into an exchange with him; their criticisms and priority claims showed the *Annalen* to be a risky place to publish casual statements about heat.[23] Because of the ubiquitous appearence of the principles of heat, physicists working on a variety of topics encountered and possibly contradicted—or worse, overlooked—Boltzmann's and Clausius's statements and had to deal with their criticisms in the pages of the *Annalen* or elsewhere.[24]

10.3 Characteristics of Research

It was still possible to publish notices in the *Annalen* of incidental observations of nature alongside those of carefully prepared phenomena in the laboratory. During a hailstorm, for example, it occurred to Johann Müller to look at polarization in hailstones, and he reported what he saw to the *Annalen*, which was that each stone is made up of many pieces of ice oriented in various directions; but as he had not been

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[19] For example, Magnus on radiant heat (vol. 139, 431–57, 582–93), Kundt on anomalous dispersion (vol. 142, 163–71), and Lommel on the relation of chlorophyll to light (vol. 143, 568–85).

[20] For example, the phenomena of galvanic arcs, fluorescence, and induced magnetism by, respectively, Bezold (vol. 140, 552–60), Lommel (vol. 143, 26–51), and Kirchhoff (supplementary vol. 5, 1–15).

[21] Wilhelm Weber called attention to a property of his fundamental law of electric action (vol. 136, 485–89), Bezold to analogies between the laws of photometry and the laws of gravitational attraction (vol. 141, 91–94), and Clausius to the mechanical principles underlying the second fundamental law of heat theory (vol. 142, 433–61).

[22] Clausius, vol. 141, 124–30; vol. 142, 433–61.

[23] Clausius's work led Ludwig Boltzmann to publish a priority claim in the *Annalen* (vol. 143, 211–30). This was one of many papers by Boltzmann in the *Annalen* in these years, all in response to papers by others published there. He worked in Austria and published most of his researches there. The Stettin secondary school teacher Robert Most provoked Boltzmann by his "simple proof" of the second law of heat theory, which he regarded as "simpler" than the first law (vol. 136, 140–43). Boltzmann pointed out that Most assumed at the start that dQ/T is a complete differential, so it was "naturally not hard" for him to prove the second law (vol. 137, 495). What Most regarded as clear about his proof Boltzmann regarded as simply wrong (vol. 140, 635–44).

[24] As, for example, Bezold did in connection with his study of electrical condensers (vol. 137, 223–47. Clausius responded (vol. 139, 276–81).

prepared for a hailstorm, he was not in a position to make a thorough study.[25] As an institute director, Müller was assured space in the *Annalen*, to which he sent a steady stream of three-page notices on unconnected topics. Another contributor began his notice with "On the night of . . ., I saw, as I walked to the window after finishing my work . . .," and he reported his observations of some remarkable thunder and lightning.[26] On walks in and around Frankfurt, another contributor had long listened closely to the interval, rhythm, and pitch of the two tones of the call of the cuckoo, a refrain, he said, which every child knew and which Beethoven had included in the *Pastoral Symphony*.[27] Poggendorff valued the occasional uncomplicated observations; he himself extracted from a French journal an account of an uncommon snowfall" in France, the equal of which only one elderly person could remember from 1804 or 1805.[28]

The odd or charming phenomenon was not the usual substance of a paper in the *Annalen* around 1870. The unadorned description of an apparatus and its use was far more common. Sometimes the apparatus was meant to be used in lecture demonstration, or at least in part: Friedrich Kohlrausch kept the needs of "lectures" in mind; Poggendorff introduced a powerful apparatus capable of producing effects visible to the far corners of lecture halls; Müller gave a "much simpler" experimental proof than Coulomb's of the inverse-square force of magnetism, which had more to do with teaching than with physical advance.[29] Generally, the apparatus was meant to be used strictly in research. As the subjects of research came in great variety, so did the apparatus; the index of the *Annalen* for the 10 years centering on 1870 listed over one hundred kinds of apparatus and instruments. They included a large number of measuring instruments, the points of contact between the laboratory and mathematical work, the names of which often ended in "meter," expressing their purpose: barometer, calorimeter, electrometer, and the like. The *Annalen* published a great many measurements by Kohlrausch using Weber's "meters": bifilar galvanometer, bifilar dynamometer, and magnetometer. The names of other instruments often ended in "scope," expressing the need of physicists not only to measure but to "see" the phenomena or the numbers on the meter: chromatoscope, chronoscope, electroscope, and erythroscope. Like the phenomena it was designed to study, the apparatus itself had to be understood exactly, since it was an interacting part of nature, operating under the same physical principles. Heavy foldout pages at the end of each issue of the *Annalen* contained elaborate drawings of the apparatus used in the researches reported there.

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[25] Johann Müller, vol. 144, 333–34.

[26] Hoh, vol. 138, 496.

[27] Oppel, vol. 144, 307–9.

[28] Poggendorff, vol. 139, 510–11.

[29] Friedrich Kohlrausch, vol. 136, 618–25, on 625; Poggendorff, vol. 141, 161–205, on 203; Johann Müller, vol. 136, 154–56.

Experimentalists commonly believed that certain phenomena they studied were still insufficiently understood to be explained by existing theory. Wüllner's spectra of gases, for example, were highly complex. By passing electric current through tubes containing hydrogen at different pressures, he obtained four kinds of spectra; to understand the relations between the spectra, Kirchhoff's theoretical law relating emission and absorption was of no help, and Wüllner approached the problem directly, by experiment.[30] Heinrich Buff, to take another example, thought that because of the complexity of hydraulic pressure experiments, the extensive mathematical theories of the subject did not give an accurate picture, and he set out to determine experimentally the "facts of the phenomena."[31] Experimentalists risked criticism by claiming theoretical knowledge of phenomena that were still being ordered experimentally.[32]

When approaching certain mathematical problems, physicists used the continuum view of matter,[33] and to other problems they brought the discrete molecular view, which had far-reaching implications for the mathematical methods of theoretical physics.[34] They used the word "molecule" in preference to the word "atom" though they occasionally used the latter. By "molecule" they sometimes meant simply the smallest particle under consideration but at other times something more complicated, such as a structured group of particles.[35] They regarded molecular reasoning as indispensable for understanding many of the intimate phenomena of matter: the motion of liquids over liquids, liquids over solids, solids through gases, and the inner friction of gases and liquids. They sometimes invoked molecular actions to explain what happens when electricity, light, and radiant heat are passed through bodies and, above all, to explain the heat relations of bodies. Where an otherwise good theory does not agree with certain phenomena, molecular behavior

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[30] Wüllner, vol. 137, 337–61, on 347–48.

[31] Buff, vol. 137, 497–517, on 497.

[32] Magnus was criticized for making a statement that had no empirical basis but was simply a generalization from the theory of gases. Knoblauch, vol. 139, 150–57, on 152–53.

[33] For example, the theory of a vibrating string based on the analysis of a volume element of the string rather than on molecular forces (Reinhold Hoppe, vol. 140, 263–71); and the theory of induced magnetism based on the analysis of volume and surface elements of the magnetized body (Kirchhoff, supplementary vol. 5, 1–15).

[34] Drawing on Gauss's method, J. Stahl had recently derived the fundamental equations of capillarity. According to Boltzmann, Stahl's derivation shared the defect of Gauss's method, which is to compute finite sums by integrating over all pairs of molecules. This procedure assumes that each molecule contributes a vanishingly small part of the sum, regardless of how far it is from any given molecules. Since it is likely that the contribution of a neighboring molecule is finite, Boltzmann reasoned, the use of the integral calculus is not justified here, and he showed how to develop capillary theory using only finite sums and replacing the integration symbol $\int$ by the symbol Σ (vol. 141, 582–90).

[35] Lommel explained that he was using "molecule" in the "chemical sense": a group of atoms characterized by their nature, number, and relative positions (vol. 143, 568–85, on 573); Budde proceeded from the understanding that the "molecule" of most simple gases consists of two atoms (vol. 144, 213–19).

might explain why.[36] In many physics institutes, the effect of molecular actions was studied: notions of molecular behavior entered qualitatively in experimental reasoning[37] and mathematically in the assumptions of a theory.[38]

Molecules are bits of matter with inertia, the motion of which is governed by the laws of mechanics. The two sets of ideas, molecular and mechanical, were seen jointly to point physics in a decided direction: it was to seek to reduce all phenomena to "purely mechanical concepts," and the solution of problems by the mechanical motions of molecules was the "final and highest task of physics."[39] Thomas Young's theory of capillarity was judged deficient for failing to give the constants entering it a "precise mechanical molecular-theoretical significance,"[40] the existence of which was assured by the understanding that the phenomena of capillarity are caused by molecular forces.[41] In optics as in capillarity, a theory was judged deficient if it failed to "give a theoretical basis" to its constants, which meant developing them from molecular and mechanical considerations. The explanation of optical absorption, dispersion, and anomalous dispersion relied on the forces between molecules of bodies and particles of the ether; these forces and the resulting motions were analyzed by the customary laws of motion belonging to the mechanics of mass points.[42] In kinetic theory, the behavior of a gas was analyzed by mechanical collisions of molecules with the walls of the container and with other molecules and, perhaps, by the rotations and internal vibrations of the molecules.[43] Molecular considerations entered theoretical discussions in heat,[44] in electricity,[45] and in other parts of physics.

Research published in the *Annalen* in 1869–1871 recorded a parallel direction in theory and experiment: in theory it was to understand phenomena at the level of the smallest parts and actions; in experiment it was to reach ever smaller quantities of

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[36] As set out by Gustav Hansemann (vol. 144, 82–108).

[37] Jochmann attributed the departure of Cauchy's reflection theory from observations on reflection and refraction in thin metal sheets to "molecular" properties of the metal surface, which affect the optical constants (supplementary vol. 5, 620–35, on 632–33).

[38] For example, in a theoretical investigation of the "internal constitution of gases" (Hansemann, vol. 144, 82–108).

[39] Heinrich Schröder went on to say that it was premature to explain the action of a gas on solids or liquids by molecular motions, so that for now he had to renounce the highest task of physics (supplementary vol. 5, 87–115, on 114–15).

[40] Paul du Bois-Reymond, vol. 139, 262–75, on 267.

[41] Lüdtge, vol. 139, 620–28, on 620.

[42] Ketteler, vol. 140, 1–53, 177–219, on 200; Lommel, vol. 143, 26–51; Sellmeyer, vol. 143, 272–82; Glan, vol. 141, 58–83.

[43] Hansemann, vol. 144, 82–108; Recknagel, supplementary vol. 5, 563–91; Narr, vol. 142, 123–58.

[44] Clausius's central concept of disgregation was related to molecular arrangements (Budde, vol. 141, 426–32).

[45] Faraday's concept of charge as a peculiar molecular position was introduced (Knochenhauer, vol. 138, 11–26, 214–30).

space, time, and energy. Molecules could not be made individually visible or tangible, but with the help of physical assumptions, the tiny sphere of action of their mutual forces[46] and the tiny width of their vibrations could be measured.[47] Whereas the "millimeter" still satisfied the measuring needs of most physics, with the powerful magnifications obtainable by new microscopes there was need for a smaller unit in micrography and physical optics, the "micron," 1,000th part of a millimeter.[48] With such instruments, previously unsolvable problems involving the fine texture and structure of bodies could now be approached.[49] In experimental mechanics, new methods of measuring extremely small intervals of time enabled the previously unsolvable problem of the duration of elastic collisions to be solved.[50] The miniscule amount of heat received on Earth from an individual star[51] and the incredibly small work done by the air on the ear at the threshold of hearing[52] were among the small quantities that the *Annalen* reported regularly in these years.

To physicists who studied theories, the appeal to the principle of conservation of energy had become second nature, and no theory that ignored it could be taken seriously anymore. An *Annalen* author rejected all explanations of the absorption of light based on Cauchy's theory, since it violated the principle of energy conservation. The use of potential instead of force facilitated the energetically correct formulation of problems in all parts of physics.[53] Related to energy considerations was an interest in developing theories from the principle of least action or some similar principle.[54]

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[46] Quincke determined that for glass, silver, water, and several other substances, the radius of action of molecular forces is a very small but non-vanishing length, of the order of 0.000050 mm, or approximately one-tenth the average wavelength of light (vol. 137, 402–14, on 413); Robert Lüdtge accepted this result (vol. 139, 620–28, on 620).

[47] Boltzmann and Toepler determined that the width of an air particle's vibration at the limits of hearing is about one-tenth of the wavelength of green light, showing how astonishingly sensitive the organ of hearing is (vol. 141, 321–52, on 349–52).

[48] Listing, vol. 136, 467–72.

[49] Ibid., 473–79.

[50] Schneebeli explained that although the laws of collision had been known since the seventeenth century, the actual process of collision remained "rather mystical" owing to the short duration of collisions. With a new method, he determined the time of collision of steel cylinders and spheres; for a cylinder colliding with a fixed bar, the time was 0.00019 s (vol. 143, 239–50).

[51] From Britain, William Huggins reported to the *Annalen* his attempt to determine the very small amount of heat the earth receives from individual stars, using an apparatus consisting of a sensitive galvanometer and various thermopiles (vol. 138, 45–48).

[52] From Graz, Boltzmann and August Toepler sent the *Annalen* an experimental investigation of sound vibrations using a new optical stroboscopic method. They determined the mechanical work per second done by the air on the ear at the limits of hearing to be 1/3,000,000,000 kilogram-meter (vol. 141, 321–52, on 352).

[53] Wilhelm Weber's law (vol. 136, 485–89). Examples of theories developed in terms of potentials are Bezold's in electricity (vol. 137, 223–47) and photometry (vol. 141, 91–94) and Kirchhoff's in magnetism (supplementary vol. 5, 1–15).

[54] For example, Clausius, vol. 142, 433–61, on 449; Boltzmann, vol. 143, 211–30, on 220, 228.

As in the 1840s, in 1869–1871 German physicists often referred to British and, above all, to French mathematical-physical work. They had a strong interest in French optical theory, especially in Cauchy's and Fresnel's.[55] They recognized Poisson's contributions to nearly all parts of physics, if as often for their deficiencies as for their merits.[56] Laplace still entered the *Annalen* for his theory of capillarity.[57] Among British theories, German physicists referred to those by Young, Faraday, George Green, G. G. Stokes, and Maxwell.[58]

What was new in 1869–1871 was the extent to which German physicists now drew on German sources. They cited Gauss on capillarity,[59] Weber on waves, sound, and electrodynamics,[60] and Neumann on crystallography, optics, capillarity, and magnetism.[61] The German work they cited most often was recent. Kirchhoff's

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[55] In Cauchy's theories of reflection (Jochmann, vol. 136, 561–88) and of dispersion (Ketteler, vol. 140, 1–53, 177–219); in Fresnel's formulas for reflection for light intensity (Kurz, vol. 141, 312–17), and his hypothesis of ether drag (Ketteler, vol. 144, 109–27, 287–300, 363–75, 550–63).

[56] In Poisson's theories of acoustics (Kundt, vol. 137, 456–70), of capillarity (Quincke, vol. 139, 1–89; J. Stahl, vol. 139, 239–61), of elasticity (Heinrich Schneebeli, who was Kundt's student in Switzerland, vol. 140, 598–621), of pendulum motion (O. E. Meyer, vol. 142, 481–524), of magnetism (Kirchhoff, supplementary vol. 5, 1–15), of Earth temperature (Fröhlich, vol. 140, 647–52), and (if the *Journal für die reine und angewandte Mathematik* is included here) of heat (Lorberg, vol. 71, 53–90) and of hydrodynamics (O. E. Meyer, vol. 73, 31–68).

[57] For Laplace's theory of capillarity (J. Stahl, vol. 139, 239–61; Quincke, vol. 139, 1–89), but also for his electrodynamics (Hittorf, vol. 136, 1–31, 197–234).

[58] Young's theory of capillarity (Quincke, vol. 139, 1–89; Paul du Bois-Reymond, vol. 139, 262–75), Faraday's theory of charges (Knochenhauer, vol. 138, 11–26, 214–30), Stokes's theory of frictional fluids (Warburg, vol. 139, 89–104; vol. 140, 367–79), Green's and Stokes's theories of pendulum motion (O. E. Meyer, vol. 142, 481–524), and Maxwell's theories of colors (J. J. Müller, vol. 139, 411–31, 593–613) and electromagnetism (Kirchhoff, supplementary vol. 5, 1–15).

[59] Paul du Bois-Reymond, vol. 139, 262–75; J. Stahl, vol. 139, 239–61; Boltzmann, vol. 141, 582–90.

[60] Wilhelm Weber's work on waves (Quincke, vol. 139, 1–89; Kundt, vol. 140, 297–305; Matthiessen, vol. 141, 375–93), on sound (Warburg, vol. 136, 89–102; vol. 137, 632–40; vol. 139, 89–104; J. J. Müller, vol. 140, 305–8), and on electrodynamics (Wilhelm Weber, vol. 136, 485–89, calling attention to a publication of his in the *Annalen* on electrodynamics over 20 years before).

[61] Franz Neumann's theoretical work entered German physics in oblique ways in 1869–1871. He had long before stopped publishing, but his work was used and cited even in cases where he had not published it himself. August Kurz cited an actual, if old, publication of 1834 on crystallography (vol. 141, 312–17), but Emil Jochmann found Neumann's formula for metallic reflection in a Swiss publication by Heinrich Wild, who had studied for a while with Neumann, and in an abstract. Jochmann thought that no derivation or statement of the suppositions of Neumann's formula had been published, and he assumed that they rested on the supposition of Neumann's other optical work, which is that the ether has the same density but different elasticity in different media (vol. 136, 561–88). Quincke, who also had studied with Neumann, stated a capillary law governing the spread of one fluid over another, which he thought Neumann was the first to express (vol. 139, 1–89). Paul du Bois-Reymond, who had spent some time with Neumann, called this law the "third principal law of capillarity" and supposed that the only place it was published was in his own dissertation in 1859 (vol. 139, 262–75). Riecke tested Neumann's law for the magnetism of an ellipsoid but gave no reference to any publication on it by Neumann (vol. 141, 453–56).

wide-ranging and influential contributions included mathematical theory in acoustics, heat, hydrodynamics, elasticity, and electricity.[62] Clausius's contributions included the mechanical theory of heat and the kinetic theory of gases;[63] so strongly had Clausius developed this branch of physics that at times his only sources were his own previous publications.[64] Of other German mathematical theory,[65] Helmholtz's contributions were cited by far the most often, mainly concerning his work on acoustics, but also touching on a range of his other work including hydrodynamics, color theory, galvanism, and geometry.[66] The German contribution to exact physics can be illustrated by a detail: an *Annalen* author observed that the electric force entering his topic, the formation of Lichtenberg figures, involved "only electrostatic action at a distance, that is, the first term of Weber's fundamental law";[67] he regarded the electrodynamic law that Weber had derived and tested as the law of electric force and Coulomb's electrostatic law as its first term.

10.4 Other Journals

The proceedings and for longer works the monograph series of societies and academies also frequently published work by physicists. The proceedings were usually not specialized; although the physical-mathematical and the philosophical-historical classes of the Prussian Academy of Sciences met separately for serious work, their proceedings ran together papers on all subjects, the Vatican manuscripts side-by-side with an electrical machine.[68] They got sorted out, if they did at all,

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[62] On Kirchhoff's work in acoustics (Seebeck, vol. 139, 104–32), in heat (Wüllner, vol. 137, 337–61; Magnus, vol. 139, 431–57, 582–93; Lommel, vol. 143, 26–51), in hydrodynamics (Paul du Bois-Reymond, vol. 139, 262–75), in elasticity (Adolf Seebeck, vol. 139, 104–32; Schneebeli, vol. 140, 598–621), and in electricity (Knochenhauer, supplementary vol. 5, 146–66).

[63] Budde, vol. 141, 426–32; vol. 144, 213–19; Narr, vol. 142, 123–58; Hansemann, vol. 144, 82–108; Bezold, vol. 137, 223–47; Recknagel, supplementary vol. 5, 563–91.

[64] Clausius, vol. 141, 124–30; vol. 142, 433–61.

[65] For example, Riemann's work in electricity (Bezold, vol. 137, 223–47) and in geometry (J. J. Müller, vol. 139, 411–31, 593–613), Krönig's in kinetic theory (Hansemann, vol. 144, 82–108; Recknagel, supplementary vol. 5, 563–91) and Carl Neumann's in optics (Ketteler, vol. 140, 1–53, 177–219) and in electrodynamics (Wilhelm Weber, vol. 136, 485–89). If the *Journal für die reine und angewandte Mathematik* is included here, Jochmann's and Lorberg's recent work in electrodynamics enters (Helmholtz, vol. 72, 57–129).

[66] On Helmholtz's work in acoustics (Warburg, vol. 139, 89–104; Adolf Seebeck, vol. 139, 104–32; Sondhauss, vol. 140, 53–76, 219–41; Glan, vol. 141, 58–83; Lommel, vol. 143, 26–51; Boltzmann and Toepler, vol. 141, 321–52) in hydrodynamics (Warburg, vol. 140, 367–79; Paul du Bois-Reymond, vol. 139, 262–75), in color theory and in geometry (J. J Müller, vol. 139, 411–31, 593–613), and in galvanism (Bernstein, vol. 142. 54–88).

[67] Bezold, vol. 144, 337–63, 526–50, on 535.

[68] The full title of the Prussian Academy of Sciences' proceedings is *Monatsberichte der königlich preussischen Akademie der Wissenschaften zu Berlin*.

only upon republication in specialized journals. The physics published by the Prussian Academy was almost all experimental, as was to be expected from its authors, who included Magnus, Poggendorff, Reusch, Heinrich Wilhelm Dove, and Peter Riess; even Helmholtz's one paper for the academy was experimental. Kirchhoff published one non- experimental paper there, which he also published in a mathematics journal. Nearly all of the other physics papers published by the academy were also published in the *Annalen*. The same was true of the many papers submitted by Beetz and Bezold to the Bavarian Academy of Sciences for publication in their proceedings.[69] The considerable quantity of physics research published in the proceedings of the Gőttingen Society of Sciences covers the range of experiment and theory. Similar societies in Leipzig, Bonn, and elsewhere published physics too.

It was generally recognized that the *Annalen* was not always the appropriate place to publish a mathematical physics paper. Physicists often published their more mathematically detailed work in journals they shared with mathematicians. Bezold published his experiments on optical illusions in the *Annalen*, but he published their mathematical theory elsewhere, observing that readers of the *Annalen* did not all take to mathematical deductions.[70] Leopold Pfaundler believed that Clausius's derivation of the fundamental equation of the molecular-kinetic theory of gases was too difficult for many readers of the *Annalen*, especially for chemists who did not know the integral calculus, so he published a simpler derivation of it there.[71] In journals for mathematics and mathematical physics, physicists' work appeared alongside mathematicians' on physical, usually mechanical, problems. *Mathematische Annalen*, which began publishing only in 1869 contained some mathematical physics, most of it written by its cofounder Carl Neumann.[72] Several German and Austrian physics institute directors published papers in the *Zeitschrift für Physik und Mathematik*, though they did not publish their major work there.[73] With few exceptions, the papers published in this journal were mathematical in nature. Mathematical physics of greater significance went to the *Journal für die reine und angewandte Mathematik*. Just as mathematicians generalized their methods, so did physicists in the work they published in the *Journal*. Kirchhoff, for example, generalized William Thomson and P. G. Tait's

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[69] The full title of the Bavarian Academy's proceedings is *Sitzungsberichte der königlich bayerischen Akademie der Wissenschaften zu München*.

[70] Bezold, vol. 138, 554–60.

[71] Pfaundler, vol. 144, 428–38.

[72] Carl Neumann published a number of brief notices in his new journal, the *Mathematische Annalen*, but only one substantial work in mathematical physics, which had to do with crystal optics (vol. 1, 325–58). Karl Von der Mühll published a mathematical physics paper there (vol. 2, 643–49), as did his Giessen colleague Alexander Brill (vol. 1, 225–52).

[73] Clausius and Bezold published brief notes in the *Zeitschrift*, and Lommel published an "elementary presentation" of some mathematical methods. Boltzmann published a paper on Ampère's law that was unusual for its length and its experimental nature; but it was a republication of a paper in the *Sitzungsber. Wiener Akad.* the year before.

treatment of the motion of a body of rotation immersed in a fluid,[74] and he generalized Helmholtz's method of treating discontinuous fluid motion.[75] Boltzmann generalized Kirchhoff's derivation of the apparent forces between two rings in a moving fluid; Hermann Lorberg generalized Kirchhoff's equations for the motion of electricity in conductors; Helmholtz generalized Neumann's potential for two current elements.[76] So it went: in physics as in mathematics, the more general the theorem, the more powerful it was understood to be.

The importance of the *Journal* for German physicists is shown by the work they published there on electrical action. Electricity in general dominated research in Germany in 1869–1871, as it had for a long time.[77] The *Annalen* contained a good deal of research on the interactions of the ether and matter, but little on actions in the ether itself.[78] Readers were told, and Berlin physicists were shown by demonstration, how a tuning fork disturbs nearby smoke and flame, suggesting that attractions and repulsions may be due to motions in the air or ether and not to action at a distance,[79] but physicists required more than a suggestion to be persuaded of this fundamental point. In their publications in the *Annalen,* German physicists took little notice as yet of Maxwell's replacement of distance action by contiguous action in the ether. Kirchhoff derived equations relating electric and magnetic quantities that were the same as Maxwell's electromagnetic field equations of 1865; but he was studying iron under induced magnetism and not the free ether, and he was guided by Poisson's magnetic theory and did no more than acknowledge Maxwell's derivation.[80] It was in the *Journal* at this time where German physicists examined the propagation of electric action in the medium, the choice of journal reflecting the mathematical stage of the research at this time. Here

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[74] Relaxing the restrictions that the body does not rotate around its axis and that its axis remains parallel to a fixed plane, Kirchhoff showed that the problem could still be solved, though this more general motion leads to elliptical integrals (*Journal* 71, 237–62).

[75] Kirchhoff, *Journal* 70, 289–98.

[76] Boltzmann relaxed Kirchhoff's assumption of the circular cross section of the rings (*Journal* 73, 111–34). Lorberg relaxed Kirchhoff's assumption that the electricity in the conductor is not acted on by outside forces (*Journal* 71, 53–90). Helmholtz's general potential reduces to Neumann's when one current is closed (*Journal* 72, 57–129).

[77] The ten-year index volumes of the *Annalen* show this. Half of the topics with entries taking up an entire column or more in the index are electrical, with light and heat far behind. This measure applies alike to the indices for the 10 years centering on 1860 and for the 10 years centering on 1870.

[78] Physicists still struggled with the question of whether in a light wave the oscillation of the ether is in the plane of polarization or normal to it. See Jochmann, vol. 136, 561–88.

[79] The Berlin secondary school teacher K. H. Schellbach showed his experiments to Quincke, Poggendorff, and Magnus; the latter evidently made them known to the young physicists working with him (vol. 139, 670–72).

[80] Kirchhoff, *Ann.*, supplementary vol. 5, 1–15.

Kirchhoff developed an analogy between pressure forces acting on rings in a fluid and electrodynamic forces acting on closed currents,[81] and here Boltzmann examined Kirchhoff's analogy further.[82] Helmholtz, in the most influential of the studies, examined the medium between bodies and with it Faraday and Maxwell's view of mediated electric action. He recognized that Maxwell's theory of a polarized medium gives the same equations as distance-action theories of Poisson's type[83]; unlike Kirchhoff, he did not stop with this formal recognition but drew its physical consequences. He called attention to Maxwell's "striking result" that electrical disturbances in dielectrics travel as transverse waves with the velocity of light and to the "extraordinary significance that this result could have for the further development of physics."[84] The correctness of Helmholtz's expectation will become clear from the discussion of theoretical and experimental work in Germany through the rest of this book.

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[81] Kirchhoff, *Journal* 71, 263–73. Kirchhoff studied two rings with infinitely small, circular cross sections placed in an infinite, frictionless, incompressible fluid. By using familiar assumptions about the motion of the fluid, he showed that the kinetic energy of the fluid has the same form as the potential of Ampère's law for the electrodynamic interaction of two electrical currents. The rings exert "apparent," or pressure, forces on one another that are the same as the forces that would act if electric currents were to flow through the rings.

[82] Boltzmann pointed out that Kirchhoff's conclusion about the formal identity of apparent fluid forces and Ampèrian forces is not generally valid, and he devoted considerable discussion to the electrical side of the analogy (*Journal* 73: 111–34).

[83] Helmholtz, *Journal* 72, 57–129; repr. in Hermann von Helmholtz, *Wissenschaftliche Abhandlungen* (Leipzig, 1882–1895) vol. 1, 545–628, discussion on 556–58.

[84] Helmholtz, *Wiss. Abh.*, vol. 1, 557.

Chapter 11
Positions in Theoretical Physics

There were practical reasons, as Hertz described them to Voigt, for not making the position for a second physicist a position for a specialist in theoretical physics at this time. In the eyes of many physicists, there was no need to, for the best qualified young physicists could meet the demands of a theoretical position or an experimental position in roughly equal measure. Whether the position for a second physicist was for theoretical or for experimental physics, the accomplishments and the qualities looked for in the evaluations of physicists were much the same; the candidate for a theoretical position was expected to have demonstrated experimental skill, and the candidate for an experimental position was expected to be knowledgeable in theory. Helmholtz placed as much weight on experimental as on theoretical work when he considered a physicist for a theoretical position: evaluating Ferdinand Braun, he wrote about his accomplishments as an experimenter as well as about his thorough knowledge of theory,[1] and he recommended Felix Auerbach even though he praised him mainly for his experimental work and thought his theoretical-mathematical abilities less adequate.[2] Clausius in his recommendation of Auerbach discussed his researches area by area, noting his “solid knowledge,” “eager scientific striving,” skill and diligence in experimenting, and success in experimentally verifying Helmholtz’s and others’ theories, thereby extending and completing existing knowledge in his areas of physics.[3] Georg Quincke in considering Philipp Lenard for a theoretical position at Heidelberg was impressed by his experiments on cathode rays, and he required of him no more evidence of mathematical ability than his editorial role in the publication of Hertz’s *Principles of Mechanics*.[4]

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[1] Helmholtz to Prussian Minister of Culture Falk, 10 May 1877, STPK, Darmst. Coll. 1912.236.

[2] Helmholtz to Prussian Minister of Culture Gossler, 10 February 1888, STPK, Darmst. Coll. 1913.51.

[3] Clausius to Director Greiff, 15 August 1887, STKP, Darmst. Coll. 1913.51.

[4] Quincke and Leo Königsberger to the Baden Ministry of Education, 11 June 1896, Heidelberg UA.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_11

11.1 Helmholtz's and Kirchhoff's Moves to Berlin

It was significant for theoretical physics that Germany's most eminent man of science in mid-career turned primarily to teaching and research in physics. In the 1870s and 1880s, Helmholtz was the most influential physicist for the development of theoretical physics in Germany, the result of his position as director of the Berlin physics institute and his research and teaching there. His standing within German physics enabled him to make his view of theoretical physics effective in physics appointments.

Before Helmholtz became professor of physics at Berlin, in connection with his physiological studies of optics and tones he treated a variety of physical questions, drawing on researches we have discussed; for example, in his work on the activity of muscle nerves, he made use of Gauss and Weber's measuring methods, and in his work on the fine bones of the inner ear, he drew on Kirchhoff's theory of vibrations of infinitely thin rods. His physiological work gradually led him to direct his attention almost entirely to physics, above all to "problems of theoretical physics in which he showed his mastery." Early in this book, we discussed the Weber brothers' comprehensive study of waves, and through the century German physicists worked on problems of fluid mechanics. Helmholtz studied wave motions in a variety of fluids: water, air, and ether. In his theory of the weather, he treated waves in the atmosphere; in acoustics, he dealt with sound waves in air in open tubes; in electromagnetism, he treated the vibrations of the light ether viewed as an incompressible fluid.[5] In 1858, he published his first work on hydrodynamics, a theoretical study of the motion of a frictionless liquid whose particles rotate, a type of motion he called a "vortex." To render this new kind of motion "vivid," he drew attention to a "remarkable analogy" between water vortices and the magnetic action of an electric current, though he made no physical hypothesis about electromagnetism on this account. He was content to show that hydrodynamic quantities and electric current and magnetic masses are mathematically identical. More suggestive of physical applications was his conclusion that vortex motion can neither arise nor disappear; once a vortex exists in the liquid, it remains for all time.[6] This property gave rise to speculations about permanent atoms as vortex rings, uniting the discrete

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[5] Wilhelm Wien, "Helmholtz als Physiker," *Naturwiss.* 9 (1921): 694–99, on 695–97.

[6] Wangerin, notes to Hermann von Helmholtz, *Zwei hydrodynamische Abhandlungen*, Ostwald's Klassiker der exakten Wissenschaften, ed. A. Wangerin (Leipzig, 1896), 51–55. In solving hydrodynamic equations, physicists generally assumed the existence of a velocity potential. Helmholtz studied the consequences of excluding a velocity potential, which occur with the rotation of a fluid about an axis in which every element has the same angular velocity. (The concept of velocity potential was known in the eighteenth century, and Helmholtz gave it the name "potential" in analogy with Gauss's introduction of that name.) (Paul Volkmann, "Hermann von Helmholtz," *Schriften der Physikalisch-ökonomischen Gesellschaft zu Königsberg* 35 [1894]: 73–81, on 75).

and continuous view of matter. In another work on hydrodynamics in 1859, Helmholtz studied friction in fluids from a theoretical standpoint, while an associate in his laboratory did the corresponding experiments. His object was to include friction in the derivation of the fundamental equations of hydrodynamics, which in principle would allow every problem in fluid motion to be solved mathematically.[7] In practice very few could be solved, but his work on fluid friction led to further experimental and theoretical work on the subject by Stefan, O. E. Meyer, and others.[8] In 1868, he returned to hydrodynamics with a study of discontinuous fluid motion, which he introduced with an imperfect analogy. The partial differential equations for the interior of a frictionless, incompressible fluid are identical with those for stationary electric currents and for heat flow in conductors with uniform conductivity. But the flow of the fluid differs from the flow of electricity and heat[9]: a fluid passing through an opening with a sharp edge into a larger space of fluid remains a compact stream, whereas electricity and heat diverge after the opening. Helmholtz recognized that there is a discontinuity of motion in the fluid: neighboring layers of the fluid move past each other with a finite relative velocity, establishing a surface of separation. In this and in his earlier work on fluids, Helmholtz revealed an eye for analogies and a strong talent in mathematics, which was recognized by both mathematicians and physicists. Kirchhoff regarded Helmholtz's paper on vortex motion and a sequel to it the following year as his greatest contribution to mathematical physics.[10]

Helmholtz's name first appeared on a list of candidates for a Prussian chair of physics in 1868. This came about not only because of his reputation in physics but also because he had united different parts of science. The mathematician Rudolph Lipshitz, who took the initiative in the effort to bring Helmholtz to Bonn after the death of its physics professor, emphasized the "characteristic quality" of Helmholtz's "great mind." The faculty, he insisted, should make clear to the Prussian minister of culture that "certain fundamental ideas of [Helmholtz's] researches belonged to a region that is common to the sciences of inorganic and of organic nature." He had shown this from the beginning of his work in science with his memoir on the conservation of force in 1847. Lipshitz's characterization of Helmholtz did not impress the scientists at Bonn, who saw him more as a specialist

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[7] That year (30 August) Helmholtz wrote to William Thomson that he was busy working on experiments to test the form he had given to hydrodynamic equations (Leo Königsberger, *Hermann von Helmholtz,* 3 vols.[Braunschweig: F. Vieweg, 1902–1903], vol. 1, 343).

[8] Helmholtz was not the first to treat viscous motion; the French mathematical physicists Navier in 1823 and Poisson in 1831 and the Cambridge mathematician and physicist Stokes in 1844 had studied it. Wangerin, in Helmholtz, *Zwei hydrodynamische Abhandlungen*, 55.

[9] Wien, "Helmholtz als Physiker," 695–97.

[10] Hermann von Helmholtz, "Ueber discontinuirliche Flüssigkeiten," *Monatesber. Berlin Akad.* (1868): 215–28; trans. in *Philosophical Magazine* 36 (1868): 337–46, on 337–39; Leo Königsberger, *Hermann von Helmholtz*, trans. F. A. Welby (Oxford: Clarendon Press 1906), 161.

who happened to have two specialties, recommending him for his "classical investigations in the area of physical physiology" and for his outstanding achievements in "pure physics, especially in the mathematical direction." This formulation displeased Lipchitz because he thought it was based on a wrong understanding of Helmholtz's importance.[11]

Helmholtz had been teaching physiology for two decades, but physics had always been his preference, he told the Bonn curator, and he was glad for the chance to bring his job into line with his main interest. His achievements in physiology were all based on physics, but he could no longer pass on to his students what was best in his work, since they no longer knew enough mathematics and physics to follow it. He believed that he could still accomplish something in physics. He explained that the few great physicists in Germany were near the end of their careers, and no new generation had risen to take their place. Physics, the "true basis of all proper science," was not advancing as it should, and that was true especially of "mathematical physics." If he were to come to Bonn, he would want to teach not only experimental physics but also mathematical physics, and if through his teaching, students were to take up the work that needed to be done in physics, he would have succeeded in his goal.[12] Helmholtz's move to physics at Bonn seemed certain, since Prussia stood to gain a "mark of glory" by attracting him to one of its universities,[13] but the ministry of culture dragged out the negotiations, and in the end it failed to make Helmholtz a sufficiently attractive offer. For a time, he remained where he was, at Heidelberg University, as professor of physiology.[14]

Two years later, in 1870, Prussia returned to Helmholtz in its search for an illustrious physicist. Helmholtz's friend and then rector of Berlin University Emil du Bois-Reymond informed him of Magnus's death and of his own hope that he would succeed Magnus as director of the Berlin physics institute.[15] Helmholtz responded that while he had been waiting for a decision on the appointment to

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[11] Rudolf Lipschitz, "Entwurf eines Votums der mathematisch-naturwissenschaftlichen Section," n.d. [1868]; and draft by the natural science faculty, also n.d. [1868]; and Lipschitz, "Separatvotum," 13 July 1868; Plücker Personalakte, Bonn UA.

[12] Helmholtz to Bonn U. Curator Beseler, n.d. [summer 1868], quoted in Königsberger, *Helmholtz*, vol. 2, 115–16, on 116. In a letter to the physiologist Carl Ludwig, Helmholtz compared physics and physiology: he could lecture on "all parts" of physics with "completely independent judgment," whereas the manipulations and methods of physiology had diverged so that neither he nor any other one person could keep up with them all (Helmholtz to Ludwig, 27 January 1869, quoted in Königsberger, *Helmholtz*, vol. 2, 118–19, on 119).

[13] Bonn University Curator Beseler to Prussian Ministry of Culture Mühler, 4 August 1868, quoted in Königsberger, *Helmholtz*, vol. 2, 116.

[14] By 28 December 1868, Helmholtz had been offered the Bonn physics chair at a salary of 3600 thaler; by 3 January 1869, he had declined the offer. Helmholtz Personalakte, 1858/1907, Bad. GLA, 76/9939.

[15] Emil du Bois-Reymond to Helmholtz, 4 April 1870, quoted in Königsberger, *Helmholtz*, vol. 2, 178.

Bonn University, he had "thrown" himself into physical and mathematical studies again, and as a consequence he now knew even better than he had at the time of the Bonn call that he had become indifferent to physiology and was now interested only in "mathematical physics." But Helmholtz did not think of himself as a trained mathematical physicist who had proven himself in the subject. Kirchhoff was such a physicist, and Helmholtz thought that Berlin needed him. He urged Du Bois-Reymond to do all in his power to secure Kirchhoff's appointment and, above all, to keep the Prussian ministry officials away from him, for otherwise they would certainly fail to persuade him to move.[16] For his part, he would be satisfied to succeed Kirchhoff at Heidelberg University by moving from one chair to another, from physiology to physics.

The Berlin philosophical faculty regarded Helmholtz as the more "productive" and "more gifted and universal" of the two, but they preferred Kirchhoff, as Helmholtz had anticipated. They gave their reasons: Kirchhoff was trained as a physicist and had experience; his lectures were a model of "lucidity and finish"; he was better at supervising beginners' exercises than Helmholtz; and he had a greater "love of teaching."[17] The offer came to Kirchhoff, who turned it down, mainly, as he explained to Du Bois-Reymond, because he was unsure of his health, and he also liked the working conditions at Heidelberg, which the Baden ministry improved because of his call to Berlin.[18]

Du Bois-Reymond persuaded Helmholtz to state the conditions under which he would take the Berlin physics professorship, and this time the Prussian ministry of culture was ready to agree.[19] His conditions summed up the understanding of the needs of university physics that physicists of the previous decades had gradually arrived at. First, aside from an ample salary, Helmholtz wanted the ministry's assurance that a new physics institute would be built for him, equipped with everything he needed for teaching, for his own researches, and for practical student exercises.[20] Second, he wanted the ministry to agree that he alone would be in charge of the institute and the instrument collection, and that it would be his

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[16] Helmholtz to Du Bois-Reymond, 7 April and 17 May 1870, STPK, Darmst. Coll. F 1 a 1847. Helmholtz and Du Bois-Reymond were concerned that because of penny pinching, Prussia might appoint a physicist of secondary importance, someone like Quincke.

[17] Berlin Philosophical Faculty's recommendation of Kirchhoff to the Prussian Minister of Culture, n.d. [1870], quoted in Königsberger, *Helmholtz*, vol. 2, 179–80.

[18] Kirchhoff to Du Bois-Reymond, 9 June 1870, STPK, Darmst. Coll. 1924.55. To remain at Heidelberg, Kirchhoff asked for an increase in his institute budget to hire an assistant and an increase in his salary. The Baden Ministry of the Interior regarded both requests as "modest" in light of Kirchhoff's "significance" as a scholar of the first rank. Baden Ministry of the Interior, 10 June 1870, Kirchhoff Personalakte, Bad. GLA, 76/9961.

[19] Helmholtz to Du Bois-Reymond, 12 June, 25 June, and 3 July 1870, STPK, Darmst. Coll. F 1 a 1847.

[20] Helmholtz to Du Bois-Reymond, 12 June 1870.

decision to what extent and under what conditions the other physics teachers would be allowed to use it. Part of the second condition was that the physics institute auditorium would be reserved for his exclusive use, so that he could set up complicated instrumental arrangements and leave them in place. His third condition was that the institute would contain the director's living quarters. The actual construction of the new institute had to be deferred because of the Franco-Prussian war, which began during Helmholtz's negotiation with the Prussian government. With proper assurances, in December 1870, Helmholtz accepted the Berlin job.[21] Something unheard-of had happened, Du Bois-Reymond said: a professor of medicine and physiology had been named to the most important physics chair in Germany.[22] Helmholtz was fifty, and from then to the end of his life, his principal field was physics.

In Berlin Helmholtz soon had occasion to publicize his understanding of the proper training and practice of physics. Addressing the Prussian Academy of Sciences on his predecessor Magnus, he explained that a good experimenter needs thorough theoretical training and that a good theorist needs wide practical training.[23] Regularly consulted on appointments, he impressed his view of the complementary nature of experimental and theoretical approaches on faculties and ministries. Wien spoke of Helmholtz's particular service in striving to bring theoretical and experimental physics together into "one great science."[24]

Beginning around 1870, while Helmholtz was still at Heidelberg, and continuing through his first several years at Berlin, he carried out a critical study of the laws of "electrodynamics." This research, like his earlier one on the conservation of energy, originated in his physiological work. To understand the propagation of nervous impulses, he needed to understand the motion of electric currents in extended conductors and, in particular, the induction of currents in incomplete, or open, circuits. The electrodynamic laws proposed by Neumann, Weber, and Maxwell accounted for most electrical experiments involving closed circuits, but they had not yet been tested for open circuits, for which they predicted different results, as Helmholtz showed. By giving Neumann's electrodynamic potential for a pair of current elements a form that included the potential from Weber's and Maxwell's theories, he cast the three laws for electromagnetic induction as a single mathematical expression, distinguishing them by a numerical parameter. With this

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[21] Helmholtz to Du Bois-Reymond, 17 October 1870, STPK, Darmst. Coll. F 1 a 1847; Königsberger, *Helmholtz*, vol. 2, 186.

[22] Quoted in Königsberger, *Helmholtz*, vol. 2, 187.

[23] Hermann von Helmholtz, "Gustav Magnus. In Memoriam," in *Popular Lectures on Scientific Subjects*, trans. E. Atkinson (London, 1881), 1–25, on 19. Helmholtz spoke about the need to avoid a divorce between theoretical and experimental physics in, for example, his foreword in 1874 to the German translation of John Tyndall's *Fragments of Science*, repr. as "The Endeavor to Popularize Science," in *Selected Writings of Hermann von Helmholtz*, ed. R. Kahl (Middletown, CT: Wesleyan University Press, 1971), 330–39, on 337.

[24] Wien, "Helmholtz als Physiker," 697.

statement of the problem, he hoped to show how a decision between the laws might be made.[25]

In his first paper on the problem, Helmholtz developed an electrodynamic theory consistent with energy conservation, which made use of action at a distance and excluded forces that depend on motion. Neumann's potential, discussed earlier, between differential elements Ds and $D\sigma$ of two linear conductors s and σ, separated by distance r and carrying currents of intensities i and j, is:

$$Aij\,\frac{\cos\left(Ds, D\sigma\right)}{r}\,DsD\sigma,$$

where A is Weber's conversion ratio from electromagnetic to electrostatic units, numerically equal to the reciprocal of the velocity of light in the electrostatic system of units.[26] To form the complete potential, one not restricted to closed currents, Helmholtz included another term that acknowledges the unequal distribution of electricity and the forces acting on the ends of current elements. The result is:

$$-\frac{1}{2}A^2\frac{ij}{r}\left[(1+k)\cos\left(Ds, D\sigma\right)+(1-k)\cos\left(r, Ds\right)\cos\left(r, D\sigma\right)\right]DsD\sigma,$$

where k is a provisional constant.[27] The complete potential reduces to Neumann's law for $k = 1$, to Maxwell's for $k = 0$, and for Weber's and Carl Neumann's for $k = -1$. If the potential is integrated around a closed circuit, k drops out, showing that the predictions of the theories differ only in the case of open circuits. Helmholtz found that negative k allows for a continuous increase in the motion of electricity leading to infinite velocities and infinite electrical densities, in conflict with the energy principle. Positive k allows electricity to move in longitudinal waves with the velocity of light. Helmholtz concluded his paper of 1870 by examining the case for vanishing k. Drawing on Poisson's theory, regarding dielectric polarization as arising from both electrostatic and electromagnetic forces, he derived expressions

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[25] Early in 1870, Helmholtz published a first announcement of his theory of electrodynamics: "Ueber die Gesetze der inconstanten elektrischen Ströme in körperlich ausgedehnten Leitern," in *Verh. Naturhist.-med. Vereins zu Heidelberg*, vol. 5, 84–89; repr. in Helmholtz, *Wissenschaftliche Abhandlungen,* 3 vols. (Leipzig, 1882–95), vol. 1, 537–44. Later that year he followed it by the first part of the theory: "Ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper," *Journ. f. d. reine u. angewandte Math.* 72 (1870): 57–129, reprinted in *Wiss. Abh.*, vol. 1, 545–628, to which our discussion refers. Helmholtz's theory of electrodynamics is discussed in A. E. Woodruff, "The Contributions of Hermann von Helmholtz to Electrodynamics," *Isis* 59 (1968): 300–311, especially on 300, 302; also in M. Norton Wise, "German Concepts of Force, Energy, and the Electromagnetic Ether: 1845–1880," in *Conceptions of Ether: Studies in the History of Ether Theories 1740–1900*, ed. G. N. Cantor and M. J. S. Hodge (Cambridge: Cambridge University Press, 1981), 269–307, on 295–301.

[26] Helmholtz, "Bewegungsgleichungen," 562–63.

[27] Ibid., 567.

for longitudinal and transverse waves in a dielectric, which propagate with different velocities. By assuming a large dielectric constant, he showed that the transverse waves travel with nearly the velocity of light in air. Helmholtz concluded that the "remarkable analogy between the motions of electricity in the dielectric and those of the luminiferous ether does not depend on the special form of Maxwell's hypotheses but arises also in an essentially similar manner if we retain the older view." Electric "action at a distance" with finite velocity appears possible "without central changes in the foundation of accepted electrodynamic theory." Mathematically, Helmholtz's theory was the same as Maxwell's, but physically they were different. In addition to their different ideas about forces, in Helmholtz's theory charge is a substance, and in Maxwell's theory it is a displacement discontinuity in the ether. Despite the differences in physical meaning, German physicists regarded Helmholtz's theory as the way to arrive at Maxwell's theory, expressed in terms that were familiar to them, action at a distance and charge as a substance.[28]

The problem of devising experiments for testing the several electrodynamic laws proved difficult, and Helmholtz devoted much inconclusive effort to it after 1870.[29] According to his potential law for $k = 0$, corresponding to Maxwell's theory, no open electric currents can exist, as Maxwell pointed out; in 1875 Helmholtz decided that every electric motion in a conductor that leads to an accumulation of electricity at its surface continues into the surrounding insulator as an equivalent motion consisting of dielectric polarization, in conformity with Maxwell's theory.[30] Helmholtz published no experimental work on electrodynamics between 1871 and 1875, but he regularly linked his published theoretical discussions in these years to proposals for experiments, describing the arrangements for them and the results he expected from them. As it turned out, only for negative k, Weber's law, could the existing experimental means lead to a decision.

Helmholtz found that in the case of moving conductors, his potential law implies forces that are not present in the other theories unless the polarizability of the medium is very large. From an experiment on open circuits, he concluded that the new forces either do not exist or else the vacuum has to be polarizable, as it is in Maxwell's theory. He proposed a pair of prize questions on the subject through the Prussian Academy of Sciences, one of which was to decide experimentally if Maxwell's dielectric polarization has the same electrodynamic effects as an electric

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[28] Ibid., 558, 628. Jed Z. Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago and London: University of Chicago Press, 1985), 182–86, 192–93.

[29] Hermann von Helmholtz, "Ueber die Fortpflanzungsgeschwindigkeit der elektrodynamischen Wirkungen," *Sitzungsber. Preuss. Akad.*, 1871, 292–98; repr. in *Wiss. Abh.*, vol.1, 629–35; "Vergleich des Ampère'schen und Neumann'schen Gesetzes für die elektrodynamischen Kräfte," *Sitzungsber. Preuss. Akad.*, 1873, 91–104; repr. in *Wiss. Abh.*, vol.1, 688–701, on 701; Helmholtz to a "colleague," 8 February 1875, ETHB, Hs 87–402.

[30] Helmholtz, "Versuche über die im ungeschlossenen Kreise durch Bewegung inducirten elektromotorischen Kräfte," *Ann.* 158 (1875): 87–105; repr. in *Wiss. Abh.*, vol.1, 774–90, on 787–88.

current. He wanted his outstanding student Hertz to take up the question. Hertz decided that the means did not exist at the time to produce sufficiently rapid electric oscillations to answer it, but he kept it in mind, and later he answered it.[31]

Helmholtz's work on electrodynamics in the 1870s illustrates major concerns of theoretical physics: the derivation and generalization of a fundamental law, criticism of existing laws, clarification of a field of research, and experimental guidance in settling a theoretical dispute. He chose as his subject the foundations of a branch of physics, electrodynamics. He examined critically the law of electromagnetic induction, for which there were several candidates. To formulate his own law, he made use of the potential law, which covered the entire experimentally known domain of electrodynamics, generalizing it to encompass the conflicting laws. He saw that until the correct law was settled, the field was in disarray. Identifying this as the central problem of electrodynamics, he directed attention to it by making it a prize problem in the Prussian Academy and by allowing several experimenters working in his laboratory to select problems related to it.

In Berlin, as professor of physics, Helmholtz continued to lecture on physiological optics and acoustics as well as give the required experimental physics lectures, practical exercises in the laboratory, and occasional public lectures on general topics. In addition, each semester he lectured three or four times a week on mathematical physics at an advanced level, requiring his auditors to know mathematics at least through the calculus.[32] This was the first regular teaching of theoretical physics in Berlin, according to Planck. Helmholtz soon decided that its adequate instruction at Berlin demanded an ordinary professor for the subject, a conclusion the Prussian government accepted.[33]

Once again Berlin looked to Kirchhoff. In the past he had been reluctant to leave Heidelberg, in part because he had formed a "mathematical-physical school" with the mathematician Leo Königsberger.[34] He had turned Berlin down as Magnus's successor in 1870 and again in 1874 when he was invited to direct the new state solar observatory.[35] But he did have a continuing connection with Berlin through the Prussian Academy of Sciences, which had made him a corresponding member in 1861, and a "foreign" member in 1870, acknowledging his contributions to "mathematical physics" in the "disciplines of elasticity of solid bodies, electricity,

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[31] Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford: Oxford University Press, 2000), 231, 263.

[32] Berlin University, *Index Lectionum*, for the years immediately after 1871.

[33] Max Planck, "Das Institut für theoretische Physik," in Max Lenz, *Geschichte der Königlichen Friedrich-Wilhelms-Universität zu Berlin*, 4 vols. in 5 (Halle a. d. S.: Buchhandlung des Waisenhauses, 1910–18), vol. 3, 276–78, on 276.

[34] Kirchhoff to the Baden Ministry of the Interior, 16 December 1874, Kirchhoff Personalakte, Bad. GLA, 76/9961. Königsberger left Heidelberg in 1875.

[35] Again Baden raised Kirchhoff's salary to keep him. Baden Ministry of the Interior, 10 March 1874, Kirchhoff Personalakte, Bad. GLA, 76/9961.

and hydrodynamics."[36] In 1874 the academy approached him again, this time offering him a full-time, salaried position for research with no teaching duties attached, and he accepted. The terms were then modified: Prussia appointed Kirchhoff to a newly founded ordinary professorship for "mathematical physics" at Berlin University, with his salary being paid in part by the Prussian Academy. With this understanding, in 1875 Kirchhoff moved to Berlin, trusting that the faculty would receive him kindly even though he was placed among them without their initiative.[37] Faced with losing Kirchhoff, Heidelberg had designs on Helmholtz as his replacement, but Prussia improved Helmholtz's working conditions, and so it came about that Helmholtz and Kirchhoff again taught in the same university, bringing distinction to Berlin as they had before to Heidelberg, now both in physics.[38] In the summer of 1875 Kirchhoff began his teaching at Berlin with lectures on mechanics. From fourteen students at the start, his class grew to thirty-one after a few weeks, and the continuation of the course in the winter semester on mathematical optics drew fifty-six. With these numbers, Kirchhoff felt "very satisfied."[39]

We see that near the end of his career, Kirchhoff acquired a position in the subject that he had desired at the beginning. For a quarter of a century he held positions in experimental physics while carrying out researches in mathematical physics. His situation changed when the needs of physics instruction at Berlin made possible the creation of a senior position in mathematical physics, and his poor health made it reasonable for him to accept a call to it (as Ohm's poor health had made it reasonable for him briefly at the end of his career to hold a similar position). Kirchhoff was only the second (or the third, if Ohm is counted here) after Listing in Göttingen to become an ordinary professor for mathematical physics in Germany. (Neumann had taught mathematical physics as ordinary professor of physics and mineralogy.) Like Listing (and Ohm), Kirchhoff obtained the position under highly special circumstances, but unlike Listing, Kirchhoff was predominantly a mathematical physicist, a circumstance which attached to his move to Berlin the additional significance of furthering the recognition of the field in Germany.

Just as a field needs a body of knowledge and methods to give it substance, it needs paying positions to attract young, talented scientists to ensure its continuation. Planck, the foremost representative of theoretical physics in Germany in the next generation, began his university studies just as Kirchhoff took up a position in mathematical physics equal in rank to a position in experimental physics. To Planck, who attended Kirchhoff's lectures, Kirchhoff's example would have

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[36] "Wahlvorschlag für Gustav Robert Kirchhoff (1824–1887) zum AM," 10 March 1870, in *Physiker über Physiker*, ed. Christa Kirsten and Hans-Günther Körber (Berlin: Akademie-Verlag, 1975), 77–79.

[37] Kirchhoff to Eduard Zeller, 5 January 1875, Tübingen UB, Md 747/373.

[38] Kirchhoff to Emil du Bois-Reymond, 30 October, 18 November, 13 and 17 December 1874, STPK, Darmst. Coll. 1924.55.

[39] Kirchhoff to Königsberger, 1 and 26 May 1875, STPK, Darmst. Coll. 1922.87; Kirchhoff to Robert Bunsen, 25 November 1875, Heidelberg UB.

pointed to the goal, if not yet to a regular route to achieving it, of a career in the new specialty.

In his research, Kirchhoff did not look for what was new, but for what he could treat rigorously, both experimentally and theoretically; to complete, not to begin. He made fewer observations than other experimenters, but those he made were after thorough theoretical preparation, each a "masterwork" of method. Voigt thought that Kirchhoff's law of emission and absorption of radiation showed his gift in the clearest way: Kirchhoff recognized what was suggested here and there by others, and "raised it through theoretical proof and systematic observation to a scientific theorem." With reference to the same work, Boltzmann said that Kirchhoff ordered the splintered ideas around him into a "unified whole," leading to discoveries in physics, chemistry, and astronomy. Like that of his teacher Neumann, Kirchhoff's work was characterized by the "sharpest precision of hypotheses." He hesitated to pass judgment on new problems or to set students to work on them. Formal in manner and exacting in his standards, he was accessible to young physicists but he did not try to draw them close, and he was given to cross-examining students when they had questions. His extreme caution was admired, but it did not create followers. Among his favorite words were "probably" and "perhaps," which he used unless he was absolutely certain. His experimental demonstrations were "precise and elegant," and his lectures were "carefully thought out, no word too many, none too few," and perfect. (Too perfect to Graetz, who after hearing Kirchhoff's first lectures on mechanics wrote to a friend that they offered "absolutely nothing" that was not in his book, a "waste of time." To Planck, they sounded "like a memorized text, dry and monotonous." Later Planck's own lectures were described as dry, impersonal, and faultless.) Kirchhoff's influence was exerted mainly through his published lectures and researches. He lived a typical life of a German professor, devoted to lectures and researches. Nothing could disturb his "inner harmony."[40]

11.2 Junior Positions in Mathematical/Theoretical Physics

The main obstacle in the path of an aspiring physicist in the German university system was the shortage of attractive junior positions. The problem was how to make good use of the physics institute for teaching and research and, at the same time, to give the next generation of university teachers and institute directors the experience that would qualify them one day to succeed to ordinary professorships.

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[40] Woldemar Voigt, "Zum Gedächtniss von G. Kirchhoff," *Abh. Ges. Wiss. Göttingen* 35 (1888): 3–10, on 8–10; Ludwig Boltzmann, *Gustav Robert Kirchhoff* (Leipzig, 1888), iv-vi, 21–22, 30; Max Planck, *Wissenschaftliche Selbstbiographie* (Leipzig: Johann Ambrosius Barth-Verlag, 1948), 7–34, on 8; repr. in Max Planck, *Physikalische Abhandlungen und Vorträge,* 3 vols. (Braunschweig: F. Vieweg, 1958), vol. 3, 374–401; Graetz to Auerbach, 6 May 1877, Auerbach Papers, STPK; Arthur Schuster, *The Progress of Physics During 33 Years (1875–1908)* (Cambridge: Cambridge University Press, 1911), 4–5.

The humanities, theology, and law could accommodate junior teachers whenever ministries or faculties wanted them. The principle of the Prussian ministry of culture early on was that a Privatdocent's promotion to extraordinary professor was not to fill a gap in instruction but to recognize the candidate's qualification to become an ordinary professor someday, an accommodation which could be met at no greater cost than a small salary.[41] That principle was not well suited to the natural sciences and medicine because of the scientist's double responsibility as professor and institute director. As ordinary professor, a physicist could not do his job well if he did not have the material means provided by an institute; as administrator of a state institute, he could not do his job well if an extraordinary professor had the right to use the institute.

For young physicists who wanted to teach theoretical physics, there was another obstacle, a difference of opinion over just what kind of science "theoretical physics" was. Oskar Emil Meyer, a recent graduate of Königsberg, provides us with an example. In a letter in 1861 to his teacher, Neumann, he explained why he had had not followed Neumann's advice, which was to habilitate at Breslau and there begin his teaching career as "Privatdocent for theoretical physics." Although there was need for the "mathematical treatment of physics" at Breslau, as there was at "every other university," it would be next to impossible for a Privatdocent to interest students who had no inkling of the existence of theoretical physics. The experimental physics professor Moritz Frankenheim encouraged Meyer to remain at Breslau to lecture on "theoretical physics" and crystallography, but when they discussed the prospect, Meyer realized that Breslau had a quite different idea about theoretical physics than his own. Theirs was that "the whole amount of mathematics that is to be applied reduces to a few interpolation formulas by which constants are given as a function of temperature." Meyer said that it would be superfluous for him to lecture on material like that, since it was covered in experimental physics. Meyer summed up his criticism of Breslau: they had "no idea of that which is meant by theoretical physics in Königsberg," and therefore they could have no interest in that "branch of science."[42] In the end, it worked out for Meyer. He habilitated at Göttingen, and after 2 years there as Privatdocent, he returned to Breslau as extraordinary professor and successor to the mathematician Rudolf Lipchitz. He

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[41] The rule of promotion of the Prussian ministry of culture was discussed in a faculty debate at Bonn University, 16 October 1827, Plücker Personalakte, Bonn UA. The rule was not generally accepted in Germany later on. Robert von Mohl, at Tübingen, gave the following definition in 1869: "Extraordinary professors: younger but already experienced teachers for whom there is no ordinary position for the time being. Their uses are various: to fill gaps that do not allow for the establishment of a new chair; to supplement ordinary professors who are no longer fully capable of service; to multiply courses." (Württemberg, Statistisches Landesamt, *Statistik … Tübingen*, ed. K. Statistisch-Topographisches Bureau [Stuttgart, 1877], 25). With this understanding, extraordinary professorships for theoretical physics began to be created around this time.

[42] Meyer to Neumann, 6 February 1861, Gőtt. U. Lib., Ms. Dept.

was expected to give mathematics lectures, but, by informal agreement, he considered himself the successor to Kirchhoff, who had left Breslau in 1854 and had not been properly replaced.[43] He considered it his "real task," he told Neumann, to recover for Breslau what it had lost when Kirchhoff moved away, instruction in mathematical physics, and to that end he would direct his mathematics lectures as well.[44] In 1865 he was promoted to ordinary professor, and in 1867, after Frankenheim retired, he was appointed director of the physics institute. Meyer's publications dealt mainly with mathematical and experimental investigations of problems of theoretical physics.

Most physics institutes in this period were limited to a few rooms, a small laboratory space, if any, and a small budget, not enough usually for one physicist and certainly not enough for more than one. The most important measuring instruments were expensive, so they could hardly be acquired in duplicate or triplicate. Often instruments were the handiwork of the institute director himself, designed specifically for his own researches and possibly even paid for with his own money; they were not in any case the kind of instruments he could be called upon to share with a colleague. Once his measuring instruments were set up and adjusted for a particular experimental investigation, neither they nor the space they occupied could be shared with another physicist for the duration of the research, which might be months or even years. Given the material restraints, there was no way around the arrangement of putting university institutes at the complete disposal of one physicist. With specific reference to anatomical institutes, but applying equally to "every other collection," Helmholtz observed in 1860 that there was "always a considerable difficulty . . . if two teachers are to use their collection as ordinary professor, that is, with equal official entitlement."[45] For this among other reasons, Kirchhoff spoke of the "scientific versatility and depth" required of the "ordinary and lone representative of physics" at a university.[46] As the demands of physics teaching increasingly led to the appointment of a second, often already established, physicist to aid the director of the institute, the opportunities for "collisions" over the use of the facilities of the institute multiplied. In the same measure as the research needs of

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[43] Hermann Marbach had been promoted to extraordinary professor following Kirchhoff's departure, but he was not regarded in Breslau as Kirchhoff's replacement.

[44] Meyer to Neumann, 16 July 1864.

[45] Reinhard Riese, *Die Hochschule auf dem Wege zum wissenschaftlichen Grossbetrieb*, ed. Werner Conze (Stuttgart: Ernst Klett, 1977), criticizes the German university structure for keeping young scholars from advancing. But he does not take account of the factors that set some fields such as physics apart from others; for example, the material limitations we have described, which made the continuation of that structure useful for the time being. See his chapter "Die Nichtordinarienfrage," 153–92, particularly the first two sections. Riese quotes Helmholtz on p. 115.

[46] Kirchhoff to a colleague, 25 December 1865, STA, Marburg, Bestand 305a, 1864/66 Melde.

the second physicist were recognized, the arguments against the division of the director's sole responsibility for the institute workshop were sharpened.[47]

At the same time, the needs of beginning physicists—instruments, laboratories, funds, to say nothing of salaries—had to be met, and yet they rarely could be. Prussian physics graduates had to wait for two, later for three, years after their doctorate before they could become Privatdocenten. Those who needed to earn money during the waiting time usually took a teaching job, preferably one near a university with a good institute; those with some money of their own or with unusual persistence, used the two or three years to do research in a university physics institute, if the director and the accommodations allowed it, and to publish as frequently as they were ready. Becoming a Privatdocent anywhere hardly improved a young physicist's financial situation, and it could deprive him of access to a university institute, reducing his opportunity for research. About the only chance a young physicist had of entering physics teaching was by lecturing on mathematical physics, Weber wrote in 1873. But mathematical physics brought little in the way of student fees and it did not prepare young physicists for their future duties. As a consequence, "even those who preferably wanted to devote themselves to physics nevertheless habilitated in . . . mathematics or, if they were somewhat removed from that, in . . . chemistry."[48] Their loss had "proven to be detrimental for the experimental pursuit of physics" at German universities, and only the increasing number of assistantships at physical institutes in the early 1870s gave Weber hope that the problem might be alleviated.[49] Gerling's idea, dating

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[47] Another argument against two physicists at one institute, especially with the second one in a junior position, was that it made it difficult to appoint the best man in case of a vacancy in the senior position. It usually meant bringing in someone from the outside and placing him above the physicist already at the institute, under which circumstances the new physicist might hesitate to accept the job; the combination might become troublesome because of the junior physicist's frustrated hopes of advancing to the senior position himself or because of his possible loss of earlier accesses to facilities, students, and teaching subjects, which the new director might wish to claim for himself. Riese, *Hochschule*, 133.

[48] Weber to Göttingen U. Curator Warnstedt, 26 October 1873, Weber Personalakte, Göttingen UA, 4/V b/95a. Young physicists sometimes tried to combine university teaching with teaching at secondary or trade schools, as, for example, Hermann Schaeffer did at Jena, or Marbach at Breslau. When they received no offers of professorships from other universities and were passed up for the local professorship, they sometimes left academic life, as L. F. Ofterdinger did at Tübingen in 1851, reconciling themselves to a secondary school professorship. Others took paying jobs outside physics: Wilhelm Feussner at Marburg supported himself by working also as a librarian, O. J. Seyffer at Tübingen also worked as the editor of a newspaper, and C. H. Tielle at Kiel studied medicine while he was a Privatdocent for physics and then left physics to establish himself as a physician.

[49] Weber to Göttingen U. Curator Warnstedt, 26 October 1873. Under earlier conditions of physics institutes, there had been obstacles to the use of assistants; for example, they presupposed ready-made instruments, hence a certain affluence of the institute, as Weber explained to the Göttingen U. Curator, 10 May 1851, Göttingen UA, 4/V h/21.

back to 1848, that the employment of assistants could constitute a "nursery" for physics was proving to be correct.[50] Assistantships allowed physicists waiting out their two or three years until habilitation to earn a small living without leaving research.

The training of physicists before the doctorate was not much affected by having only one ordinary physics professor at a university. Students working toward doctorates were still few in number at most universities, and their research was easily supervised by one professor. They were also encouraged to study at other universities with other physicists, who even welcomed them into their laboratories, particularly if it helped them in their doctoral research. Wüllner was one of the students who took this path, doing research with Magnus's methods in Magnus's Berlin laboratory for a dissertation at Munich. By this time, some of the better institutes could specialize in the areas of research of their directors, facilitating the work of students in these areas.

The dominant institutional development in theoretical physics at the German universities after 1870 was the creation of extraordinary professorships for the subject. As the activities in the physics institutes multiplied and the number of students in the experimental lectures grew in the 1870s and 1880s, the arrangement by which regular instruction in mathematical, or theoretical, physics was left to a second, usually younger physicist was introduced at almost every German university; at about a third of the universities, this arrangement was put on a permanent basis, with a salaried extraordinary professorship created for a lecturer on theoretical physics.

At first the purpose of extraordinary professorships was solely to support the ordinary professor of physics, not to acknowledge a new specialty. They were planned as transitional positions for young physicists, whose ultimate destination was an ordinary professorship of experimental physics. In pursuit of it, they needed to do experimental research; so that although the junior physics positions almost never came with separate institutes, they generally came with the privilege of limited use of the facilities of the physics institute at the director's discretion.

Most of the new extraordinary professorships were at Prussian universities. Since several of them were not specifically designated, it might be thought that the new positions were merely a continuation of the old Prussian pattern of multiple appointments, but that is not the case. Whether so designated or not, the new extraordinary professorships were used invariably to supplement the work of the ordinary professors rather that to compete with it, and in any dispute the ministry supported the ordinary professor. It worked this way outside of Prussia as well. When designated, the new extraordinary professorship might be called mathematical physics, though increasingly it was called theoretical physics.

The proper organization of university physics could best be realized in a new university, with a newly appointed faculty and a new institute facility. At Strassburg

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[50] Christian Ludwig Gerling, *Nachricht von dem mathematisch-physicalischen Institut der Universität Marburg* (Marburg, 1848), 21.

University, which Prussia acquired through its war with France, the physics institute became the model for training physicists in the early 1870s. The first ordinary professor of physics and director of the physics institute there was August Kundt, who had acquired experimental skill as a student in Magnus's laboratory in Berlin. In his inaugural lecture, he said that he had not acquired a comparable training in "theoretical physics," and he came to regret his one-sided education and to advocate a balance between experimental and theoretical physics. The modern experimental physicist, he said, could hope for success only if he took theory as his guide in selecting problems for research.[51]

From the beginning, the Strassburg physics institute made provision for a separate position for teaching mathematical physics. Emil Warburg, Kundt's former student and collaborator in Magnus's laboratory, was appointed extraordinary professor for mathematical physics in 1872, and Kundt left its teaching entirely to him.[52] Warburg's stay at Strassburg was the only time in his career when he came close to being exclusively a mathematical theorist, not only in his teaching but also in his research. His reunion with Kundt resulted in joint investigations of the kinetic theory of gases, which had been prepared by Kundt's discovery of a simple method of measuring the velocity of sound; in their application of the method, Warburg did the mathematical work.[53] When Warburg left Strassburg in 1876 to take up the physics chair at Freiburg, he was succeeded by W. C. Rőntgen, another former student and assistant of Kundt's. Rőntgen in turn was succeeded by Ferdinand Braun and Friedrich Kohlrausch, both of whom quickly moved on to ordinary professorships of physics. Then in 1884, for the first time, the position went to a predominantly theoretical physicist, Emil Cohn, another former student of Kundt's. Cohn taught theoretical physics for over 30 years in the Strassburg extraordinary professorship, a record that was contrary to the original conception of such a position.[54]

As at Strassburg, at Königsberg the Prussian ministry established a formal extraordinary professorship in 1876 or soon after, and it did so again at Marburg in 1877.[55] At other universities, particularly where the ordinary professor of physics was primarily a mathematical physicist or had an interest in teaching mathematical physics himself, the new extraordinary professorships were left unspecified; that

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[51] August Kundt, "Antrittsrede," *Sitzungsber. Preuss. Akad.*, 1889, pt. 2, 679–83, on 682; Wilhelm von Bezold, "Gedächtnissrede auf August Kundt," *Verh. Phys. Ges.* 13 (1894): 61–80.

[52] Eduard Grüneisen, "Emil Warburg zum achtzigsten Geburtstage," *Naturwiss.* 14 (1926): 203–7; *Deutscher Universitäts-Kalender* (1872–1876) (Berlin, 1872-[semiannual]).

[53] Friedrich Paschen, "Gedächtnissrede des Hrn. Paschen auf Emil Warburg," *Sitzungsber. Preuss. Akad.*, 1932, cxv-cxxiii, on cxvii.

[54] Strassburg, University, *Festschrift zur Einweihung der Neubauten der Kaiser-Wilhelms-Universität Strassburg* (Strassburg, 1884), 143.

[55] Marburg, University, *Catalogus professorum academiae Marburgensis*, ed. F. Gundlach (Marburg: Elwert, 1927), 394–95; Helmholtz to Prussian Minister Falk, 10 May 1877, STPK, Darmst. Coll. 1912.236; Prussian Ministry of Culture to Marburg U. Curator, 15 April 1880, STA, Marburg, Bestand 310 Acc. 1975/42 Nr. 2037.

way the younger man might be used to teach other parts of the physics curriculum as well as mathematical physics, depending on where he was needed at the time. But if at these universities the ordinary professorship passed into the hands of an experimentalist, they too followed the model of Strassburg.

Bonn University is a case in point. There in 1874 Eduard Ketteler, who had been elevated from Privatdocent to extraordinary professor at his own request 2 years before, received a newly created, salaried extraordinary professorship. He was employed, at a little additional salary, in assisting Bonn's ordinary professor of physics Clausius in running the laboratory practice course. When Clausius died in 1888, Ketteler was temporarily assigned the lectures on experimental physics and the direction of the physics institute, which placed him in line for a possible permanent advancement. Wanting to strengthen theoretical physics at Bonn, the philosophical faculty decided it was time to establish an ordinary professorship for the subject. As evidence of the need, they pointed to the universities that had preceded theirs in establishing two physics positions: Berlin with Dove and Magnus and, following them, Helmholtz and Kirchhoff; Gőttingen with Weber and Listing and, following them, Eduard Riecke and Woldemar Voigt; and Königsberg with Neumann and Ludwig Moser. The faculty proposed a division of teaching assignments and facilities resembling Strassburg's: one of the two professors was to lecture on the general foundations of theoretical and experimental physics, give the lectures on experimental physics, and direct the exercises in the physics laboratory; the other professor was to lecture on individual branches of theoretical physics. The former professor would be in charge of the physics institute, while the latter would receive certain rooms in it for his experimental research.[56] The position the faculty had in mind for the theorist was less important than that of the experimentalist, and the theorist was to be dependent on the experimentalist for space and equipment for his teaching. On the whole, neither professor could have found the arrangement to his liking. In the event, it was carried out only for a short time.

On the same day in January 1889 on which the ministry appointed Heinrich Hertz to the chair of experimental physics at Bonn, it appointed Ketteler ordinary professor for theoretical physics. Hertz was told that he had Clausius's professorship, which carried the obligation "to represent experimental physics in lectures and exercises, and to direct the physics institute"; Ketteler was assigned only theoretical physics, forestalling any difficulties. By agreement between Hertz and the ministry, Ketteler was to receive two rooms in the institute from Hertz. But the ministry apparently had little interest in perpetuating two ordinary professorships for physics anywhere but in Berlin or where they were already long established as at Göttingen; in October 1889 Ketteler was given the ordinary professorship of physics at

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[56] Prussian Ministry of Culture to Bonn U. Curator Gandtner, 24 October 1888, Ketteler Personalakte, Bonn UA; Lipschitz to Dean Lubbert of the Bonn U. Philosophical Faculty, 10 November 1888, Akten d. Phil. Fak., Bonn UA.

Münster University, and the position he had held briefly at Bonn reverted to a properly subordinate one, that of an extraordinary professor.

The physicist Hertz acquired was Hermann Lorberg,[57] who being in his late fifties and relatively inexperienced would not have expected to use the position as a stepping stone to an ordinary professorship of any kind. He had taught secondary school until 1889, when he became Privatdocent at Strassburg, but he fitted Hertz's description of someone capable of teaching the subject, having published a number of theoretical papers on electrodynamics and a physics textbook for secondary schools. "I would like it best," Lorberg wrote deferentially to Hertz before coming to Bonn for his first semester as extraordinary professor, "if I could give a four-hour a week lecture course on 'Theory of Static Electricity and Magnetism' as well as a two-hour a week public lecture course 'On Recent Investigations in the Area of the Theory of Electricity'—if you are not reserving that for yourself—or on physical chemistry; or also a private lecture course on mechanical heat theory or mechanics—if Prof. Lipschitz does not want to lecture on these."[58] A much older man than was usually hired for the second physics position, Lorberg was at first given a temporary appointment, which was then made permanent; through Hertz's long illness and then through his own, Lorberg remained extraordinary professor for theoretical physics at Bonn to the end of his career.

At Breslau University and Halle University, the extraordinary professorships were kept equally flexible, as the position at Bonn had been before 1890. As late as 1894, the appointment of Philipp Lenard as extraordinary professor at Breslau still defined his duty as teaching physics in "lectures and exercises by supplementing the teaching of the ordinarius [ordinary professor] appointed for the subject and according to a more detailed agreement with him."[59] Halle University had an extraordinary professor from 1878, the former Privatdocent for theoretical physics Anton Overbeck. In 1884, Overbeck was promoted to personal ordinary professor for theoretical physics because he had received an offer from Karlsruhe Polytechnic, but like Ketteler at Bonn, Overbeck was moved to the ordinary professorship of physics at another university the next year. His title at Halle—that of a personal ordinary professor, not of an ordinary professor—went to Ernst Dorn, probably because Dorn was already ordinary professor in his previous position and could hardly be demoted. When Dorn became the ordinary professor of experimental physics at Halle in 1895, the second position was returned to an extraordinary professorship, and its occupant, Karl Schmidt, was charged with teaching theoretical physics "in accordance with a more specific agreement with Prof. Dr. Dorn."[60]

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[57] Bonn U. Curator to Lorberg, 20 June 1890, and other documents in Lorberg Personalakte, Bonn UA.

[58] Lorberg to Hertz, 14 July 1890, Ms. Coll., DM, 2971.

[59] *Chronik der Königlichen Universität zu Breslau*, 1894–1895, 6–7.

[60] For this and other information on the development of theoretical physics at Halle University, we are grateful to the director of the Halle University Archive, Dr. H. Schwabe.

Of the non-Prussian German universities, only two acquired an extraordinary professorship for theoretical physics during this period: Munich in 1886 and Jena in 1889. These new positions were created late, perhaps because these universities and also Leipzig enjoyed regular and competent instruction in theoretical physics for some time before they made formal arrangements for it.

Leipzig University attracted the mathematical physicist Karl Von der Mühll in 1867. The opportunity arose because the mathematics faculty was then losing one of its members just when the presence of about forty students of mathematics and physics required a full complement of teachers. By offering to teach both mathematics and mathematical physics, Von der Mühll made himself acceptable to the faculty. The ordinary professor of physics, Wilhelm Hankel, taught mathematical physics as well as his regular subjects, but he welcomed a second physicist to take over some of the burden.[61] In 1872 Von der Mühll was promoted to extraordinary professor of "physics," but he continued to teach mathematical physics and mathematics. He offered a fairly comprehensive program, which included "theoretical physics," elasticity theory, potential theory, topics from mechanics and from optical theory, electrodynamics, and mechanical heat theory, and he also participated in the direction of the mathematical seminar, where he took up subjects related to physics.[62] From 1878 to 1886, he was joined at Leipzig by a second extraordinary professor, Eilhard Wiedemann, who also taught mathematical physics.[63] Von der Mühll, perhaps handicapped because he did little original research and almost none in physics, remained in his position at Leipzig for nearly 20 years without ever attaining formal recognition for his field of mathematical physics. Finally, in 1889, he returned home to Switzerland to a university post there.[64]

At Jena University, an informal but satisfactory arrangement for teaching theoretical physics was transformed into a regular extraordinary professorship for theoretical physics in 1889, which was filled by the Breslau Privatdocent Felix Auerbach.[65] In the early 1880s, Munich University had two Privatdocenten for theoretical physics, Max Planck and Leo Graetz. In 1886, after Planck had left,

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[61] Von der Mühll to Franz Neumann, 29 November 1867, Neumann Papers, Göttingen UB, Ms. Dept.

[62] Saxon Ministry of Education to Leipzig U. Philosophical Faculty, 19 December 1872, and Felix Klein's report on Von der Mühll's teaching, dated 15 March 1886, in Von der Mühll Personalakte, Leipzig UA, Nr. 759.

[63] Saxon Ministry of Education to Leipzig U. Philosophical Faculty, 1 February 1878, and the draft report of the Philosophical Faculty on Wiedemann's teaching, dated 28 January 1878, in Eilhard Wiedemann Personalakte, Leipzig UA, Nr. 1060.

[64] Carl Neumann reported on Von der Mühll's activities at Leipzig on 8 May 1886 with reference to a promotion to ordinary honorary professor for his teaching of over twenty years. He noted Von der Mühll's lack of productivity, citing only three publications. Saxon Ministry of Education to Leipzig U. Philosophical Faculty, 5 January 1889. Both in Von der Mühll Personalakte, Leipzig UA, Nt. 759.

[65] Dean Kalkowsky, minutes of the meeting of Jena U. Philosophical Faculty on 7 August 1889, and Ministry to the "whole university," 27 September 1889, "Allgemeine Fakultätsacten," Jena UA, Nr. 621a.

Munich established a regular position: the Privatdocent for physics Friedrich Narr was appointed extraordinary professor with the duties of holding regular lectures on the "discipline of theoretical physics" and of directing the practice course in the institute and in the seminar.[66]

At Kiel University, the last of the Prussian universities to acquire an extraordinary professorship for theoretical physics between 1870 and 1890, the new position received a different interpretation by its initiators and by its first two occupants. In the fall of 1882, in connection with deficiencies of mathematical instruction at the university, Kiel considered establishing a new position for mathematical physics. The Kiel mathematician L. A. Pochhammer argued against a proposal to create an ordinary professorship for the combination of mathematics and mathematical physics, since the position would deprive mathematics ultimately of its second position. Whoever held it would have little interest in teaching elementary mathematics, which would mean a significant loss at Kiel where there were almost no Privatdocenten in mathematics. As mathematical physics was an independent and large subject, and as its representative would naturally want to stay within it, Pochhammer urged the creation of an extraordinary professorship restricted to mathematical physics.[67]

In December 1882, the Kiel philosophical faculty applied for an extraordinary professorship for "theoretical" physics, offering the following reasons for their request. In the past, lectures on theoretical physics had been given by teachers of mathematics and physics, whose own subjects were still sufficiently limited to allow it. Now theoretical physics was recognized as a necessary specialty at many universities, and not just at the largest, valued as a link between mathematics and the natural sciences and an enrichment of both. Mathematics students were deflected from becoming too narrow, since theoretical physics allowed them to put their abstract mathematical knowledge to use, and students of the natural sciences were motivated to complete their knowledge of mathematics when they studied theoretical physics. Theoretical physics also prepared students to study other sciences, such as those taught in the medical faculty. Finally, the philosophical faculty had a practical reason for wanting a theoretical physicist: whenever the ordinary professor of physics could not attend examinations, there would now be someone else to represent his field.[68]

The Kiel curator forwarded the faculty's request together with the observation that the growth of the natural sciences since the time of C. H. Pfaff (who had taught several sciences at Kiel) made it impossible now for a professor to teach more than one field of science. He did not know if there was an urgent need to establish an extraordinary professorship for theoretical physics, but he knew that Gustav

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[66] Bavarian Ministry of the Interior to Munich U. Senate, 2 August 1886, Munich UA, E II-N, Narr.

[67] Schöne to Althoff, n.d. [1882], and Pochhammer to Schöne, 14 October 1882, DZA, Merseburg.

[68] Kiel U. Philosophical Faculty to Prussian Minister of Culture Gossler, 14 December 1882, DZA, Merseburg.

Karsten, who was the only person teaching physics at Kiel at the time, was unable to give extensive lectures on the subject. He left the decision to the ministry. Noting that there were some forty students of mathematics at Kiel, the ministry thought that the faculty's request was justified but that a paid Privatdocent might be the answer. (In recent years the Privatdocent Leonard Weber had given lectures on parts of theoretical physics, but he had left.) In any case, the ministry would list the extraordinary professorship for the 1884–1885 budget.[69]

The man they hired for Kiel was Henrich Hertz. Originally intending to become an engineer, Hertz read Wüllner's textbook on physics in his spare time while working for construction engineers in Frankfurt am Main after leaving secondary school. With his interest in the natural sciences rekindled, he attended lectures at the Frankfurt Physical Society and soon after that he briefly attended the polytechnic school in Dresden. He regarded mathematics as his favorite subject at this time. After a year of military service, he entered the polytechnic school in Munich, where he soon decided that he liked physics better and that he wanted to make it his profession instead of engineering. In asking his father's permission to change plans, he explained that he might have been happy as an engineer but equally so as a book binder or woodworker or "anything ordinary." Engineering relied on practical talents and practical knowledge, which did not interest him, whereas science promised him lifelong study and the possibility of becoming an important investigator.[70]

In Munich, Hertz moved freely between the university and the polytechnic, where he took laboratory practice. Bezold and the professor for technical physics at the polytechnic advised Hertz not to turn to physics too early but to get a good mathematics education first, common wisdom then.[71] On the advice of the university physics professor, Philipp Jolly, Hertz studied mechanics and mathematics on his own from the classical French works by Lagrange, Laplace, and Poisson. Finding Lagrange "horribly abstract" and more recent writers of little help, Hertz despaired of grasping the "individual parts of contemporary mathematics in their connection." He believed that in nature everything is mathematical if properly understood, but at the same time he believed that non-Euclidean geometry, geometry of four or more dimensions, elliptic functions (the subject of special lectures he

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[69] Kiel U. Curator Mommsen to Prussian Ministry of Culture, 27 December 1882, DZA, Merseburg.

[70] Hertz to his parents, 1, 7, and 25 November 1877, in Heinrich Hertz, *Erinnerungen*, *Briefe*, *Tagebücher*, ed. J. Hertz, 2nd rev. ed. by M. Hertz and C. Süsskind (San Francisco: San Francisco Press, 1977), 62–72.

[71] For example, Helmholtz advised his son Robert, the future physicist, who at the time was starting his university studies, to study mathematics before studying physics. Helmholtz said of himself: "As far as mathematics is concerned, my interest developed only through its applications, especially those of mathematical physics, and I studied everything I know of mathematics only occasionally for purposes of application. But that is a method that takes a great deal of time and through which one reaches complete knowledge only very late" (Anna von Helmholtz, *Anna von Helmholtz. Ein Lebensbild in Briefen*, ed. Ellen von Siemens-Helmholtz [Berlin: Verlag für Kulturpolitik, 1929], vol. 1, 249).

attended in Munich), and, in general, the new mathematics from about 1830 on held "no great value for the physicist, however beautiful" they may be in themselves.[72]

After a year in Munich, Hertz was impatient to move on. The physics professor at the polytechnic, Wilhelm Beetz, told him that he could now expect to find a physics laboratory at any university he chose. He considered Leipzig and Bonn, "where Clausius is," and then decided for Berlin. There he attended Helmholtz's lectures on mathematical acoustics and Kirchhoff's on mechanics, which held nothing new for him, sometimes skipping them. Although he wrote his dissertation on a theoretical problem concerning electrical induction in spheres rotating between magnets, his main interest then was experimental physics.[73] Following graduation, he stayed on as Helmholtz's assistant for 3 years, supervising practice exercises connected with general physics and heat theory and doing experimental and theoretical research of his own on problems in electrodynamics and elasticity.[74] His independent researches at Berlin were "in part more of a theoretical, in part of an experimental nature; they did not arise in systematic pursuit of a greater goal but from the incidental stimulus" he received from his teachers and collaborators.[75] During these years, he published a dozen or so researches, an impressive achievement, which held the promise of a better job.

About to appoint a physicist as Privatdocent for theoretical physics at Kiel, the Prussian ministry asked the advice of Helmholtz, Kirchhoff, and the mathematician Karl Weierstrass. They told the ministry about Hertz, and they also told Hertz that the Kiel faculty had requested an extraordinary professorship and that there was a good chance that he might be offered it in 2 years when money for it became available. They also told Hertz about the complications of the appointment: since the Kiel faculty wanted an extraordinary professor, they might resent a Privatdocent, and the Kiel physics professor Gustav Karsten disliked the Berlin faculty and would devalue any recommendation they made. Helmholtz was lukewarm about the job, for he believed that a mathematical physicist should have means to do experimental research, and Hertz would have none at Kiel. But Helmholtz did not advise Hertz to decline or accept the job, as Hertz wished; instead he advised Hertz to visit Kiel and learn firsthand the faculty's mood and the ministry's intention.[76]

Despite his belief that Berlin was the center of physics, where he could "compare his powers" with those of other researchers, Hertz was ready to move on again. He wanted to begin teaching, and there were already too many Privatdocenten in Berlin;[77] Kiel's job looked to him like a solution. He found that the Kiel faculty

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[72] Hertz to his parents, 25 November 1877, in Hertz, *Erinnerungen*, 68–72, on 70; Max Planck, "Gedächtnissrede auf Heinrich Hertz," *Verh. phys. Ges.* 13 (1894): 9–29; repr. *Phys. Abh.*, vol. 3, 268–88, on 277.

[73] Heinrich Hertz, *Ueber die Induction in rotierenden Kugeln* (Berlin, 1880); Hertz to his parents, 4 November 1878, in Hertz, *Erinnerungen*, 114–16.

[74] Hertz, "Vita," 14 March 1883, LA Schleswig-Holstein, Abt. 47.7 Nr. 8.

[75] Ibid.

[76] Hertz to his father, 1 March 1883, in Hertz, *Erinnerungen*, 176–78.

[77] Hertz to his parents, 8 March 1881 and 17 February 1883, in Hertz, *Erinnerungen*, 144, 172–74.

was pleased that the gap in their curriculum was to be filled and that they were satisfied with a Privatdocent for the purpose. The ministry, for its part, was satisfied that Hertz had, in addition to a thorough knowledge of physics, a sufficient mathematical education to meet the requirements for what its university official Friedrich Althoff called a "so-called theoretical physicist."[78]

Once installed at Kiel, Hertz lectured on theoretical physics while he did theoretical research on a variety of topics. His diary entries soon after his arrival reveal that he read Maxwell and worked steadily on electrodynamic questions. The outcome was a critical study in 1884 of the foundations of electrodynamics, in which Hertz concluded that Maxwell's version does not contain within itself the proof of its incompleteness and the opposing electrodynamics does.[79] We will look at this work, which foreshadows his later research on electric waves.

Like Helmholtz, Clausius, and other physicists working on electrodynamics, Hertz retained the customary mechanical principles where possible. To analyze the equations for closed currents, he invoked the principles of the conservation of energy, the equality of action and reaction, and the superposition of actions. To these he added two principles, the "unity of electric force" and the "unity of magnetic force," which he believed were implicit both in the theory of Faraday and Maxwell's and in the opposing theories of electrodynamics. He illustrated what he meant by the "unity of electric force" by an example: the force with which a friction rod attracts a charged piece of wood is the same force as that with which a changing magnet induces a current in a conductor. In both cases, the force is the electric force, from which it follows that a changing magnet attracts the charged piece of wood and that, by the principle of action and reaction, the charged wood attracts the changing magnet. It follows, too, that a changing magnet attracts another changing magnet with an electric force in addition to their mutual magnetic force; by the older electrodynamics, only the magnetic force is recognized.[80] Carrying out this analysis mathematically for ring magnets and closed currents of varying intensity, Hertz arrived by an iterative method at "Maxwell's equations."[81]

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[78] Hertz to Kiel U. Philosophical Faculty, 14 March 1883, LA Schleswig-Holstein, Abt. 47.7 Nr. 8; Hertz to Althoff, 15 March 1883, and marginal note by Althoff, DZA, Merseburg.

[79] Hertz, diary entries for January–July 1884, in Hertz, *Erinnerungen*, 188–94; Heinrich Hertz, "Über die Beziehungen zwischen den Maxwell'schen elektrodynamischen Grundgleichungen und den Grundgleichungen der gegnerischen Elektrodynamik," *Ann.* 23 (1884): 84–103; repr. in *Ges. Werke*, vol. 1, 295–314, on 313–14.

[80] Max Planck, "Gedächtnissrede auf Heinrich Hertz," *Verh. phys. Ges.* 13 (1894): 9–29; repr. in *Physikalische Abhandlungen und Vorträge*, 3 vols. (Braunschweig: F. Vieweg, 1958), vol. 3, 278–79.

[81] The starting point of Hertz's mathematical analysis of electrodynamics, like Helmholtz's, was Neumann's "vector potential," which Hertz applied to both the electric current and the magnetic current to obtain the electric and magnetic potentials. In tracing the implications for these potentials of varying electric and magnetic current densities, Hertz departed from the older electrodynamics. He showed that a small correction term is needed in the expression for the ponderomotive action of the order of A^2 where $1/A$ is equal to $c/\sqrt{2}$, which is the same as the velocity of light, c being Weber's constant. He then showed that by the principle of the

"If the choice rests only between the usual system of electromagnetics and Maxwell's," he reasoned, "the latter is certainly to be preferred."[82] Like Helmholtz, Hertz tried to clear up the confusion in electrodynamics by a theoretical analysis, here by an appeal to principles standing above the conflicting theories.

It has been said of Hertz's 1884 paper that "there may be no more dazzling combination of thought experiment and general principles in the whole of the nineteenth century physics."[83] Planck thought that it had received too little attention, especially since it was a first-rate piece of theoretical work, as impressive in its way as Hertz's later experimental work, which eclipsed it.[84] It did draw some interest, for example, from Boltzmann and Lorberg, but it had little impact on work in electrodynamics other than, it would seem, Hertz's own. Although Hertz began his later experimental work with Helmholtz's formulation with its denial of the unity of the dynamic and static electric forces, midway through it he adopted the approach of the unity of fields, which recalled his principle of the unity of forces in 1884.[85]

Hertz was restless at Kiel. At his own expense he set up a laboratory in his house, but it was not the same as an institute; his means for research were meager, and it took an eternity, it seemed, to get a single length of platinum wire or a glass tube.[86] He had always done experimental as well as theoretical work, a pattern he wished to continue. He did not see himself as a theoretical specialist, which if he were to stay in Kiel he seemed likely to become, in teaching certainly and, by circumstance, perhaps in research as well.

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conservation of energy, this term entails another correction in the inductive action, which in turn entails another correction in the ponderomotive action. Ultimately, in this way, Hertz arrived at a convergent infinite series of correction terms in increasing powers of A; the series yields electric and magnetic potentials that satisfy the standard equation for waves propagated in free space with velocity $1/A$. The electric force $\mathbf{E}$ and the magnetic force $\mathbf{H}$ satisfy the same wave equation as do the potentials. Hertz arrived at a system of equations that he identified as Maxwell's:

$$\nabla^2\mathbf{H} - A^2\frac{d^2}{dt^2}\mathbf{H} = 0, \mathrm{div}\,\mathbf{H} = 0,$$

$$\nabla^2\mathbf{E} - A^2\frac{d^2}{dt^2}\mathbf{E} = 0, \mathrm{div}\,\mathbf{E} = 0.$$

(The vector notation is modern, not Hertz's.) Hertz also wrote the first-order differential equations mixing the electric and magnetic forces. In this paper, the equations for the electric and magnetic forces were expressed in "symmetric form" for the first time (Salvo D'Agostino, "Hertz's Researches on Electromagnetic Waves," *HSPS* 6 [1975]: 261–323, on 291).

[82] Hertz, "Über die Beziehungen," 313.

[83] Darrigol, *Electrodynamics*, 237.

[84] Planck, "Hertz," 278; Max Planck, "James Clerk Maxwell in seiner Bedeutung für die theoretische Physik in Deutschland," *Naturwiss*. 19 (1931): 889–94; repr. in *Phys. Abh.*, vol.3, 352–57, on 356.

[85] D'Agostino, "Hertz's Researches," 295, 322.

[86] Hertz to his parents, 27 October 1883, in Hertz, *Erinnerungen*, 186.

In November 1884 the ministry proposed to the Kiel faculty that Hertz now be given the planned extraordinary professorship for theoretical physics, and in December the faculty sent a report agreeing with the ministry. Hertz speculated that the faculty voted this way so that a compensating offer could be made to him if Karlsruhe Polytechnic offered him a job, then known to be a likelihood. Hertz wanted to improve his conditions of work, and so when Karlsruhe offered him a position as ordinary professor of physics and director of a decent physics institute, he accepted without hesitation. He was "convinced," he told the dean of the Kiel philosophical faculty, "that there was hardly any other choice."[87]

To fill the new extraordinary professorship for theoretical physics, the Kiel faculty recommended Planck. Of all the younger teachers of theoretical physics, Planck had the "longest and most successful activity" to his credit, and the faculty was as impressed by his publications as by his teaching.[88] In May 1885 the government approved their choice. Planck agreed to teach all of mathematical physics and, if necessary, to help out in experimental physics.[89]

For Planck, the move to Kiel was more in keeping with the direction of his work than it had been for Hertz. It was also a move to a place he knew well, having spent his boyhood in Kiel. From there, his family had moved to Munich, where his father taught law at the university and he attended the gymnasium. He considered making a career in music, but he decided that he lacked talent for composition; he also considered the humanities, and he later supposed that he might have made a passable philologist or historian.[90] As a student at Munich University, he attended mathematical lectures, which inclined him towards the exact natural sciences. Near the end of his life, he wrote that his "original decision" to devote himself to physics stemmed from his recognition that the laws of thought conform to the impressions

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[87] Hertz to his mother, 6 and 12 December 1884, and diary entries for December 1884, in Hertz, *Erinnerungen*, 198–200; Hertz to Dean of the Kiel U. Philosophical Faculty, 31 December 1884, LA Schleswig-Holstein, Abt. 47.7 Nr. 8.

[88] The ministry requested another suggestion from the faculty via the curator on 2 January 1885; the curator agreed with the faculty's recommendation of Planck in February. Kiel U. Curator Mommsen to Prussian Minister of Culture Gossler, 12 February 1885, and Dean of Kiel U. Philosophical Faculty to Gossler, 13 February 1885, DZA, Merseburg.

[89] In April 1885 Kiel received an additional 3060 marks in its budget for an "extraordinary professor for theoretical physics"; it was for a salary of 2400 marks and 660 marks for rent. Planck saw the Prussian Ministry of Culture official Friedrich Althoff in Munich the same day and signed an agreement according to which he got 2000 marks in salary plus the 660 marks. That is, Planck received 400 marks less than the university was receiving as salary for him (Ministry document dated 10 April 1885, and Prussian Ministry of Culture to Planck, 2 May 1885, DZA, Merseburg; Kiel U. Curator Mommsen to Kiel U. Philosophical Faculty, 6 May 1885, LA Schleswig-Holstein, Abt. 47.7 Nr. 8).

[90] Armin Hermann, *Max Planck in Selbstzeugnissen und Bilddokumenten* (Reinbeck b. Hamburg: Rowohlt, 1973), 11; Max Born, "Max Karl Ernst Ludwig Planck 1858–1947," *Obituary Notices of Fellows of the Royal Society* 6 (1948): 161–88; repr. in Max Born, *Ausgewählte Abhandlungen*, ed. Akademie der Wissenschaften in Göttingen (Göttingen: Vandenhoeck und Ruprecht, 1963), vol. 2, 626–46, on 627.

we receive from the outer world and that by pure thought we can discover those laws.[91]

At Munich, Planck studied physics, and there under Jolly he performed his only experiments before transferring to theoretical physics. In time, he no doubt received the same advice as Hertz, for he too left Munich for Berlin to continue his studies. There he met Helmholtz and Kirchhoff, who impressed him for their world reputation, showing that physics in Munich had only "local significance." Although he netted "no perceptible gain" from their lectures,[92] he acquired a sound knowledge of physics from their writings and from writings by other recent masters. He chose a topic suggested by Clausius for his dissertation, which he submitted in Munich in 1879. He stayed on there teaching theoretical physics as a Privatdocent, waiting impatiently for a call. When he was offered a job teaching physics at a forestry academy, he went to Berlin to discuss it with Helmholtz. Encouraged by Helmholtz's favorable expectations for eventual jobs in theoretical physics,[93] Planck elected to remain in Munich for the time being. Theoretical physics "had not as yet come to be recognized as a special discipline," he recalled in an autobiographical sketch. In light of that, Kiel's invitation to him to teach theoretical physics as Hertz's replacement "came as a message of deliverance." Being pessimistic about the chances for a timely recognition of accomplishment in physics, he did not attribute the offer so much to his scientific work as to the circumstance that his father was a close friend of the physics professor at Kiel, Gustav Karsten. For Planck as for Hertz, Kiel was a stepping stone to a better job; after Kiel, their careers diverged. Hertz moved on to a job in experimental physics, which corresponded to his first interest, whereas Planck's teaching assignments at Kiel corresponded to his research, and he moved on to another job in theoretical physics.

Kiel was a small university remote from the centers of German physics, and like similar Prussian universities it suffered material neglect by a ministry whose main attention was devoted to Berlin. With Karsten as its director, the Kiel physics institute was almost devoid of significance for experimental research for a half-century, but in the establishment of theoretical physics in German universities, Kiel played a practical part: its position for theoretical physics furthered the early careers of Hertz and Planck, two major contributors to theoretical physics in Germany.

When Kirchhoff died in 1887, Helmholtz was concerned to replace him with a physicist having comparable qualifications, one who was primarily interested in mathematical physics but who was also knowledgeable in experimental physics. Berlin tried, in vain, to attract Boltzmann and also Hertz, who since moving to Karlsruhe from Kiel had gained renown for his experimental researches on electric waves. Berlin settled for Planck, who stood directly behind Hertz on their list of candidates. Planck, who had not yet done research equal to Boltzmann's or Hertz's, was appointed extraordinary professor, which meant that for the time being the

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[91] Planck, *Wissen. Selbstbiog.*, 7.

[92] The text was Kirchhoff's own. Planck's account, *Wissen. Selbstbiog.*, 8–9.

[93] Hermann, *Planck*, 18.

teaching of physics at Berlin reverted to the usual arrangement: beside the ordinary professor who taught experimental physics, the second physicist taught theory as an extraordinary professor.

Despite his rank, Planck had a more important position at Berlin than was usual for the second physicist. In 1889, Helmholtz proposed an institute for theoretical physics, and Planck became its first director. The institute was extremely modest, practically precluding any experimental work. It was established with a budget of 570 marks a year, which went mostly to the institute's library, and there was an assistant, who was used for reading and correcting the students' written exercises. Few physicists would have been satisfied with the new institute, but it was sufficient for Planck, who was content to teach and do research in theoretical physics, as he had done at Munich and Kiel before coming to Berlin. Apart from the research expected of him, his principal duty was to give a cycle of lectures on theoretical physics.[94]

Planck was a specialist of a kind that Helmholtz found acceptable, or at least adjusted to. Although Planck did not do experiments himself, he followed closely the work of experimentalists, reproducing their tables of measurements in his papers and suggesting ways for them to develop their observations. Upon arriving in Berlin as a determined "theorist," Planck felt that the assistants in the main physics institute kept him at arm's length at first, but he was welcomed by the most important physicist there, the director of the institute, Kundt, who had just replaced Helmholtz. Kundt wrote to a colleague at the time that "in Planck, I believe we have made an excellent acquisition; in every respect, he appears to be a splendid man."[95]

11.3 Institutional Reinforcement Through Technical Physics

Several leading researchers in theoretical physics lectured on their subject at polytechnics or, as they came to be renamed in the late nineteenth century, technical institutes (Technische Hochschulen). Hertz, as we have seen, both studied and taught at such institutes, and it was at one of them, Karlsruhe, where he carried out the experiments on electric waves that altered the direction of theoretical physics in Germany. These institutes have a place in our study if not as large a one as the universities.

In the late nineteenth century, Germany had nine technical institutes, located at Aachen, Berlin, Braunschweig, Darmstadt, Dresden, Hannover, Karlsruhe, Munich, and Stuttgart, and in the early years of the twentieth century two more were added, at Breslau and Danzig. At these institutes, as at the universities, the size of the physics staff and the range of physics courses varied considerably from place to

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[94] Planck, "Das Institut für theoretische Physik."

[95] Planck, *Wissen. Selbstbiog.*, 16; Kundt to Graetz, 26 May 1889, Ms. Coll., DM, 1933, 9/18.

place. For example, at the technical institute at Berlin at the turn of the twentieth century, physics including mathematical physics and mechanics was taught by two professors, four Docenten, five Privatdocenten, and three assistants. At the technical institute at Munich, physics was also well represented. By contrast, the technical institute at Breslau relied entirely on the local university for its physics faculty; Braunschweig had only one professor of physics along with one assistant; and Aachen had only one professor and one Docent.[96]

The main responsibility of physicists at technical institutes was to teach basic physics within what was often called the "general department," a complement of the professional engineering schools, as we saw at Zurich Polytechnic.[97] Much like the philosophical faculties of universities, the general departments of the technical institutes, led by those of Saxony and the southern states, developed teaching goals of their own beyond providing engineers with a general education. In the 1860s, for example, the technical institute at Dresden established within its general department a professional school for training teachers of mathematics, physics, and other natural sciences. Darmstadt, Karlsruhe, Munich, and Stuttgart also offered partial or complete training for teachers. Later the Prussian technical institutes followed their lead, as Prussia's new testing ordinance of 1898 allowed teaching candidates in

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[96] By around 1880, the German polytechnics had achieved an official position as "higher schools." The corresponding change of name from "Polytechnic" to "Technische Hochschule" occurred at different times at different schools. The full equality of these schools with universities together with the right to confer the engineering doctorate came about in the 1890s (Karl-Heinz Manegold, *Universität, Technische Hochschule und Industrie*, vol. 16, Schriften zur Wirtschafts- und Sozialgeschichte, ed. W. Fischer [Berlin: Duncker und Humblot, 1970], 72–74). Numbers of physics teachers at German technical institutes are given in *Das Unterrichtswesen im Deutschen Reich*, ed. Wilhelm Lexis, vol. 4, pt. 1, *Die technischen Hochschulen im Deutschen Reich* (Berlin: A. Asher, 1904), 217, 296; also in Paul Forman, John L. Heilbron, and Spencer Weart, "Physics *circa* 1900. Personnel, Funding, and Productivity of the Academic Establishments," *HSPS* 5 (1975): 1–185, on 10. The technical institutes used the same principal titles for teachers as the universities: ordinary and extraordinary professor and Privatdocent. They also used "Docent," which stood for a teacher who was salaried in contrast with a Privatdocent who usually was not. Otto Lehmann, for example, moved to Aachen in 1883 as Docent for physics and assistant to Adolph Wüllner, the physics professor. Lehmann's classes were small: he had two students in his lectures on the mechanical theory of heat, four in his lectures on experimental physics, three in his laboratory, and none in his class on applied physics. The space for research in the laboratory was so small that he could not find room for the little apparatus he had brought with him from Mühlhausen, where he had been teaching in the secondary school. It took him ten times longer to do research at Aachen than at Mühlhausen but he felt compensated by the better collection of apparatus at Aachen and the increased time he had for research there (Lehmann to Warburg, 6 November 1883, STPK; K. L. Weiner, "Otto Lehmann, 1855–1922," in *Geschichte der Mikroskopie*, vol. 3, ed. H. Freund and A. Berg [Frankfurt a.M.: Umschau, 1966], 261–71, on 262).

[97] Of the subjects represented by the general department, physics was regarded as the "most important natural-scientific field of instruction," since it related to all other subjects taught in the technical institutes (Robert Fricke, "Die allgemeinen Abteilungen," in *Das Unterrichtswesen im Deutschen Reich*, ed. W. Lexis, vol. 4, pt. 1, 49–62, on 54). To the physicists Max Wien and Jonathan Zenneck at Danzig, it was "one of the most beautiful tasks of the technical institutes to introduce the exact methods of physics into the treatment of technical problems" (Wien and Zenneck to the Danzig Technical Institute Senate, 15 February 1906, Ms. Coll., DM).

mathematics and the physical sciences to spend half of their time at technical institutes, and Aachen soon provided teaching for this purpose.[98]

The technical institutes did not have the right to grant doctorates in physics, assuring that they would have less importance for physics than the universities. Even so, they provided an opportunity for a good many physicists to study, teach, and practice physics. Like the universities, the technical institutes came to recognize the need for a "second" physicist; by 1891, for example, Aachen, Dresden, and Karlsruhe had teachers for theoretical physics, all extraordinary professors, and in time the other technical institutes also acquired their theoretical physics lecturers.[99] Eventually, as at the universities, the extraordinary professorships for theoretical physics were replaced by ordinary ones.

The increasing opportunities for physicists to work at technical institutes corresponded to the growth in number, size, and stature of these institutions. Their growth, in turn, reflected the advanced technological needs of an industrializing nation. With the rise of big industries, the conquest of space and time, and the whole movement toward the "technical mastery of lifeless nature," physics and chemistry were seen as driving forces of the modern age.[100] Physicists accepted and often encouraged this association. At the fiftieth anniversary of the Berlin Physical Society in 1896, its president Emil Warburg contrasted the quiet, unnoticed condition of physics at the founding of the Society with its present world renown, a difference he attributed not to physical discoveries but to the impact of physics on economic life, especially through electrotechnology.[101] Wilhelm Wien, speaking on science and German universities in 1914, observed that physics together with chemistry had "created the firm foundations on which the pillars of our industry stand, which support a great part of our economy."[102] The understanding of the age as one of applied science was given historical expression by the Deutsches Museum in Munich, built in 1908 from public and private sources to symbolize the "mutual influence" of technical and scientific work in the past and present.[103]

The idea of applied physics was not new; it was a category in the *Fortschritte* from its beginning in the 1840s. It was understood that to each branch of basic

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[98] Fricke, "Die allgemeinen Abteilungen," 58–61; Paul Stäckel, "Angewandte Mathematik und Physik an den deutschen Universitäten," *Jahresber. d. Deutsch. Math-Vereinigung* 13 (1904): 313–41, on 323–24, 335. At Dresden, for example, where the general department had thirty-seven students in 1903–4, the teaching candidates would take the same mathematics and mechanics courses as the engineers in their first four semesters, after which they would take more advanced courses in the mathematical-physical fields.

[99] Lehmann's 1891 survey of budgets for German physics institutes, Bad. GLA, 235/4168.

[100] For example, by Oscar Hertwig, "Die Entwicklung der Biologie im 19. Jahrhundert," *Verh. Ges. deutsch. Naturf. u. Ärzte* 72, pt. 1 (1900): 41–58, on 41–42.

[101] Emil Warburg's address in *Verh. phys. Ges.* 15 (1896): 30–31, on 30.

[102] Wilhelm Wien, *Die neuere Entwicklung unserer Universitäten und ihre Stellung im deutschen Geistesleben* (Würzburg: Stürtz, 1915), 17.

[103] Note on the new building for the Deutsches Museum in *Internationale Wochenschrift* 2 (1908): 608.

physics, there was a corresponding branch of technical physics,[104] and from the 1860s and 1870s, technical optics, technical electricity, and other technical physical fields began their strong development alongside and within technical institutes.[105] Physics was also applied in medicine.

In the introduction to his published lectures on technical mechanics, the Munich technical physicist August Főppl explained why such courses were needed and how they differed from courses for physicists and mathematicians. The rigorous demands that the physicist and the mathematician placed on mechanics could not be met in practice; the technologist who was under pressure to come up with useful results had no choice but to resort to provisional, approximate theories. In their attempt to encompass all physical phenomena within a single formula, such as Hamilton's principle of least action, the physicist and the mathematician were of no help to the technologist, who had reason to be "rather mistrustful" of it.[106] The Gőttingen technical physicist Hans Lorenz said that students by and large still learned physics in the old-fashioned experimental lectures, in associated laboratory courses, and in theoretical lectures that were strongly mathematical and not at all technical. In the latter, he said, students learned a "small number of the most general, sharply formulated laws," which they were unable to apply in practice.[107] Physicists who taught technical physics were expected to recognize the practical needs of their audience and to accommodate them.

Courses in "applied" or "technical" physics fell to the teachers of theoretical physics at several universities and technical institutes. This might seem an unlikely pairing, but at the time there was no regular preparation for a teacher of applied physics, and there were practical advantages in having theoretical and technical physics taught by the same man, usually the second physicist. The doubling up offered a route to the creation of two separate positions and perhaps of a new institute, a credit to the university or technical institute.[108] For the theoretical

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[104] Georg Gehlhoff, Hans Rukop, and Wilhelm Hort, "Zur Einführung," *Zs. f. techn. Physik* 1 (1920): 1–4.

[105] Wilhelm Hort, "Die technische Physik als Grundlage für Studium und Wissenschaft der Ingenieure," *Zs. f. techn. Physik* 2 (1921): 132–40; Friedrich Klemm, "Die Rolle der Mathematik in der Technik des 19. Jahrhunderts," *Technikgeschichte* 33 (1966): 72–91.

[106] August Föppl, *Vorlesungen über technische Mechanik*, vol. 1, *Einführung in die Mechanik*, 5th ed. (Leipzig: B. G. Teubner, 1917), vi, 1, 9–10. The first edition (1898) was based on lectures that Föppl gave to second semester students in technical mechanics. Föppl to Arnold Sommerfeld, 29 March 1902, Sommerfeld Correspondence, Ms. Coll., DM.

[107] Hans Lorenz, *Technische Mechanik starrer Systeme* (Munich: Oldenbourg, 1902), v-xi. This text is based on lectures Lorenz gave at Halle and Göttingen in 1899–1902; it is the "foundation" and the first of several volumes covering the areas of "technical physics." Hort, "Die technische Physik," 134.

[108] The Karlsruhe physics professor Lehmann, for example, in 1891 anticipated an electrotechnical institute with an independent director, but he thought that such an institute would not be granted for several years. He therefore proposed as a "transition phase" the creation of a separate chair for theoretical physics, "pure as well as applied." The chairholder, he explained, "could then, as soon as the opportunity arose, be appointed director of the new electrotechnical institute and would have the opportunity of preparing its arrangements without rush, completely according to his wishes" (Lehmann to the Director of Karlsruhe Technical Institute, 3 June 1891, Bad. GLA, 235/4168).

physics lecturer, the doubling up had the benefit of increasing his income, since his lectures on technical physics were often well attended and brought him more fees than his theoretical lectures. He might even take the initiative of adding technical physics to the curriculum. Walter Kőnig, when asked about accepting the extraordinary professorship for theoretical physics at Heidelberg University in 1899, urged the Baden ministry of education to introduce applied physics, especially electrotechnology. He explained that in recent years many universities had offered introductory lectures on these subjects, which were taught either by a specialist or by the second physicist, who lectured on theoretical physics; because of the great importance of applied physics for practical life, there was a definite need, particularly in the training of teachers, to give students some knowledge of the subject.[109] When discussing the extraordinary professorship for theoretical physics at Würzburg in 1903, Friedrich Pockels said that he might like to give lecture courses meant for a larger audience, one of which could be on the foundations of electrotechnology, a subject Theodor Des Coudres had previously taught in the same position.[110] At Halle, from 1895 on the extraordinary professor for theoretical physics Karl Schmidt taught applied physics, which as a personal ordinary professor he continued to do after 1915 with the aid of a "provisional laboratory for pure and applied physics."[111] At Erlangen, the extraordinary professor from 1904 to 1906 Arthur Wehnelt, who had studied engineering before physics, already had some teaching experience in electrotechnology.[112] His successor, Rudolph Reiger, lacked such experience, but as an assistant in the Erlangen physics institute and as an acquaintance of Wehnelt's, he was thought to have adequate qualifications for teaching the subject.[113] As late as 1931, the Hannover theoretical physicist Erwin Fues could speak of the "separation of this [theoretical physics] discipline which until recently had been united with the applied physics" in many universities and technical institutes.[114]

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[109] Walter König to "Geheimrat," 12 February 1899, Bad. GLA, 235/3135.

[110] Pockels to Wilhelm Wien, 14 March 1903, Wien Papers, Ms. Coll., DM.

[111] In 1921 Schmidt's laboratory was named "laboratory for applied physics," which agreed with the direction of his work. Upon Schmidt's departure, the Halle faculty in 1927 called for an ordinary professor of technical physics (Communication from D. H. Schwabe, Director of the Halle University Archive).

[112] Wehnelt's vita submitted to Erlangen U. in 1904; recommendation by the Erlangen U. Philosophical Faculty and the Prorector that Wehnelt be appointed extraordinary professor, at a salary of 3180 marks, with the teaching assignment for "theoretical and applied physics," 3 October 1904; and approval of the request by the Bavarian government, 18 November 1904; Wehnelt Personalakte, Erlangen UA.

[113] Reiger's vita; Erlangen U. Philosophical Faculty to Academic Senate, recommending Reiger's appointment as extraordinary professor with the same salary and teaching assignment as Wehnelt's, 7 March 1907; and approval by the Bavarian government, 4 May 1907; Reiger Personalakte, Erlangen UA.

[114] Quoted in Forman, Heilbron, and Weart, "Physics *circa* 1900," 32.

For physicists who taught or did research in theoretical physics, technical physics did not have to mean only a source of extra income. They frequently took an interest in the scientific problems suggested by technical subjects. Clausius, who combined theoretical and technical physics in his teaching, published on the theory of the steam engine during his researches on heat theory in the 1850s, and he published on the theory of the dynamo-electric machine during his researches on electrodynamic theory in the 1880s. He valued similar work by other physicists; for example, he recommended Auerbach for work that belonged to the earliest measuring researches on those "interesting machines," the dynamos.[115] Most German university physicists preferred to work on problems of basic physics, but they did not disparage the work of technical physics for that reason.

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[115] Clausius to Director Greiff, 15 August 1887, STPK, Darmst. Coll. 1913.51.

Chapter 12
Methods of Theoretical Physics

12.1 Molecular Mechanics

We discussed methods of research in theoretical physics in a general way in the first chapter. In this chapter we look at specific examples of methods as they entered research and teaching: Boltzmann's use of the molecular method in the mechanical theory of heat; Hertz's use of the method of mathematical phenomenology in electrodynamics; Planck's use of principles as a method in physical chemistry; Helmholtz's and Boltzmann's use of the method of analogy in developing heat and electromagnetic theory; and Neumann's, Kirchhoff's, and Helmholtz's methods of presenting theoretical physics in lectures.

In the physics laboratory at Berlin, Boltzmann tested Maxwell's electromagnetic theory by measuring the refractive index, obtaining results he regarded as confirmative. But by far his most important work was theoretical and focused; in a series of publications in 1866–1877, he advanced the kinetic theory of gases and helped lay the foundations of statistical mechanics. In going beyond the work on the subject by Krőnig, Clausius, and Maxwell, he confronted increasingly difficult mathematical problems, which called into play his considerable mathematical skills.

During these years, Boltzmann followed the usual career path of a physicist, academic teaching. After his graduation at Vienna in 1866, he worked for a time as his supervisor Stefan's assistant, and he lectured as a Privatdocent. His opportunity came after the Austrian university at Graz acquired a second physics position for an extraordinary professor for mathematical physics. In 1869 Boltzmann was appointed to this position, which was then elevated to an ordinary professorship for mathematical physics. He was only twenty-five at the time. In 1873 he returned

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_12

to Vienna as ordinary professor for mathematics, and in 1876 he returned to Graz as ordinary professor of physics and director of the physics institute.[1]

Boltzmann's dissertation was on the kinetic theory of gases. In 1866, in his first paper on the subject, he derived the second fundamental law of the mechanical theory of heat from molecular mechanics, but to do so he had to make unrealistic assumptions about the motions of the molecules of a gas. Soon afterwards he read Maxwell's studies on the kinetic theory of gases, which included his law of the distribution of velocities among the molecules of a gas in thermal equilibrium. He agreed with Maxwell that since gas molecules pass through all possible states of motion, it was of the "highest importance" to know the probability of the various states, and in 1868 he generalized the distribution law that Maxwell had derived for molecules considered as single material points to the more realistic case of a gas composed of polyatomic molecules, capable of internal motions.[2] The law takes the following form. The probability of the state of a molecule at a given moment is determined by a distribution function that depends on the positions and velocities, $\xi_1, \eta_1, \ldots, \omega_r$, of all the constituent atoms: $f = Ae^{-h\phi}$, where A and h are constants. In Maxwell's version, ϕ is the kinetic energy. Boltzmann's change was to make ϕ the sum of the kinetic energy and the potential energy, or total energy, of a molecule. From this, the "Maxwell-Boltzmann" distribution function, the average value of any property of the gas, as represented by a function X of the positions and velocities of the atoms, can be calculated: $1/N \cdot \int X dN$, where N is the number of molecules per unit volume of the gas, and $dN = f(\xi_1, \eta_1, \ldots, \omega_r) d\xi_1 d\eta_1 \ldots d\omega_r$ is the fraction of molecules whose atoms have positions and velocities falling within the intervals ξ_1 to $\xi_1 + d\xi_1$, η_1 to $\eta_1 + d\eta_1, \ldots, \omega_r$ to $\omega_r + d\omega_r$. Boltzmann explained the need for average values, still a relatively new understanding in physics. Physicists did not observe molecules performing the lively motion that the mechanical theory of heat attributes to them; instead they observed the lawful behavior of heated bodies, a consequence of the "most random events" among molecules, which under the same conditions always lead to the same average values. For calculating average values, physicists had recourse to the "probability calculus," but this did not mean that the mechanical theory of heat is any less secure.

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[1] The extraordinary professorship for mathematical physics was created in 1863. Austrian Ministry of State, Section for Culture and Education, and Ministry of Finance reports, 10 July 1863, Öster. STA, 5 Phil, Physik. Hans Schobesberger, "Die Geschichte des Physikalischen Institutes der Universität Graz in den Jahren von 1850–1890" (manuscript), 16, Graz UA; Ministry of Culture and Education to Dean of the Philosophical Faculty, 15 April 1869; report by the Ministry to the Emperor, 28 June 1869; all in Öster. STA, 5 Phil. Physik; Theodor Des Coudres, "Ludwig Boltzmann," *Verh. Sächs. Ges. Wiss.* 85 (1906): 615–27, on 623; Woldemar Voigt, "Ludwig Boltzmann," *Gött. Nachr.*,1907, 69–82, on 72.

[2] Ludwig Boltzmann, "Studien über das Gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten," *Sitzungsber. Wiener Akad.* 58 (1868): 517–60, reprinted in *Wiss. Abh.*, vol. 1, 49–96.

Maxwell believed that the second law of thermodynamics has only strong probability, not absolute certainty, but he did not develop a statistical theory to show how a system of molecules behaves in conformity with the second law. Boltzmann made progress in that direction in 1871 with a new proof of the second law, making use of the generalized distribution function to determine average values of atomic motions. Representing the absolute temperature T of a body by the average kinetic energy of an atom, he proved that a small quantity of heat added to the body δQ divided by T is a complete differential, the infinitesimal change in entropy, dS. He showed that the resulting expression is useful by calculating the entropy of an ideal gas, and he applied his analysis to the specific heats of solids, deriving further conclusions about the molecular world.[3]

With his latest proof of the existence of an entropy function, Boltzmann had not completely clarified the molecular basis of the second fundamental law, since he had considered it only in relation to equilibrium. He now addressed the more difficult problem of non-equilibrium.[4] In 1871 he had proved that *if* the distribution law has Maxwell's form, it cannot be changed by the collision of molecules or by the motion of their constituent atoms. He regarded this as the first "rigorous" proof that the distribution law meets all of the conditions that the real distribution of states of gas molecules has to satisfy. In 1872 he set out to prove that the distribution law *must* have this form; that is, it not only gives results that agree with experience, but the equilibrium distribution of velocities it describes is the only one that does not change over time owing to collisions between molecules. In the course of proving that the distribution law is necessary, he advanced a theorem that provided the first insight into the nature of irreversible processes.[5] (If a physical system undergoes an "irreversible process" in changing its state, it cannot be returned to its original state without an expenditure of energy. The reason is that at the molecular level, a certain amount of thermal energy is dissipated through molecular interactions. Friction is an example of an irreversible process. In truth, all natural processes are irreversible; reversible processes are idealizations.)

To establish the uniqueness of the distribution law, Boltzmann analyzed the effect of collisions between gas molecules two at a time on the distribution function f. (He studied the partial derivative of f with respect to t, a special case of the "Boltzmann equation" for transport phenomena). To prove that *any* distribution f always tends in time toward Maxwell's distribution, Boltzmann introduced an auxiliary function, $E = \int\int \ldots f \log f dx_1 dy_1 \ldots dw_r$, and showed that $-E$ behaves

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[3] Ludwig Boltzmann, "Analytischer Beweis des zweiten Hauptsatzes der mechanischen Wärmetheorie aus den Sätzen über das Gleichgewicht der lebendigen Kraft," *Sitzungsber. Wiener Akad.* 63 (1871): 712–32; repr. in *Wiss. Abh.*, vol. 1, 288–308, on 308.

[4] Boltzmann, "Weitere Studien"; Martin J. Klein, *Paul Ehrenfest, The Making of a Theoretical Physicist*. (Amsterdam and London: North-Holland, 1970), vol. 1, 100.

[5] Boltzmann, "Über das Wärmegleichgewicht," 254–55; Martin J. Klein, "Maxwell, His Demon, and the Second Law of Thermodynamics," *American Scientist* 58 (1970): 84–97, on 92.

similarly to Clausius's entropy function. E can never increase over time; it is proportional to the negative of the entropy, which can never decrease.[6] (Later Boltzmann would write H for E, and the associated theorem would be known as the "H-theorem.")[7] To be precise, Boltzmann proved that if the initial distribution of velocities among the molecules of the gas is not in equilibrium, that is, if it is not described by Maxwell's law, the function E will decrease over time until temperature equilibrium is established, at which point E will have reached its constant minimum value, and the molecular velocities will be distributed according to Maxwell's law. With this result, Boltzmann provided an "analytic proof of the second fundamental law in an entirely different way," which embraces the irreversible processes we observe in nature and not just the "ideal" reversible processes.[8] Boltzmann had completed, or so it seemed, his fundamental study, having provided a molecular interpretation of the second law, at least in its application to thin monatomic and polyatomic gases.[9]

In his work on the molecular foundations of the mechanical theory of heat, Boltzmann started from theoretical studies, but he also followed closely the experimental work bearing on the subject, especially the confirmation by his Vienna colleagues Josef Stefan and Josef Loschmidt of Maxwell's temperature-dependent constants for diffusion and conduction in gases.[10] Because the "intramolecular motion" was still largely unknown, the theory behind the constants had a shaky foundation, and Boltzmann regarded Stefan's experimental determination of the heat conduction constant as far superior to the theoretical one in exactness.[11] More molecular work remained to be done.

In 1876 Loschmidt presented a paper to the Vienna Academy of Sciences expressing doubts about the "possibility of a purely mechanical proof of the second fundamental law." The proof referred to any attempt to derive irreversible behavior from the reversible laws of motion of mechanics. The problem was that for every process leading to an increase in entropy, the laws of mechanics allow a process with reversed molecular velocities, resulting in a decrease in entropy, contradicting

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[6] Boltzmann, "Weitere Studien," 369–402, especially 393. The reasons for Boltzmann's choice of the letter E, in this form of writing the second law, and of the sign reversal (the entropy increases while E decreases in the passage to equilibrium) together with Boltzmann's 1872 paper in general are discussed in Thomas S. Kuhn, *Black-Body Theory* and *the Quantum Discontinuity 1894-1912* (New York: Oxford University Press, 1978), 42–46, 269n13.

[7] For a time this statement was known as "Boltzmann's minimum theory," then as "Boltzmann's H-theorem" (Stephen G. Brush, *The Kind of Motion We Call Heat: A History of the Kinetic Theory of Gases in the 19th Century*, vol. 1, *Physics and the Atomists* [Amsterdam and New York: North-Holland, 1976], 238; see 235–38 for Brush's discussion of Boltzmann's 1872 paper).

[8] Boltzmann, "Weitere Studien," 345.

[9] Klein, *Ehrenfest*, 102.

[10] Ludwig Boltzmann, "Über das Wirkungsgesetz der Molekularkräfte," *Sitzungsber. Wiener Akad.* 66 (1872): 213–219; repr. *Wiss. Abh.*, vol. 1, 309–15.

[11] Boltzmann, "Weitere Studien," 368.

experience. Boltzmann took Loschmidt's objection seriously, and in showing that it does not invalidate the molecular-mechanical interpretation of the second law, he deepened his understanding of it. One had to recognize, Boltzmann explained, that the irreversibility of processes governed by the second law arises from the nature of the initial state of a molecular system and not from the equations of motion themselves, which conduct the system from that state to subsequent states in reversible ways. Boltzmann conceded that from improbable initial states, a system could evolve with decreasing entropy, but he observed that there are infinitely more initial states from which a system evolves with increasing entropy. With his discussion of initial conditions, Boltzmann underscored the probabilistic considerations in the molecular approach to the mechanical theory of heat.[12]

Following up an earlier suggestion of his, in a paper for the Vienna Academy in 1877 Boltzmann developed a powerful new method for determining heat equilibrium by calculating the probabilities of the various allowed molecular states. The method arose from his understanding that over time a system in an improbable initial state proceeds through more probable states to the most probable state, corresponding to heat equilibrium. Since the entropy of the system also increases as it approaches equilibrium, Boltzmann showed that entropy can be identified with probability.[13]

To make clear what is meant by the probability of states, Boltzmann supposed for the moment that any molecule of a gas can take on only a finite number of discrete values of kinetic energy: 0, ε, 2ε, . . ., $p\varepsilon$. This "fiction," which facilitated the calculation of probabilities, he later replaced with the realistic case of continuous allowable energies. The detailed assignment of kinetic energy to each of the molecules constituting the system is, in Boltzmann's terminology, a "complexion": the assignment, for example, of energy 2ε to the first molecule, of energy 6ε to the second molecule, and so on, defines a specific complexion. He assumed that any one complexion is as likely as any other, the condition of randomness. By contrast with the assignment of energy molecule by molecule, the "distribution" specifies only the gross numbers of molecules belonging to each of the allowed energy values: in a given distribution for a system of n molecules, ω_0 is the specific number of molecules with value 0, ω_1 the number with value ε, ω_2 the number with value 2ε, and so on. By the calculus of probabilities the number of complexions corresponding to any given distribution is:

$$P = \frac{n!}{(\omega_0)!(\omega_1)!\cdots}.$$

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[12] Ludwig Boltzmann, "Bemerkungen über einige Probleme der mechanischen Wärmetheorie," *Sitzungsber. Wiener Akad.* 75 (1877): 62–100; repr. *Wiss. Abh.*, vol. 2, 112–48, especially 116–22; Klein, *Ehrenfest*, 102–4. Loschmidt's statement, which came to be called the "reversibility paradox," had been discussed by William Thomson in 1874 (Brush, *Motion We Call Heat*, vol. 1, 238–39).

[13] Ludwig Boltzmann, "Über die Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung respektive den Sätzen über das Wärmegleichgewicht," *Sitzungsber. Wiener Akad.* 76 (1877): 373–435; repr. *Wiss. Abh.*, vol. 2, 164–223, on 165–66.

Boltzmann defined the probability W of a given state distribution, ω_0, ω_1, . . ., as the ratio P/J, where J is the sum of the complexions of all possible state distributions. Since J is a constant for a given molecular system, the maximum value of P determines the state of greatest probability, corresponding to heat equilibrium and maximum entropy. For mathematical convenience, Boltzmann investigated the maximum value of log P instead of P, and he replaced the discrete set ω_r by the continuous velocity distribution function f (u, v, w). This logarithm is, but for a constant,

$$\Omega = -\int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} f(u,v,w)\; l[og]\, f(u,v,w)\; du\, dv\, dw,$$

consistent with a constant total kinetic energy and a given number of molecules of the system. Boltzmann called Ω the "measure of permutability"; it is, as we see, the negative of the H function, so that it acquires its maximum value when f is the Maxwell distribution function, as Boltzmann had shown several years before. From his new understanding of the probability of a physical state, Boltzmann derived the result that Planck would use in introducing the quantum theory in 1900: entropy is proportional to the logarithm of the probability.[14]

At the close of his 1877 paper Boltzmann pointed out that the permutability measure extends the range of applicability of entropy. In thermodynamics, entropy is defined only for a system in thermal equilibrium, and so if a gas is not in equilibrium before and after undergoing a change of state, the entropy cannot be calculated, but its permutabability can, since it is defined for every state, equilibrium or otherwise. Boltzmann believed that the new understanding of entropy applies not only to gases, which he could confirm by calculation, but also to liquids and solids, though mathematical difficulties still prevented their exact treatment.[15] The molecular and probabilistic methods he used to analyze the second law of thermodynamics became standard tools of the theoretical physicist.

12.2 Phenomenology

At Karlsruhe, where he was professor of physics, Hertz found his direction in late 1886, and for nearly three years he worked steadily on researches related to electric waves and not, as usual, on a variety of topics. These researches contained his experimental confirmation and subsequent theoretical study of Maxwell's electromagnetic theory of light.

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[14] Boltzmann, "Über die Beziehung," 168, 175–76, 190–93. Boltzmann's reasoning in his 1877 paper is analyzed in detail, for example, in René Dugas, *La théorie physique au sens de Boltzmann et ses prolongements modernes* (Neuchâtel-Suisse: Griffon, 1959), 192–99; Klein, *Ehrenfest*, 105–8; Kuhn, *Black-Body Theory*, 47–54; Salvo D'Agostino, *A History of the Ideas of Theoretical Physics, Essays on Nineteenth and Twentieth Century Physics* (Dordrecht: Kluwer Academic Publishers, 2000), 212–13.

[15] Boltzmann, "Über die Beziehung," 217–18, 223.

Working in the laboratory of the Karlsruhe physics institute, Hertz made use of two related experimental arrangements, which he arrived at partly by theoretical reasoning and partly by experimental insight, if in his case the two can meaningfully be separated. To detect electric waves, he used the resonance principle, tuning a secondary circuit to receive waves radiated by a primary one, and to produce waves of manageably short length in the primary circuit, he discharged an induction coil across a spark gap between two spheres.[16] By November 1886 he had propagated an electric induction between two open current loops across a space of a meter and a half, and by December he had produced a resonance between two electric oscillations. With an assembly of primary and secondary circuits, spark gaps, and blocks of paraffin and other dielectrics, he demonstrated the existence of inductive effects of a polarization current in dielectrics. (In 1887, the Prussian Academy of Sciences accordingly awarded him its prize for solving the problem that Helmholtz had posed in 1879.) At first he interpreted his experiments with reference to Helmholtz's understanding that electrostatic and electromagnetic waves propagate with different velocities.[17] Later in the course of his experiments, he came to see that the propagation of electric waves in space or in air with the velocity of light is the central point of Faraday and Maxwell's theory, and that its confirmation would solve the rest of Helmholtz's problem. (Earlier the Prussian Academy had judged the demonstration that air and empty space behave like dielectrics as too demanding and had removed it from the original problem.)[18]

At the end of 1887, Hertz measured the velocity of electric waves in air. His first attempts suggested that the velocity is greater than that of light, perhaps infinite, and for a time he discontinued his experiments. He resumed them in the belief that a disproof of Maxwell's theory would be as important as a confirmation. He moved his experiments from the laboratory to the lecture hall where there was more room.[19]

As quickly as Hertz completed his experiments, he wrote them up, sending them to Helmholtz for publication by the Prussian Academy. At the request of the editor,

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[16] Max Planck, "Gedächtnissrede auf Heinrich Hertz," *Verh. phys. Ges.* 13 (1894): 9–29; repr. in *Physikalische Abhandlungen und Vorträge,* 3 vols. (Braunschweig: F. Vieweg, 1958), vol. 3, 268–88, on 281–82.

[17] Hertz abandoned his earlier approach to Maxwell's theory, in 1884, which treated electrical waves in the air and the ether without a physical hypothesis about the ether. In its place, he adopted Helmholtz's dielectric polarization, which was important for his course of experiments. He retained his belief that Maxwell's theory was superior, but Helmholtz's approach allowed him to prove it experimentally without presuming its truth (D'Agostino, *History of the Ideas of Theoretical Physics*, 138, 149, 177–78, 181–82; Olivier Darrigol, *Electrodynamics from Ampère to Einstein* [Oxford: Oxford University Press, 2000], 238).

[18] Planck, "Hertz," 282. In time, in agreement with Maxwell, Hertz recognized only one electric force, and he turned his attention from the electromagnetic effects of changing polarizations in material dielectrics to the free propagation of electric waves.

[19] Hertz to his parents, 13 November and 23 December 1887, 1 January 1888, in Heinrich Hertz, *Erinnerungen, Briefe, Tagebücher*, ed. J. Hertz, 2nd rev. ed. M. Hertz and C. Süsskind (San Francisco: San Francisco Press, 1977), 236–48.

he also published them in the *Annalen der Physik*. Early in 1888 he reported in the *Annalen* an indication of a "finite velocity of propagation of electric distance actions," renewing his confidence in his work.[20] In subsequent papers that year, he said that his experiments made electric waves in air "almost tangible," and that they promised a "decision between the conflicting theories" of electrodynamics and their differing predictions for open currents.[21] He concluded later that year that Maxwell's theory explains all the facts that he had investigated and that it is superior to the other theories.[22] By this theory, the production of electric waves depends not only on the source but also on the condition of the surrounding space, the seat of the electromagnetic energy. By means of large parabolic mirrors, lenses, gratings, and prisms, Hertz demonstrated that electric waves exhibit all of the main properties of light waves. In a paper in the *Annalen* in 1889, which he regarded as a "natural end" of the experimental series, he reported that his experiments had removed "any doubt as to the identity of light, radiant heat, and electromagnetic wave motion": electric waves are light rays of long wavelength.[23] Helmholtz informed the Berlin Physical Society of Hertz's demonstration of electric waves with these words: "Gentleman! I have to communicate to you today the most important physical discovery of the century."[24] In summing up the significance of Hertz's experiments, Helmholtz said that they showed that light and electricity are "most closely connected" and that "apparent actions-at-a-distance really consist of a propagation of an action from one layer of an intervening medium to the next."[25]

With this achievement, Hertz linked two principal directions of electrodynamic research in Germany. One direction was concerned with the motion of electricity in wires, and it was pursued mainly by experimentalists. The other, less developed direction was concerned with electrical propagation in space, and it was pursued by mathematical physicists rather than by experimentalists. The difference is explained in part by the very limited availability of experimental means for the latter direction. When Hertz invented an appropriate means, he showed how electrical waves in wires and electrical waves in space interact, experimentally and theoretically bridging the "two research traditions."[26]

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[20] Heinrich Hertz, "Ueber die Einwirkung einer gradlinigen elektrischen Schwingung auf eine benachbarte Strombahn," *Ann.* 34 (1888): 155–70, on 169.

[21] Heinrich Hertz, "Ueber die Ausbreitungsgeschwindigkeit der elektrodynamischen Wirkungen," *Ann.* 34 (1888): 551–69, on 568–69; "Ueber elektrodynamische Wellen im Luftraume und deren Reflexion," *Ann.* 34 (1888): 610–23, on 610.

[22] Heinrich Hertz, "Die Kräfte elektrischer Schwingungen behandelt nach der Maxwell'schen Theorie," *Ann.* 36 (1888): 1–22, on 1.

[23] Heinrich Hertz, "Ueber Strahlen elektrischer Kraft," *Ann.* 36 (1889): 769–83, on 781.

[24] Eugen Goldstein, "Aus vergangenen Tagen der Berliner Physikalischen Gesellschaft," *Naturwiss.* 13 (1925): 39–45, on 44.

[25] Helmholtz's preface to Hertz, *Gesammelte Werke*, vol. 3, *Die Prinzipien der Mechanik, in neuem Zusammenhange dargestellt*, ed. P. Lenard (Leipzig, 1894); *The Principles of Mechanics Presented in a New Form,* ed. D. E. Jones and J. T. Walley (London, 1899; reprint New York: Dover, 1956).

[26] D'Agostino, *History of the Ideas of Theoretical Physics*, 122–23.

Hertz's "external successes," as he put it, brought him an invitation to speak at the German Association meeting in Heidelberg in 1889, definitely a distinction for someone so young. His subject was the relations between light and electricity. They are not affirmed by direct testimony of the senses, he said, but are accessible to our intuition through mathematical physics, the key to the hidden unities of nature. When one studies Maxwell's theory, Hertz said, one feels "as if the mathematical formulas have an independent life and intelligence, as if they are wiser than we, even wiser than their discoverer, as if they give us more than he put into them."[27]

In a theoretical paper in 1890, Hertz stated as postulates the mathematical connections between the electric and magnetic forces, the main quantities of Maxwell's theory. He made no attempt to derive the equations from a physical hypothesis about the ether, which was still "entirely unknown"; instead he regarded it as "expedient to start from these equations in search of such further conjectures respecting the constitution of the ether."[28] Formulating the mathematical connections between the electric and magnetic forces to express the logical structure of Maxwell's theory, Hertz stated the equations for the free ether, in Gauss's absolute units:

$$A\frac{dL}{dt}=\frac{dZ}{dy}-\frac{dY}{dz},A\frac{d\mathcal{X}}{dt}=\frac{dM}{dz}-\frac{dN}{dy},$$

with corresponding equations for the y and z components. X, Y, Z are the components of the electric force, L, M, N those of the magnetic force, and A is the reciprocal of the velocity of light. Hertz added supplementary equations that distinguish the ether from ponderable matter:

$$\frac{dL}{dx}+\frac{dM}{dy}+\frac{dN}{dz}=0,\ \frac{d\mathcal{X}}{dx}+\frac{dY}{dy}+\frac{dZ}{dz}=0$$

From these equations, Hertz deduced the principal electric, magnetic, and optical phenomena: Ohm's law, Kirchhoff's laws of circuits, Neumann's potential for the ponderomotive force between currents, law of electrostatic force which was the starting point for theories like Weber's but was a "remote final result" in Maxwell's theory, induction in open circuits which was the "richest region of all" and one still little explored experimentally (Hertz cited his papers on electric waves), and more.[29]

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[27] Heinrich Hertz, *Ueber die Beziehungen zwischen Licht und Elektricität* (Bonn, 1889); in *Ges. Werke*, vol. 1, 339–54, on 339–40, 344, 352–53.

[28] Heinrich Hertz, "On the Fundamental Equations of Electromagnetics for Bodies at Rest," 1890, in *Electric Waves, Being Researches on the Propagation of Electric Action with Finite Velocity through Space,* trans. D. E. Jones (New York, 1893; repr. New York: Dover, 1962), 201.

[29] Hertz, "Fundamental Equations … at Rest," 201. These became a standard form of the equations of Maxwell's theory; Hertz's left-handed coordinate system determines the sign. Hertz's introduction of these equations is analyzed in Tetu Hirosige, "Electrodynamics before the Theory of Relativity, 1890-1905," *Jap. Stud. Hist. Sci.* 5 (1966), 1–49, on 2–6; and in P. M. Heimann, "Maxwell, Hertz and the Nature of Electricity," *Isis* 62 (1970): 149–57.

Hertz's theoretical work on Maxwell's theory, quite apart from the clarity it brought to the subject, held an interest as a method of theoretical physics. By starting from the differential equations describing experimental results rather than from detailed physical assumptions, he offered physicists a model of what Boltzmann called "mathematical phenomenology." Ernst Mach wrote to Hertz that he admired his way of doing physics, since it followed the "ideal of a physics free of mythology," which he had been advocating.[30]

Hertz's phenomenology was a provisional stage of research. The bare equations for the electromagnetic field were not the end of the physics in his judgment. In his Heidelberg talk, he singled out three "ultimate" problems, which he thought would be solved by starting from the views of Faraday and Maxwell's together with the understanding that light and electricity have a common ether. The first problem is gravitation, the one remaining action at a distance, which Hertz believed would be shown to be finitely propagated too. The second is the nature of electricity, which is now understood to extend "over all of nature." The third is the "nature, the properties of the space-filling medium, of the ether, its structure, its rest or motion, its infinite or bounded extent." The latter is the "all-important question," the answer to which will reveal the nature of electricity and matter. "Today's physics," Hertz said, "is inclined to ask the question if all that exists has not been created from the ether." He closed his talk with the goal of a comprehensive physical theory of the ether, the substratum of the entire phenomenal world, of light, electricity and magnetism, gravitation, and the rest.[31] Like the German physicists at the beginning of the nineteenth century, he believed that physics could discover the "nature" of things; the significant difference between then and Hertz's day was the theory and method of Faraday and Maxwell's, which made the goal appear reachable.

12.3 Principles

When Planck moved to Berlin as extraordinary professor for theoretical physics, he was just thirty and in the middle of a series of researches on the thermodynamics of chemical processes. These impressed Helmholtz, who thought that physical chemistry would dominate the next development of chemistry, leading at last to a true theory. Helmholtz regarded Planck as the most capable young researcher working on the subject, a matter he could judge expertly, having recently worked on it himself. In its evaluation, the Berlin philosophical faculty was impressed by Planck's researches, as exemplified in his physical chemistry work, which carried through the "strong consequences of thermodynamics without interference from

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[30] Ludwig Boltzmann, "Über die Entwicklung der Methoden der theoretischen Physik in neuerer Zeit," in *Populäre Schriften* (Leipzig: J. A. Barth, 1905), 198–227, on 221; Mach to Hertz, 25 September 1890, Ms. Coll., DM, 2976.

[31] Hertz, *Ueber die Beziehungen*, 353–54.

other hypotheses." The faculty also praised Planck for his "original ideas," which were what Helmholtz always looked for in candidates for physics jobs.[32]

Beginning in 1887, while he was still at Kiel, Planck published four papers under the title "On the Principle of Increase of Entropy," in which he applied the second law of thermodynamics to chemical problems. His goal was to carry further the "grand generalization" begun by Helmholtz, Gibbs, and others: like the first principle of the mechanical heat theory, the second, the "Carnot-Clausius," principle applies throughout physics and chemistry, and because it applies to irreversible, or "natural," processes as well to reversible ones, it applies to all processes whatsoever. Planck's starting point for studying the path of chemical reactions was Clausius's statement of the entropy principle: in all processes involving a change of state of bodies, the sum of their entropies increases or remains the same and never decreases. With the help of this principle and through the introduction of thermodynamic potentials, in the first of his series of papers Planck developed a general theory of chemical equilibrium, the details of which he filled in in subsequent papers. His method was carefully laid out: it was to determine the laws from facts rather than from "definite ideas of the nature of molecular motion."[33] One of the later papers of the series Einstein considered to be outstanding, and what he admired about it was the generality of its formulas, which contained all that can be derived from pure thermodynamic principles.[34]

At the German Association meeting in 1891, Planck gave an invited talk on recent progress in heat theory, in which he discussed the dissociation of gases and other problems he had solved in his recent work. He recalled the daring hypotheses of Krőnig, Clausius, and Boltzmann, who had tried to answer the final questions of the mechanics of atoms through the kinetic theory of gases; after its initial successes, this theory had not fulfilled the high expectations it had aroused, in Planck's opinion, and the subsequent efforts expended on it were disproportionate to the results. Planck believed that a deeper insight into the world of molecules is obtainable from the general laws of thermodynamics together with the method of "ideal processes" (reversible processes). They are a "special triumph of the human mind," a "pathfinder," which can direct us to connections between natural laws in regions closed to direct experimentation. This was all the more remarkable because

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[32] Report by the Berlin U. Philosophical Faculty, 29 November 1888, DZA, Merseburg; quoted in Armin Hermann, *Max Planck in Selbstzeugnissen und Bilddokumenten* (Reinbek b. Hamburg: Rowohlt, 1973), 21–22.

[33] Max Planck, "Ueber das Princip der Vermehrung der Entropie. Erste Abhandlung. Gesetze des Verlaufs von Reactionen, die nach constanten Gewichtsverhältnissen vor sich gehen," *Ann* 30 (1887): 562–82; repr. in *Phys. Abh.*, vol. 1, 196–216, on 196–200; Max Born, "Max Karl Ernst Ludwig Planck 1858-1947," *Obituary Notices of Fellows of the Royal Society* 6 (1948): 161–88; repr. in *Ausgewählte Abhandlungen*, ed. Akademie der Wissenschaften in Göttingen (Göttingen: Vandenhoeck und Ruprecht, 1963), vol. 2, 626–46, on 629.

[34] Einstein referred to the third paper of the series: Max Planck, "Ueber das Princip der Vermehrung der Entropie. Dritte Abhandlung. Gesetze des Eintritts beliebiger thermodynamischer and chemischer Reactionen," *Ann.* 32 (1887): 462–503; repr. in *Phys. Abh.*, vol.3, 232–73; Albert Einstein, "Max Planck als Forscher," *Naturwiss.* 1 (1913): 1077–79, on 1077.

ideal processes cannot be directly proven by experiment.[35] When a few years later, in 1894, Helmholtz, Kundt, and Bezold proposed Planck as an ordinary member of the Prussian Academy of Sciences, they discussed his research, eleven papers by then, all of which had appeared in the *Annalen der Physik,* mostly related to thermochemistry; they were impressed by the results he had derived from general principles alone.[36]

12.4 Analogies

We saw how Ohm drew on Fourier's mathematical theory of imponderable heat to develop by analogy a mathematical theory of another imponderable substance, electricity. Like Ohm, guided by Fourier's theory, the British physicist William Thomson developed the method of analogy further in the 1840s, extending it to the laws of fluids, elastic solids, and again electricity. Again like Fourier and Ohm, he thought that a common mathematical representation had physical significance, revealing an underlying connectedness of phenomena across the branches of physics. The method of analogy took on increasing significance in the second half of the century as a way of connecting different areas of physics.

Helmholtz made extensive use of the method of analogy. In several papers in 1884, he showed that the limited transformability of heat energy has an analogy in the behavior of mechanical systems containing hidden "inner cyclic motions." These are a class of motions that preserve the kinetic energy and the total energy of a system, while the individual parts of the system rapidly change their positions. What is important for the analogy is that only the cyclic velocities, which correspond to the rapid molecular heat motions of a gas, enter the expression for the energy of the system, not the coordinates defining the positions. Helmholtz assumed that any changes in the cyclic velocities produced by external forces are brought about relatively slowly; and slowly varying coordinates, such as those that define the volume of a gas, enter equations that are identical to those relating heat to work. He illustrated the motions of his analogy by a spinning top: the hidden rotation of

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[35] Max Planck, "Allgemeines zur neueren Entwicklung der Wärmetheorie," *Zs. f. phys. Chemie* 8 (1891): 647–56; repr. *Phys. Abh.*, vol. 1, 372–81, quotations on 380–81.

[36] Proposal of Planck as ordinary member of the Prussian Academy of Sciences, signed by Helmholtz, Kundt, and Bezold. Document Nr. 23, in *Physiker über Physiker*, ed. Christa Kirsten, and Hans-Günther Körber (Berlin: Akademie-Verlag, 1975.), 125–26. The proposal is undated; Planck's election was on 11 June 1894.

the top corresponds to the hidden motion identified with heat, which is rapid compared with the observable slow precession of the axis of the top.[37]

To formulate the analogy exactly, Helmholtz analyzed the simplest case, a "monocyclic" system. For it, only one cyclic velocity q enters; the system may contain many inner cyclic motions, but if it is monocyclic, they all must depend on one parameter. In this system, the analog of the introduction of heat from the outside is the performance of work dQ by an external force tending to increase the cyclic velocity. For this work, Helmholtz wrote $dQ = qds$, where s is shorthand for $- \partial H/\partial q$. (H enters Lagrange's equations of motion as the difference between the potential energy and the kinetic energy of the system, the negative of the Lagrangian function.) Assuming that the measure of temperature is kinetic energy, Helmholtz transformed the equation to read $dQ = LdS$, where the integrating factor L is the kinetic energy and S, a function of s, is the measure of entropy. With this familiar result, Helmholtz observed, the "Carnot-Clausius principle" is no longer a principle derived from experience but appears as a special case of a law derived from the "general principles of mechanics." In response to a criticism by Clausius, Helmholtz clarified his intention: it was to draw attention to "analogies" between monocyclic and thermal motions rather than to claim "'an explanation' of the second principle of the mechanical theory of heat." The analogies concern only the "most general conditions" under which the "most general physical characteristics of heat motion" can be represented by mechanical motion, rather than a detailed mechanical model. This work interested Boltzmann, who in several papers in 1884 and 1886 examined the relationship of monocyclic motions to his own interpretation of the second law. Hertz also found Helmholtz's monocyclic studies instructive as the first general treatment of hidden motions, on which Hertz based his new principles of mechanics.

In another application of Helmholtz's method of analogy, Boltzmann introduced cyclic motions in a different branch of physics. In the first part of his university lectures on Maxwell's electromagnetic theory, he interpreted motion in the ether and in ponderable bodies as Helmholtz's cyclic motion, giving rise to electric currents. For a "bicycle," a system of masses for which the motion and position

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[37] Maxwell was the first to work with general cyclic systems, applying them to electromagnetism. They were applied to heat by Maxwell, Rankine, and Helmholtz (Ludwig Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, vol. 2 [Leipzig: J. A. Barth, 1904], 166). The properties of monocyclic systems and Helmholtz's reasons for studying them together with his conclusions are discussed in Martin J. Klein, "Mechanical Explanation at the End of the Nineteenth Century," *Centaurus* 17 (1792): 58–82, on 63–67; Leo Königsberger, "The Investigations of Hermann von Helmholtz on the Fundamental Principles of Mathematics and Mechanics," *Annual Report of the ... Smithsonian Institution ... to July, 1896* (1898): 93–124, on 120–23; and, the main source of our discussion, Helmholtz's Prussian Academy papers on the subject and his recapitulation of them in his Berlin lectures on the theory of heat: "Studien zur Statik monocyklischer Systeme," *Sitzungsber. preuss. Akad.*, 1884, 159–77, 311–18, 755–59, and *Vorlesungen über theoretische Physik*, vol. 6, *Vorlesungen über die Theorie der Wärme*, ed. Franz Richarz (Leipzig: J. A. Barth, 1903), 338–70. We acknowledge discussions with Stephen M. Winters, who has made a study of Helmholtz's physics, including his monocyclic systems and least-action principle.

are determined by two "cyclic" coordinates, Boltzmann wrote Lagrange's equations of motion, which he illustrated by a model, essentially two independent cranks linked by a movable middle shaft; representing the two cyclic coordinates by the angular positions of the cranks, he showed that the motion of the bicycle forms a complete analogy with two interacting electric currents. To keep his audience from confusing the apparatus with reality, Boltzmann returned to the methodological lesson with which he introduced mechanical analogies: the mechanism of the electric current is completely different from that of the apparatus and, moreover, it is completely unknown to us. That the apparatus has only a gross analogy with nature does not detract from its usefulness, however, since it offers the advantage of working with a well-defined, well-understood mechanical system. To develop the consequences of Maxwell's equations, Boltzmann introduced new analogies, or "mechanical pictures," which he distinguished from the "mechanical foundations" of the theory. In general, throughout his lectures, Boltzmann constantly reminded his audience that he was doing theoretical physics according to the preferred new method, not describing reality. He showed, that is, how to build theories of physics in what he regarded as Maxwell's way, which he supported and which found considerable sympathy in Germany.[38]

As the conservator of a mathematical-physical collection, Boltzmann added some pieces of electrical apparatus, which included his own "bicycle," a model for demonstrating the mutual induction of electric currents.[39] When in 1892 the German Mathematical Society published a catalog of instruments and mathematical and physical models, including Boltzmann's, Boltzmann used the occasion to write up his thoughts on the methods of theoretical physics. Material models, a recent supplement to lectures in physics, symbolized for him the most promising new method. The older, supplanted method of theoretical physics Boltzmann associated above all with the French, who believed that physical theory "explains" the phenomena and that the electric and material points and central forces on which they built their theories correspond to reality. He agreed with Kirchhoff, who corrected the French by pointing out that theory only "describes" the phenomena, not explains them. By this newer understanding, it was still proper to develop physical theory from mechanical conceptions, as the French had done, but now the mechanisms had to be recognized as "pictures," or "analogies," not as reality. Maxwell had early grasped this point and, by appealing to mechanical analogies, he had developed electromagnetic equations of "unbelievable magical power." Maxwell had been impressed by the analogies of nature, by the reappearance of the same patterns throughout nature, as represented by the same laws and differential equations. He had taught physicists that to understand is to see analogies, and his method

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[38] Ludwig Boltzmann, *Vorlesungen über Maxwells Theorie der Elektricität und des Lichtes*, vol. 2, *Verhältniss zur Fernwirkungstheorie; specielle Fälle der Elektrostatik, stationären Strömung und Induction* (Leipzig, 1893), 22, 50.

[39] Arnold Sommerfeld, "Das Institut für Theoretische Physik,"in *Die wissenschaftlichen Anstalten der Ludwig-Maximilians-Universität zu München,* ed. Karl Alexander von Müller (Munich: R. Oldenbourg und Dr. C. Wolf, 1926), 290–91, on 290.

was the new preferred method of theoretical physics. Mechanical analogies can be displayed visually by demonstration apparatus, the reason why Boltzmann set store by them.[40]

At the time of his essay on the new methods of theoretical physics, in 1892, Boltzmann presented a paper to the Bavarian Academy of Sciences on the properties of the ether as characterized by Maxwell's electromagnetic equations. As Maxwell did, he represented the ether by mechanical analogies, first as a continuous, incompressible, weightless fluid with mass and inertia, and second as a homogeneous, isotropic, elastic solid. By use of the laws of continuum mechanics and the energy principle, Boltzmann showed that the forces acting within the fluid and the solid give rise to equations that, when the symbols are appropriately interpreted, are identical with Maxwell's. There is no suggestion in Boltzmann's discussion that he thought that the ether really is an incompressible fluid or an elastic solid.[41]

At the German Association meeting in 1893, Boltzmann reported on new theories of electricity and magnetism. There, in addition to classifying several theories of the ether developed by German and British physicists and sketching a new theory of his own, he cautioned against following Hertz's phenomenological treatment of Maxwell's theory. He acknowledged that Hertz's presentation of Maxwell's equations as the experimentally given could be useful, but physics would be held back if physicists gave up their search for mechanical analogies. Recalling Maxwell's observation that an infinite number of mechanical conceptions are possible, Boltzmann said that a good number had already been tried and still more should be, since mechanical analogies had great value for illustrating and discovering facts, as Maxwell's own career of discovery had shown.[42]

Physicists held different ideas about analogies. Hertz wrote to Walter Kőnig in 1889 about an analogy the latter had made between optics and electricity involving Stokes's formulas. Hertz observed that it was not an analogy "between things but between mathematical expressions," and that it called for complete clarity: "1) how much lies simply in the agreement of the differential equations? 2) how come only certain terms agree and not others?" Hertz was interested in "how much analogy there is between the old and the new," and he would have looked for more himself if he had not been caught up in his researches on electric waves. He accepted Kőnig's

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[40] Ludwig Boltzmann, "Über die Methoden der theoretischen Physik" (1892), in *Populäre Schriften*, 1–10. Boltzmann approved of Maxwell's expression "dynamical illustration" for mechanisms representing the electromagnetic field. He discussed this and related points in Ludwig Boltzmann, *Vorlesungen über Maxwells Theorie der Elektricität und des Lichtes*, vol. 1, *Ableitung der Grundgleichungen für ruhende, homogene, isotrope Körper* (Leipzig, 1891), 13, 35, and elsewhere in his lectures.

[41] Ludwig Boltzmann, "Über ein Medium, dessen mechanische Eigenschaften auf die von Maxwell für den Electromagnetismus aufgestellten Gleichungen führen," *Sitzungsber. bay. Akad.* 22 (1892): 279–301; repr. in *Wiss. Abh.*, vol. 3, 406–27.

[42] Ludwig Boltzmann, "Ueber die neueren Theorien der Elektrizität und des Magnetismus," *Verh. Ges. deutsch. Naturf. u. Ärzte* 65 (1893): 34–35; repr. in *Wiss. Abh.*, vol. 3, 502–3.

analogy between "optical *things* and electrical *things*" but was critical of his analogy between "formulas." The differential equations were "only *similar*, not equivalent formulas."[43]

12.5 Methods of Presentation: Lectures

For theoretical physics as a field, teaching is as important as research. Without the regular renewal of trained researchers, the field would stagnate and eventually come to an end. This is one of the reasons we give attention to arrangements for teaching within physics institutes and within universities and to otherwise boring lists of courses. In this section, we look at how theoretical physicists presented their field to their auditors, a few of whom would go on to become theoretical physicists. Teaching belonged to the work of theoretical physics, and a good part of it was preparing and reworking lectures from one cycle to the next.

Most of the theoretical physicists we discuss published their lectures, usually on the branches of physics that especially interested them; for example, Boltzmann published his lectures on gas theory and Planck published his lectures on thermodynamics. In the case of three of them, Neumann, Kirchhoff, and Helmholtz, their lectures on the whole or nearly the whole of theoretical physics were eventually published.

Even though Neumann was the second physics professor at Königsberg, charged with teaching the specialized lectures that nobody was required to attend, he nevertheless managed to cover all the areas of physical theory in a sequence of courses and keep them going for over forty years. His lectures were relatively well-attended, attracting twice as many students as his seminar in the early years before 1860.[44] The first volume of Neumann's published lectures, *Einleitung in die theoretische Physik* [*Introduction to Theoretical Physics*], conveys the idea of theoretical physics as he presented it to his students. He begins the course "with the mechanical part of physics, because it builds the foundation for all the other branches and contains the principles which find their application there." Unlike many lecturers after him, he does not develop mechanics axiomatically and systematically; instead he discusses gravity and introduces the important laws and concepts of mechanics as needed. To show how theoretical investigations and experimental observations are brought together, he discusses extensively the principles and operation of instruments, beginning with the pendulum, and recommending a work on pendulums by his colleague Bessel as the "best model

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[43] Hertz to Kőnig, 20 April 1889, Bonn (DM 3195).

[44] In 1834–1839, 41 individuals attended Neumann's private lectures, but only 18 attended his seminar. In 1840–1849, 56 individuals attended his private lectures, 11 his seminar. For 1850–1859, 52 attended his private lectures, 34 his seminar. After 1860, the seminar became stronger: in 1860–1869, 84 attended the private lectures, 61 his seminar.

for study." He discusses at length a second precision instrument, the balance, which next to the pendulum is the "most important physical instrument." He mentions a large number of researchers primarily for their use of instruments and for their experimental data. He refers to Gauss's method of bifilar suspension of a magnet for determining the moment of inertia, Weber's balance as used by Bessel in pendulum observations, and Henry Cavendish's torsion balance used to determine the density of the Earth. He makes reference to many "French physicists," Laplace, Biot, Clairaut, Regnault, Borda, Arago, Gay-Lussac, Carnot, and others, largely appreciatively; it was they, for example, who made the most exact observations of pendulums before Bessel. The reviewer in the *Fortschritte* said that in the *Einleitung,* Neumann succeeded in presenting "the mechanical foundations of this discipline [theoretical physics]." Neumann's subsequent lectures on magnetism, electric current, elastic bodies, and other parts of physics included extensive mathematical developments, but always in relation to physics. His lectures in themselves were a considerable achievement. When he started out, with the exception of mathematical treatments of mechanics, there were no textbooks on mathematical physics. He had to put together his course from scattered sources; for example, Lagrange and Poisson on mechanics, Laplace and Gauss on capillarity, Gauss and Green on potential theory, and Fourier on heat theory.[45]

In 1875, when Neumann stepped down from lecturing on mathematical physics, his prize student, Kirchhoff, moved to Berlin as professor of mathematical physics, carrying the torch with him. No longer having an institute for experimental work, Kirchhoff experimented in the private laboratory of a friend, but most of his research was purely theoretical, a narrowing forced on him in part by poor health. He published a paper or two a year, though his main achievement at Berlin was his lectures on mathematical physics. He intended to publish his lectures on all parts of mathematical physics, but he succeeded in bringing out only the part on mechanics; [46] his other lectures appeared after his death under various editors as "Lectures on Mathematical Physics."

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[45] Franz Neumann, *Vorlesungen über mathematische Physik, gehalten an der Universität Königsberg*. *Einleitung in die theoretische Physik*, ed. Carl Pape (Leipzig, 1883), vii, 1, 4, 78, 110, 117. The editor used lectures he took notes on in the winter 1858–1859, Paul Volkmann, *Franz Neumann. 11. September 1798, 23. Mai 1895* [Leipzig, 1896], 38). Woldemar Voigt, "Zur Erinnerung an F. E. Neumann, gestorben am 23. Mai 1895 zu Königsberg i/Pr.," *Gött. Nachr.*, 1895, 248–65, on 256, 258; repr. "Gedächtnissrede auf Franz Neumann," in *Franz Neumanns Gesammelte Werke*, edited by his students, 3 vols. (Leipzig: B. G. Teubner, 1906–28), vol. 1, 3–19; Review of Neumann's *Einleitung in die theoretische Physik* in *Die Fortschritte der Physik im Jahre 1883* 39 (1883): 166–67.

[46] Gustav Kirchhoff, *Vorlesungen über mathematische Physik*, vol. 1, *Mechanik*, 3rd ed. (Leipzig, 1883), quotation from the preface to the first edition in 1876. The volume is based on lectures Kirchhoff gave at Heidelberg just before moving to Berlin. The third edition is almost unchanged from the first. Ludwig Boltzmann, *Gustav Robert Kirchhoff* (Leipzig, 1888), 22. Robert Helmholtz, "A Memoir of Gustav Robert Kirchhoff," trans. J. de Perott, in *Annual Report of the ... Smithsonian Institution ... to July, 1889*, 1890, 527–40, on 531.

In the volume of lectures he completed, Kirchhoff defines his subject as "pure mechanics," the study of the motion of bodies, ignoring all other physical changes of the bodies. He is conscious that his presentation differs from the usual one, in which mechanics is defined as the science of forces, taken as primitive things that create and destroy motion. To him, the idea of forces is a metaphysical concept, which has only historical importance, and its continuance in mechanics causes confusion; he makes a place for forces, but only as a means of simplifying equations, not of explaining nature. He likewise dispenses with the standard concepts of mass and causality, explaining that all he needs to formulate the general equations of mechanics are space, time, and matter. With the object of bringing clarity to mechanics, he defines its task as the complete and simplest description of motion. In his lectures in general, he couples his emphasis on conceptual rigor with frequent reference to experimental facts, especially where they are in conflict with theory.[47]

The equations of motion show what Kirchhoff has in mind by the criterion of simplicity. The motion of a material point is completely determined if at the beginning its position and velocity are given and if for every value of time its acceleration is given. The acceleration is the second derivative with respect to time of the three coordinate variables, and the only reason why the third and higher differential quotients are not also needed is experience and simplicity; if they were introduced this would only complicate matters, violating the criterion of the simplest description of motion.[48]

The next lectures of the series, on mathematical optics, were published fifteen years after the mechanics volume, and three years after Kirchhoff's death.[49] At the start, Kirchhoff takes up two classes of optical theories, the emission and the wave theories. Though the wave theory has supplanted the emission theory, the latter is still important, Kirchhoff says, because it accounts for the most obvious optical phenomena, the propagation of light in straight lines and the aberration of light, in the simplest way. But in his lectures, he is concerned with the wave theory, which is founded on the hypothesis that light consists of oscillations of an ether, a medium that fills empty space and the space between the ponderable molecules making up bodies. He bases his discussion of light on the equations of infinitely small motions of the ether, which he has derived in his mechanics lectures. In an aside in his heat lectures, Kirchhoff explains that the hypothesis of light as oscillations is the reason why specifically optical concepts are reducible to mechanical concepts: the kinetic energy of oscillations stipulates the intensity of light, the duration of which stipulates the color, and the direction of which stipulates the state of polarization.

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[47] Kirchhoff's preface to *Vorlesungen über Mechanik*.

[48] Kirchhoff, *Vorlesungen über Mechanik*, chapter 1.

[49] Gustav Kirchhoff, *Vorlesungen über mathematische Physik*, vol. 2, *Vorlesungen über mathematische Optik*, ed. K. Hensel (Leipzig, 1891). This volume is based on lectures he gave at Berlin in 1876–77 and 1885–86.

In the same year as his optical lectures, Kirchhoff's lectures on electricity and magnetism were published. Planck, who edited these lectures as well as the lectures on heat, chose to bring out the former first because the theory of electricity was a "mighty, and daily expanding field." Kirchhoff speaks of the "causes" of electrical and magnetic phenomena, which he locates in two kinds of electrical fluids and two kinds of magnetic fluids, and he connects them by means of distance forces and their potentials instead of by mechanical motions in the ether. He acknowledges that the view he has assumed of forces is not a general view, for according to Faraday and Maxwell all electric and magnetic forces are transmitted by a medium. Planck noted that Kirchhoff treated only in passing Faraday and Maxwell's theory, "which at present perhaps offers most hope for a fruitful development."[50]

The final volume of Kirchhoff's lectures deals with the theory of heat. He begins it with a lengthy discussion of the mechanical view of nature, which he regards as a world picture in the making, still incomplete. The justification of the mechanical view of nature he paraphrases from his lectures on the theory of mechanics: the task of physics is to order physical phenomena "clearly" and to arrange them "as simply as possible," and the simplest physical phenomena are those of motion. But if the reduction of heat phenomena to motion satisfies the criteria of simplicity, it does not satisfy the criterion of clarity: the idea of heat motion is "unclear," he says, and even in its most developed form, in the theory of gases, the assumed molecular collisions remain "obscure." In the first of the heat lectures, and again in later lectures, Kirchhoff goes to lengths to show the extent to which heat is, and is not yet, ordered under the concept of mechanics.[51]

In the early heat lectures, which deal solely with the distribution and variation of temperature, Kirchhoff treats matter as continuous in the space of bodies, a viewpoint which does not allow him to reduce temperature changes to mechanics. He explains that it is best to define the central concept of the theory, temperature, independently of any body, for then the "laws of heat actions will assume their simplest form." He brings in "pure mechanics" at this stage only as a guide in developing the formulas of "pure heat theory," which neglects as insignificant any motions accompanying changes in temperature. Just as he introduces accessory concepts such as velocity and force in mechanics, in the theory of heat he introduces accessory concepts such as quantity of heat and specific heat, and he does so for the same reason: the resulting laws are simpler. The units for heat are the same as those for mechanics: space, time, and mass, with the addition of units for changes in heat and temperature. He refers to a good many experimental publications, the majority of them appearing in the *Annalen*.[52]

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[50] Gustav Kirchhoff, *Vorlesungen über Electricität und Magnetismus*, ed. M. Planck (Leipzig, 1891).

[51] Gustav Kirchhoff, *Vorlesungen über mathematische Physik*, vol. 4, *Vorlesungen über die Theorie der Wärme,* ed. Max Planck (Leipzig, 1894), 1–5. This volume is based on lectures he gave at Berlin in 1876, 1878, 1880, 1882, and 1884.

[52] Ibid., 5, 10, 12, 57.

Not until he is a quarter way into his lectures does Kirchhoff introduce the "mechanical theory of heat," or "thermodynamics," which deals with motions as well as with temperature changes, but even here he holds to his initial assumption that temperature is a property of matter that does not "need to be reduced to motion." He introduces the principle of energy conservation and its place in heat theory, calling the law of equivalence of heat and work the "first fundamental law." He next introduces the "second fundamental law," the law of entropy. He treats the theory of "ideal" gases as an "application" of the mechanical theory of heat. He gives ample space to Van der Waal's equation of state for gases that depart from the ideal, along with similar equations, including one by Clausius, which agrees well with measurements. On the topic of his last lectures, the kinetic theory, he proceeds cautiously. Up to this point, he has treated the concepts of temperature and quantity of heat as independent of mechanics, as he must so long as he regards matter as continuously filling the space of bodies and motion as continuously changing, as appears to be the case. Now he interprets temperature and heat in terms of molecular motion, while cautioning that the "leap" from the phenomena that molecules are intended to explain to the molecules themselves is so great that it is hard to decide on the right assumptions for drawing strong conclusions. Nevertheless, he says, it is possible to build a theory from molecular assumptions that "represents many properties of *gases* in a satisfactory way and offers a valuable guide to the further investigation of these properties." Since the properties depend on average values and not on individual molecules, he introduces Maxwell's velocity distribution along with the "concept of probability" to deal with the "statistical" nature of the subject, and he introduces the appropriate branch of mathematics, the "probability calculus," for carrying out the calculations.[53]

Kirchhoff's lectures are largely concerned with mathematical "methods" of deriving and solving equations that express physical laws. An example is the one-dimensional diffusion equation, which Fourier treated in his book on the theory of heat, and which was also treated in the book of Riemann's lectures on partial differential equations: $\frac{\partial \theta}{\partial t} = a^2 \frac{\partial^2 \theta}{\partial x^2}\cdot$ where θ is temperature, t is time, and a is a constant. Kirchhoff solves the equation for a variety of boundary conditions and for the case where a body radiates heat from the surface into the surroundings. He applies it to the heat of the Earth, and he cites publications on the variation of temperature with depth and on the cooling of the Earth from its molten state. In a later lecture, he treats the three-dimensional diffusion equation with radiation at the surface and applies it to heat conduction in a rod of infinitely small, rectilinear cross section, and here he cites measurements of heat conduction in an iron rod by Wilhelm Weber.[54] The diffusion equation is the heart of the heat conduction theory, and Kirchhoff gives appropriate space to it in his lectures.

In Kirchhoff's presentation of mathematical physics, the basic equations of one branch of physics reappear in other branches, suggesting analogies between diverse

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[53] Ibid., 51, 60–61, 69, 97, 102, 134–36.

[54] Kirchhoff, *Theorie der Wärme*, 2nd and 3rd lectures.

phenomena, and he calls attention to them in many places in the lectures. For example, for "clarity," he gives an analogy between electric lines of force and the velocity potential in hydrodynamics; he shows that to each electrical problem there corresponds a stationary motion in an incompressible fluid, with electric lines of force corresponding to flow lines in the fluid.[55] Analogies are one way Kirchhoff's lectures show how the parts of physics are connected. There are other ways: the recurrence of mechanical concepts and laws wherever they are justified, the ever-presence of potentials and differential equations and their integrals, the picture of continuous matter with exceptions, a common system of measures with additions, the lawful interactions of different physical areas such as the magnetic, thermal, and chemical effects of electric currents, the universal laws, and the criterion of simple description.

What appealed to readers of Kirchhoff's mechanical lectures is their compelling logic,[56] Boltzmann said. He continued: Kirchhoff's lectures embody his method in physics, which is to avoid "bold hypotheses" and to "build the equations that correspond to the phenomenal world as truly as possible and quantitatively correctly, unconcerned with the essence of things and forces." Planck considered Kirchhoff's lectures to be a model for work in exact science.[57]

After Helmholtz gave up his physics chair at Berlin University, he offered a small lecture course in theoretical physics, which covered the main parts in six successive semesters, a practice he followed until a year before his death. Having begun teaching physics with the conviction that theoretical physics was in need of encouragement in Germany, he ended his teaching by lecturing on it exclusively. In 1892 he let it be known that he wanted to see his lectures published, and he revised parts of them. Over the next fifteen years they appeared in handsomely produced volumes, one for each part of the cycle of lectures, edited mainly by former students of his.

Helmholtz's introductory lectures, published as *Einleitung zu den Vorlesungen über theoretische Physik*, consist not of the expected lectures on mechanics but of more fundamental matters, recalling the philosophical introduction to his memoir on the conservation of force. These are epistemological and methodological principles that apply to all parts of physics, which he justifies including on the grounds that "we must investigate the instrument we work with." He explains that the creation of concepts, hypotheses, and laws and their formulation in differential equations and integrals, have to be understood if the work of theoretical physics is to be understood. Our "ideas" and "wishes," our "consciousness" and "will," cannot influence the phenomena we know through experience. Belonging to the "outer world," they exist independently of us and are subject to natural laws, which are sometimes called "forces." They mean no more than that the laws will show

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[55] Kirchhoff, *Electricität und Magnetismus*, 11.

[56] Boltzmann, *Kirchhoff*, 25.

[57] Planck's foreword to Gustav Kirchhoff, *Vorlesungen über mathematische Physik,* vol. 3, *Vorlesungen über Elektrictät und Magnetismus*, ed. M. Planck (Leipzig, 1891).

themselves "in each case where the conditions for the phenomena are given." Physicists assume that all changes in bodies obey laws and have intelligible causes, or forces, which are the subject of general mechanics, or "dynamics."[58] Helmholtz says that the totality of theoretical physics can be developed with the aid of the concept of force.[59]

Helmholtz brings up elementary matters which in conventional presentations of physics are usually assumed without comment. For example, he discusses the axioms of arithmetic, pointing out that their applicability to physical magnitudes has to be decided by experiment; he discusses the various kinds of magnitudes that enter physics; and he discusses the conceptual basis for the quantitative description of physical phenomena. He does not present the mathematical methods of theoretical physics here; he does that in later lectures, in which he develops the laws of physical phenomena.

In his introductory lectures, he discusses in general terms the two ways physicists have of treating bodies. Depending on the problem at hand, they regard bodies either as aggregates of material points or as volume elements filled with matter. The difference between the two ways is far-reaching, and Helmholtz devotes a separate semester – and volume of lectures – to each. The difference between the two is evident from the calculation of density of matter, or the ratio of mass to volume: within the picture of continuously distributed masses, we can imagine a closed volume of diminishing smallness in which the density approaches a limiting value at a given place; by contrast, within the picture of discrete masses, we can imagine a sufficiently small closed volume containing only a few mass points, so that in this case it makes no sense to speak of a limiting value of the density.[60] The concept of continuously distributed masses corresponds to our direct sense impressions of sight and touch, but we cannot conclude from this that matter is actually continuous. Of the final division of matter we know nothing, and all we can do is form hypotheses about it to explain the properties of bodies, mechanical, thermal, and chemical.[61]

Helmholtz underscores the distinction between the concepts of discrete mass points and of continuously distributed masses by distinguishing between the concepts of "unordered" and "ordered" motions. In unordered motion, each molecule carries out its motion independently of that of its neighbors. In the case of continuously distributed masses, adjacent volume elements press against one another and cannot move independently, and the concept of unordered motion does not apply.

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[58] Hermann von Helmholtz, *Vorlesungen über theoretische Physik,* vol. 1, *Einleitung zu den Vorlesungen Über theoretische Physik,* ed. A. König and C. Runge (Leipzig: J. A. Barth, 1903), pt. 1, 1, 7, 10–11, 14–16. Helmholtz's introductory lectures were given at Berlin in 1893.

[59] Helmholtz, *Einleitung*, 21.

[60] Hermann von Helmholtz, *Vorlesungen über theoretische Physik,* vol. 2, *Vorlesungen über die Dynamik continuirlich verbreiteter Massen,* ed. Otto Krigar-Menzel (Leipzig: J. A. Barth, 1902), 1–2. The volumes of Helmholtz's lectures on the mechanics of mass points and of continuously distributed masses were based on lectures he gave at Berlin in 1893–94.

[61] Ibid., 2–3.

To illustrate the distinction, he invokes a homely image: he likens unordered motions to the motions of individual flies within a swarm and ordered motions to the change in shape and position of the whole swarm. In his lectures on elasticity theory, he is interested in the swarm only.[62]

The concept of unordered motion, which Helmholtz introduces in his lectures on the dynamics of continuously distributed masses but does not use there, is central to his lectures on the mechanical theory of heat. For heat is the "unordered motion" of the smallest particles of bodies; the motion, that is, of the flies, in which the velocity and displacement of any individual fly has no relation to those of its neighbors. Since we cannot describe the motions of individual molecules or mass points, we can only calculate their average values. On this understanding, Helmholtz deduces most of the properties of gases.[63]

In his lectures on the dynamics of discrete masses, Helmholtz deduces the conservation of energy principle from mechanical laws and from the assumption of conservative forces, but he points out that this derivation does not imply that the consequences of the principle belong only to mechanics. There exist various energy forms, and although efforts have been made to “reduce the nonmechanical energy forms through hypothetical ideas to the mechanical as the original forms,” the hypothetical ideas do not belong to the contents of the energy principle. The principle stands as an independent fact of experience: it is completely general and “governs all phenomena of nature.”[64] The energy principle is highly useful in solving dynamical problems, but it is an insufficient basis for carrying through the calculations, and other “comprehensive principles” must be considered. These include Hamilton’s least action principle, Helmholtz’s special area. His lectures include much discussion about this and about material from other researches of his.

The remaining volumes of Helmholtz’s lectures on theoretical physics treat acoustics, optics, and electrodynamics and magnetism. He presents acoustics as an application of ordinary mechanics, since it is the study of small oscillations in bodies under the action of conservative forces. He begins his optical lectures with the mechanical optical theory because of its “great historical interest” and the “enormous quantity of factual knowledge” expressed in its concepts and language, but the title for these lectures is “Electromagnetic Theory of Light,” not the customary “Optics.” He explains that Hertz’s experiments leave no doubt of the reality of the medium for electric oscillations and of their finite velocity of

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[62] Ibid., 7–8.

[63] Hermann von Helmholtz, *Vorlesungen über theoretische Physik,* vol 6, *Vorlesungen über die Theorie der Wärme*, ed. Franz Richarz (Leipzig: J. A. Barth, 1903), 256–58. This volume was compiled from his notebooks for the summer semester of 1890, from stenographic notes taken of his lectures in the summer semester of 1893, and from notes taken by the editor of the volume in the early 1880s.

[64] Helmholtz, *Dynamik discreter Masseenpunkte,* 231.

propagation: they "possess all objective properties of light oscillations," justifying Helmholtz's emphasis in these lectures on the electromagnetic theory.[65]

Helmholtz's lectures on electrodynamics and magnetism were the last to appear in print, in 1907. Probably because since the time he had delivered these lectures there had been a great development of the subject, this volume of lectures was not reviewed in the *Physikalische Zeitschrift,* as other volumes had been, but only acknowledged as the completion of a "lasting memorial of the great master."[66] Wien, one of Helmholtz's students, regarded the Berlin lectures on theoretical physics as a great accomplishment, not least because they were so completely Helmholtz's creation, free from previous models.[67]

Helmholtz's audience at Berlin came away from his course of lectures with the view of physics as a well-connected science, not as so many isolated explanations of phenomena. Physics was ordered by fundamental laws governing each of its parts, and the whole was connected by general laws of dynamics. "General," "universal," "comprehensive," and "invariant," these words stand out in Helmholtz's presentation of theoretical physics and give it its distinctive form. In this respect, Helmholtz's lectures and researches constitute a whole.

Max von Laue, formerly Planck's assistant in the institute for theoretical physics, said that more than most scientists Helmholtz was intellectually drawn to the poetry of Goethe, and he thought that Helmholtz's two volumes of popular lectures could be set beside the masterworks of German literature such as Goethe's. He called Helmholtz a "classicist [Klassiker]" of science, perhaps the last.[68] The published lectures on theoretical physics by Helmholtz – who next to Neumann was the last great German theoretical physicist to have lived entirely within the nineteenth century – might be taken as a "classic" of physics.[69] To a reviewer of Helmholtz's introductory lectures, which appeared in 1903, it seemed that a

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[65] Hermann von Helmholtz, *Vorlesungen über theoretische Physik*, vol. 5, *Vorlesungen über die elektromagnetische Theorie des Lichtes*, ed. Arthur König and Carl Runge (Hamburg and Leipzig, 1897), 14–16.

[66] Hermann von Helmholtz, *Vorlesungen über theoretische Physik*, vol. 4, *Vorlesungen über Elektrodynamik und Theorie des Magnetismus*, ed. O. Krigar-Menzel and M. Laue (Leipzig: J. A. Barth, 1907). Acknowledged by Emil Bose, in *Phys. Zs.* 9 (1908): 141.

[67] Wilhelm Wien, "Helmholtz als Physiker." *Naturwiss.* 9 (1921): 694–99, on 697.

[68] Max von Laue, "Über Hermann von Helmholtz," in *Forschen und Wirken. Festschrift zur 150-Jahr-Feier der Humboldt-Universität zu Berlin 1810-1960*, vol. 1 (Berlin: VEB Deutscher Verlag der Wissenschaften, 1960), 359–66, on 360. In Wilhelm Ostwald's division of scientists into two temperamental types, "Classical" and "Romantic," Helmholtz was classical.

[69] This is not just a play on words. Lewis Pyenson suggests that Boltzmann's and Mach's use of the word "classical" to describe traditional mechanics around 1890 would have been recognized at the time as having an affinity with the word appearing in "classical" philology, referring to the languages of ancient cultures, greatly admired in educated circles. This appears in Pyenson's discussion of neo-humanism and mathematics in German education in the late nineteenth century (*The Young Einstein: The Advent of Relativity* [Bristol and Boston: Adam Hilger, 1985], 175).

revolution in physics was underway, and he welcomed the lectures as a steadying hand from the past.[70] From the perspective of a quarter century after Helmholtz's death, Wien said that with him the "classical time of German physics has found its conclusion"; he was the "last German and one of the greatest representatives of a closed epoch in the history of physics, in which causality and logical construction built the firm foundations."[71]

Physics in the nineteenth century is known as "classical." Just how one characterizes classical physics depends partly on what one is looking for, and there are varieous opinions on what it was and when and how the period ended. The word "classical" was applied to physics by Boltzmann and others at the time. Individual works of physics were "classical" if they were especially significant, independently of the concepts they contained and the methods they used. More specific meanings were attached to "classical mechanics," "classical thermodynamics," and "classical electrodynamics," terms arising from arguments about the methods of physics. Following major conceptual changes in physics after the turn of the twentieth century, physicists began to speak of earlier physics as "classical physics" as distinguished from "modern physics." The conventional, historically rather misleading meaning of the terms "classical" and "modern" was more or less fixed at an international conference convened in Brussels to address the problems of quantum physics, the first Solvay Congress in 1911: physics before Planck's theory in 1900 was classical, physics after 1900 was modern.[72]

For most of the century, physics was largely ordered and partially unified within a framework characterized by a spare number of quantitative concepts, fundamental laws and forces, universal principles, and epistemological assumptions. By the end of the century, each branch of physics in its classical version was largely completed, and though each continued to be developed, it was not radically revised or extended. Classical physics contained a number of obstinate problems, but they were not seen as underminding its foundations beyond recovery. Only with the advantage of hindsight do we see that the roots of modern physics were present by the time that Helmholtz's lectures on theoretical physics began to be published in the 1890s.

Without deciding on one of the meanings of "classical physics," we can identify a principal feature of classical theoretical physics in a time that all parties accept as "classical." In acknowledgment of the mechanical foundations of physics,

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[70] Karl Böhm, "H. von Helmholtz, *Einleitung zu den Vorlesungen über theoretische Physik*," *Phys. Zs.* 5 (1904): 140–43.

[71] Wien, "Helmholtz als Physiker," 699.

[72] The association of the classical-modern distinction with the Solvay Congress has been proposed by Richard Staley. He points out that if it is meant to imply that physicists before 1900 were working in classical physics and physicists after 1900 were working in modern physics, it is a myth. There is a loose connection with the same terms, "classical" and "modern," appearing in other parts of culture, such as art, at the same time (*Einstein's Generation: The Origins of the Relativity Revolution* [Chicago and London: University of Chicago Press, 2008], 348–49, 353, 355, 422).

Neumann, Kirchhoff, and Helmholtz began their surveys with mechanics, and throughout their lectures they brought in mechanics in a number of ways. One way was to discuss phenomena using forces; another was to introduce hypothetical mechanisms or analogies; and another was to show that phenomena can be deduced from abstract general principles of mechanics such as the principle of least action.[73]

Modern physics is based on mechanics too, only a different mechanics, one which contains a universal constant unknown to classical mechanics, Planck's constant h. Modern physics accounts for processes at the atomic scale, in which action is found to change discretely by integral multiples of h. This universal constant entered physics in a theory we discuss later in this book, Planck's theory of blackbody radiation. It was the beginning of a development that would result in a new mechanics, quantum mechanics.

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[73] Peter M. Harman, *Energy, Force, and Matter: The Conceptual Development of Nineteenth-Century Physics* (Cambridge: Cambridge University Press, 1982), 9–10.

Chapter 13
Ordinary Professorships for Theoretical Physics

13.1 Göttingen Chair for Theoretical Physics

At Listing's death in 1882, Riecke temporarily took over as director of the Göttingen mathematical physics institute.[1] In the interest of instruction in physics, Riecke and the faculty urged the government to replace Listing as promptly as possible. Riecke had in mind a proper mathematical physicist, not another experimental physicist to share teaching duties. This preference was accepted by the Göttingen faculty, who recommended that the government try to hire Clausius to continue the Göttingen "tradition" established by Gauss, Dirichlet, and Riemann.[2] Realistically they did not have much hope of getting Clausius: he was expensive, Bonn would take steps to keep him, and the Göttingen mathematical physics institute had "completely inadequate quarters."[3] At one point in the negotiations, it seemed that Clausius was actually going to accept, but he refused in the end.[4] The government then turned to the faculty's second choice, Woldemar Voigt, who did not pose a financial problem, and he was available.

Letters from the faculty commission to Voigt characterized the position of mathematical physics at Göttingen. The mathematician H. A. Schwarz stressed the advantage for mathematical physics of the traditional mathematical strength at Göttingen, where Voigt would find over 130 well-prepared, hard-working students enrolled in mathematics. Riecke wrote that he especially welcomed Voigt since

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[1] Göttingen U. Curator to Riecke, 10 January 1883, Listing Personalakte, Göttingen UA, 4/V b/108.

[2] Riecke to Voigt, 11 January 1883, Voigt Papers, Göttingen, UB, Ms. Dept. Philosophical Faculty recommendation, drafted by Riecke, for the commission appointed to decide on Listing's replacement, 12 January 1883, and Göttingen U. Curator to Prussian Minister of Culture von Gossler, 15 January 1883, Voigt Personalakte, Göttingen UA, 4/V b/203.

[3] Göttingen U. Curator to Prussian Minister of Culture von Gossler, 15 January 1883, Voigt Personalakte, Göttingen UA, 4/V b/203.

[4] Riecke to Voigt, 4 April 1883, Voigt Papers, Göttingen UB, Ms. Dept.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_13

they would complement one another. He had been lecturing only on experimental physics, and Voigt would have the “whole area of mathematical physics” to himself, corresponding to his interest.[5] The difference between the experimental and the mathematical physics institutes would be real, no longer “merely nominal,” as it had been in the past, when Listing only formally, not actually, represented the entire field of mathematical physics. Riecke would retain control of the beginners’ exercises and the examination for students of medicine, pharmacy, and agriculture, but he would divide with Voigt the advanced exercises and examinations for secondary teachers and doctoral candidates. The budget of the experimental physics institute was more than twice that of the mathematical physics institute, but the experimental one paid for heat and lighting for the whole building, which the two institutes shared. Riecke assured Voigt that if they worked together, their institutes would compare in scientific significance with the better equipped ones.[6]

Voigt had studied under Neumann at Königsberg, and when Neumann ended his lectures in 1875, Voigt continued them as extraordinary professor for mathematical physics. He had expected in time to be promoted to ordinary professor for mathematical physics, but Neumann did not make way for him as his successor. Since few universities had two ordinary professors for physics, let alone two ordinary professors representing mathematical physics, Voigt’s expectations for a promotion at Königsberg were not based so much on precedent as on the government’s earlier acceptance in principle of a double institute, one for experimental physics and the other for mathematical physics. Conditions at Königsberg did not develop as he had hoped, so that when Göttingen offered him Listing’s ordinary professorship in 1883, he was receptive, even though the Königsberg faculty took steps to keep him.[7]

In August 1883 Voigt became Göttingen’s ordinary professor for theoretical physics and director of the mathematical-physics institute, and soon after this he was also appointed co-director of the physical department of the mathematical-physical seminar.[8] Unlike his predecessor Listing, Voigt was trained in theoretical physics and regarded it as his field; accordingly, his appointment at Göttingen was an important step in the establishment of theoretical physics in German universities.

The Göttingen faculty had a good idea of the man they hired. They knew Voigt as an “independent researcher in the area of mathematical physics,” who had attained “beautiful results” through the “connection of the theoretical and the experimental sides of physical research.” Furthermore, because Voigt’s research had centered on elasticity theory and optics, his direction would complete that of

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[5] Riecke to Voigt, 9 January 1883, Voigt Papers, Göttingen UB, Ms. Dept.

[6] Riecke to Voigt, 11 and 22 January 1883, Voigt Papers, Göttingen UB, Ms. Dept.

[7] Neumann to a Prussian government official (draft), n.d. [soon after 20 April 1883], Neumann Papers, Göttingen UB, Ms. Dept.

[8] Voigt’s salary was 4200 marks with an additional 540 marks for rent. Prussian Ministry to Göttingen U. Curator von Warnstedt, 3 September 1883; Voigt to Curator, 4 October 1883; Curator to Voigt, 9 October 1883; Voigt Personalakte, Göttingen UA, 4/V h/203.

physicists already working at Göttingen, and in this respect "none of the physicists who might be named could be regarded as equal to him."[9]

In April 1883, as he was negotiating for the Göttingen job, Voigt published his first major contribution to the elastic-solid theory of light,[10] linking the two subjects, elasticity and optics, which the Göttingen faculty had drawn attention to in their evaluation. This work is a good example of Voigt's preferred method of theoretical research, which was to draw mathematical consequences from a few general principles rather than from special pictures or mechanisms.[11] Theories of optics from the early nineteenth century were found to be unsatisfactory in certain respects. For one, the optical relations of matter could not be understood by a simple elastic-solid theory in which the only consideration is the influence of matter on the density or elasticity of the ether. Some physicists in the middle of the nineteenth century began to work on optical problems independently of a theory of the ether, while some began to consider the interaction of the ether and matter; around 1865 Neumann, for example, began to include simultaneous equations of motion for the ether and matter in his optical lectures. But it was only from the discovery of anomalous dispersion in 1870 that the theory of the mutual reaction of ether and matter was systematically developed. This part of optical theory was cultivated in Germany in the 1880s, and it was Voigt's starting point in his reformulation of the elastic-solid theory of light.[12]

13.2 Munich Chair for Theoretical Physics

When Kirchhoff died in 1887, the theoretical physicist of comparable standing was Boltzmann, who was then teaching physics at Graz in Austria with growing dissatisfaction. His lecture course on elementary physics attracted mainly students of medicine and pharmacy, providing him with little "stimulation or leisure for theoretical lectures," and Graz had few students in any case who were prepared for theoretical physics. Boltzmann also felt held back in his research by the "burden" of directing the physics institute. In contrast to the usual director who complained of

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[9] Göttingen Philosophical Faculty recommendation, 12 January 1883.

[10] Karl Försterling, "Woldemar Voigt zum hundertsten Geburtstage," *Naturwiss*. 38 (1951): 217–21, on 219; Woldemar Voigt, "Theorie des Lichtes für vollkommen durchsichtige Media," *Ann*. 19 (1883): 837–908.

[11] Försterling, "Voigt," 217–18.

[12] Glazebrook discusses primarily German work in the part of his report dealing with theories of the mutual reaction of ether and matter (R. T. Glazebrook, "Report on Optical Theories," in *Report of the Fifty-Fifth Meeting of the British Association for the Advancement of Science* [London: J. Murray, 1886], 157–261, on 212–33). Jed Z. Buchwald, *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century* (Chicago and London: University of Chicago Press, 1989), 309–10.

the limitations of his institute, Boltzmann found his institute too big, quite unsuited for conditions at Graz.[13] He was in a mood to leave, and so when Berlin offered him Kirchhoff's job, he accepted, or so it seemed. He asked about his teaching assignment and when it was to begin, and he even selected his room in the Berlin physics institute with Kundt's consent. But then he began to talk about his problems; he had eye trouble, he had bad nerves, and he was master of only parts of theoretical physics, not the whole, as Kirchhoff had been. He had private reservations about going to work in Berlin too, about living among Prussians, who seemed dour to him. In the end, he asked to be released from his commitment to Berlin, even though it meant giving up a salary nearly twice what he was paid at Graz.[14] As we have seen, the position went to Planck instead.

Hardly had Boltzmann informed Berlin that he was not coming than Munich approached him, offering him a job like Berlin's, an ordinary professorship for theoretical physics. Because it was a new position at Munich, and a nearly unheard-of position elsewhere, the Munich philosophical faculty had to justify it carefully. The first of their arguments, which were written up by the physics professor Lommel and the mathematics professor Gustav Bauer, was that a "progressive separation of theoretical from experimental physics" was taking place as a natural outcome of their "difference in methods," calling for a division of labor: "while experimental physics in its inductive work requires the knowledge and practice of an experimental art that becomes more and more intricate, theoretical physics uses mathematics in its deductive process as its main tool and demands intimate familiarity with all means of this quickly advancing science." With the rapid growth of physics, the argument continued, fewer and fewer physicists would be able to master both methods to the same perfection. This meant that physicists had no choice but to specialize in one or the other, and universities had to provide specialized chairs for both. Their second argument for the establishment of a chair of theoretical physics was that it would bring to Munich a physicist whose research belonged to theoretical physics. Several men already taught the subject at Munich, including an extraordinary professor who gave a year's course covering the entire subject, but a theoretical physics teacher was expected to do more than simply transmit knowledge of the subject to students. He needed to be an independent researcher who contributed "new truths" to theoretical physics, and Munich's

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[13] Boltzmann to a colleague [Lommel or Bauer] in Munich, 3 November 1889, Munich UA, E II-N, Boltzmann.

[14] Boltzmann to Althoff, 6 and 24 June 1888, and Boltzmann to Prussian Ministry of Culture, 24 June 1888, STPK, Darmst. Coll. 1913.51. In Berlin, Boltzmann's salary would have been 13,700 marks, or 8220 fl. In Graz, his salary was 4240 fl., which the Austrian government raised by 1000 fl. (Hans Schobesberger, "Die Geschichte des Physikalischen Institutes der Universität Graz in den Jahren von 1850–1890," 102. This 150-page manuscript is in the Graz U. Archive, "Hausarbeit aus dem Fach Geschichte.).

lecturers did not do this and were in any case more interested in experimental physics. The philosophical faculty wanted Boltzmann.[15]

Boltzmann's researches dealt almost exclusively with theoretical physics, the Munich faculty noted. Because of his outstanding talent for research together with his "most thorough mathematical education," he was able "to develop further and to supplement the theories of Maxwell, Clausius, and Helmholtz." His publications on elasticity, hydrodynamics, electricity, and, above all, the mechanical theory of heat and the kinetic theory of gases had earned him a reputation as one of the best theoretical physicists. The faculty singled out for praise his work on the mechanical interpretation of the second law of thermodynamics and on the relationship of that law to probability theory, on the theory of gas friction and diffusion, on heat conduction and equilibrium in gases, and on the nature and the velocity of gas molecules.[16] Boltzmann was eager for the new Munich position for the same reason that the Munich faculty wanted him: it brought into agreement the areas of his teaching and research.[17] By combining the salaries of two lapsed professorships, Munich could afford Boltzmann, and in August 1890 he became their "ordinary professor of theoretical physics."[18]

When early in 1893 Josef Stefan at Vienna University died, Austria tried to entice Boltzmann to return as his successor in the "ordinary chair of theoretical physics." To hold him in Munich, the Bavarian government gave him a substantial raise, a title, and an assistant. Boltzmann declined the Austrian offer for the present. He quietly continued to negotiate with the Austrian ministry of culture, which considered it a "question of honor" to bring back a native who was one of the "most outstanding representatives of the subject of theoretical physics." The ministry did not grant his request for a salary of 9000 florins (plus another 2000 florins in additional income), but it did offer him the highest salary then paid to any Austrian university professor, 6000 florins, and eventually it added enough income from other sources to bring the total up to just over 9000 florins anyway. During the negotiations with Boltzmann, Stefan's teaching duties had been assigned to an extraordinary professor for theoretical physics, Gottlieb Adler. When Adler died in the spring of 1894, Boltzmann and the Austrian government reached a final agreement, and Boltzmann took the occasion to break the news to the Bavarian government that he would be leaving. In July he obtained the official release from his duties at Munich, where he had

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[15] Dean von Baeyer of Section II of the Munich U. Philosophical Faculty to Munich U. Senate, 24 November 1889, Munich UA, E II-N, Boltzmann.

[16] Dean von Baeyer to Munich U. Senate, 24 November 1889.

[17] Boltzmann to a colleague [Lommel or Bauer], 3 November 1889.

[18] Boltzmann's salary was 7800 marks. Munich U. Senate to Bavarian Ministry of the Interior, 11 June 1890; letter of appointment by Luitpold, Prince of Bavaria, 6 July 1890, Munich UA, E II-N, Boltzmann.

taught theoretical physics for four years.[19] Boltzmann's position in theoretical physics at Munich remained vacant until well after the turn of the twentieth century.

13.3 Berlin Chair for Theoretical Physics

In 1872, the idea of a state research institute for technology was proposed. Helmholtz backed the idea, and when it was taken up, he advocated a scientific as well as a technical division, since "all serious scientific work must eventually find its practical application."[20] In 1888, he was appointed first president of the Physikalisch-Technische Reichsanstalt, a position created specifically for him, as Emil Du Bois-Reymond explained:

> There came a time when our great friend Werner von Siemens, with a huge donation that only he could afford, prepared the way for the founding of an imperial institute for physics and technology in Charlottenburg. Now we were not unaware that Siemens always regretted that Helmholtz had to devote a large share of his time and energy to his teaching duties instead of to the continuation of his incomparable researches, and we were also not unaware that he had intended the position of president of the institute for Helmholtz. His intention was for it to free him from all but scientific work, a situation that only a pure academic could imagine as ideal.[21]

With his new job at the Reichsanstalt, Helmholtz did not give up all teaching at the university, but he accepted a less demanding assignment tied to his retention of a salary from the Prussian Academy of Sciences. Kirchhoff's continuing serious illness left theoretical physics inadequately represented at the university, and the plan was for Helmholtz to lecture from one to three hours a week in the "area of theoretical physics" and otherwise be free of all academic duties. The imperial government gave its "revocable" permission after being assured that Helmholtz's teaching would not interfere with his duties at the Reichsanstalt, his proper job.[22]

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[19] Reports to the Austrian Ministry of Culture and Education by ministry officials on negotiations with Boltzmann, 14 July 1893, 3 December 1893, and 30 May 1894, including drafts of letters written to Boltzmann, and Boltzmann's reply, 26 December 1893, Öster. STA, 4 Phil, Physik, 1375/1849. Boltzmann to the Munich U. Rector, 9 May 1894, Munich UA, E II-N, Boltzmann. Dean Zittel to the Munich U. Philosophical Faculty, 21 May 1894, Munich UA, OCI 20. Boltzmann received his official dismissal on 14 July 1894.

[20] Leo Königsberger, *Hermann von Helmholtz*, trans. F. A. Welby (Oxford: Clarendon Press, 1906), 369–70.

[21] Emil du Bois-Reymond quoted in Königsberger, *Helmholtz*, vol. 2, 346.

[22] Prussian Minister of Culture Gossler to Otto von Bismarck, 20 May 1887, quoted in Leo Königsberger, *Hermann von Helmholt,* 3 vols. (Braunschweig: F. Vieweg, 1902–1903), vol. 2, 352–53. To make the job of presidency of the Reichsanstalt acceptable to Helmholtz, the imperial government had to offer him an income comparable to the one he had at the university. This meant that, as usual, the desired sum had to be brought together from here and there. How this was done and how Helmholtz's teaching assignment entered into it are discussed in David Cahan, "The Physikalisch-Technische Reichsanstalt: A Study in the Relations of Science, Technology and Industry in Imperial Germany (Ph.D. diss., Johns Hopkins University, 1980), 200–204.

During the negotiations over Helmholtz's move from the university and the Prussian payroll to the Reichsanstalt and the imperial payroll, officials were concerned about Helmholtz's student fees from his lectures. If the imperial government were to match exactly Helmholtz's income at the university, which would include student fees, he would then be paid twice for the same thing. The officials did not realize that the fees would be entirely inconsiderable because the enrollment would be so small.

Through Kirchhoff's and Helmholtz's efforts, theoretical physics was well established at Berlin University by the time Planck was hired in 1889 to head its new institute for theoretical physics. After three years as extraordinary professor, he was promoted to ordinary professor, his predecessor Kirchhoff's rank and the customary one of an institute director. In 1894 Helmholtz died, and Planck was no longer the theoretical physicist "beside Helmholtz," as Kundt had described him when he first came to Berlin.[23] Now *the* theoretical physicist in Berlin, he acquired an official responsibility for theoretical physics for all of Germany, replacing Helmholtz as the designated advisor on theoretical physics for the *Annalen der Physik*.[24] When in 1894 Kundt also died, he was replaced by another experimental physicist, Warburg, and so Planck's position as the university's principal theorist remained unchanged.

The budget for Planck's institute for theoretical physics was a miniscule fraction of that for the physics institute.[25] The main activity of the institute was teaching, which meant lecturing, assigning and correcting written exercises, and directing some advanced work. Enrollments in Planck's general lecture course showed a gradual increase from an average of fifty-five in 1896–1897 to an average of 135 in 1903–1904. The number of participants in exercises or independent work showed a comparable increase: from eighteen in 1890, to eighty-nine in 1900, to 143 in 1909. Planck's obligatory annual reports on the activities of the institute were terse. Besides enrollments, they stated the odd extra expense such as the repair and tuning of the institute's harmonium and the replacement from time to time of the assistant. By contrast, reports on the Berlin physics institute by Warburg typically contained two full pages of published dissertations and papers as well as impressive attendance figures. Warburg lectured to audiences of 300, and sometimes more wanted

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[23] Kundt to Graetz, n.d. [December 1888], Ms. Coll., DM, 1933 9/18.

[24] In 1895, in volume 54 of the *Annalen der Physik*, Planck's name replaced Helmholtz's on the masthead as editorial collaborator; the editors at this time were Gustav and Eilhard Wiedemann.

[25] In 1909 Planck's institute received 700 marks, while the physics institute received 26,174 marks (Max Lenz, *Geschichte der Königlichen Friedrich-Wilhelms-Universität zu Berlin*, 4 vols. in 5 [Halle a. d. S.: Buchhandlung des Waisenhauses, 1910–1918], vol. 3, 446).

to attend than the hall could hold, and his many teaching subordinates and assistants accommodated large groups of students working in the laboratory.[26]

In Warburg's institute, research students, along with Warburg himself, all but lived as one great "family,"[27] while in Planck's institute there was little family life to speak of. That suited Planck, who worked alone and expected his students to do the same. He did not encourage students to work closely with him under his supervision.[28] His reports for the years around the turn of the century mentioned no published dissertations, and by 1910 he counted only fifteen as having come out of his institute.[29]

Outside of the institute Planck had other duties, which he appealed to when warding off new demands on his time. When he declined to work on a proposed new physics journal, he explained that any time left over after his teaching and research was taken up in presiding over the Physical Society and in editing the *Annalen der Physik*.[30] When he declined to contribute an article to an encyclopedia, he explained that his years at Berlin University had taught him that after his lectures and "eternal meetings, examinations, and written reports," what time remained he needed for his own researches,[31] which soon were to include his theory of black-body radiation.

13.4 Leipzig Chair for Theoretical Physics

Paul Drude had been extraordinary professor for theoretical physics at Leipzig since 1894, and he expected to become ordinary professor for theoretical physics alongside the professor for experimental physics, Gustav Wiedemann. It did not work out that way. When Wiedemann stepped down in 1898, Boltzmann received a confidential letter from the Leipzig physical chemist Wilhelm Ostwald informing him about a prospective position for an ordinary professor of theoretical physics, and on the same day Ostwald met with the commission to draw up a list of candidates for the experimental physics position. Since "in principle" the Saxon government was ready to establish the theoretical professorship, Ostwald asked

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[26] Ibid., vol. 3, 446. This discussion is based largely on the annual reports by Planck on his institute in the series *Chronik der Königlichen Friedrich-Wilhelms-Universität zu Berlin*; the years considered here are 1896–1897 through 1903–1904.

[27] James Franck, "Emil Warburg zum Gedächtnis," *Naturwiss.* 19 (1931): 993–97, on 995–96.

[28] E. Lamlla's recollection, in Alfred Bertholet, et al., "Erinnerungen an Max Planck," *Phys. Bl.* 4 (1948): 161–74, on 173.

[29] Planck, "Das Institut für theoretische Physik," in Lenz, *Geschichte . . .Universität zu Berlin,* vol. 3, 276–78, on 277.

[30] Planck to Otto Lummer, 8 January 1898, Breslau UB., Lummer Nr. 219.

[31] Planck to Sommerfeld, 11 September 1899, Sommerfeld Papers, Ms. Coll., DM. The encyclopedia in question is the *Encyklopädie der mathematischen Wissenschaften*.

Boltzmann if he would accept it. Boltzmann said that he would if Leipzig met his needs, and he rejoiced that an ordinary professorship was about to be established for his "special science, theoretical physics," which was represented at that level at "so few universities in Germany." He made no secret of his discontent in Austria, where there were "far fewer students ready for scientific work" than in Germany, and where there were few scientific meetings and societies and little scientific stimulation.[32]

Several months later, Ostwald informed Boltzmann that Drude, whose presence at Leipzig ruled out the appointment of a second professor for his subject, wanted to become ordinary professor for theoretical physics, and he had influential support. The necessary condition for Boltzmann's call to Leipzig was Drude's call away, which had not yet happened, but Ostwald counted on Drude's rising reputation.[33] The confidence that leaders of German physics placed in Drude was signaled by their intention to appoint him editor of the *Annalen der Physik*. Drude's, and with it Boltzmann's, opportunity came soon in the form of an opening for a professor of physics at Giessen. In the spring of 1900, Ostwald wrote to Boltzmann that Drude was leaving, and that the Leipzig faculty wanted him and would press their choice on the ministry.[34]

Wiedemann's successor Otto Wiener and the Leipzig faculty characterized Boltzmann to the ministry as penetrating, powerful, original, rich in ideas, and one of the last of Germany's recent great theoretical physicists, joining Helmholtz, Kirchhoff, and Clausius. They argued that at fifty-six Boltzmann was still young enough to make Leipzig a brilliant center for physics and the foremost university for the field of theoretical physics. With Drude's imminent departure, the time had come for the ministry to transform the extraordinary professorship into an ordinary professorship for theoretical physics, having had expressed its willingness to do so almost two years before. Then Leipzig would be in line with other universities, the

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[32] Ostwald to Boltzmann, 9 December 1898; Boltzmann to Ostwald, 13 December 1898; Henriette Boltzmann to Ostwald, 29 April 1899; in Wilhelm Ostwald, *Aus dem wissenschaftlichen Briefwechsel Wilhelm Ostwalds*, vol. 1, *Briefwechsel mit Ludwig Boltzmann*, *Max Planck, Georg Helm und Josiah Willard Gibbs*, ed. Hans-Günther Körber (Berlin: Akademie-Verlag, 1961), 22–30. Boltzmann regretted his move to Vienna from Munich. His wife wrote to friends about his unhappiness. He wrote to his former Munich colleagues and to his future Leipzig ones about the inferior students in Vienna, to whom he could not teach higher theoretical physics. He told Ostwald he was also dissatisfied with political conditions in Austria. Insertion in the minutes of the meeting of section 2 of the Munich U. Philosophical Faculty, 30 April 1896, Munich UA, OCI 22. Wiener for the Leipzig U. Philosophical Faculty to the Saxon Ministry of Culture and Public Education, 12 March 1900; Wilhelm Ostwald, "Beibrief an den Minister in Sachen Boltzmann," 12 March 1900; Boltzmann Personalakte, Leipzig UA, PA 326. Henriette Boltzmann to Leo Königsberger's wife, 13 January 1895, STPK, Darmst. Coll. 1922.93.

[33] Ostwald to Boltzmann, 5 May 1899, in *Briefwechsel … Ostwalds*, vol. 1, 26.

[34] Ostwald to Boltzmann, 13 March 1900, in *Briefwechsel … Ostwalds*, vol. 1, 26.

faculty claimed, pointing to positions for theoretical physics alongside positions for experimental physics at Berlin and Vienna and even at intermediate and small universities such as Göttingen and Königsberg. The faculty recommended only Boltzmann for their position, since he was the "most important physicist in Germany and beyond." Moreover, he wanted to come to Leipzig, as Ostwald confirmed. Boltzmann was offered the job in 1900 and, as expected, he accepted it.[35]

Wiener all along had argued that Leipzig had to have an "institute for the theoretical physicist," for otherwise it could not attract anyone of significance. Hertz had not gone to Berlin as a theorist, Wiener pointed out to the ministry, mainly because he would have lacked an institute there. Wilhelm Wien said that he would never take a job as a theorist without an institute. Wiener's floor plans for a new physics institute building included a theoretical physics institute, which occupied parts of three stories, an impressive setting of its kind. Since the plans had been drawn up with Drude in mind, Wiener asked Boltzmann what he thought of them.[36] "Nice" and "suitable," Boltzmann replied, but unfortunately, he explained, he had had little experience with institutes for theoretical physics. As it turned out, he never taught in the institute, for he left Leipzig after two years, before the new building was completed. While he was there, he gave a comprehensive lecture course on theoretical physics, managing to get through most of the subject, beginning with his favorite part, analytical mechanics. He had capable students, even if they were not many, and he had good relations with Wiener, yet he was unhappy in Leipzig. He asked the Saxon government to release him, giving "reasons of health," but to Wiener later he found it hard to say just why he felt unhappy. Perhaps it was the marshy climate or the North-German Protestant customs or yet some other cause. In any event, he felt better after he had returned to Vienna, where he again took up a position as theoretical physicist in the university.[37]

Boltzmann's piece of mind was short-lived. Talented lecturer that he was, he dreaded that his memory would fail him in front of his class. That was only one of many self-doubts that plagued him, adding to a burden of physical illness and recurring depression. On a summer vacation from his teaching in Vienna, he committed suicide. It was 1906; he was sixty-two.[38]

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[35] Wiener to Dean Eduard Sievers of the Leipzig U. Philosophical Faculty, 10 March 1900; letter to the Saxon Ministry of Culture and Public Education, drafted by Wiener and signed by Ostwald, Wilhelm Wundt, Heinrich Bruns, Otto Hölder, Carl Neumann, and Sievers, 12 March 1900; Ostwald, "Beibrief," 12 March 1900; Ministry to Philosophical Faculty, 4 August 1900; Boltzmann Personalakte, Leipzig UA, PA 326.

[36] Ostwald, "Beibrief," 12 March 1900; Wiener to Boltzmann, 25 April and 6 May 1900.

[37] Wiener to Boltzmann, 6 May 1900; Boltzmann to Wiener, 3 January and 7 February 1903; Wiener Papers, Leipzig UB, Ms. Dept. Saxon Ministry of Culture and Public Education to Leipzig U. Philosophical Faculty, 4 June 1902, Boltzmann Personalakte, Leipzig UA, PA 326.

[38] Engelbert Broda, *Ludwig Boltzmann. Mensch, Physiker, Philosoph* (Vienna: F. Deuticke, 1955), 26; Martin J. Klein, *Paul Ehrenfest, The Making of a Theoretical Physicist.* (Amsterdam and London: North-Holland, 1970), vol. 1, 76.

The Dutch theoretical physicist H. A. Lorentz had met Boltzmann only a few times, so he felt unequipped to portray his “strongly marked, witty, and many-sided personality” as he would have wished. He was forced to rely on his publications for his account of him, but these were a fair substitute, since Boltzmann conveyed his “entire way of thinking and feeling” in his writings. In his early experimental work, he did not look for new phenomena in unknown areas, but made measurements related exclusively to theories. Lorentz said of him that “at the bottom of his heart he was a theorist; he loved to forcefully emphasize this, in earnest and in humor, and he never ceased to point to his life task as the furthering of the clarity and firmness of the foundations of theory.” “The idea, which fills my consciousness and actions, is the construction of theory,” Boltzmann had written, making Lorentz’s point. Boltzmann was a “passionate molecular theorist,” Voigt said, and all of his most important results were in that area. His “main life work” was the kinetic theory of gases, to which he devoted more than fifty publications over thirty years. He had an eagle eye for weaknesses in others’ theories, which led to frequent controversies. He was a memorable teacher, clear and lively. Like most theoretical physicists with a strong direction, he had few special students and directed little research.[39]

13.5 Königsberg Chair for Theoretical Physics

In the new Königsberg physics institute, built in 1884–1886, the mathematical and experimental physics professors were each given a wing, and because each wing was a complete institute, the plan of the building expressed something like equality between the two halves of physics. For a small university, Königsberg provided reasonable space for its theoretical physicist, who was now Voigt’s former assistant Paul Volkmann.[40] A graduate of Königsberg, Volkmann spent his entire career there, first as Privatdocent from 1882, then as extraordinary professor from 1886, and finally as ordinary professor in 1894.

Volkmann’s enrollments increased gradually. In the physics section of the Königsberg mathematical-physical seminar, he had no students in 1892, one student the following year, and two students the year after, which encouraged him to offer regular seminar exercises after a long interruption. In his laboratory course, he had twelve students in 1899, and three years later twenty-four. Physically and mentally, Volkmann told the curator, the direction of student laboratory work

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[39] H. A. Lorentz,“Ludwig Boltzmann,” *Verh. phys. Ges.* 9 (1907): 206–38, on 206–7; Theodore Des Coudres, “Ludwig Boltzmann.” *Verh. sächs. Ges. Wiss.* 85 (1906): 615–27, on 623–24; Woldemar Voigt, “Ludwig Boltzmann.” *Gött Nachr.*, 1907, 69–82, on 69–72, 81.

[40] This discussion of Volkmann’s institute is based in part on the annual report, *Kőniglichen Albertus-Universität zu Königsberg*. The years considered here are 1892–1893 through 1901–1902 (“Neubau des physikalischen Institutes in Königsberg i. Pr.,” *Centralblatt der Bauverwaltung* 7 [1887]: 13–14).

belonged to the most demanding teaching in the university. Problems had to be assigned to members of the class in such a way that the solution of each problem required instruments that could be chosen and arranged so as not to disturb the experiments of other students. The director could not concentrate his attention in the laboratory as he could in the lecture hall, since he had to attend to the students' various needs all at the same time. The laboratory class was divided among a series of rooms, including the corridors and stairway shafts, requiring the director and his assistant to run back and forth to see that everything was going right and to answer students' questions from every side of physics. They also had to see that the instruments were properly used, since damaged ones might require weeks to be repaired. They had to examine and, in many cases, recalculate the work of the students. All of this labor in the laboratory course, Volkmann said, had its basis in "the nature of the discipline of mathematical physics."[41]

Optics was Volkmann's favorite field, continuing a Königsberg tradition. Shortly after Hertz's experiments on electric waves, Volkmann published his lectures on theoretical optics, addressing the great question that physics had to decide, whether light is an elastic or an electromagnetic phenomenon. He wrote at length on the epistemology of the natural sciences, devoting nearly fifty pages to the subject in his published lectures on mechanics. He published little research, and he reported little from his institute.[42] Primarily a physics teacher and interpreter, Volkmann did not attract many physicists. The future theoretical physicist Arnold Sommerfeld, who attended classes in the Königsberg theoretical physics institute, looked not to Volkmann but to Volkmann's assistant of many years, Emil Wiechert, as his "highest model."[43]

13.6 The Place of Theoretical Physics Within German Physics in the 1890s

In the period we consider, German physicists who specialized in theoretical problems generally knew each other and what each was working on, a not misleading way of characterizing a research field. These specialists were relatively rare but it

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[41] Volkmann to Königsberg Curator, 14 July 1902, STPK, Darmst. Coll. 1923.16.

[42] In the ten years from 1892, Volkmann reported in the Königsberg *Chronik* only one publication from his institute. Paul Volkmann, *Einführung in das Studium der theoretischen Physik insbesondere in das der analytischen Mechanik mit einer Einleitung in die Theorie der physikalischen Erkenntniss* (Leipzig, B. G. Teubner, 1900); *Vorlesungen über die Theorie des Lichtes. Unter Rücksicht auf die elastische und die elektromagnetische Anschauung* (Leipzig, 1891).

[43] Arnold Sommerfeld, "Autobiographische Skizze," AHQP.

was not because theoretical physics was underappreciated by faculties and ministries. As we have seen, at Bonn Clausius received the highest salary of any professor, and at Vienna Boltzmann received the highest salary of any professor in Austria. From at least the mid-1890s, theoretical physics in Germany was insufficiently cultivated, according to Voigt, as if brilliant experimental results were possible without theoretical preparation. Asked for recommendations of young theoretical physicists for a position in 1899, Voigt could not come up with many names and fewer yet that he could feel enthusiastic about. The reason he gave was that "Kundt's purely experimental direction" had become the "standard in Germany." Young physicists scarcely valued "exact theory," and among them there were no theorists as good as Planck, Wien, Drude, and Sommerfeld.[44] In 1899, Voigt wrote to Sommerfeld, who was about to begin teaching engineers at the technical institute in Aachen, that he feared that Sommerfeld would be lost to theoretical physics. Three years later he wrote again to say that he rejoiced that Sommerfeld was still working in theoretical physics and that he expected him to "help reconquer for *us* the world position lost since Kirchhoff etc."[45] In a letter to Sommerfeld in 1898, Wien analyzed the state of German theoretical physics in terms similar to Voigt's:

> Theoretical physics in Germany lies as good as completely fallow. That ought to be a reason to help revive it, but the situation has already gotten to the point that even the need for theoretical physics disappears more and more. The reasons for this are, first, that physicists do almost nothing but pure experiments and have hardly any interest in theory and, second, that most mathematicians have turned to entirely abstract areas and do not concern themselves with applications. This reveals itself externally in that pure theoretical physics is taught from only two chairs (Berlin and Göttingen), and such an important chair as Munich has come entirely to an end. Theoretical physics currently finds no takers. Later it will all be different again, indeed, because otherwise physics would go completely to ruin; but I must make some allowance for trends and thoroughly busy myself with purely experimental researches as long as I still have to work for an external position.[46]

Wien was an extraordinary professor at Aachen waiting for his first call to a university professorship, and positions in physics and how to get them were much on his mind. It is a measure of the slow growth of theoretical physics as an independent field that of the ordinary professors at the end of the nineteenth century only Planck at Berlin, Voigt at Göttingen, Volkmann at Königsberg, and Des Coudres, who was mainly an experimentalist, at Leipzig had institutes. Moreover, theoretical physics could still lose ground by being reduced from an independent position to a secondary one, as had recently happened at Munich.

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[44] Voigt to a colleague, 6 June 1899, STPK, Darmst. Coll. 1923.54.

[45] Voigt to Sommerfeld, 3 December 1899 and 24 November 1902, Sommerfeld Correspondence, Ms. Coll., DM.

[46] Wien to Sommerfeld, 11 June 1898, Sommerfeld Correspondence, Ms. Coll., DM.

Below the level of chairs, many subordinate jobs in physics had been established in the universities from the 1870s, as we have seen. The teaching of theoretical physics was assigned to Privatdocenten and extraordinary professors, whose duty it was to supplement the teaching of the increasingly burdened main physics professors. In time, and gradually, there came to be an awareness of the need for a properly acknowledged field, as shown by Voigt's and Wien's dissatisfaction with the imbalance in German physics. By the end of the century, the existence of many subordinate jobs in theoretical physics was no longer sufficient.

We conclude our discussion with two tables: Table 13.1 summarizes the budgets of German physics institutes in the last decade of the nineteenth century; Table 13.2 lists the physics courses taught throughout Germany at this time. They show that when Boltzmann was teaching theoretical physics at Munich, the subject was everywhere accepted as a necessary part of physics instruction. They also show that it was still taught largely by subordinate teachers. Boltzmann's presence at Munich was conspicuous, and his subsequent absence there was equally conspicuous.

Tables 13.1 and 13.2

Table 13.1 Summary of the Budgets of Physics Institutes at German Universities and Technical Institutes in 1891

	Aachen T.	Berlin U.	Berlin T.	Bonn U.	Braunschweig T.	Breslau U.	Darmstadt T	Dresden T.	Erlangen U.	Freiburg U.	Giessen U.	Görringen U.	Greifswald U.	Halle U.
Budget:	3000		3000	4400	2710	3762	1430	2700	2450	2000		4910		1325+
Is it sufficient?			No	Yes	Yes	No	No	No	No			No		No
Are you expected to cover these costs?														
Small building repairs:														
Changes in gas and water lines:										Yes				
Building appliances, furniture:				Yes		Yes			Yes	Yes		Yes		
Gas, water, heat, cleaning:				Yes 2000										
Assistant and servant salaries:				460	1500	144								
Number of assistants:	1		1	1		2	1	2	1	1		2		1
Salary of assistants:	2300		1800	1200		1200 1200	1000		1250	1200		1200 1200		1200
Number of servants:	2		1	2	1	2	2	1½	1	1		2		1
Salary of servants:	1800 1350		1380	1200 960	1500	1500 900	1100 900		750	545 + f.l.q.		1100 600		600
Central heating:	Yes		Yes		Yes	Yes		Yes		Yes				Yes
Separate building:							i.p.			Yes		Yes		Yes
Professor for electrotechnology:	Yes		Yes		Yes		Yes	Yes						
Professor for theoretical physics:	e			e		e		e				Yes		Yes
Professor for physical chemistry:														
Attendance:[1]	797	4611	1640	1386	273	1342	316	403	1678	1138	562	831	834	1483
Adjusted attendance:	797	2305	1640	693	273	671	316	403	539	569	281	415	417	741
Adjusted budget:[2]	3000		3000	1740	1210	3418	1430	2700	2250	2500		4710		1325+
Budgets per too students:	1500		183	250	445	500	455	675	415	437		1110		178+
Adjusted number of assistants:[3]	3		2	2	1	3	2	4	1	1		3		2+
Adjusted number of servants:[4]	3		2	2	2	3	2	2	1½	2		2		2+

Hannover T.	Heidelberg U.	Jena U.	Karlsruhe T.	Kiel U.	königsberg U.	Leipzig U.	Marburg U.	Munich U.	Munich T.	Mönster A.	Rostock U.	Strassburg U.	Stuttgart T.	Töbingen U.	Wörzburg U.
1450	4000	4500	1100	3200	5270		4280	2143 (4000)	3100	1620		6000	2000	6600	
Yes	No	Yes	No	No	No		No	No	Yes?			Yes	No	No	
	Yes		Yes				Yes	Yes							
	Yes	Yes	Yes					Yes	Yes			Yes			
	Yes	Yes	Yes	Yes	Yes			Yes		Yes		Yes		Yes	
	Yes	Yes	Clean.	Yes	1260		1500					Water		2000	
	1800	1700			1950									yes	
1	1	1	1	2	1	2	2	1	2	1		3	1	1	
1350	1000	800 + f.l.q.	1200	1080 + f.l.q. 480	1200		1200 1200	1200		1200		1425 + f.l.q. 900 + f.l.q. 400	1820	1280	
1	1	1	2	1	2	2	1	2		1		3	1	1	
	800 + f.l.q.	900 + f.l.q.	1600 800	1280+ f.lq.	750 + f.l.q.		1000	1398 1398		1200		1800 + f.lq. 1350 + f.l.q. 1050	1930	1560	
Yes													Yes	Yes	
		Yes		Yes			Yes	i.p.	Yes	i.p.		Yes		Yes	
Yes									Yes				Yes		
		Yes	Yes	Yes			e	Yes + e				e		e	
						Yes									
580	1089	645	585	605	717	3242	952	3551	882	377	368	917	486	1393	1422
580	544	322	585	302	358	1621	476	1775	882	188	184	458	486	696	711
1450	−400	2400	400	1000	1860		2580	3400	2900	1420		5500	2000	1760	
250	−74	745	67	330	515		545	191	325	755		1120	470	253	
2	1	2	1	3	2	3	3	3	3	1		4	2	2	
2	1	1	2	1	2	2	1	2	1	1		3	2	2	

NOTES FOR TABLE 1 (*preceding pages*)

The attendance at the time of the survey. *2*. Budget minus operating expenses of the institute. *3*. Assistants plus number of remaining physics chairs. *4*. Servants plus 1 if there is central heating. Under salaries of assistants and servants, "f.l.q." stands for "free living quarters in the institute"; under professor for theoretical physics, "e" stands for "extraordinary professor"; and under separate building "i.p." stands for "in prospect." Under Erlangen University's central heating, there is an entry that reads, evidently, "Hausm.," which we have omitted from the table. The figures are in marks. We have not altered them in any way, not have we corrected Lehmann's arithmetic.

Otto Lehmann gathered this information by direct inquiries. Having recently moved to Karlsruhe Technical Institute, he wanted to improve the arrangements for physics; his survey showed, for example, that Karlsruhe ranked below the other schools in expenditure per student. The information is clearly incomplete: for Berlin, Giessen, and Leipzig Universities, Lehmann noted that the data were "obtainable only from the government"; for Greifswald University, "the institute is completely newly organized, request not yet approved"; for Rostock University, "the head of the institute is very ill and can't reply"; for Würzburg University, "no answer received." Lehmann also noted that for Halle University, "there exist two physics institutes, forgot to ask one of them"; and that for Dresden, where Lehmann had taught before moving to Karlsruhe, "data according to my recollection." The staff and budget details that are summarized in the table contain matters we discuss throughout this study, institute by institute, period by period. We have reproduced Lehmann's summary here to give an overall impression, such as a German physics professor could gain by going to some trouble, of physics institutes throughout Germany. The time, 1891, corresponds to our last survey of research in the *Annalen der Physik*. The table, dated by Lehmann 26 June 1891, is in Bad. GLA, 235/4168.

We make several observations about the line in the table for theoretical physics, our main subject. Two of the universities, Berlin and Giessen, for which the information of the table is incomplete, had extraordinary professors for theoretical physics in 1891. Königsberg also had one; its omission here, we suspect, owes to Lehmann's neglect to write to the second physics institute at Königsberg. The responses from Jena and Kiel of "yes" instead of "e" might be misunderstood; both universities had only extraordinary professors for theoretical physics at this time. Tübingen's response of "e" was premature; that year the Tübingen Senate proposed that their Privatdocent for theoretical physics be promoted to extraordinary professor, but they were refused, and the promotion did not go through until 1895. To sum up the survey as far as it concerns theoretical physics at universities: in 1891 twelve of Germany's twenty universities had either an extraordinary or an ordinary professor for theoretical physics or, in the case of Munich, both.

NOTES FOR TABLE 2 (*opposite pages*)

Announced for the summer semester 1892 and the winter semester 1892–1893. The numbers beside the teachers' names give the hours per week required by the course. The letter (*d*) indicates that the teacher is a Privatdocent, the letter (*c*) that he is an extraordinary professor, and no letter that he is an ordinary professor. Free courses are indicated by (*gr*) (Reprinted from Lexis, ed., *Die deutschen Universitäten* 2:164–65)

Table 13.2 Summary of physics courses at German universities in 1892–1893

Universitäten	Experimental-Physik	Theoretische Physik	Theorie der Elektricitat and des Magnetismus	Warmetheorie	Praktische Uebungen im Laboratorium: I. für Anfänger II. f.Geübtere	Bemerkungen
Berlin...	Kundt 5, König (e) 4·	v.Helmholtz4, Planck 4, Glan (d) 2, Rubens (d) 2.	Glan (d) 4.	Weinstein (d) 3.	Kundt I. 7. II. 39. Planck (Institut für. theoretische Physik) 2.	Ferner war angekündigt: Die elektromagnetische Theorie des Lichts: Berlin Wien (d) 2. — Kinetische Gastheorie: Berlin Pringsheim (d) 2; Freiburg Zehnder (d) 1; Giessen Fromme (e) 3. — Ueb. elektr. u. magnet. Messmethoden: Berlin Arons (d) 2; Freiburg Zehnder (d). — Hydrodynamik: Erlangen Knoblauch (d) 2.— Krystalloptik in mathemat. Behandlung: GöttingenPockels , (d) 2. — Verwendung d. Elektrieität in der Technik and Medicin: Halle Schtmidt (d) 2. — Induct, u. Dynamomasch.: Kiel Hagen(d)2. — Spektralanalyse u. ihre Verwendung Halle Schmidt (d) 1. — Physikal.-chemische Theorien: Heidelberg Horstmann (h) 2; Leipzig Le Blanc (d) 2. — Diffusion des Lichts: Jena Abbe (h) 3. — Photometrie: Kiel Weber (e) 17. — Interferenz u. Doppelbrechung des Lichts: Leipzig
Bonn...	Hertz 4.	Lorberg(e) 4.	[Lorberg(e) 4.]	Lorberg (e) 2 gr.	Hertz I.8, II.54.	
Breslau...	Meyer 6.	Dieteriei (e) 5.	—	[Dieterici (e) 4.]	Meyer 3 u. 6. Dieterici (e) 3 u. 6.	
Erlangen...	Wiedemann 5.	Ebert (d) 2.	[Ebert (d) 2.]	[Knoblauch (d) 2].	Wiedemann II. 40.	
Freiburg...	Warburg 5.	Warburg 2. Meyer (d) 2.	—	—	Warburg II.	
Giessen...	Himstedt 5.	[Fromme (e) 3.]	—	Fromme (e) 3.	Himstedt I. 12, II. täglich.	
Göttingen.	Riecke 4.	Voigt 5, Drude(d) 2.	[Drude (d) 2.]	—	Riecke IL 48 u. 4.	
Greifswald	Holtz (e) 4.	[Oberbeck 2.]	Oberbeck 4.	—	Oberbeck I. 6, II. täglich.	
Halle...	Knoblauch 4.	[Dorn 2.]	Dorn 4.	[Dorn 4.]	Dorn 6.	des Condres (d) 2; München Donle (d) 2. — Theorie des Mikroskops and seine Anwendung: Leipzig Ambronn (e) 2. — Photogrammetrie: Heidelberg Woif. — Grandzüge der Elektrostatik: Strassburg Hallwachs (d) 2.— Potentialtheorie: Heidelberg Eisenlohr; Jena. Auerbach 3 (s. Tab.); Kiel [Weber (e) 2]; Königsberg [Volkmann(e)4];München *Boltzmann* 3; Strassburg [Reye 4]; Würzburg Selling 4 (s. Tab.).
Heidelberg	Quineke 5.	Quineke 3, Eisenlohr (e) 4.	—	—	Quincke II. tüglich.	
Jena...	Winkelmann 5. Schaeffer(h) 4.	[Auerbach (e) 1.]	Auerbach (e) 3.	—	Winkelmann II. 48.	
Kiel...	Karsten 6.	[Weber (e) 2.]	—	Weber (e) 3.	Karsten Wober} 20	
Königsberg	Pape 5.	—	Volkmann. (e)4.	[Wichert(d) 1.]	Pape Volkmann (e).	
Leipzig...	Wiedemann 6.	[Des Coudres (d) 2.]	—	—	Wiedemann 39.	Die physikalisch - mathematischen Seminares s. u. — Ausserdem waren noch Uebungen, Repetitionen etc. angekundigt in Berlin (u. a. Praktischer Karsus für Mediciner: Kundt 3), Bonn, Göttingen, Halle, Erlangen, Giessen, Heidelberg, Jena, Leipzig, Strassburg, Tübingen, Würaburg.
Marburg...	Melde 5.	[Feussner(e) 4.]	s. theor. Physik.	Elsass (e) 2.	Melde 12. Feassner(e)12.	
München...	Lounel 5.	Grütz (d) 4.	Boltzmaun 4.	[Grätz (d) 4.]	Lommel Narr} 15.	
Münster...	Ketteler 4.	Hittorf 3 (gr.)	Ketteler 2 (gr.)	[Hittorf 3].	Ketteler 9.	Oeffentliche Vorlesungen in Berlin (5), Greifswald (1), Halle(2), Jena (2), Kiel (2), Königsberg (3), Marburg (3), Münster (2), Strassburg (2).
Rostock...	Mathiessen 5.	—	—	Münuich (d) 2.	Matthiessen24.	
Strassburg	Kohlrausch 5.	[Cohn (e) 3.]	Cohn (e) 3.	[Hallwachs(d) 2].	Kohlrausch I. 12, II. 39.	
Tübingen.	Braun 5.	[Waitz (e) 3.]	Waitz (e) 3.	[Waitz (e) 3.]	Braun I. 4, II. täglich.	
Würzburg.	Röntgen 5.	Heydweiller (d) 2.	Selling 4. Heydweiller (d) 2.	Geigel (d) 2.	Röutgen I. 10, II. täglich.	

Chapter 14
Physical Research in the *Annalen* and in the *Fortschritte*

Hertz's work on Maxwell's electromagnetic theory in 1888–1890 was instrumental in bringing about its acceptance in Germany. Because of its importance for what followed, we select those years as the basis for our next partial survey of research in the *Annalen der Physik*. In addition to the electromagnetic and related optical work surrounding Hertz's, we also look at work in molecular physics. The years following 1890 were to see important developments originating in electromagnetic theory and in molecular physics, leading to—as it came to be called—"modern" theoretical physics.

14.1 Contributors and Contents

In the 20 years between the time of this survey and that of the last one, the number of contributors in Germany to the *Annalen der Physik* increased substantially, by a third. Within the increased number, certain categories of contributors grew while others declined, a redistribution which reflected two trends. One was that under Poggendorff's successors, the journal was sustained almost exclusively by physicists.[1] It continued to publish, for example, occasional discussions of the eye and the ear and of nature outside the laboratory, of rainbows, sea waves, weather, and the like,[2] and even of the history of physics (making room for Eilhard Wiedemann's historical studies of early physics, which began in good physics fashion, "Our

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[1] In 1888–1890, the *Annalen* contained contributions by twenty-six ordinary physics professors, including one emeritus, together with seven extraordinary physics professors, and at least ninety Privatdocenten, assistants, students, and recent graduates in physics. There were only seven scientists in universities and technical institutes who did not have positions in physics.

[2] Because of the large number of references, we use the abbreviated form again: author, volume of the *Annalen*, and pages. On the eye: Ebert, vol. 33, 136–55; Wolf, vol. 33, 548–54; Geigel, vol. 34, 347–61; Brodhun, vol. 34, 897–918. On the ear: Voigt, vol. 40, 652–60; Preyer, vol. 38, 131–36. On the weather: Helmholtz, vol. 41, 641–62; Pulfrich, vol. 33, 194–208.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_14

knowledge … still is in a bad way"),[3] but the contributors to these border areas of physics were often themselves physicists. The other trend was an increasing predominance in the journal of work by physicists in universities and technical institutes. From outside these two institutions, there were only about half as many contributors as before, now mostly secondary school teachers with a handful from other occupations.[4]

Earlier the major authors of purely theoretical studies in the *Annalen* were Kirchhoff and Clausius, both of whom died just before the time of this survey. Weber was no longer active, and Bezold had moved from teaching physics to directing the meteorological institute in Berlin. Of the institute directors from the earlier survey, only Lommel was still publishing theoretical work in the *Annalen*. Hertz, Warburg, and other newer institute directors combined theory with experiment in their work.[5] To these should be added two physicists who published on theory and who were near the end of their careers: Hankel at Leipzig and Helmholtz, who had just moved to an administrative position at the Physikalisch-Technische Reichsanstalt, in Berlin.

Physicists publishing in the *Annalen* in 1888–1890 showed little tendancy to specialize in pure theory, with Planck the main exception.[6] Most of the new extraordinary professors for theoretical physics published in the *Annalen* in 1888–1890, and their research included some valuable purely theoretical papers.[7] Theoretical physics was also published by Privatdocenten, assistants, and others including several experimental physicists with strong theoretical ability.[8] As before, physicists published in journals other than the *Annalen* such as the *Journal für die reine und angewandte Mathematik*.[9]

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[3] Eilhard Wiedemann, vol. 39, 110–30, and more, following on the Arabic predecessors of the founders of modern physics.

[4] Along with twenty universities—only Münster Academy was unrepresented—eight technical institutes were represented by authors in the *Annalen* in 1888–1890. There were at least twenty secondary school teachers and administrators, and there were three technical school teachers. There were two employees from the Physikalisch-Technische Reichsanstalt in Berlin and two more from the Hamburg state physics laboratory. The remaining dozen contributors no doubt included some unaffiliated persons.

[5] Besides Hertz and Warburg, they included Voigt, Braun, and Oberbeck.

[6] Lorberg, too, was doing purely theoretical research at this time.

[7] Besides Planck, they included Cohn, Volkmann, Lorberg, and, for the theory of instruments, Felix Auerbach. Other extraordinary professors for theoretical physics, such as Ketteler and Narr, published experimental work with theoretical discussions.

[8] Drude was foremost among them, but also Otto Wiener, Walter König, and—though he was soon to turn to technical physics—August Föppl.

[9] The editors of the *Journal*, Leopold Kronecker and Karl Weierstrass, on the occasion of the hundredth volume of the journal in 1887, noted that throughout its history it had published work by "many of the most significant. . .mathematicians and mathematical physicists." In the past they had included Gauss, Dirichlet, and Riemann. This, the hundredth, volume contained work by Helmholtz and Boltzmann as well as by Kronecker, Kummer, and other leading mathematicians, both German and foreign. "Vorwort zum hundertsten Bande," *Journ. f. d. reine u. angewandte Math.* 100 (1887): v–vi.

A contributor to the *Annalen* in 1888–1890 observed that physics brought the theoretical and the exact observational work characteristic of mathematics and astronomy together with the experimental work characteristic of chemistry.[10] It was an accurate statement: most physical research in the *Annalen* was experimental, often accompanied by theoretical and mathematical discussion. Now as in the past, experimental papers frequently contained separate discussions entitled "Mathematical Treatment," "Comparison between Theory and Observation," "Theoretical Investigation," "Theoretical Observations," "Theoretical Part," or simply "Theory."[11] In these discussions, laws and concepts were examined, criticized, and developed in light of experimental work. They included discussions of physical constants, the inexhaustible common ground of theory and experiment: with improved physical cabinets, physicists measured and re-measured indices of refraction and the theoretical molecular constants determining them, elastic constants of solids, light absorption constants of crystals, magnetic constants of isotropic and crystalline bodies, metal optical constants, and the constant relating electromagnetic to electrostatic units, among other constants.[12]

Occasionally, a theoretical discussion in the *Annalen* was purely qualitative; it was in Braun's theory of change of state and in Lehman's theory of crystals, which was illustrated by 100 or so polarization pictures.[13] Occasionally, too, it was presented with the aid of geometrical or graphical methods to make the calculations more visual.[14] But most often it was presented in analytical form, reminiscent of early French mathematical physics.[15]

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[10] Eilhard Wiedemann, vol. 39, 110–30, on 110–11.

[11] Wiedeburg, vol. 41, 675–711; Messerschmitt, vol. 34, 867–96; Galitzine, vol. 41, 770–800; Eilhard Wiedemann and Ebert, vol. 35, 209–64; Pockels, vol. 37, 144–72, 269–305; Karl E. Franz Schmidt, vol. 33, 534–48; and others.

[12] Ketteler, vol. 33, 353–81, 506–34; Wesendonck, vol. 35, 121–25; Voigt, vol. 36, 743–59 and vol. 39, 412–31; Drude, vol. 40, 665–80; Henri du Bois, vol. 35, 137–67; and many more places; Kundt, vol. 34, 469–89; Himstedt, vol. 33, 1–12 and vol. 35, 126–36.

[13] Braun, vol. 33, 337–53; Lehmann, vol. 40, 401–23, gave a few temperatures, but nothing more in the way of numbers.

[14] Following Kundt's example, Wiener treated the combined phenomena of double refraction and circular polarization by "geometrical" methods, imparting to the theory greater "Anschaulichkeit" (vol. 35, 1–24). Similarly, by a "graphical representation," Auerbach made a calculation in the theory of an instrument, an air pump, "anschaulich" (vol. 41, 364–68).

[15] German physicists in 1890 still discussed, criticized, and developed the work of Fresnel, Poisson, Cauchy, and other French mathematical physicists.

14.2 Molecular Work

Much physical research appearing in the *Annalen* in 1888–1890, as in the time of the previous survey, was guided by molecular reasoning. From Maxwell's theory of gases, German physicists adapted the concept of "relaxation time" to their electrical studies, and in general they drew on work on the theory of gases by Maxwell, Clausius, Boltzmann, Meyer, and others.[16]

Hertz along with Hittorf and several others made quantitative studies of electric conduction in gases, without arriving at a law for it. Since gaseous behavior is relatively simple, they were puzzled why they had not been able to describe conduction in thin gases with the same exactness as Ohm's law describes conduction in metals and electrolytes. Several physicists offered solutions to the problem, "still full of riddles," which included "mechanical theories," "ether theories," and an explanation based on the kinetic theory of gases together with the laws of electrostatics.[17] Molecular ideas entered the explanations in one way or another. Narr thought that physicists needed more experimental facts, since they were still far from an "approximately clear mechanical idea of the passage of electricity through gases."[18]

Other instances of electric conduction received their explanations and laws. Wiedemann explained Hertz's discovery of the action of ultraviolet light on an electrode by the passage of energy from the light to the molecules of the electrode and from them to the molecules of the surrounding gas, setting up a convective current. Hankel explained the main laws of galvanic currents—Ohm's law, Kirchhoff's laws of currents, and Joule's law of the heat of currents—by the electric state of material molecules, which consists of a rotary oscillation on the surfaces of the molecules. Graetz explained electrolytically conducting solids by extending Clausius's explanation of electrolytically conducting solutions, based on the assumption of freely moving charged molecules or partial molecules.[19]

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[16] Cohn and Arons, vol. 33, 13–31; Graetz, vol. 34, 25–39; Schleiermacher, vol. 34, 623–46; Föppl, vol. 34, 222–40; Galitzine, vol. 41, 770–800; Eilhard Wiedemann, vol. 37, 177–248; Wüllner, vol. 34, 647–61; Ebert, vol. 36, 466–73. The most sustained work on the kinetic theory of gases came from abroad: Ladislaus Natanson's series of purely theoretical, highly mathematical studies (vol. 33, 683–701; vol. 34, 970–80, etc.).

[17] Narr, vol. 33, 298. To explain electric conduction in gases, Föppl preferred the earlier mechanical theories of Gustav Wiedemann and Rühlmann to the ether theories of Erik Edlund, who identified the "extra-molecular ether" as the substratum of the current, and of Eilhard Wiedemann, who explained the current by the electrical deformation of ether shells surrounding molecules. The difficulty with Föppl's kinetic theory was that it did not say how two electrified molecules behave upon collision (vol. 34, 222–40).

[18] Narr, vol. 33, 295–301, on 298.

[19] Eilhard Wiedemann's "Theoretical Observations" at the end of his and Ebert's study of electric discharges (vol. 35, 209–64, on 255–64); Wilhelm Hankel, vol. 39, 369–89; Graetz, vol. 40, 18–35; Planck, vol. 34, 139–54, vol. 39, 161–86, and vol. 40, 561–76.

Physicists working in 1888–1890 did not believe that the sharply distinguishable forms of bodies in the phenomenal world necessarily have a correspondence in the molecular world. Bodies that appear to be solid, for example, may contain Graetz's "fluid" molecules or Lehmann's "liquid" crystals. The essence of crystals, for Lehmann, is not the geometrical arrangement of molecules determined by elastic forces, for that arrangement can be destroyed by external pressure without in the least affecting the crystalline nature of the bodies; rather the structure of the physical molecule explains crystal properties such as double refraction. For Voigt, crystals with directional properties and solids with identical properties in all directions may have a common molecular basis; to explain the empirical failure of Poisson's theory of isotropic bodies, Voigt called on his understanding of crystals, according to which their molecules have a certain polarity.[20]

In their optical researches, German physicists continued to apply molecular reasoning to ponderable matter and the ether. A common idea was that oscillations of ether particles cause molecules of bodies to oscillate in the same direction and with the same frequency, an instance, Wiener said, of the "striving of physics to learn to conceive of all phenomena as processes of motion." Since different types of motion are associated with different types of differential equations, Ebert reasoned that equations, even if they cannot describe spectra quantitatively, can suggest the molecular motions responsible for the observed line, band, and continuous spectra.[21] The explanation of spectra, according to Wiedemann, requires consideration of the various internal and external motions attributed to gas molecules by "new views" of the constitution of matter.[22] Physicists called upon different assumptions of the "molecular theory of radiation" to defend different views of the origin of the spectra of gases.[23] After experimentally deciding between conflicting laws of the index of refraction of compressed water, Zehnder interpreted the correct law by a molecular picture: when water is compressed, some of the less dense ether is forced out of the shells of ether surrounding water molecules, changing the index of refraction in the right direction.[24] The motion of light in bodies, the appearance of light in fluorescence and phosphorescence, and the production of light by thermal, electrical, chemical, frictional, and other processes all found interpretations within a broad molecular conception of matter and ether.[25]

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[20] Graetz, vol. 40, 18–35; Lehmann, vol. 40, 401–23; Voigt, vol. 38, 573–87.

[21] Ebert, vol. 34, 39–90.

[22] Eilhard Wiedemann, vol. 37, 177–248, on 178–79, decided that it is the oscillatory motions within gas molecules and not their rotations and translations that produce light.

[23] Ebert, vol. 34, 39–90, on 89, and Wüllner, vol. 34, 647–61, criticized one another's molecular interpretations. Wüllner criticized Kayser's view that certain molecules produce line spectra and certain others produce band spectra; his own view was that the transition between the two kinds of spectra is gradual (vol. 38, 619–40).

[24] Wiener, vol. 40, 203–43, on 241–42; Zehnder, vol. 34, 91–121, on 117–21.

[25] Voigt, vol. 35, 370–96, 524–51; Eilhard Wiedemann, vol. 34, 446–63; Wüllner, vol. 34, 647–61; Ketteler, vol. 33, 353–81, 506–34.

Physicists could not see molecules directly, but they could draw conclusions about the laws that govern them. They concluded, for example, that the same Doppler principle that applies to the greatest bodies in the universe applies as well to individual luminous molecules at the opposite end of the scale of nature.[26] By assuming, for example, that molecular forces together with the laws describing their effects are similar to what they were familiar with, they could measure their extraordinarily short range.[27]

14.3 Optical Theories

When physicists discussed optical theory in the *Annalen* in 1888–1890, it was often in connection with Neumann's theory from the 1830s and Kirchhoff's and Voigt's extensions of it. Neumann's theory merited attention, Voigt said, because it alone was supported "by the consistent connection to the laws of elasticity theory and by the use of unassailable fundamental laws of general mechanics."[28] Other physicists took Fresnel's side in an ongoing debate.[29] Wiener put forward new experiments on standing light waves as evidence for Fresnel's assumptions and against Neumann's, while others assumed that the decision in favor of Fresnel had already been made, citing a variety of phenomena in support: Newton's colored rings, Fresnel's experiment with three mirrors, phase changes with reflection of polarized light, crystal florescence, optical properties of moving bodies, and more. These same phenomena were rejected as supports for Fresnel's theory by Voigt and Drude. In fairness, Voigt said, both theories should be used and both named, and Pockels and Volkmann spoke of the "Fresnel-Neumann" laws for the propagation of light.[30] A challenge to both theories based upon J. C. Jamin's experiments with polarized light was answered by Voigt.[31] The supporters of Neumann's theory mentioned here were connected

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[26] Ebert, vol. 36, 466–73.

[27] From experiments on the thickness of oil on water, Sohncke calculated the radius of action of molecular forces to be equal to or greater than 55.75 $\mu\mu$ (vol. 40, 345–55, on 354). From Dorpat, the *Annalen* received an article containing the law of force between pairs of molecules, m_1m_2/r^2, which showed that the "same law that rules the macrocosm has also proven valid for the microcosm" (Bohl, vol. 36, 334–46, on 346). It was usual to assume a more complex and uncertain law in analyzing processes on the molecular level. Ebert, for example, wrote the force producing motions responsible for line spectra as $d^2x/dt^2 = f(x, a, b, c, \ldots)$, where $a, b, c, \ldots$ are constants that depend on the properties of the radiating molecule (vol. 34, 39–90, on 89–90).

[28] Voigt, vol. 35, 76–100, on 100.

[29] For Fresnel, we recall, the density of the ether changes in bodies, and for Neumann its elasticity changes; for Fresnel the oscillation of polarized light is perpendicular to the plane of polarization, and for Neumann it is parallel to the plane.

[30] Wiener, vol. 40, 203–43, on 240; Drude, vol. 41, 154–60; Voigt, vol. 35, 76–100, on 100; Volkmann, vol. 35, 354–60, on 355; Pockels, vol. 37, 144–72, on 151, and 269–305.

[31] It had been argued that Jamin's experiments on the reflection of polarized light invalidated the theories of light of Neumann, Voigt, and Fresnel; Voigt explained the departure from theory by a surface layer on the reflecting body (vol. 31 [1887], 326–31), and Drude (vol. 36, 532–60, 865–97) and Karl Schmidt (vol. 37, 353–71) examined this explanation.

through their training: Kirchhoff, Voigt, and Volkmann were Neumann's students, and Drude and Pockels were Voigt's students.

The foundations of optical theory were disputed. With reference to developments since Neumann's theory, Volkmann examined the mechanical interpretation of optics: he was able to reduce one part of the subject, the motion of light in unbounded transparent media, to elasticity theory "rigorously in the sense of mechanics," but he could not do the same for the problem of reflection and refraction, in which case it was necessary to leave the "mathematically rigorous basis" of "pure elasticity theory." To develop all of optical theory from "pure mechanics," he concluded, another basis had to be found; Kirchhoff, for example, sought it in the interaction of the particles of ether and matter.[32]

Because the electromagnetic theory posed a challenge to all mechanical theories of light, physicists examined the relationship between the two sets of foundations.[33] Old problems such as the ether drag by moving bodies were treated by Voigt within the framework of the mechanical theories and their disagreements,[34] while others treated optical problems from the electromagnetic side.[35] The controversy between Neumann's and Fresnel's theories appeared to be clarified by the electromagnetic theory: if light consists of an electric oscillation and a magnetic oscillation at right angles to one another, the question of the direction of the light oscillation loses its meaning.[36] For physics generally, the establishment of Maxwell's electromagnetic theory was the main task, and to this end optics was looked to for confirmation or refutation of the assumptions of the theory; we see examples in the next section.

14.4 Electrodynamics

The *Annalen* in 1880–1890 saw a continuation of the controversies of electrodynamics, which threatened to prove as irresolvable as Neumann and Fresnel's in optics.[37] The same issue of the journal contained both Hankel's proof that the fundamental electrodynamic law is a "point law" like gravitation and Hertz's recognition that his experiments are best explained by the Faraday-Maxwell view

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[32] Volkmann, vol. 35, 354–60, on 354–55, 359–60.

[33] Wiener, vol. 40, 203–43; König, vol. 37, 651–65; Geigel, vol. 38, 587–618; Drude, vol. 39, 481–554; and, from abroad, Koláček, vol. 34, 673–711.

[34] Voigt, vol. 35, 370–96, 524–51.

[35] Des Coudres, vol. 38, 71–79; Röntgen, vol. 35, 264–70.

[36] Koláček, vol. 34, 673–711; Drude, vol. 41, 154–60, on 154.

[37] For example, in connection with the long-studied phenomena of induction by the rotation of a magnet around its axis, Lorberg discussed the contending laws: Weber's, Edlund's, Clausius's, Riemann's, and Maxwell's (vol. 36, 671–92).

of electric forces as contiguous action.[38] The results of different tests of Maxwell's electromagnetic theory were inconsistent; in varying degrees, they confirmed the theory, disconfirmed it, or left the question open. Experiments on the transparency of thin metal sheets contradicted Maxwell's theoretical relationship between the optical transparency and the electrical conductivity of a body.[39] According to another result, the theoretical relationship between the dielectric constant and the index of refraction was confirmed for insulators and poor conductors but not even approximately for conducting fluids.[40] According to another, optical observations of crystals were "a proof of Maxwell's theory."[41] Experiments on electrical residues were inconclusive,[42] as were experiments on the capacity of a condenser, and it was not always possible to derive mathematical conclusions from Maxwell's theory that differed from those of other theories, even if their different physical assumptions might suggest them.[43] Moreover, Maxwell's was not the only electromagnetic theory of light.[44] A physicist reading the journal from cover to cover in these years might well have been unclear about the direction that electrodynamics would take in the near future.[45]

Incompletely tested as it was, by the late 1880s, Maxwell's theory had drawn wide notice in Germany, as it had not 20 years earlier when Maxwell's own papers were its difficult source and Maxwell's *Treatise* and its German translation did not yet exist. Most of the notices of Maxwell's theory in the *Annalen* came from physicists who were at the start of their careers, still Privatdocenten or assistants or even only advanced students. The few established physicists among them were in

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[38] Hankel discussed the ether theory he had worked on since the 1860s, according to which all electrical phenomena are oscillations (vol. 36, 73–93).

[39] Wien, vol. 35, 48–62. Drude did not count recent experiments on metal optics reported in the *Annalen* as refutation of the electromagnetic theory of light, since the optical constants for waves of the length of electric waves were all too uncertain (Drude, vol. 39, 481–554, on 553–54).

[40] Cohn and Arons, vol. 33, 13–31, on 23.

[41] Geigel, vol. 38, 587–618, on 610–11.

[42] Wüllner, in his study of electric residues, vol. 32 (1887), 19–53, doubted that the values of the dielectric constants obtained so far agreed with the Faraday-Maxwell theory. Arons regarded the accurate determination of these constants as difficult but not impossible (vol. 35, 291–311, on 307).

[43] At first, Himstedt believed that his experiments on a "Schutzring-Condensator" showed that Maxwell's capacity formula was correct and Kirchhoff's "false" (vol. 35, 126–36, on 129–30). Soon he acknowledged his error: Kirchhoff's formula was as good as Maxwell's (vol. 36, 759–61).

[44] A work on the refractive indices of fluids discussed the formula relating the index and the density from the side of the "electromagnetic theory of light," mentioning not Maxwell but Ludwig Lorenz, who independently developed an electrical theory of light, and H. A. Lorentz and Koláček (Ketteler, vol. 33, 353–81, 506–34, on 354–55).

[45] In any case, the older methods might still be seen to have advantages. Following Hertz, the Austrian physicist Stefan studied high-frequency oscillations in straight conductors, this time presenting the problem of the distribution of a varying current in a straight conductor of circular cross section "not from Maxwell's theory of the electromagnetic field but from the formulas that F. Neumann and W. Weber have laid down for the electrodynamic potential of two current elements" (vol. 41, 400–420, on 406).

their early or mid careers. Cohn, an extraordinary professor for theoretical physics, was engaged in a systematic formulation of Maxwell's theory. Röntgen and Himstedt, both institute directors, were working on experiments to confirm the action of a dielectric moving in an electric field, an effect anticipated by both Maxwell's theory of dielectric displacement and the theory of the dielectric as polarized particles; in addition, Röntgen was trying experiments from the view of the light ether as the medium of electric forces.[46] But it was only Hertz who applied the resources of his institute single-mindedly to a set of experimental problems central to the claims of Maxwell's theory. In contrast to the confusing evidence for and against the theory in the *Annalen*, Hertz's experimental work came to be seen as the decisive confirmation of the finite propagation of electric forces in air, and his theoretical work clarified these forces and the equations describing their action.

The early responses to Hertz's experiments divided into two sorts. One was a widespread interest in a discovery he made in 1887 incidental to his researches on electric waves, an action of ultraviolet light on an electrode or on the surrounding air, facilitating an electric discharge; physicists explained it as best they could, repeating and varying Hertz's experiments.[47] The other and more widespread interest was directed to the main objective of Hertz's experiments. Boltzmann demonstrated electric waves in a great auditorium before 200 viewers; Emil Wiechert demonstrated them by a similar method; and on Kundt's suggestion, Robert Ritter demonstrated them in a long passageway in the cellar of the Berlin physics institute using an ingenious modification for making them visible to a large number of viewers.[48] Variants of the experiments were tried and reported in the *Annalen*.[49]

Hertz's experiments inspired others to try original ones. Since he made the "identity of electric waves and light waves" no longer only the "results of mathematical developments but as the object of the most direct perception," the contradiction between the optical transparency and the good electrical conductivity of electrolytes appeared serious, and a reader of the *Annalen* did experiments to clarify it.[50] Hertz's demonstration of the finite propagation of electric actions inspired another reader to revive experiments of his own on the detection by electrical means of the motion of the Earth through the ether. To another reader, Hertz's production

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[46] Cohn, vol. 40, 625–39; Röntgen, vol. 35, 264–70; Himstedt, vol. 38, 560–73 and vol. 40, 720–26.

[47] Eilhard Wiedemann and Ebert, vol. 33, 241–64 and vol. 35, 209–64; Hallwachs, vol. 33, 301–12; Narr, vol. 34, 712–19; Lenard and Wolf, vol. 37, 443–56; Elster and Geitel, vol. 38, 497–514; and, from abroad, Arrhenius, vol. 33, 638–43.

[48] Boltzmann, vol. 40, 399–400; Wiechert, vol. 40, 640–41; Ritter, vol. 40, 53–54, demonstrated the oscillations in the detector of the electric waves by convulsions of a frog's leg.

[49] Classen, vol. 39, 647–48; Rubens and Ritter, vol. 40, 55–73; Waitz, vol. 41, 435–47; Elsas, vol. 41, 833–49.

[50] Cohn, vol. 38, 217–22, on 218.

of standing electric waves by combining incident and reflected waves suggested a method for producing standing light waves for the first time.[51]

Following Hertz's experimental and theoretical researches as they appeared and, in many cases, constructing and handling apparatus like his and reproducing his results, German physicists became familiar with electrical phenomena according to Maxwell's teaching. Even if they had studied Maxwell independently, after Hertz they had a guide through the complexities of the theory and a demonstration of its usefulness in exploring new phenomena.

To many German physicists, especially those at the start of their careers who were settling on a direction of research, Hertz's work pointed to Maxwell's theory as a source of rewarding problems. Drude, who discussed his favorite subject optics with an awareness of Maxwell's work in the *Annalen* in 1890, made an early reputation through his experimental study of the optical properties of Hertzian electric waves; later, as director of Germany's largest physics institute at Berlin, he continued his research on electric waves. His successor at Berlin, Heinrich Rubens, who reported quantitative experiments on Hertzian waves in the *Annalen* in 1890, directed his research at the start of his career to the study of heat radiation from the electromagnetic point of view, experimentally bridging the gap between optical and electric waves, which Hertz's experiments had made a central problem for physics. Several more future institute directors responded to Maxwell and Hertz in the *Annalen* in 1888–1890.[52] Several other physicists who wrote about about Maxwell's and Hertz's work in the *Annalen* in those years—Cohn, Drude, Főppl, and Boltzmann—introduced Maxwell's theory to German readers through textbooks, which they brought out between 1891 and 1900. The lesson of Hertz's work for readers of the *Annalen* was more than the correctness of Maxwell's theory; it was as well the indispensability of good theoretical understanding for experimental work and conversely.

14.5 Divisions of Physical Knowledge: *Fortschritte der Physik*

New physical fields were organized, representing both a furthering of specialization and, by bridging the more established fields, a corrective to it. Universities offered courses in and created positions and institutes for meteorology, geophysics, astrophysics, and physical chemistry, and physicists worked on problems originating in these fields. Examining the Earth's atmosphere from the standpoint of hydrodynamics, Helmholtz made fundamental studies in meteorology, a science characterized as the "physics of the atmosphere." Bezold, who moved from a position in physics to the first professorship in meteorology in Germany, strove to make the

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[51] Des Coudres, vol. 38, 71–79, on 72; Wiener, vol. 40, 203–43.

[52] They included Lenard, Des Coudres, König, Wiechert, and Wiener.

physics of the atmosphere an exact science through the application of thermodynamics. For years, Dorn collected observations made in Königsberg on the temperature of the Earth. Leonhard Weber built meteorological instruments, collected weather information, did research on the upper atmosphere, trained meteorologists, and, in general, directed his research toward meteorology and geophysics. Pockels studied the magnetization of basalt, the accumulation of rain in mountains, and other problems of interest to geophysicists, meteorologists, and mineralogists. Johann Königsberger worked extensively in mineralogy. Karl Zőppritz worked almost entirely in geophysics, eventually moving from a position in theoretical physics to one in geophysics. Emil Wiechert made a similar move, becoming director of a geophysics institute and an authority on seismology.[53] Physical chemistry, the most important of the neighboring sciences for physicists, acquired a fundamental physical theory through the application of thermodynamics. Some physicists and chemists anticipated that physical chemistry would dominate chemistry in its next stage, perhaps reducing it to a chapter of physical theory.[54] Helmholtz did extensive work on the thermodynamics of chemical reactions, as did other physicists in the 1880s and after, including Clausius, Kirchhoff, Boltzmann, and Planck. For their part, chemists who worked in physical chemistry required a good knowledge of parts of physics. The physical chemist Walther Nernst thought that the proper task of physical chemistry was to apply the "methods of theoretical physics" to chemical problems, and he maintained close working relations with theoretical physicists.[55] Physical chemistry was not an appendage to physics, but given their connections, the teaching of theoretical physics and that of physical chemistry were sometimes combined.[56]

From its beginning in 1845, the Berlin Physical Society's abstracting journal *Fortschritte der Physik* gave over an entire section to meteorology, and from 1849 another section to physical geography, combining the two in 1850, and in 1852

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[53] Paul Volkmann, "Hermann von Helmholtz," *Schriften der Physikalisch-ökonomischen Gesellschaft zu Königsberg* 35 (1894): 73–81, on 77; C. Voit, "Wilhelm von Bezold," *Sitzungsber. bay. Akad.* 37 (1907): 268–71, on 270–71; Albert Wigand, "Ernst Dorn," *Phys. Zs.* 17 (1916): 297–99, on 297; Johann Königsberger, "F. Pockels," *Centralblatt für Mineralogie*, *Geologie und Paläontologie* (1914), 19–21, on 20; Joachim Schroeter, "Johann Georg Königsberger (1874–1946)," *Schweizerische Mineralogische und Petrographische Mitteilungen* 27 (1947): 236–46; Charlotte Schmidt-Schönbeck, *300 Jahre Physik und Astronomie an der Kieler Universität* (Kiel: F. Hirt, 1965.), 99–101; Gustav Angenheister, "Emil Wiechert," *Gött. Nachr., Geschäftliche Mitteilungen aus dem Berichtsjahr* (1927–1928): 53–62.

[54] Eduard Riecke, "Rede," in *Die physikalischen Institute der Universität Göttingen*, ed. Göttinger Vereinigung zur Förderung der angewandten Physik und Mathematik (Leipzig and Berlin: B. G. Teubner, 1906), 20–37, on 35.

[55] Walther Nernst, "Antrittsrede," *Sitzungsber. preuss. Akad*, 1906, 549–52.

[56] For example, Gerhard Schmidt, who was trained primarily in chemistry and who was Gustav Wiedemann's assistant, alternated lectures on theoretical physics with lectures on physical chemistry and astronomy as Privatdocent at Erlangen in 1896–1900. In 1901, he was appointed extraordinary professor of physics with an assignment for both theoretical physics and physical chemistry at Erlangen. Gerhard Schmidt Personalakte, Erlangen UA.

combining with them Earth magnetism to constitute a section entitled "Physics of the Earth." The *Fortschritte* for 1880 published a separate volume on "Physics of the Earth," and in 1890 the volume was renamed "Cosmic Physics," the parts of which, astrophysics, meteorology, and geophysics,[57] its editor said, "belong without doubt to physics," even if not in all respects to the working region of "pure physics." This becomes evident, he said, from a few examples. It is in spectroscopy and stellar photography that optics has had its most outstanding successes. In the theory of winds and other weather topics, the laws of the mechanical theory of heat have played a major part. In geodesy and in the determination of gravity, the laws of mechanics and the theory of measures have proven indispensable. In sum, the editor said, in the sciences comprising cosmic physics, pure physics has found not only extensive applications but perhaps confirmation of its conclusions.[58] In 1903 the *Fortschritte* brought together in a single section the scattered chapters on physical chemistry, explaining that the subject had grown into a large research area and that the "representatives of this discipline" did not yet have their own yearly report on the literature. At the same time, the Physical Society decided to eliminate from the *Fortschritte* all reports on purely chemical and purely technical publications.[59]

In teaching and in research, the principal divisions of physics imposed a degree of specialization. In addition to what each division shared with the rest of physics, it had its general and special theories, concepts, measures, and apparatus specific to its needs. Each division had a tradition of work on detailed problems, which led to new methods, concepts, and facts, to theoretical and experimental problems, and to disputes, dissertations, and the rest of what made up the practice of physics. Each of the principal divisions of physics in turn contained narrower specialties. In addition there were hyphenated specialties such as "thermal-electricity," which acknowledged theoretical and experimental bridges across the divisions. Occasionally an entire principal division of physics was subsumed under another, or it was judged no longer to belong to physics proper. The classifying scheme of physics was fluid; it changed as physical understanding changed, as the categories of the *Fortschritte* repeatedly showed.

The *Fortschritte* started out with six main divisions, each with its subdivision for "theory": general physics, which included mechanics; acoustics; optics; heat, which

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[57] The term "geophysics" was also introduced in 1890. The term "astrophysics" was introduced earlier, in 1873; in that year, astrophysical topics, which before had been included in the chapter "Meteorological Optics" were given prominence in a new chapter "Astrophysics and Meteorological Optics," and 5 years later, in 1878, they received their own chapter, "Astrophysics."

[58] Richard Assmann, et al., ed., "Vollendung des 50. Jahrganges der 'Fortschritte,'" *Fortschritte der Physik des Aethers im Jahre 1894* 50 (1896): i–xi. The remarks by Assmann, editor of the cosmic physics department, are on viii-xi. Although the title "cosmic physics" appeared in the 46th volume, 1890, the volumes for the years 1893 and 1894 actually came out first, the reason why the editor discussed it there (Richard Assmann's "Vorwort," *Fortschritte der Physik* 46 [1890]).

[59] "Tagesereignisse," *Phys. Zs.* 3 (1902): 559–60; Karl Scheel, "Vorwort," *Fortschritte der Physik* 59 (1903): iii-iv. The elimination of pure chemistry and technology is announced in *Verh. phys. Ges.* 3 (1901): 130.

included radiant heat; electricity, which included magnetism; and applied physics, which was distributed among the other categories in the next report. Over the years these divisions underwent modification. The report for 1882 recognized certain long-standing connections by combining optics, heat, and electricity under the designation “Physics of the Ether” and acoustics and general physics under the designation “Physics of Matter.”[60] In 1902 the Physical Society decided that the *Fortschritte*’s ordering no longer corresponded to the state of physics, that it was now appropriate to place electricity and magnetism before instead of after optics and heat and to extend optics to optics of the total spectrum.[61] That organizational change corresponded to the major change in physical understanding associated with the names of Maxwell and Hertz.

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[60] E. Rosochatius, “Vorwort,” *Fortschritte der Physik* 38 (1882): v–vi; Emil Warburg, “Zur Geschichte der Physikalischen Gesellschaft,” *Naturwiss*. 13 (1925): 35–39, on 37.

[61] Scheel, “Vorwort,” iii. The new ordering appeared in the *Fortschritte* for 1903.

Chapter 15
Foundations and Connections

Through the nineteenth century, experimental physics was generally the driving force in physical advance, and this was particularly true from the 1880s into the beginning of the twentieth century. Technical developments and applied physics such as electrotechnology played an important part in this.[1] Hertz's experiments on electric waves in the late 1880s were followed in the 1890s by experimental discoveries of other sorts of radiations. From his physics laboratory at Würzburg in 1895, W. C. Rőntgen reported on a "new kind of rays," the penetrating X, or Röntgen, rays, the nature of which was unclear at the time. It was a true discovery, something Röntgen, who was experimenting with electrical discharges in evacuated tubes at the time, did not anticipate. At about the same time, in experiments using similar kinds of apparatus, the long-disputed nature of "cathode rays" was decided. They are not disturbances propagated through the ether, as Hertz and other German physicists thought, but streams of corpuscles, as British physicists argued and as Jean Perrin in France demonstrated in 1895. The most impressive confirmation of their corpuscular nature was reported in 1897 from the physics laboratory at Cambridge University, where J. J. Thomson measured the velocity and the ratio of charge to mass of the negatively charged corpuscles, or "electrons." Thomson was led to study the nature of cathode rays by his work with Röntgen rays in the ionization of gases, and it was through an investigation of the relations of Röntgen rays to his subject, florescence, that in 1896 the French physicist Henri Becquerel came across penetrating radiations emitted by a uranium salt. This was another true discovery, that of radioactivity. It drew the attention of his French colleagues Pierre and Marie Curie, who soon discovered new radioactive elements, and of the New Zealander Ernest Rutherford, who was then working with Röntgen rays in Thomson's laboratory at Cambridge. Becquerel, the Curies, Rutherford and his

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[1] Robert D. Purrington, *Physics in the Nineteenth Century* (New Brunswick, London: Rutgers University Press, 1997), xiv.

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
DOI 10.1007/978-3-319-49565-1_15

coworkers, and other experimenters gradually brought clarity to the complex of radiations and transmutations characteristic of radioactivity. The extensive experimental work on Röntgen rays, electrons, and radioactivity posed questions and presented opportunities for theoretical physics. Later experiments with radioactive sources led to ideas about the structure of the atom, followed by an exact atomic theory based on a new mechanics. Theoretical physics was also stimulated by—as it in turn stimulated—experiments of a different sort, not discoveries of new things, but accurate measurements of known phenomena, for example, spectral lines and their splitting, blackbody radiation, photoelectric emissions, and the motion, if any, of the Earth through the ether.

In addition to the stimulus from experimental physics, theoretical physicists responded to problems arising from their own methods and objectives. For example, at their meetings and in their publications, they debated over the mechanical foundation of physics and alternatives to it. In 1899, Boltzmann observed that the new theory of physics "has not yet proved totally successful in unifying all of these phenomena into one theoretical structure as the old [theory] used to." Five years later he thought that it could almost be said that theoretical physics was "in process of revolution."[2]

"World picture" will be important in this chapter. Kant "gave philosophical currency to the notion of world-view (Weltanschauung) and, indirectly, world-picture (Weltbild),"[3] giving rise to an intellectual tradition, which was continued by theoretical physicists. Since Kant, Planck said, it has been accepted that there is no way of proving the difference between "world view" and "world picture," and out of caution, scientists have preferred "world picture."[4] In 1913, he associated world view with philosophy, which has many coexisting and changing forms, and world picture with science, which is singular. He said that world view cannot be based solely on science, but that any world view that ignores science is condemned from the start. Physicists, he said, need to believe in "some sort of reality outside" themselves to support their "aimless groping," uplift their spirits "wearied by failure," and urge them on to further searching.[5] He characterized the world picture

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[2] Ludwig Boltzmann, "On the Development of the Methods of Theoretical Physics in Recent Times," trans. Y. Elkana, in *Philosophical Forum,* n.s. 1 (1968–69): 97–120, on 107; "The Relations of Applied Mathematics," in *International Congress of Arts and Science: Universal Exposition St. Louis, 1904*, vol. 1, *Philosophy and Mathematics* (Boston and New York: Houghton, Mifflin, 1905), 591–603, on 591.

[3] Jordi Cat, "The Unity of Science," *Stanford Encyclopedia of Philosophy* (16 May 2013), http://plato.stanford.edu/entries/scientific-unity (accessed 13 June 2015).

[4] Max Planck, "The Unity of the Physical Universe," 1908, in *A Survey of Physical Theory*, ed. R. Jones and D. H. Williams (New York: Dover Publications, 1960), 1–26, on 25.

[5] Max Planck, "Ansprache," *Sitzsber. Pr. Akad. d. Wiss.*, 1913, pt. 1, 73–76, esp. 75; "New Paths of Physical Knowledge," 1913, in *A Survey of Physical Theory*, 45–55, on 54.

is a “synthesis of concepts and theorems,” which replaces the world of the senses.[6] He considered it a valuable source of hypotheses for physicists in their work. From researches discussed in this chapter, physicists concluded that a new world picture was needed.

15.1 Mechanics

The nineteenth century was the “golden age” of the “mechanical view of nature,” which Planck defined as the “view that all physical phenomena can be completely reduced to movements of invariable and similar particles or elements of mass.”[7] In keeping with this expectation, the physicists in our study repeatedly extended the reach of mechanical concepts and laws outside of mechanics proper. Helmholtz’s contributions were the most comprehensive and influential in this regard, and we start with them. In an earlier chapter, we saw that he used mechanics to derive the universal principle of conservation of energy, which came to be known in one of its applications as the first law of thermodynamics. In his later career as a physicist at Berlin, he studied the second law of thermodynamics from the standpoint of mechanics, building on the concept of cyclic motions. He showed in his monocyclic studies that the laws of reversible heat processes can be expressed in the form of Lagrange’s equations of motion and therefore in the form of a certain “minimal law,” which he referred to as “Hamilton’s principle of least action.”[8] In 1886 he undertook a systematic study of the least-action principle itself. Upon comparing it with other formulations of the laws of mechanics, he concluded that it holds the advantage, and that it encompasses the others as special cases. He held high expectations for this principle as a way of providing a uniform dynamical representation of the laws of all parts of physics. In this respect, his interest in the principle was a continuation of the general direction of his research, as it was of certain British physicists whose work he followed with interest, Maxwell’s above all.

Helmholtz undertook his study of the least-action principle as a direct result of his interest in the form of the kinetic potential required by Maxwell’s

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[6] Max Planck, “The Scientist’s Picture of the Physical Universe,” in *Where Is Science Going?* trans. J.V. Murphy (New York: W.W. Norton, 1932), 84–106, on 85.

[7] Max Planck, “The Place of Modern Physics in the Mechanical View of Nature,” 1910, in *A Survey of Physical Theory*, 27–44, on 28.

[8] The least action principle went back to the eighteenth century. In Hamilton’s later form of it, the action that is a minimum is expressed by a time integral of the difference of kinetic and potential energies, not just the kinetic energy as in earlier forms. By this principle, the action of the actual motion of a mechanical system is less than the action of any other motion consistent with the conditions of the problem. The principle is independent of coordinate systems. Another class of least action principles such as Gauss’s and Hertz’s is differential, which applies to a moment (element) of time, and which depends on the choice of coordinate system. Hamilton’s principle is mechanical, but Helmholtz’s generalization of it is universal in its applications (Planck, “The Principle of Least Action,” 1915, in *A Survey of Physical Theory,* 69–81, on 72–73, 77–78).

electromagnetic equations. “Kinetic potential” is the name Helmholtz used for the function H entering the least-action principle and the resulting Lagrangian equations of motion. For each area of physics there is a kinetic potential, which is constructed from variables of state and has the dimensions of energy, or work, though the individual parts of the kinetic potential are not specified as kinetic energy and potential energy. The principle in its analytical formulation is a variational equation determining the entire course of a system[9]:

$$\delta\Phi = 0, \text{where}$$

$$\Phi = \int_{t_0}^{t_1} dt \left\{ H + \sum_a (P_a p_a) \right\}.$$

Helmholtz added to the usual statement of the least-action principle the above term $\sum_a (P_a p_a)$, where P_a are forces that depend on time and p_a are the corresponding coordinates. In this generalized form, the principle applies to systems acted on by friction, galvanic resistance, and other processes that lie outside mechanics proper. One of the consequences of the principle is what Helmholtz called “exchange relations,” or “reciprocity laws,” between forces, a property of any system that has a kinetic potential. One set of exchange relations holds between forces and

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[9] The minimal principle states that the negative mean value of the kinetic potential acquires its minimum value on the actual path that a system takes relative to all of the neighboring paths proceeding from the point of origin to the end point in the same time. Helmholtz gives his modified version the name “minimal law of the negative kinetic potential.” The kinetic potential entering the modified least-action principle is no longer limited to its original form, the difference between potential energy as a function of position and kinetic energy as a quadratic homogenous function of velocity; the potential energy now may be a function of velocity as well as of position, and the kinetic energy need not be a homogenous function and may depend on position and on terms linear in velocity. The function H is required only to have finite first and second derivatives with respect to the coordinates and velocities. The kinetic potential in general consists of two series of variables, p and q; among the p are parameters that determine the positions of the masses, whose acceleration refers to ponderomotive forces; the q need not refer to the velocities of ponderable masses but may refer to functions of the temperature, intensities of electric currents, and changes of dielectric and magnetic polarizations, among other physical quantities. The calculus of variations yields the Lagrangian equations of motion, which are, in Helmholtz’s notation,

$$0 = P_a + \frac{\partial H}{\partial p_a} - \frac{d}{dt}\left[\frac{\partial H}{\partial q_a}\right].$$

Here $q_a = dp_a/dt$ are the velocities of the system, and P_a are the forces acting to change the coordinates. Proceeding from these equations, Helmholtz examined the behavior of mechanical, thermodynamic, and electrodynamic systems (Hermann von Helmholtz, “Über die physikalische Bedeutung des Princips der kleinsten Wirkung,” *Journ. f. d. reine u. angewandte Math.* 100 [1887]: 137–66, 213–22, on 139–40, 145).

accelerations, another between forces and velocities, and another between forces and coordinates. The relation between forces and velocities for a great many cases is:

$$\frac{\partial P_a}{\partial q_b} = -\frac{\partial P_b}{\partial q_a},$$

which asserts that if the force P_b increases with increasing velocity q_a, the force P_a decreases with a corresponding increase in velocity q_b. Helmholtz offered the following thermodynamic law as an example of exchange relations: if with an increase in the temperature of a given system, the pressure increases, then a compression of the system will increase the temperature. He showed that with different physical meanings attached to the generalized forces, velocities, and accelerations, these abstract exchange relations express connections between diverse phenomena: mechanical, electrodynamic, thermodynamic, thermoelectric, and electrochemical. This is a reason why he valued them.[10]

In his last researches, from 1892 until his death in 1894, Helmholtz studied electrodynamics from the standpoint of the generalized least-action principle. Previously he had applied it to Neumann's potential law and other limited electrodynamic relations, and now he extended it to Maxwell's more complete equations, especially in Hertz's version. By assuming that the pure ether is a frictionless, incompressible fluid without inertia, Helmholtz derived Hertz's electromagnetic equations for bodies in motion. He derived as well the ponderomotive forces of the theory, though these posed complications that he did not fully resolve.[11]

In an address to the Prussian Academy of Sciences in 1887, Helmholtz compared the least-action principle with the conservation of energy principle. The former principle completes the latter, since it yields the latter as a consequence, and in addition it determines the path of a process.[12] The two principles had similar histories: both originated in the mechanics of ponderable bodies, both were subsequently extended to heat and electricity, and both found wide applications in research. In the middle of the nineteenth century, Kirchhoff, Boltzmann, and Thomson found the least-action principle useful for solving problems in hydrodynamics and elasticity. Clausius, Boltzmann, and recently Helmholtz in his studies of

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[10] Ibid., 161–66.

[11] Hermann von Helmholtz, "Das Princip der kleinsten Wirkung in der Elektrodynamik," *Ann.* 47 (1892): 1–26; repr. in *Wissenschaftliche Abhandlungen*, 3 vols.(Leipzig, 1882–95), vol. 3, 476–504; "Folgerungen aus Maxwell's Theorie über die Bewegungen des reinen Aethers," *Ann.* 53 (1893): 135–43; repr. in *Wiss. Abh.*, vol. 3, 526–35; "Nachtrag zu dem Aufsatze: Ueber das Princip der kleinsten Wirkung in der Elektrodynamik" (1894); repr. in *Wiss. Abh.*, vol. 3, 597–603.

[12] The reason for the difference in the two principles can be seen in their mathematics. The energy principle gives one equation only. To solve completely the problem of motion, there must be as many equations as there are variables, which in the case of a free particle are three. The principle of least action gives as many equations as there are variables, and it does this through one formula, since it is a variation principle, unlike the principle of energy (Planck, "The Principle of Least Action," 70–71).

monocyclic systems applied the principle to the second law of thermodynamics. Franz and Carl Neumann, Weber, Clausius, Riemann, and abroad Maxwell and others applied it to electrodynamics. The only limitation to the validity of the least-action principle is that it does not apply to irreversible processes, but Helmholtz thought that irreversiblility is a consequence of our inability to trace the irregular motions of individual atoms, not a reality of nature. The least-action principle appealed to him for several reasons. It compresses in the narrowest space, that of a single equation, all of the conditions of a physical problem, and it applies throughout physics, providing a guide for formulating the laws of new classes of phenomena wherever they occur.[13] It also agrees with Helmholtz's preference to work with differential equations expressing general principles rather than with hypotheses. In his late studies, he was drawn to the principles that govern the "flux of the eternally indestructible and uncreatable energy supply of the world." The circumstance that laws of the constancy of energy and of least action, the two most general principles of physics, speak only of energy and not of forces he considered to be of high importance to the "final principal questions of science."[14]

In 1891, on the occasion of Helmholtz's seventieth birthday, Hertz wrote that it was not well known that Helmholtz had recently returned to his earlier interests: having extended the energy principle to all forces, he was now extending the least-action principle to all of nature. Helmholtz was pursuing a lonely path, Hertz said, and he speculated that it would be years before a follower appeared to continue his work of tracing all phenomena to the least-action principle.[15] The most notable of Helmholtz's followers in this direction was Max Planck, who believed that the goal of "theoretical inquiry" in physical science is a single principle embracing all natural phenomena, allowing past and future to be calculated, and that the least-action principle most closely approaches the goal.[16] Helmholtz had reasoned from the principle to a "unified conception," Planck said, adding that the "future must bring the realization of his ideas."[17] Planck regarded the question of the significance

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[13] Helmholtz, "Über die physikalische Bedeutung," 142–143; "Rede über die Entdeckungsgeschichte des Princips der kleinsten Action," in *Geschichte der Königlich preussischen Akademie der Wissenschaften zu Berlin*, ed. Adolf Harnack, 3 vols. (Berlin: Reichsdruckerei, 1900), vol. 2, 282–96, on 283–87. Helmholtz did not publish this address, which he gave on 27 January 1887 at the Prussian Academy of Sciences, because he learned that the theme had already been treated by the mathematician Adolf Mayer.

[14] Helmholtz, "Rede über die Entdeckungsgeschichte," 287; Helmholtz's preface to *Principles of Mechanics Presented in a New Form,* trans. D. E. Jones and J. T. Walley (London, 1899; repr. New York: Dover, 1956).

[15] Heinrich Hertz, "Hermann von Helmholtz," in supplement to *Münchener Allgemeine Zeitung*, 31 August 1891; repr. Hertz's *Miscellaneous Papers,* trans. D. E. Jones and G. A. Schott (London, 1896), 332–40, on 340.

[16] Planck, "The Principle of Least Action," 69.

[17] Max Planck, "Helmholtz's Leistungen auf dem Gebiete der theoretischen Physik," *ADB* 51 (1906): 470–72; repr. in Planck, *Physikalische Abhandlungen und Vorträge*, vol. 3 (Braunschweig: F. Vieweg, 1958), 321–23, on 323.

of the least-action principle for the whole of physics as worthy of his, as of Helmholtz's, best efforts.

After finishing his researches on electric waves, by the end of 1890 Hertz had completed the arrangements of his laboratory at Bonn University, giving him more time for research. He tried a variety of experiments, then growing tired of repeated failures, he looked for a new direction. In early 1891 he found it, and for the next 3 years he devoted himself to writing "something good and above all lasting" on the subject of mechanics. "Entirely occupied" with the work, he rewrote each word three or four times and rewrote the whole as often, he said, with the goal of clarifying the subject.[18]

The result was a book, *Die Principien der Mechanik* [*Principles of Mechanics*], published in 1894, which opens with the observation that "all physicists agree that the problem of physics consists in tracing the phenomena of nature back to the simple laws of mechanics." But since all physicists did not agree on what those simple laws are, it was "premature to attempt to base the equations of motion of the ether upon the laws of mechanics." That would have to come later. For now Hertz set about to reformulate the laws of mechanics with the object of describing all known motions and excluding all unknown ones. In this project, he was indebted to Mach's *Science of Mechanics* and to Thomson and Tait's and other treatises on mechanics, but he owed most to Helmholtz's recent investigations of the least-action principle, which he called the "furthest advance of physics at the present time." He did not, however, make use of the least-action principle in his mechanics.[19]

The first part of *Principles* treats geometry and kinematics and is "completely independent of experience," concerned only with statements about the paths and connections of material particles that satisfy the demands of thought. The second part treats mechanics proper and draws on experience through the "fundamental law," which is a statement about the path followed by a free system.[20] Force, the presumed invisible something responsible for the visible motions of masses, Hertz regarded as logically obscure, belonging to the superseded physics of action at a distance. Dispensing with force as one of the fundamental concepts of mechanics, he worked directly with motions and masses. His motions and masses, which differ from the moving masses that physicists observe in that they are too fine for their senses, are related to the "concealed motions" and "concealed masses" that Helmholtz had introduced in his mechanical studies. They are also related to the notion of

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[18] Hertz to Cohn, 31 December 1890, Ms. Coll., DM, 3204; Hertz's diary in Hertz, *Erinnerungen, Briefe, Tagebücher,* ed. J. Hertz, 2nd rev. ed. M. Hertz and Charles Süsskind (San Francisco: San Francisco Press, 1977), 314; Hertz to Arthur Meiner, 12 November 1893, Ms. Coll., DM, 3245. Hertz to Sarasin, 19 May 1893, Ms. Coll., DM, 3149.

[19] Hertz, *Principles of Mechanics*, author's preface and 17.

[20] Hertz's fundamental law reads: "Every free system persists in its state of rest or of uniform motion in a straightest path." *Principles of Mechanics*, 144. Here the expression "free system" refers to a material system with only internal and normal, or time-independent, connections; and "straightest path" refers to a path of smallest curvature.

the ether after Maxwell's theory: the idea that electromagnetic forces arise from concealed motions of the ether had "become almost a conviction" among physicists. To formulate mechanics from masses and motions without introducing mysterious forces agrees with the "present tendencies of physics." "This is the leading thought from which we start," he said.[21]

On receiving Hertz's book, Helmholtz thought that his mechanical principles might lead to a new understanding of forces, and he admired the logic and the mathematics, but he regretted that Hertz did not give concrete examples to show how the mechanism of hidden masses works.[22] Hertz's principles were applied by others to some problems, but physicists by and large looked at them as Helmholtz did, with respect and suspended judgment. The subject of his mechanics came up in discussions of the foundations of physics, but it was his epistemological and methodological introduction to the book that drew greatest attention. There, in a criticism of the alternative representations of mechanics, Hertz closely analyzed physicists' "pictures" of mechanics and the criteria by which they are to be judged.

When physicists spoke of mechanics as the "foundation" of physics, they did not mean that pure mechanical theory explains all physical phenomena; they meant that mechanics contains the laws of motion that hypothetical explanations of phenomena in the various branches of physics must obey. J. F. Fries clarified this point as early as 1822, as we saw in Chap. 2. In 1896 the Leipzig physical chemist Wilhelm Ostwald said that recent textbooks on physics and chemistry confirmed his impression that the prevailing scientific world picture was formed of mechanical hypotheses.[23] In one way or another, if not always in the way Ostwald had in mind, physics textbooks in the late nineteenth century ascribed to mechanics a leading role in physical theory. This was the case with such standbys as Adolph Wüllner's textbook on experimental physics, which noted that physicists strove increasingly to found all phenomena on "one and the same fundamental cause, on motion," the science of which was mechanics, and Müller-Pouillet's textbook on physics and meteorology, which noted that much of heat theory was united with mechanics and that mechanics was applied to light and electricity as well. Leopold Pfaundler, the editor of the Müller-Pouillet text, concluded that "mechanics is really not a part of physics, but conversely the individual disciplines are parts or applications of mechanics." The newer textbooks on experimental physics by Kundt and Warburg made a similar point. Kundt introduced mechanics as the "foundation of the whole of physics and all natural sciences," and Warburg characterized "mechanics or the theory of motion as building the foundation of physics," explaining that physicists perceived many phenomena directly as motion, and that they successfully applied

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[21] Hertz, *Principles of Mechanics*, 26.

[22] Helmholtz's preface to Hertz, *Principles of Mechanics*.

[23] Wilhelm Ostwald, "Zur Energetik," *Ann.* 58 (1896): 154–67, on 160.

the laws of motion to other phenomena that they did not perceive as motion such as those of light, electricity, and heat.[24]

We have met similar statements about mechanics in lectures and texts on theoretical physics, which we briefly recall. Franz Neumann began his published lectures with mechanics, since it was the "foundation for all remaining branches" of physics. Volkmann in his introductory text on theoretical physics treated mechanics as the "fundamental discipline within the physical system." Voigt devoted roughly the first half of his compendium of theoretical physics to the mechanics of rigid and non-rigid bodies, developing "mechanical theories" for the most important phenomena of all parts of physics. In his textbook on mechanics, Boltzmann repeatedly expressed his understanding that mechanics is the foundation of physics and of all natural science. Likewise, Kirchhoff, Helmholtz, and Hertz regarded mechanics as the foundation of physics. Even an ancillary text such as Heinrich Weber's edition of Riemann's lectures on the mathematical methods of physics might assert that "mechanics lies at the foundation of our entire theoretical natural science."[25]

Despite the seeming wide agreement, the role of mechanics came under repeated questioning at the end of the nineteenth century. In his inaugural lecture at the Prussian Academy of Sciences in 1894, Planck discussed the problem. He observed that the task of theoretical physicists in seeking a comprehensive view of nature was much harder than it had been a generation back, when it had been to reduce all natural phenomena to mechanics; "today this direct striving for the highest goal has come to a halt, a certain sobering has occurred." The reason is not that mechanics lacks sufficient concepts, Planck explained, but that it allows too many explanations for any process, each complicated and none clearly superior to the others. He believed that only new ideas such as the communication of forces through a medium might decide between the mechanical choices. The example of the whole recent development of thermodynamics, which needs only its two

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[24] Adolph Wüllner, *Lehrbuch der Experimentalphysik*, vol. 1, *Allgemeine Physik und Akustik* (Leipzig, 1882), 8; *Müller-Pouillet's Lehrbuch der Physik und Meteorologie,* ed. Leopold Pfaundler, 9th rev. ed., vol. 1 (Braunschweig, 1886), 15; August Kundt, *Vorlesungen über Experimentalphysik*, ed. K. Scheel (Braunschweig: F. Vieweg, 1903), xii; Emil Warburg, *Lehrbuch der Experimentalphysik für Studirende* (Freiburg i.B. and Leipzig, 1893), 1.

[25] Franz Neumann, *Vorlesungen über mathematische Physik, gehalten an der Universität Königsberg; Einleitung in die theoretische Physik*, ed. Carl Pape (Leipzig, 1883), 1; Paul Volkmann, *Einführung in das Studium der theoretischen Physik insbesondere in das der analytischen Mechanik mit einer Einleitung in die Theorie der physikalischen Erkenntniss* (Leipzig: B. G. Teubner, 1900), 41–43, 349; Woldemar Voigt, *Kompendium der theoretischen Physik*, vol. 1, *Mechanik starrer und nichtstarrer Körper*, *Wärmelehre* (Leipzig, 1895); Boltzmann, *Prinzipe der Mechanik*, vol. 1, 1; "Antritts-Vorlesung. Gehalten in Leipzig im November 1900," in "Zwei Antrittsreden," *Phys. Zs.* 4 (1902–1903): 247–56, on 248; Gustav Kirchhoff, *Vorlesungen über die Theorie der Wärme*, ed. Max Planck (Leipzig, 1894), 2; Hermann von Helmholtz, *Vorlesungen über die Dynamik discreter Massenpunkte*, ed. Otto Krigar-Menzel (Leipzig, 1898); Hertz, *Principles of Mechanics*; Heinrich Weber, *Die partiellen Differential-Gleichungen der mathematischen Physik. Nach Riemann's Vorlesungen*, 4th rev. ed., 2 vols. (Braunschweig: F. Vieweg, 1900–1901), vol. 1, 283.

fundamental principles, might seem to indicate that physics was moving away from the "mechanical view of nature" altogether. But this was to forget that the great connection of all the forces of nature is in its innermost nature an "identity" and that it can never be arrived at in physics better than through mechanics. Planck guardedly reaffirmed the continuing mechanical direction in theoretical physics, aware of the contemporary questioning of that direction in light of the success of his own preferred branch of physics, thermodynamics.[26]

There was another reason for questioning the mechanical direction in physics from the side of thermodynamics. Josef Loschmidt had stated it in one form in the 1870s, discussed earlier. It was stated again in another form in 1896 by Planck's assistant Ernst Zermelo. Drawing on a recent theorem by Henri Poincaré, which states that a mechanical system moving under conservative forces must in time return arbitrarily close to its original state, Zermelo argued that the science of irreversible processes, thermodynamics, could not be reduced to mechanics, at least not to mechanics in its present form. For the second law of thermodynamics to have general validity, it would be necessary to assume highly improbable initial conditions, which would contradict the requirement of causality and, in general, the "spirit of the mechanical view of nature itself." Unless there were to be a new mechanics, for example, Hertz's mechanics, a "mechanical derivation of the second law" must be assumed impossible.[27]

Zermelo's questioning of the place of mechanics in thermodynamics was answered by Boltzmann in 1896. Zermelo had misunderstood him, he said. From the standpoint of molecular theory, the second law is a "mere law of probability," not a theorem of ordinary mechanics, and so it does not require giving up mechanical pictures. The recurrences predicted by Poincaré's mechanical theorem do occur, but they are exceedingly rare and do not invalidate the probabilistic interpretation of the second law.[28] In a letter to Graetz in 1897, Planck gave his opinion on the questions that divided Zermelo and Boltzmann, judging them to be the "most important questions that presently concern theoretical physics." The probability calculus applies to the most probable state, Planck said, but only mechanics can determine the state succeeding any given improbable state, and there is no reason to assume that physical changes always take place in the direction of greater probability. Planck believed that it was preferable to regard the second law as a natural

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[26] Max Planck, "Antrittsrede zur Aufnahme in die Akademie vom 28. Juni 1894," *Sitzungsber. preuss. Akad.*, 1894, 641–44; repr. in *Phys. Abh.*, vol. 3, 1–5, quotations on 1–2, 4.

[27] Ernst Zermelo, "Ueber einen Satz der Dynamik und die mechanische Wärmetheorie," *Ann.* 57 (1896): 485–94, quotations on 492–94.

[28] Ludwig Boltzmann, "Entgegnung auf die wärmetheoretischen Betrachtungen des Hrn. E. Zermelo," *Ann.* 57 (1896): 773–84; repr. in *Wissenschaftliche Abhandlungen,* ed. Fritz Hasenöhrl, 3 vols. (Leipzig: J.A. Barth, 1909; reprint, New York: Chelsea, 1968), vol. 3, 567–78, on 567. There was another round of responses between Zermelo and Boltzmann; the whole exchange is discussed in Stephen G. Brush, *The Kind of Motion We Call Heat: A History of the Kinetic Theory of Gases in the 19th Century*, vol. 2, *Statistical Physics and Irreversible Processes* (Amsterdam and New York: North-Holland, 1976), 632–37.

law that is strictly valid than to try to save the kinetic theory of gases by assuming particular initial conditions of the world, which would then have no further use. In all of this, Planck was in agreement with Zermelo, though he thought that his assistant had gone too far in claiming that the "second law, as a law of nature, is altogether incompatible with any mechanical conception of nature." Planck said that the problem looked different if one worked with continuous matter rather than with discrete mass points as in the theory of gases: "I believe and even hope that in this way we will be able to find a rigorously mechanical interpretation of the second law; but this matter is obviously very difficult and requires time."[29]

The first lectures Planck published after coming to Berlin, in 1897, covered the "entire field of Thermodynamics" presented from a "uniform point of view," and included much of his own recent research. He was concerned to place thermodynamics on the firmest possible foundations, which are laws rooted in our universal experience of nature. He said that no one any longer questioned the first law of thermodynamics, which is the principle of energy conservation applied to processes involving heat exchanges, but the second law did not get off so easily. The reason for this is that the law of increase of entropy, the "universal measure" of the irreversibility of all finite physical and chemical processes, is more complex than the first law. He allowed that the second law might have only limited validity, but if so, the limits are in nature and not in us; for the reasoning behind the law makes use of ideal processes, which have nothing to do with our skill in experiment. He sharply separated the foundations of thermodynamics in experience from its foundations in the "mechanical view of nature." He chose not to present the subject as a branch of mechanical theory, either as the kinetic theory, which penetrates "deepest" but meets with "essential difficulties," or as Helmholtz's more general mechanical theory, which views heat as motion but leaves unspecified the type of motion. He thought that the mechanical theories had proven less fruitful than his own, which makes no assumption about the nature of heat, but he did not consider his theory as necessarily final. One day it might be replaced by another that would correspond to "our aspiration for a uniform theory of nature, on a mechanical basis or otherwise." In this theory, the two thermodynamic laws "would not be introduced as independent, but would be deduced from other more general propositions."[30]

Planck had serious disagreements with Ernst Mach, the experimental physics professor at Prague University, but the two men were in agreement that thermodynamics is founded on experience rather than on mechanics. Mach believed that the "majority of modern scientists" subscribed to Laplace's ideal of a total atomistic-mechanistic determinism, both parts of which, the atomistic and the

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[29] Planck to Graetz, 23 May 1897, Ms. Coll., DM, 1933 9/30. This letter is discussed and quoted in part in Thomas S. Kuhn, *Black-Body Theory and the Quantum Discontinuity 1894–1912* (New York: Oxford University Press, 1978), 27–28.

[30] Max Planck, *Vorlesungen über Thermodynamik* (Leipzig, 1897), trans. as *Treatise on Thermodynamics*, by A. Ogg (London, New York and Bombay: Longmans, Green, 1903), vii, ix–x, 38, 77–79, 86, 103.

mechanistic, met with Mach's principled skepticism. He had come to regard atoms only as a convenient aid to the researcher and not to be taken as "realities behind phenomena." In *Science of Mechanics,* published in 1883, he wrote that "the view that makes mechanics the basis of the remaining branches of physics, and explains all physical phenomena by mechanical ideas, is in our judgment a prejudice. . . . We have no means of knowing, as yet, which of these physical phenomena go *deepest*, whether the mechanical phenomena are perhaps not the most superficial of all, or whether all do not go equally *deep*." The "mechanical theory of nature" is historically understandable, but it is nonetheless an "artificial conception," destined sooner or later to be supplanted.[31] For the goal of physical science is the simple, economical expression of facts, which takes the form of concise, mathematical "descriptions," or "natural laws," rather than mechanical hypotheses. Mach allowed that mechanical ideas might be usefully applied in the other branches of physics such as heat and electricity, but they are applied in the spirit of an analogy only. For Mach, physics is comparative, not mechanical.[32] He continued this argument in 1896 in a book on the science of heat, which he found a ready subject for further questioning mechanical explanations in physics. Physicists might find mechanical analogies useful, but analogies are not identities, and so they have no place in the physicists' presentations of their researches. Mechanical explanations of the second law of thermodynamics are not proof of the mechanical nature of the processes but only a further development of the analogy between heat and kinetic energy. The principle of entropy goes deeper, and it and the energy principle, not molecular mechanics, are the foundations of thermodynamics.[33]

15.2 Energy

The principle of conservation of energy provided Mach with an object lesson in the way physical science advances. Theoretical ideas such as the identification of heat with motion played a part in the genesis of the principle, but once it was established, it described an extensive set of facts directly and economically,

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[31] Ernst Mach, *Die Mechanik in ihrer Entwickelung. Historisch-kritisch dargestellt* (Leipzig, 1883), trans. as *The Science of Mechanics. A Critical and Historical Exposition of Its Principles,* from the 2nd rev. German edition of 1889 by T. J. McCormack (Chicago, 1893), 495–96.

[32] Ernst Mach, "Economical Nature of Physical Inquiry," talk at the Academy of Sciences in Vienna in 1882, in *Popular Scientific Lectures,* trans. T. J. McCormack (Chicago, 1895), 186–213, on 193, 207; *Science of Mechanics*, 498; "On the Principle of Comparison in Physics," invited talk for the general session at the 1894 German Association meeting in Vienna, in *Popular Scientific Lectures*, 236–58, on 249–50.

[33] Ernst Mach, *Die Principien der Wärmelehre. Historisch-kritisch entwickelt* (Leipzig, 1896), 363–64, quotation on 364.

without the need for any theoretical ideas.[34] In the second edition of *Science of Mechanics* in 1889, Mach acknowledged Georg Helm's book on the energy principle, *Die Lehre von der Energie*, which laid down "a general science of energetics." Mach said that he had "seldom read anything that . . . appealed in an equal degree to my mind."[35]

Helm was only then beginning his career; in 1888, the year following the appearance of his book on energy, he became extraordinary professor and 4 years later ordinary professor at the technical institute in Dresden. There while he lectured on mechanics and mathematical physics, he promoted a program of energetics, which sought to base physical science on the understanding that with each possible physical change the energy remains constant. He founded the mathematical development of energetics on what he called the "energy principle," a differential law that applies to all physical systems; the integral law of conservation of energy follows from the differential law. In 1890, he attempted to reduce mechanics to energetics by deriving the differential equations of motion of a material point from the energy principle. He explained why to a colleague: a "unified construction of natural science from energetic ideas must, above all, be able to bring the most secure knowledge, mechanics, under this viewpoint." By providing mechanics with its general founding principle, energetics would replace mechanics as the foundation of physics.[36]

In his efforts to establish the energetic viewpoint, Helm had an influential ally in Wilhelm Ostwald.[37] While teaching chemistry at the polytechnic school in Riga, Ostwald had become convinced that certain chemical processes could only be understood in energetic rather than atomistic terms. In his inaugural lecture at Leipzig in 1887, he proposed a program of energetics as an alternative to atomism,

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[34] Mach, "On the Principle of Comparison," 247–48.

[35] The appendix to the 2nd German edition of 1889 appears in Mach, *Science of Mechanics*, 517. With the words quoted here, Mach commended in addition to Georg Helm's book, *Die Lehre von der Energie* (Leipzig, 1887), also Josef Popper's *Die physikalischen Grundsätze der elektrischen Kraftsübertragung. Eine Einleitung in das Studium der Electrotechnik* (Vienna, 1884). Mach approved of energetics for its rejection of atomism and mechanism, but he was critical of its own metaphysical tendencies. John T. Blackmore, *Ernst Mach. His Work, Life, and Influence* (Berkeley, Los Angeles, and London: University of California Press, 1972), 118–19.

[36] From $T = \frac{1}{2} \cdot m(x'^2 + y'^2 + z'^2)$, Helm wrote the differential kinetic energy of a mass point: $dT = mx''dx + my''dy + mz''dz$; and he wrote the (in general, not complete) differential of the work associated with the force X, Y, Z acting on the mass point: $dA = Xdx + Ydy + Zdz$. Then, from the "energy principle," $dT = dA$, he wrote the equation $(mx'' - X)dx + (my'' - Y)dy + (mz'' - Z)dz = 0$. For it to hold for all dx, dy, dz, Helm reasoned that the quantities in the brackets must all vanish: $mx'' = X, my'' = Y, mz'' = Z$, which are the equations of motion of a freely moving mass point (Georg Helm, "Ueber die analytische Verwendung des Energieprincips in der Mechanik," *Zs. f. Math. u. Phys.* 35 [1890]: 307–20; Helm to Ostwald, 20 January 1891, in *Briefewechsel . . . Ostwalds*, 73; Niles R. Holt, "A Note on Wilhelm Ostwald's Energism," *Isis* 61 [1970]: 386–89, on 386–87).

[37] Helm and Ostwald did not agree on all points. In particular, Helm did not regard energy as a substance, whereas Ostwald regarded it as the single ultimate substance.

and in the second edition of his textbook on physical chemistry in 1892, he worked with energetic ideas while avoiding atomistic ones. In a series of papers around this time, he argued against the reduction of physics and chemistry to mechanics and for the reduction of the mechanical concept of mass or matter to the concept of energy. He proposed changing the measures of exact natural science from Gauss and Weber's absolute system to an energetic system in which the basic units are energy, length, and time.[38] In a general address at the German Association meeting in 1894, he acknowledged that particular phenomena of heat and chemistry can be explained by the use of mechanical analogies, but he denied that the totality of phenomena can be explained this way. It is evident, he said, from the reversibility of the equations of motions that mechanics cannot explain the irreversible events in our experience, and that the mechanical world picture can no longer be defended scientifically. The object of science is not to construct mechanical pictures but to connect measurable quantities with one another. All that we experience directly is energy, the most general invariant known to science. In energetics, energy replaces force and matter, offering a unified world picture as clear as the mechanical one and without its difficulties.[39]

A debate on energetics took place at the German Association meeting in 1895, and it was continued in the pages of the *Annalen der Physik* the following year. Concerned about the energetic treatment of the "most cultivated discipline of theoretical physics, mechanics," Boltzmann proceeded to set matters right. He pointed out an elementary mathematical error in Helm's derivation of the laws of mechanics,[40] and he showed that Ostwald was out of touch with current thought in physics. Denying Ostwald's claim that "today everyone thinks of atoms and forces as the final reality," he explained that the "view that no other explanation can exist except that of the motion of material points, the laws of which are determined by central forces, had generally been abandoned." Mechanics provides only a picture, but it is the only rigorously developed picture to date, one which might be capable of being brought to perfection in the future. Boltzmann allowed that energetics upon further development might

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[38] Wilhelm Ostwald, *Lehrbuch der allgemeinen Chemie*, vol. 1, *Stöchiometrie*, 2nd ed. (Leipzig, 1891); "Studien zur Energetik," *Verh. Sächs. Ges. Wiss.* 43 (1891): 271–88, and 44 (1892): 211–37; Erwin N. Hiebert, "The Energetics Controversy and the New Thermodynamics," in *Perspectives in the History of Science and Technology*, ed. D. H. D. Roller (Norman: University of Oklahoma Press, 1971), 67–86, on 75; Holt, "A Note."

[39] Wilhelm Ostwald, "Die Ueberwindung des wissenschaftlichen Materialismus," *Verh. Ges. Deutsch. Naturf. u. Ärtze*, vol. 67, pt. 1, 1st half (1895): 155–68.

[40] In the equation Helm derived from the energy principle, $(mx'' - X)dx + (my'' - Y)dy + (mz'' - Z)dz = 0$, he regarded dx, dy, dz as independent variables, pointed out in note 34 above. By setting dy and dz, but not dx, equal to zero, he obtained $mx'' - X = 0$, but by setting $dy = dz = 0$, he also assumed $y' = z' = 0$, since $dy = y'dt$, and $dz = z'dt$. So Helm proved that for the special case in which the material point has a non-vanishing velocity in the x-direction only, the equation $mx'' - X = 0$ holds. He did not prove that the three equations for the three coordinate directions all hold. Boltzmann pointed out that Helm confused the differential dx with the variation δx (Ludwig Boltzmann, "Ein Wort der Mathematik an die Energetik," *Ann.* 57 [1896]: 39–71, on 40–41).

"still be of the greatest use in science," and he was interested in the analogies between the various forms of energy, but his enthusiasm did not extend to Helm and Ostwald's belief that energy laws are the "fundamental laws of theoretical physics."[41]

Planck did not take part in the debate at the Lübeck meeting, but he had corresponded with Ostwald on the subject, and in light of the wide interest following the debate, he felt that it was time to make his views on energetics public. For his well-known preference for the methods of thermodynamics over those of the kinetic theory of gases, he might have been expected to side with Helm and Ostwald and to oppose Boltzmann, but that is not where he came down on the issue.[42] In a criticism of the "new energetics" in the *Annalen,* Planck did not defend the mechanical world picture, which he said would require a deep and difficult study, but instead he pointed out the inadequacy of energetics, a simpler task. His most serious objection was that energetics failed to recognize the fundamental distinction between reversible and irreversible processes in nature. In general, he regarded energetics as lacking sound methods and foundations, as confusing its proofs with disguised definitions, and as being occupied with metaphysics instead of science. Its only success had been to incite younger scientists to "dilettantish speculations instead of a thorough absorption in the study of the present masterworks and thereby to lay fallow for years a broad and fruitful area of theoretical physics."[43]

Energetics, for all its vulnerability, stimulated a valuable discussion of the foundations of theoretical physics. Boltzmann and Planck objected to energetics for what they saw as its errors and its theoretical ambitions. Boltzmann claimed that Helm did not give a simpler, clearer, and more comprehensive form of the theory of mechanics, and that he did not avoid "pictorial representations." In reality, he said, Helm relied on the "theoretical picture" of material bodies as constituted of material points, which exert mutual attractions and repulsions. For his part, Helm gave a spirited defense of his ideas. In a second book, in 1898, he presented energetics as a "unified development of thought," to "be understood as a whole," not piecemeal as its opponents tended to view it. Of the two directions of energetics, the mechanical and the thermodynamic, he was partial to the latter, which was free of mechanical pictures, closer to experience. He thought that physicists who used mechanical pictures still silently yearned

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[41] Boltzmann, "Ein Wort," 64, 71; Ludwig Boltzmann, "Ein Vortrag über die Energetik," 11 February 1896, *Vierteljahresber. d. Wiener Ver. z. Förderung des phys. u. chem. Unterrichts* 2 (1896): 38; repr. in Boltzmann, *Wiss. Abh.*, vol. 3, 558–63, on 558.

[42] Max Planck, "Allgemeines zur neueren Entwickelung der Wärmetheorie," *Zs. f. phys. Chemie* 8 (1891): 647–56; repr. in *Phys. Abh.*, vol. 1, 372–81, on 372–73; Hiebert, "Energetics Controversy," 73.

[43] Planck had the same concern as Boltzmann, who observed that young scientists "who do not have the mathematical criticism necessary for successful work in the area of theoretical physics" had begun to look to energetics for its promise of quick and easy rewards (Boltzmann, "Ein Wort," 64; Planck, "Gegen die neuere Energetik," 465).

for a real mechanical world behind them. He considered atomism and mechanical pictures as heuristic aids, nothing more. He said that all we know of the physical world is phenomena and their changes, and a proper theory is a quantitative description of their relations.[44]

Although the specific program of energetics did not command a large following,[45] German physicists did not dispute the importance of the concept of energy for physics. Around the turn of the twentieth century, certain energetic notions entered lectures and textbooks on physics, making it clear to the audience that through the conservation principle and the transformability of the different forms of energy, energy holds physics together.

What advantages energetics might have as an alternative to mechanical physics arose from thermodynamics, to which it assigned a reforming role for all of physical science. Energetics had little to say about electromagnetism, which was beginning to be seen by many physicists as the theoretical foundation of their science, as an alternative to mechanics. Like thermodynamics and the energy principle, electromagnetism generated a widespread discussion about the methods of theoretical physics and the coherence they brought to the field of physics.

15.3 Electromagnetism

Early German writers on Maxwell's electromagnetic theory presented its relationship to mechanics with varying emphases. Boltzmann derived the equations of the theory from mechanical ideas in his textbook of 1891–93. Arthur Korn wrote that it was the duty of every theorist working in electricity to try to derive Maxwell's equations, preferably from mechanical hypotheses, as Boltzmann had done.[46] Hermann Ebert wrote that the great advantage of Maxwell's over other electrical

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[44] Robert Deltete, "Helm's History of Energetics: A Reading Guide," to Georg Helm, *The Historical Development of Energetics,* trans. with intro. R. J. Deltete (Dordrecht, Boston, London: Kluwer Publishers, 2000), 4–45, on 11, 16–17, 26–28.

[45] Einstein, for example, said that Planck's criticism of energetics "undoubtedly exercised a significant influence on his colleagues" because it showed that "energetics is worthless as a heuristic method" (Albert Einstein, "Max Planck als Forscher," *Naturwiss*. 1 [1913]: 1077–79, on 1077).

[46] Arthur Korn, *Eine Theorie der Gravitation und der elektrischen Erscheinungen auf Grundlage der Hydrodynamik*, 2 vols. (Berlin, 1892, 1894), vol. 2, 1. This work was the basis of Korn's Habilitationsschrift at Munich University, which the physics professor Lommel commended as belonging to the "ever increasing efforts to derive the phenomena mentioned, especially the electric and magnetic ones, from the principles of mechanics" (Dean to Munich Philosophical Faculty, 1 July 1895, Munich UA, OCI 21).

theories is that its equations could be derived mechanically. Helm, Voigt, Sommerfeld, Richard Reiff, and others published mechanical studies of Maxwell theory in the early 1890s.[47]

By contrast, Főppl in his published lectures on Maxwell's theory based the electromagnetic equations first of all on experimental facts; following Boltzmann, he provided the equations with a mechanical derivation too, but at the end of his text, not at the beginning. Drude similarly derived the equations of electromagnetism from experimental facts in his text on the physics of the ether. He did not think that beginning students needed to study the ether from mechanical principles, especially since it was an open question whether the equations of the ether should be referred back to the equations of mechanics or whether the reverse would prove more useful.[48] Wien criticized attempts by Maxwell and his successors to found the electromagnetic system of equations on Newtonian mechanics, considering them too complicated or hypothetical to correspond to the canons of good physical theory. If physicists followed Hertz and Oliver Heaviside, who regarded Maxwell's equations and concepts as closed, they would see that Maxwell's system is completely analogous to the system of pure mechanics, and that their only connection is through the concept of energy.[49] Over the next years Wien did not change his opinion about the undesirability of mechanical approaches to electromagnetism, but he changed his opinion about their connection. With a number of other physicists, he came to view electromagnetism as the foundation of mechanics.

In his published lectures on thermodynamics in 1897, Planck observed that thermodynamics might eventually be derived from an electromagnetic theory if not from a mechanical one. Two years later he concluded a talk on Maxwell's electromagnetic theory by projecting its relationship to thermodynamics:

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[47] Hermann Ebert "Versuch einer Erweiterung der Maxwell'schen Theorie," *Ann.* 48 (1893): 1–24, on 4; Georg Helm, "Die Fortpflanzung der Energie durch den Aether," *Ann.* 47 (1892): 743–51; Woldemar Voigt "Ueber Medien ohne innere Kräfte und über eine durch sie gelieferte mechanische Deutung der Maxwell-Hertz'schen Gleichungen," *Ann.* 52 (1894): 665–72; Richard Reiff, "Die Fortplanzung des Lichtes in bewegten Medien nach der electrischen Lichttheorie," *Ann.* 50 (1893): 361–67; Arnold Sommerfeld, "Mechanische Darstellung der electromagnetischen Erscheinungen in ruhenden Körpern," *Ann.* 46 (1892): 139–51.

[48] August Föppl, *Einführung in die Maxwellsche Theorie der Elektricität* (Leipzig, 1894), v–xi, 266–73; Paul Drude, *Physik des Aethers auf elektromagetischer Grundlage* (Stuttgart, 1894), vi.

[49] Wilhelm Wien, "Ueber die Bewegung der Kraftlinien im electromagnetischen Felde," *Ann.* 47 (1892): 327–44, on 328. Another example from this time of the questioning of the mechanical direction in electrical theory is a treatise on mathematical physics by the mathematician Carl Neumann, who thought that mechanical explanations in physics were incomplete, contradictory, and overly complex. Having studied the analogies between electrodynamics and mechanics, specifically hydrodynamics, which Helmholtz, Kirchhoff, Boltzmann, and others had developed, Neumann convinced himself that the analogies lacked "deep foundations." Moreover, the explanation of heat phenomena requires *specifically* thermal principles, and since heat is intimately related to electricity, Neumann did not expect "merely mechanical principles" to succeed with electricity either (Carl Neumann, *Beiträge zu einzelnen Theilen der mathematischen Physik, insbesondere zur Elektrodynamik und Hydrodynamik, Elektrostatik und magnetischen Induction* [Leipzig, 1893], iii–iv, 205–6).

"Perhaps one day we could succeed to an electromagnetic theory of heat—not by means of special new hypotheses, but simply by the further development of Maxwell's ideas on the connection between light and electricity." For some time this prospect guided Planck in a series of researches on the second law of thermodynamics.[50]

In his new researches, Planck applied thermodynamics to radiation processes. In his inaugural lecture at the Prussian Academy of Sciences in 1894, he spoke of his interest and his method: "there is hope that we will also be able to achieve a more detailed understanding of those electrodynamic processes that are directly conditioned by temperature—as in heat radiation in particular—without having to make the laborious detour through the mechanical explanation of electricity."[51] The following year, he submitted his first paper on the subject to the Prussian Academy. Building on Hertz's treatment of electric oscillations using Maxwell's theory, he gave an explanation of thermal equilibrium based on the absorption and emission of electromagnetic waves by electric resonators of dimensions small relative to the wavelength. He followed this first paper with another at the Prussian Academy in 1896, in which he introduced the notion of the damping of electric oscillations through radiation. The most important property of the damping is that it is conservative, which suggested to Planck the "possibility of a general explanation of irreversible processes through conservative actions—a problem that faces theoretical-physical research with greater urgency every day."[52]

Planck incorporated his results of 1895 and 1896 in his next work entitled "On Irreversible Radiation Processes," which appeared as a series of papers in the proceedings of the Prussian Academy between 1897 and 1901.[53] He gave a full statement of his objective: the principle of conservation of energy and the principle of the increase of entropy should be placed on the same foundations, and since the energy principle requires that all natural processes be governed by conservative

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[50] Planck, *Thermodynamics*, ix; "Die Maxwell'sche Theorie der Elektricität von der mathematischen Seite betrachtet," *Jahresber. d. Deutsch. Math-Vereinigung* 7 (1899): 77–89; repr. in *Phys. Abh.*, vol. 1, 601–13, on 613.

[51] Planck, "Antrittsrede," 3.

[52] Max Planck, "Absorption und Emission electrischer Wellen durch Resonanz," first published in the Prussian Academy's proceedings in 1895, then in *Ann.* 57 (1896): 1–14; repr. in *Phys. Abh.*, vol. 1, 445–58; "Über elektrische Schwingungen, welche durch Resonanz erregt und durch Strahlung gedämpft werden," first published in the Prussian Academy's proceedings in 1896, then in *Ann.* 60 (1897): 577–99; repr. in *Phys. Abh.*, vol. 1, 466–88, on 469–70.

[53] This series of papers together with the preliminary papers in 1895 and 1896 is analyzed by Martin J. Klein in several publications, which include "Max Planck and the Beginnings of the Quantum Theory," *Arch. Hist. Ex. Sci.* 1 (1962): 459–79, on 460–64; "Thermodynamics and Quanta in Planck's Work," *Physics Today* 19 (1966): 23–32, on 25–26; *Paul Ehrenfest*, *The Making of a Theoretical Physicist* (Amsterdam and London: North-Holland, 1970), vol. 1, 218–24; by Hans Kangro, *Vorgeschichte des Planckschen Strahlungsgesetzes* (Wiesbaden: Franz Steiner, 1970), 125–48; and by Kuhn, *Black-Body Theory*, 34–37, 72–91. In our discussion of Planck here and, later on, of Planck and the quantum theory, we are indebted to the fundamental studies of Klein, Kangro, and Kuhn.

forces, the "fundamental problem" of theoretical physics is to refer the entropy principle to conservative forces too. Citing Zermelo's publications, Planck noted that the kinetic theory of gases assumes conservative forces, but because of the reversibility of the motions of molecules, every state of a system must eventually recur, so that this theory cannot completely solve the problem. In place of the kinetic theory, he introduced his picture of an electrical resonator in interaction with electromagnetic waves. The resonator is excited by absorbing waves and is damped by emitting them, and the incoming and the outgoing waves differ in form: an absorbed plane wave is emitted as a spherical wave, an irreversible change brought about by the conservative action of radiation damping of the resonator.[54] To facilitate the mathematical analysis, Planck imagined a geometrically simple arrangement: electromagnetic spherical waves are contained within a hollow reflecting sphere, at the center of which is an infinitesimal, linear resonator with a long, fixed wavelength and small damping.

Boltzmann was naturally interested in Planck's new approach to the second law of thermodynamics, which differed fundamentally from his own. He thought that Planck's might prove useful, but he was critical of an error in the reasoning: the equations of electrodynamics, like those of mechanics, could not do what Planck wanted of them, for they do not prohibit a reversal of the course of absorbed and emitted waves. If Planck wanted to explain irreversibility by electromagnetic waves, he had to assume a particular arrangement for their initial states.[55] Boltzmann's criticism led Planck to introduce the concept of "natural radiation," an electromagnetic analog of Boltzmann's molecular disorder. He excluded from consideration as not occurring in nature all radiation processes that lack the property of irreversibility; this required eliminating correlations between the phases of the electromagnetic waves and averaging the amplitudes of the field. He could now show that a certain function, which he called the combined "entropy" of the radiation and the resonator, can only increase. He thought that the extension of his theory to other cases would "succeed finally to a purely electromagnetic definition of entropy and with it also of temperature."[56] In his work in the 1890s Planck did not yet make use of the electron concept, which when joined to the electromagnetic field would result in a well-defined program for placing physics on electromagnetic foundations.

Before the advent of the electron, ideas of charged particles, or "ions," were commonly used as a theoretical aid by German physicists, who recognized that the interaction of magnetism and light in a transparent medium and other optical phenomena could not be dealt with without them. Helmholtz was important in furthering this method of research, making essential use of ions in his influential

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[54] Max Planck, "Über irreversible Strahlungsvorgänge. Erste Mittheilung," *Sitzungsber. preuss. Akad.*, 1897, 57–68; repr. in *Phys. Abh.*, vol. 1, 493–504, on 493–95.

[55] Ludwig Boltzmann, "Über irreversible Strahlungsvorgänge I," *Sitzungsber. preuss. Akad.*, 1897, 660–62; pt. 2, 1897, 1016–18; pt. 3, 1898, 182–87.

[56] Max Planck, "Über irreversible Strahlungsvorgänge. Vierte Mittheilung," *Sitzungsber. preuss. Akad.,* 1898, 449–76, repr. in *Phys. Abh.*, vol. 1, 532–59, on 533, 536, 556.

work on dispersion, and in his Faraday lecture in 1881 he spoke of "atoms of electricity" in connection with electrolysis. For a time, following Hertz's work, German physicists were reluctant to combine Maxwell's theory with atomistic ideas, preferring to treat the electromagnetic field from a phenomenological standpoint. This changed in the late 1890s when they became interested in the electron theory, especially in the form given to it by the Dutch theoretical physicist H. A. Lorentz, the principal source of which being the summary he published in German in 1895. In a text on the physics of the ether in 1894, Drude treated his subject from a purely phenomenological viewpoint, as expected from a pupil of Voigt, but in his text on optics in 1900, he presented ionic theories of dispersion, magneto optics, and optics of moving bodies, and that same year he produced a highly original electron theory of metals. Abandoning phenomenology, he took up Lorentz's ideas in his theories and lectures.[57] Lorentz's electron theory impressed German physicists for its clarity and its wide coverage of optical and electrodynamic phenomena.

Primarily as a result of his electron theory, Lorentz acquired a reputation in Germany comparable to Boltzmann's, and German faculties looked to him as they did to Boltzmann to fill ordinary professorships in theoretical physics. Lorentz declined the offers from Germany, but he maintained close working relationships with German physicists. From the start of his career, he was strongly attracted to theoretical physics. The subject of his dissertation at the University of Leiden in 1875 was physical optics, which he treated systematically from the standpoint of the new electromagnetic theory of light, the first to do so. His starting point was Helmholtz's action-at-a-distance theory of electromagnetism, which he regarded as less dependent on unconfirmed hypotheses than Maxwell's contiguous-action theory. The quality of his dissertation and his scientific promise in general were recognized, and in 1877 he was appointed to the new chair of theoretical physics at the University of Leiden, the first position of its kind in Holland. Subsequently, following Hertz, Lorentz approached electromagnetism through contiguous action rather than distance forces, but he departed from Hertz on the properties of the ether. Whereas Hertz assumed that moving ponderable bodies carry the ether with them, Lorentz regarded the ether as stationary and completely transparent to bodies moving through it. Having no mechanical connection with matter, Lorentz's ether interacts with matter solely through small, ponderable, rigid bodies carrying positive or negative charges, which are assumed to be contained in all molecules of ordinary bodies. Lorentz referred to them first as "charged particles," then in 1895 as "ions," and after 1899 as "electrons," from which his theory derived its permanent name. By means of his stationary ether and electrons, Lorentz constructed a consistent electrodynamics, which rejected action at a distance and retained electric particles, a fusion of British and Continental electrodynamics, as he characterized his theory. He accepted the

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[57] Jed Z. Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago and London: University of Chicago Press, 1985), 199–200; Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford: Oxford University Press, 2000), 319, 321, 331–32.

crux of Maxwell's theory, the propagation of electric action at the speed of light, and retained the clear understanding of electricity of Weber's and Clausius's theories.[58]

From the beginning, a central concern of the electron theory was the optics of moving bodies, the subject of Lorentz's most original work. Because the stationary ether of his theory is not dragged by a body moving through it, the Earth has an absolute velocity relative to it, which ought to be detectable by optical or electromagnetic means.[59] The effects of this ether "wind," which are measured by the ratio of the Earth's motion v to the speed of light c, were not observed, and Lorentz had to explain why. He showed that by the theory, an unexpected compensation of actions eliminates all effects of the ether wind to a first-order approximation (neglecting terms involving the very much smaller second and higher powers of v/c). But second-order experiments had been performed, the most important being A. A. Michelson's interferometer experiment in 1881 and his and E. W. Morley's more accurate repetition of it in 1887, which did not detect the uncompensated actions. Lorentz's answer to these experiments was a hypothetical contraction, which G. F. Fitzgerald proposed independently at about the same time: if the arms of the interferometer contract by a factor of $\sqrt{}(1-v^2/c^2)$ in the direction of the Earth's motion through the ether, the second-order effect of the ether wind is eliminated. Lorentz regarded the hypothesis as dynamic, requiring the molecular forces that determine the shape of the interferometer arms to propagate through the ether analogously to electric forces.[60]

Independently of Lorentz, the electron theory was introduced and developed by other physicists, notably by Emil Wiechert, who was just beginning his teaching career at Göttingen, and by Joseph Larmor at Cambridge. They all assumed that the ether is stationary, and that the interaction between matter and the ether takes place through charged particles. In whichever formulation, the electron theory received a strong impetus from the experimental measurements of electric particles by J. J. Thomson in 1897. That same year several German physicists—Wiechert, Wien,

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[58] H. A. Lorentz, "La théorie électromagnétique de Maxwell et son application aux corps mouvant," *Arch. néerl.* 25 (1892): 363, repr. in *Collected Papers*, 9 vols. (The Hague: M. Nijhoff, 1934–39), vol. 2, 164–343; Tetu Hirosige, "Origins of Lorentz' Theory of Electrons and the Concept of the Electromagnetic Field," *HSPS* 1 (1969): 151–209; Russell McCormmach, "Einstein, Lorentz, and the Electron Theory," *HSPS* 2 (1970): 41–87; "H. A. Lorentz and the Electromagnetic View of Nature," *Isis* 61 (1970): 459–97; "Lorentz, Hendrik Antoon," *DSB* 8 (1973): 487–500.

[59] H. A. Lorentz, *Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern* (Leiden, 1895); repr. in *Collected Papers*, vol. 5, 1–137.

[60] In 1904, Lorentz gave a general proof, using the "Lorentz transformations," of the undetectability of the earth's absolute motion through the ether, replacing the separate explanations for the absence of first-order and of second-order effects ("Electromagnetic Phenomena in a System Moving with Any Velocity Less Than That of Light," *Proc. R. Acad. Amsterdam* 6 (1904): 809; repr. in *Collected Papers*, vol. 5, 172–97). The Michelson and Morley interferometer experiment and its relationship to the electron theory and the contraction hypothesis are discussed in Loyd S. Swenson, Jr., *The Ethereal Ether: A History of the Michelson-Morley-Miller Aether-Drift-Experiments, 1880–1930* (Austin and London: University of Texas Press, 1972).

and Walter Kaufmann, an assistant at the Berlin physics institute—experimentally established to their satisfaction that cathode rays consist of electric particles, and these were identified with the hypothetical particles of the electron theory, universal atoms of electricity, the same whatever their source and however produced.

Before his experiments on electric particles, Thomson calculated a result having an important bearing on their interpretation. On the basis of Maxwell's theory, he showed that the self-induction of a moving charged sphere results in an effective mass that varies with the velocity of the sphere. This was confirmed experimentally in Germany by Philipp Lenard and Walter Kaufmann. In 1898 at Göttingen, Theodor Des Coudres raised the possibility that the mass of streaming negatively charged particles is only "apparent," arising entirely from self-induction. The same year Wiechert repeated his earlier suggestion that all matter consists of positive and negative particles and, as a result, that not only the mass of cathode ray particles but all mass may be electromagnetic in origin rather than an original property of matter.[61] Here then was the prospect of a new mechanics based on an electromagnetic interpretation of mass and, beyond that, of an electromagnetic foundation of all of physics.

Of the great forces of nature, only gravitation seemed to remain outside the electron theory. In 1900, Lorentz discussed a way of including it too, based on Mossoti's gravitational theory, which had recently been revived by Weber and Zőllner. According to this theory, a ponderable particle is a compound of two opposite electric atoms, and the attraction between two ponderable particles is greater than their repulsion. Substituting electrons for the older electric atoms and states of the ether for distance forces, Lorentz derived a gravitational law that is identical with Newton's when there is no motion between the two attracting bodies; but if there is motion, the new law of attraction also involves velocity-dependent terms, which, Lorentz noted, are analogous to those appearing in the electrodynamic laws proposed by Weber, Clausius, and Riemann. Applying his law to the secular motion of the planet Mercury, he found that it does not fit as well as Weber's law, but he was not especially troubled, since his main purpose was to show that gravitation can be understood as an action that propagates through the ether. Convinced that the properties of the ether, as revealed in electromagnetic investigations, set conditions on all theories, he supposed that the ether does not allow the communication of any action—electromagnetic, molecular, or gravitational—at a

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[61] Theodor Des Coudres, "Ein neuer Versuch mit Lenard'schen Strahlen," *Verh. Phys. Ges.* 17 (1898): 17–20. Following Helmholtz's proposal of 1881 of an atomistic constitution of electricity, Wiechert speculated that the "electric atom" is a local modification of the ether and that the mass of matter is partly or wholly electromagnetic in origin ("Bedeutung des Weltäthers," *Physikal.-ökonom. Ges. zu Königsberg* 35 [1894]: 4–11). In 1896, he introduced an electron theory, in which he showed that upon the assumption that matter consists of charged particles, a complete electrodynamic theory of the interaction of matter and field can be formulated ("Über die Grundlagen der Elektrodynamik," *Ann.* 59 [1896]: 283–323). In 1898, Wiechert continued to speak of the possibility that all inertia is electromagnetic, but he believed that it was premature to assert that atoms of ordinary matter are nothing but aggregates of simpler electrical atoms ("Hypothesen für eine Theorie der elektrischen und magnetischen Erscheinungen," *Gött. Nachr.* [1898], 87–106).

speed greater than that of light.[62] In a paper in 1900 Wien set about to bring together the "now completely isolated areas of mechanical and electromagnetic phenomena and to deduce each valid differential equation from a common foundation." Wien's approach was the reverse of Hertz's. Whereas in reformulating mechanics, Hertz sought to supply proper mechanical principles for describing electromagnetic as well as mechanical phenomena, Wien sought to show that Newton's laws of motion together with his law of gravitation are only approximately correct, and that the correct laws are those of the electron theory.[63]

The electron theory was the electrodynamics of greatest authority in 1900, and its implications for the foundations of physics in general were widely acknowledged. Boltzmann, in his inaugural lecture at Leipzig in 1900, pointed out that mechanical explanations had extended their domain throughout all natural science only to lose favor at home, in theoretical physics, with physicists seeking to replace them with electromagnetic explanations by deriving the laws of mechanics from the theory of electromagnetism.[64] In 1901, Kaufmann related his experiments with Becquerel rays emitted by radium salts to efforts then under way to establish an electromagnetic physics, which he saw as the continuation of a 30-year historical development going back to Weber.[65] In 1905, Kaufmann's colleague at Göttingen the Privatdocent for theoretical physics Max Abraham placed electron dynamics on an electromagnetic basis, while acknowledging that the "electromagnetic world picture is so far only a program."[66]

Owing to certain formulas in the electrodynamics of moving bodies, the expectation arose that electrons cannot travel faster than light, and since ordinary matter contains electrons the expectation extended to it as well, as Lorentz argued. Des Coudres, whose interest in the problem was related to the question of electron mass, tried in vain to accelerate electrons to velocities exceeding the velocity of light. On theoretical grounds, the prospect of success had seemed unlikely to him, since such

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[62] H. A. Lorentz, "Elektromagnetische Theorien physikalischer Erscheinungen," *Phys. Zs.* 1 (1899–1900): 498–501, 514–19; repr. in *Collected Papers*, vol. 8, 333–52; "Considérations sur la pesanteur," *Versl. Kon. Akad. Wet. Amst.* 8 (1900): 603; repr. in *Collected Papers*, vol. 5, 198–215.

[63] Wilhelm Wien, "Ueber die Möglichkeit einer elektromagnetischen Begründung der Mechanik," *Arch. néerl.* 5 (1900): 96–104; repr. in *Ann.* 5 (1901): 501–13, on 501, 504.

[64] Boltzmann, "Zwei Antrittsreden," 255, 276.

[65] Walter Kaufmann, "Die Entwicklung des Elektronenbegriffs," talk given in a common session of the two main groups at the 1901 German Association meeting, published in *Phys. Zs.* 3 (1902): 9–15, on 14–15.

[66] Max Abraham, *Theorie der Elektrizität*, vol. 2, *Elektromagnetische Theorie der Strahlung* (Leipzig: B. G. Teubner, 1905), especially 143–47.

velocities would require infinite energy.[67] In 1905 Sommerfeld approached the problem theoretically, and failing to find an unforced motion for electrons moving faster than light, he concluded that this might be the only example of a "sensibly posed physical problem" without a solution; like others, he suspected that such a motion is impossible.[68]

At the meeting of the German Association in 1906, Planck's student Hans Witte gave a report on the mechanical interpretation of electromagnetism. Physicists had long been trying to found all of the branches of their science on a "unified system of concepts," he said. Through the energy conservation principle, they had attained a measure of success, so that now there were only two separate branches, mechanics and electrodynamics, thermodynamics having been fitted under the other two. That left as possible routes to the desired unity the reduction of electrodynamics to mechanics, the reverse reduction, or the derivation of both from a common foundation, or *Urprinzip*. Identifying and investigating nine types of mechanical theories, he concluded that a mechanical explanation of electromagnetism is impossible on the assumption of a continuous ether and unacceptable on the assumption of a discontinuous one. He eliminated from consideration mechanical explanations that do not depend on the assumption of an ether, since he was confident that physics could not do without it. The discussion following the report at the meeting shows that physicists were far from agreement on the fundamental point. Abraham did not think that Witte had laid to rest the debate over the mechanical foundations of physics. Gustav Mie doubted that mechanical theories were important for the further development of electrodynamics, but neither did he think that a "purely electromagnetic explanation of nature" was possible. The direction that the foundations of physics would take was still very much up in the air.[69]

The electron theory seemed close to becoming a universal physics, but the questions framed in the search for electromagnetic foundations could not all be adequately answered. Meanwhile, the rapid development of experimental atomic physics in other areas in the early years of the twentieth century brought into question the viability of any comprehensive theory founded on laws and concepts of contemporary physics. It was the same with the quantum theory of blackbody radiation and the theory of relativity. The latter theory called for the abandonment

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[67] Theodor Des Coudres, "Zur Theorie des Kraftfeldes elektrischer Ladungen, die sich mit Ueberlichtgeschwindigkeit bewegen," *Arch. néerl.* 5 (1900): 652–64. Des Coudres referred to Oliver Heaviside's early discussion, in 1888, of velocities greater than the velocity of light and to J. J. Thomson's theoretical argument in 1893 against their possibility and also to G. F. C. Searle's in 1897.

[68] Arnold Sommerfeld, "Zur Elektronentheorie. 2. Grundlagen für eine allgemeine Dynamik des Elektrons," *Gött Nachr.,* 1904, 363–439, on 384–402; "Zur Elektronentheorie. 3. Ueber Lichtgeschwindigkeits-Elektronen," *Gött. Nachr.,* 1905, 201–35, on 201–4.

[69] Hans Witte, "Über den gegenwärtigen Stand der Frage nach einer mechanischen Erklärung der elektrischen Erscheinungen," paper given at the 1906 meeting of the German Association, published in *Phys. Zs.* 7 (1906): 779–86, quotation on 784; this includes the discussion following Witte's report on 785–86.

of the ether, a concept integral to the electromagnetic world picture; it gave another reason for the dependence of mass on velocity; and in its application to electrodynamics, it yielded all of the testable laws of motion of electrons without having to make decisions about their shape, substance, and charge distribution. We look next at the introduction of the quantum and relativity theories and what they portended for the foundations of physics.

15.4 Thermodynamics and the Elementary Quantum of Action

Planck's research at the turn of the twentieth century was to prove his most consequential. As part of his electromagnetic investigations in thermodynamics, he introduced into physics a law of blackbody radiation containing two new universal constants, which would enter ubiquitously in atomic physics. Berlin was the center of experimental research on blackbody radiation, and so his new location of work turned out to be convenient. Research was also carried out in Hannover, which he kept in touch with through correspondence.

At the Physikalisch-Technische Reichsanstalt, work on blackbody radiation was performed by the optical group of the scientific section, which was seeking a useful primary luminous standard and a scientific foundation for the study of radiation. This research represented only a minor part of the work of the Reichsanstalt as a whole, but the optical group devoted a fair amount of effort to it in the 1890s and, from 1898, all of its resources.[70]

While Wien was employed in the optical group, he carried out fundamental theoretical studies of radiation, the most important of which he did unofficially.[71] In 1893 he derived a new relation between blackbody radiation and the second law of thermodynamics, later called the "displacement law": from Boltzmann's proof of the existence of radiation pressure and from Stefan's law for the total energy radiated at all frequencies from a unit surface of a blackbody in unit time, Wien showed that "in the normal emission spectrum of a blackbody, with a change in

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[70] David Cahan, "The Physikalisch-Technische Reichsanstalt: A Study in the Relations of Science, Technology and Industry in Imperial Germany," Ph.D. diss., Johns Hopkins University, 1980, 292–311.

[71] Wien also took an interest in the experimental side of the problem: in 1895 he and Lummer published a method for investigating the laws of blackbody radiation. To improve on the earlier techniques, which had to make do with imperfectly "black" bodies, they proposed to bring a hollow body to uniform temperature and to observe the radiation through a small opening. To try their proposal, they acquired hollow spheres of porcelain and metal capable of reproducing the radiation of a theoretical blackbody to "any approximation." Their "blackbody" was not a cavity enclosing radiating bodies, as Kirchhoff originally described it; the empty hollow body itself created the radiation (Wilhelm Wien, "Temperatur und Entropie der Strahlung," *Ann.* 52 [1894]: 132–65; Wilhelm Wien and Otto Lummer, "Methode zur Prüfung des Strahlungsgesetzes absolut schwarzer Körper," *Ann.* 56 [1895]: 451–56, on 453; Kangro, *Vorgeschichte*, 106).

temperature each wavelength is displaced such that the product of the temperature and the wavelength remains constant."[72] Three years later, making use of Maxwell's distribution law for molecular motions in thermal equilibrium, he derived a law for the distribution of energy in the emission spectrum of a blackbody: the intensity Φ_λ of radiation in the interval λ to λ + *d*λ is

$$\Phi_\lambda = \frac{C}{\lambda^5} e^{-c/\lambda\theta},$$

where C and c are constants and θ is the absolute temperature. Before the publication of his paper containing this law, Wien received a partial confirmation of his derivation from the Hannover physicist Friedrich Paschen, who from his experimental results independently derived a law equivalent to Wien's.[73] The same year that Wien announced his distribution law, he accepted an extraordinary professorship at the technical institute in Aachen, making it impossible for him to undertake the experimental testing of it. Others would do this, but since it required improved absolute measuring methods and more perfect blackbodies, it would be some time before new measurements were available.[74]

Wien had derived the radiation law from doubtful molecular assumptions, and Planck derived it from improved ones. The confidence he placed in his derivation is evident from his remarks in the fifth paper of his series on irreversible radiation processes: "the limits of validity of this law, in case there are any at all, coincide with those of the second fundamental law of the theory of heat." Further experimental tests of the law had "all the more fundamental interest" for being at the same time tests of the second law.[75] Planck worked on the law from the theoretical side, leaving the experiments entirely to others, a division of labor that proved to be no barrier to, and probably facilitated, progress in understanding blackbody radiation.[76]

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[72] Wilhelm Wien, "Eine neue Beziehung der Strahlung schwarzer Körper zum zweiten Hauptsatz der Wärmetheorie," *Siztungsber. preuss. Akad.* (1893): 55–62, on 62. This and other radiation studies by Wien are analyzed by Kangro, *Vorgeschichte*, 45–48, 93–113.

[73] Wilhelm Wien "Ueber die Energievertheilung im Emissionsspectrum eines schwarzen Körpers," *Ann.* 58 (1896): 662–69.

[74] Hans Kangro, "Das Paschen-Wiensche Strahlungsgesetz und seine Abänderung durch Max Planck," *Phys. Bl.* 25 (1969): 216–20; *Vorgeschichte*, 149–79.

[75] Planck, "Über irreversible Strahlungsvorgänge. Fünfte Mittheilung," 597.

[76] For the history of blackbody research leading to the quantum theory, there exist a number of detailed studies. Among the older studies, Léon Rosenfeld, "La première phase de l'évolution de la Théorie des Quanta," *Osiris* 2 (1936): 149–96. Among later studies, Martin J. Klein, "Max Planck and the Beginnings of the Quantum Theory"; "Thermodynamics and Quanta in Planck's Work"; *Ehrenfest*, 217–30; and "Planck, Entropy, and Quanta, 1901–1906," *The Natural Philosopher* 1 (1963): 83–108; Armin Hermann, *The Genesis of the Quantum Theory (1899–1913)*, trans. C. W. Nash (Cambridge, MA: MIT Press, 1971), 5–28; Kuhn, *Black-Body Theory*, 92–113; Kangro, *Vorgeschichte* 149–223. Here we do not give another self-contained history of blackbody research leading to the quantum theory, but draw on historical studies by others to illustrate features of the practice of theoretical physics around the turn of the century. In this we are guided by Kangro's observation that the route to Planck's law of blackbody radiation is an excellent example of the interaction between experiment and theory in modern physics, but our discussion is indebted to all of these sources.

Early in 1899 the experimentalists Otto Lummer and Ernst Pringsheim began to report on systematic departures of blackbody measurements from Wien's law (see Fig. 15.1).[77] Early the next year, they said that the departures, which increased with the temperature of the blackbody and with the wavelength of the radiation, "gained in theoretical interest" as a result of Planck's improvement over Wien's derivation of the law.[78] Using special prisms, improved bolometers, new forms of blackbodies, absolute measuring methods, and different arrangements, the experimentalists were able to extend their measurements to ever longer wavelengths and higher temperatures.[79] They also proposed variants of the law for the extended spectrum of blackbody radiation, for which they used the shorthand "black radiation."[80] At meetings and in technical publications, experimentalists and theorists critically discussed the measurements and the assumptions underlying the law.[81] At the German Association meeting in late 1900, when Wien suggested that recent experiments on blackbody radiation might be in error owing to the absorption of long waves by the atmosphere, he was answered by Pringsheim: "This source of error I consider completely

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[77] Otto Lummer and Ernst Pringsheim, "Die Vertheilung der Energie im Spectrum des schwarzen Körpers," *Verh. Phys. Ges.* 1 (1899): 23–41, on 34. Paper given at a meeting of the German Physical Society on 3 February 1899.

[78] Otto Lummer and Ernst Pringsheim, "Ueber die Strahlung des schwarzen Körpers für lange Wellen," *Verh. Phys. Ges.* 2 (1900): 163–80, on 166. Paper given by Pringsheim at a meeting of the German Physical Society on 2 February 1900.

[79] Kangro, "Das Paschen-Wiensche Strahlungsgesetz und seine Abänderung durch Max Planck."

[80] The expression "black radiation" came into common use at this time. Max Thiesen, one of the blackbody experimenters, justified it: "The concept of the (complete) blackbody was used by Kirchhoff in 1862; at the same time, he showed how the radiation from such a body can be realized independently of an actual blackbody. Since then, one has become accustomed to regard radiations independently of the radiating body, and I therefore recommend that radiation with the properties of that emitted by a blackbody be designated with a special name, most simply *black radiation*; the paradox of the expression disappears upon closer consideration, since the trivial and the scientific concept of blackness are not coextensive." ("Über das Gesetz der schwarzen Strahlung," *Verh. Phys. Ges.* 2 [1900]: 65–70, on 65. Paper given at a meeting of the German Physical Society on 2 February 1900).

[81] The critical "theorists" here were Planck and Wien. Planck wrote on 22 March 1900 that "the questions pending between the observers are also an inducement for me to state clearly, and to undertake a sharp criticism of, the theoretical suppositions that lead to the expression for the radiation entropy" ("Entropie und Temperatur strahlender Wärme," *Ann.* 1 [1900]: 719–37, on 720). In this paper Planck retracted his earlier conclusion that Wien's law is the only law that satisfies the second law of thermodynamics, but he still thought that Wien's law is the correct one, or that the experimental evidence against its general validity was not yet compelling. Wien wrote on 12 October 1900 that "lately the theoretical and experimental investigations of the radiation of the blackbodies have been the subject of many discussions" and that he "would like to discuss this question critically in still greater detail." ("Zur Theorie der Strahlung schwarzer Körper. Kritisches," *Ann.* 3 [1900]: 530–39, on 530). In this paper Wien repeated criticisms he had recently made of Planck's derivation of his, Wien's, radiation law. Wien's derivation was in turn criticized by Eugen Jahnke, Otto Lummer, and Ernst Pringsheim, "Kritisches zur Herleitung der Wien'schen Spectralgleichung," *Ann.* 4 (1901): 225–30; submitted on 12 December 1900.

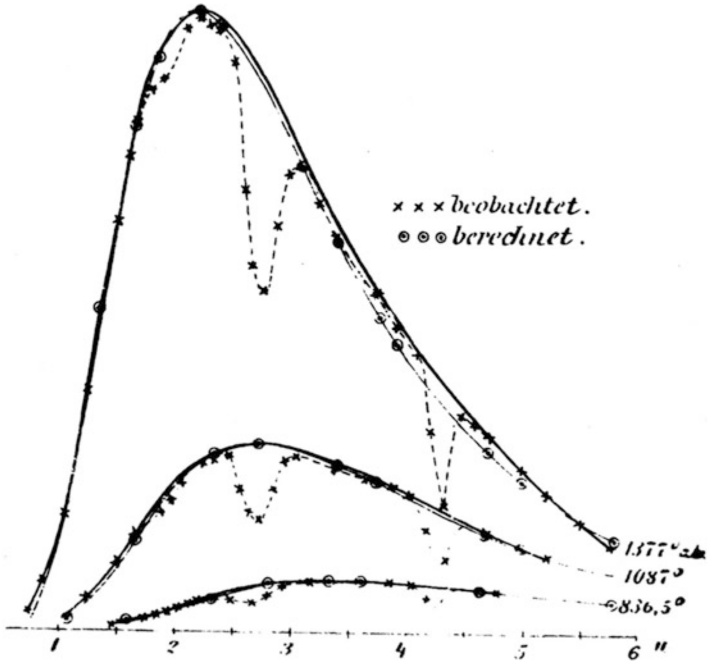

Fig. 15.1 Blackbody radiation curves, 1899. The *crosses* are observed values; the *circles* are calculated values (Reprinted from Otto Lummer and Ernst Pringsheim, "Die Vertheilung der Energie im Spectrum des schwarzen Kőrpers," *Verh. phys. Ges.* 1 (1899): 23–41, on 34)

excluded by the type of experiment." In a similar vein, when Wiener asked, "Is it really certain that even with the longest wavelength the absorption of the bolometer is taken precisely into account?" Pringsheim answered, "One knows that very precisely. We have checked that very precisely. For long waves it is much easier to produce a blackbody. There soot is already 92 % of a blackbody, porcelain 90 %." When Wiener persisted, "But for very long waves soot becomes transparent," Pringsheim answered, "That may be, but it does not matter here," and that was the final word on the matter at this point.[82] Blackbody experimenters placed confidence in the precision of their measurements.

At a meeting of the German Physical Society in October 1900, Planck proposed another "improvement," not a different derivation of Wien's law this time but a different law. He referred to the experimentalists Ferdinand Kurlbaum and Heinrich Rubens's earlier determination that Wien's law does not have "general significance" but is only a "boundary law," good for short waves and low temperatures,

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[82] Discussion following Wien's report, "Die Temperatur und Entropie der Strahlung," at the Aachen meeting of the German Association in September 1900, submitted on 20 October 1900 to the *Phys. Zs.* 2 (1900): 154–55.

and that a nearly linear relationship between energy and temperature applies to the other limit of long waves and high temperatures. On the basis of these empirical findings, Planck offered a formula in agreement with both limits. This improved law depended on a new expression for the entropy of a linear resonator in equilibrium with radiation, which Planck found by constructing "completely arbitrary expressions for the entropy," then selecting one that appealed to him for its simplicity. Using it together with the second law of thermodynamics and Wien's displacement law, he derived his new formula for the intensity of blackbody radiation:

$$E = \frac{C\lambda^{-5}}{e^{c/\lambda t} - 1},$$

where c and C are constants. Although more complicated than Wien's law, Planck's formula "satisfied the demands of thermodynamics and electromagnetic theory as completely," and he recommended it to the experimentalists.[83] On the same night following the meeting, Rubens compared his measurements with Planck's formula, and the next morning he called on Planck to tell him that he had found a satisfactory agreement.[84] Planck's law looked important.

The agreeable formula was only a beginning for Planck, who did not regard its relative simplicity the same as understanding. The form of the law had "only a very limited value," he said in his Nobel prize lecture, since even if it proved accurate it was at best "happily guessed"; from the start, he "was occupied with the problem of obtaining for it a true physical meaning."[85] A few days after his presentation to the Physical Society he received a letter from Lummer and Pringsheim containing a new formula they had worked out. "I immediately started to work on deciphering the theoretical meaning of your new formula," he replied to Lummer, "but unfortunately I have absolutely no idea what to make of it." From their formula, Planck determined the corresponding entropy, his own theoretical starting point, finding it

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[83] Max Planck, "Ueber eine Verbesserung der Wien'schen Spectralgleichung," *Verh. Phys. Ges.* 2 (1900): 202–4; paper given at a meeting of the German Physical Society on 19 October 1900. In place of the earlier connection between entropy S and energy U, $d^2S/dU^2 = \text{const.}/U$, from which Planck had derived Wien's law, he now wrote $d^2S/dU^2 = \alpha/U \cdot (\beta + U)$, from which he derived his improved distribution law replacing Wien's. On 7 Oct 1900, 12 days before the meeting at which Planck presented his new radiation formula and Ferdinand Kurlbaum and Heinrich Rubens presented their new measurements, he spoke with Rubens about their experiments then in progress (Max Laue, "Rubens, Heinrich," *Deutsches biographisches Jahrbuch*, vol. 4, *Das Jahr 1922* [1929]: 228–30, on 230; Kangro, *Vorgeschichte,* 200).

[84] Max Planck, *Wissenschaftliche Selbstbiographie* (Leipzig: J. A. Barth, 1948); repr. in *Phys. Abh.*, vol. 3, 394; "Scientific Autobiography," in *Scientific Autobiography and Other Papers*, trans. F. Gaynor (New York: Philosophical Library, 1950), 13–51.

[85] Max Planck, *Die Entstehung und bisherige Entwickelung der Quantentheorie* (Leipzig: J. A. Barth, 1920). This talk was given in Stockholm on 2 June 1920; reprinted in *Phys. Abh.*, vol. 3, 121–34, quotation on 125.

"so colossally complicated that probably even the most skillful mathematician would not succeed in putting it into a useful form." Planck wrote to Lummer:

> If the prospect should exist at all of a theoretical derivation of the radiation law, which I naturally assume, then, in my opinion, this can only be the case if it is possible to derive the expression for the probability of a radiation state, and this, you see, is given by the entropy. Probability presumes disorder, and in the theory I have developed this disorder occurs in the irregularity with which the phase of the oscillation changes even in the most homogeneous light. A resonator, which corresponds to a monochromatic radiation, in resonant oscillation will likewise show irregular phase changes, and on this the concept and the magnitude of its entropy are based. According to my formula, the entropy of the resonator would come to:
>
> $$S = \alpha \log \frac{(\beta + U)^{\beta+U}}{U^{U.}}$$
>
> and this form very much recalls expressions occurring in the probability calculus. After all, in the thermodynamics of gases, too, the entropy S is the log of a probability magnitude, and Boltzmann has already stressed the close relationship of the function X^X, which enters the theory of combinatorials, with the thermodynamic entropy. I believe, therefore, that the prospect would certainly exist of arriving at my formula by a theoretical route, which would then also give us the physical significance of the constants C and c.[86]

As he said, Planck "naturally" assumed that a theoretical derivation of his new radiation law could be found and that the route to it lay through Boltzmann's "theory of combinatorials." His letter to Lummer 1 week after the meeting of the Physical Society agrees with his recollection at the time of his Nobel prize: the question of a theoretical derivation of the law "of itself led me to a consideration of the connection between entropy and probability, thus to Boltzmann's train of ideas; after several weeks of the most strenuous work of my life, the darkness lifted, and a new, unexpected perspective began to dawn on me."[87]

By then Wien and Paschen had conceded that their original law failed for long waves, as Lummer and other experimentalists had previously reported. Only Wien still held to part of his original derivation, believing that there might have to be two sets of theoretical hypotheses, one for his law, which was confirmed for short waves, and the second for another law for long waves.[88] Planck was not interested in this but in a single set of hypotheses for a single law, which he soon found and referred to in a letter to Wien: "I now also have a theory for [my new formulae], which I will lecture on here at the Physical Society in 4 weeks."[89]

At this meeting of the Physical Society, on 14 December 1900, Planck opened his well-known paper: "Gentleman! Several weeks ago I had the honor of directing your attention to a new equation that seemed suitable to me for expressing the law

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[86] Planck to Lummer, 26 October 1900, Breslau UB, Lummer Nr. 222.

[87] Planck, *Die Entstehung und bisherige Entwickelung der Quantentheorie*, 125.

[88] Wien, "Zur Theorie der Strahlung schwarzer Körper. Kritisches."

[89] Planck to Wien, 13 November 1900, Wien Papers, STPK, 1973. 110.

of the distribution of radiating energy over all areas of the normal spectrum."[90] He went on to develop the theory for the entropy formula in his letter to Lummer, in the direction he had indicated there, using Boltzmann's combinatorials.[91] "Entropy means disorder," he said, and he located the disorder in the amplitude and phase of resonators. To determine the entropy of a resonator, he distributed the total energy over the many resonators constituting the system: energy E to the group of resonators vibrating with frequent ν, energy E' to a different group of resonators vibrating with the frequency ν', and so on. He then looked at the number of ways the energy of each group can be distributed among the resonators belonging to it. If each energy E is continuously divisible, there is an infinite number of ways in which the energy can be distributed. The "most essential point of the whole calculation," Planck said, is that E is made up of a large but finite number of "energy elements" ε, and that this ε is proportional to the frequency through a "constant of nature," $h = 6.55 \times 10^{27}$ erg. sec. With this hypothesis, he derived the equilibrium distribution of energy over all of the resonators, which enabled him to write the resonator entropy (following Boltzmann) as the logarithm of the equilibrium distribution, or probability, multiplied by a "second natural constant," k. In the outline of the theory in his first paper, Planck omitted most of the mathematical steps. If they are filled in, the average energy of the resonators with frequency ν comes out to be $U_\nu = h\nu/(e^{h\nu/kT}-1)$, having the right form. From the expression for the entropy of resonators as a function of U_ν and the energy element $h\nu$, the law of the spectral energy distribution u_ν of blackbody radiation follows:

$$u_\nu = \frac{8\pi h\nu^3}{c^3} \cdot \frac{1}{e^{h\nu/k\theta} - 1}.$$

This formula agrees with Planck's earlier "happily guessed" formula for the intensity of blackbody radiation, only now the constants c and C are replaced by h and k, universal constants on which he placed great significance. In the derivation, Planck departed from Boltzmann principally in his hypothesis, setting the energy element proportional to frequency and not allowing it to go to 0 in the limit, and he also determined the probabilities of the distribution of energy elements differently than Boltzmann. In a later paper Planck deduced the energy element from the theory rather than introducing it as a hypothesis. The way in which Planck brought

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[90] Max Planck, "Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum," *Verh. Phys. Ges.* 2 (1900): 237–45, on 237; paper given at a meeting of the German Physical Society on 14 December 1900.

[91] Rosenfeld inferred the existence of an entropy formula similar to the one Planck cited in his letter to Lummer early in Planck's search for a theoretical derivation, from which Planck worked backwards to the combinatorial formula ("La première phase de l'évolution de la Théorie des Quanta," 165–66). Klein recognized this as Planck's starting point in "Max Planck and the Beginnings of the Quantum Theory," 469–70. It is clear from Planck's remarks in his letter to Lummer that the resemblance of the entropy formula to formulas of the probability calculus pointed him in the direction of Boltzmann's approach to counting complexions in his probabilistic definition of entropy, as Kuhn argues in *Black-Body Theory*, 100–101.

together various parts of physics to derive the radiation law was original and subject to reinterpretation, which was not long in coming.[92]

That same month, December 1900, Planck's formula received further confirmation from Rubens and Kurlbaum, who extended their measurements to "as large a temperature range as possible." They noted that "for short waves and low temperatures, [Planck's] equation approximates Wien's, for long waves and high temperatures Lord Rayleigh's equation, and encompasses both as limiting cases."[93] At the German Association meeting the next year, Pringsheim noted that "Planck has replaced his theoretical derivation of Wien's equation by another train of thought, which makes his new spectral equation … seem theoretically probable"; and although from an experimental standpoint the question of whether or not Planck's law is a "complete expression of black radiation" was not yet closed, it "deserves to be preferred to all other spectral equations established so far and in any case comes very close to the truth."[94] Future experimental tests confirmed Planck's law with ever greater completeness (Fig. 15.2).

In 1901 Planck published a second theoretical derivation of the new blackbody radiation law in the *Annalen der Physik* and several more papers having to do with blackbody theory. After that, he published nothing more on the subject until he

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[92] Planck, "Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum," 238–40, 242. The constant c in the equation is the velocity of light. The first factor in the law, which comes from Maxwell's theory, entered Planck's earlier version of Wien's law; the resonator energy U_ν is converted to field energy u_ν through the relation $u_\nu = 8\pi\nu^2/c^3 \,.\, U_\nu$. The constant k in the second factor relates the entropy to the logarithm of the probability in Boltzmann's formulation of the second law. Boltzmann had not introduced this constant, and Planck calculated it here for the first time from his radiation law. With a value for k, Planck calculated Avogadro's constant and the "Boltzmann-Drude constant," or the average kinetic energy of an atom at unit absolute temperature; and with Avogadro's constant he calculated the charge of the electron. The exactness of these calculated constants depended on that of k, which Planck considered sufficient to conclude that his values surpassed "by far all previous determinations of these magnitudes"; he regarded the experimental test of these values as an important and necessary "problem for further research" (245) (Klein, *Ehrenfest*, 226, 229; Kuhn, *Black-Body Theory*, 105–9). As to why Planck departed from Boltzmann's treatment of the energy element, there are at least two interpretations. Klein thinks it is because Planck was unaware of the equipartition theorem of statistical mechanics. Kuhn thinks that Planck did not apply the energy element to the resonators and so did not have energy quantization in mind in 1900–1901, and that he did not come to an understanding of it until 1908; this is the likely explanation. What is certain is that Einstein and Ehrenfest soon recognized that Planck's theory required discrete energy, a break with the classical theories that Planck used to derive his radiation law (Purrington, *Physics in the Nineteenth Century*, 156–57).

[93] Heinrich Rubens and Ferdinand Kurlbaum, "Über die Emission langwelliger Wärmestrahlen durch den schwarzen Körper bei verschiedenen Temperaturen," *Sitzungsber. preuss. Akad.,* 1900, 929–41, on 931, 933. Paper presented to the Prussian Academy of Sciences on 25 December 1900. In 1900, Lord Rayleigh showed that the equipartition theorem of statistical mechanics implies that the distribution function of blackbody radiation is proportional to $\nu^2 T$, which he thought might apply to low frequencies.

[94] Otto Lummer and Ernst Pringsheim, "Temperaturbestimmung mit Hilfe der Strahlungsgesetze," submitted on 9 October 1901 to the *Phys. Zs.* 3 (1902): 97–100, on 97–98. Paper given by Pringsheim at the Hamburg meeting of the German Association in September 1901.

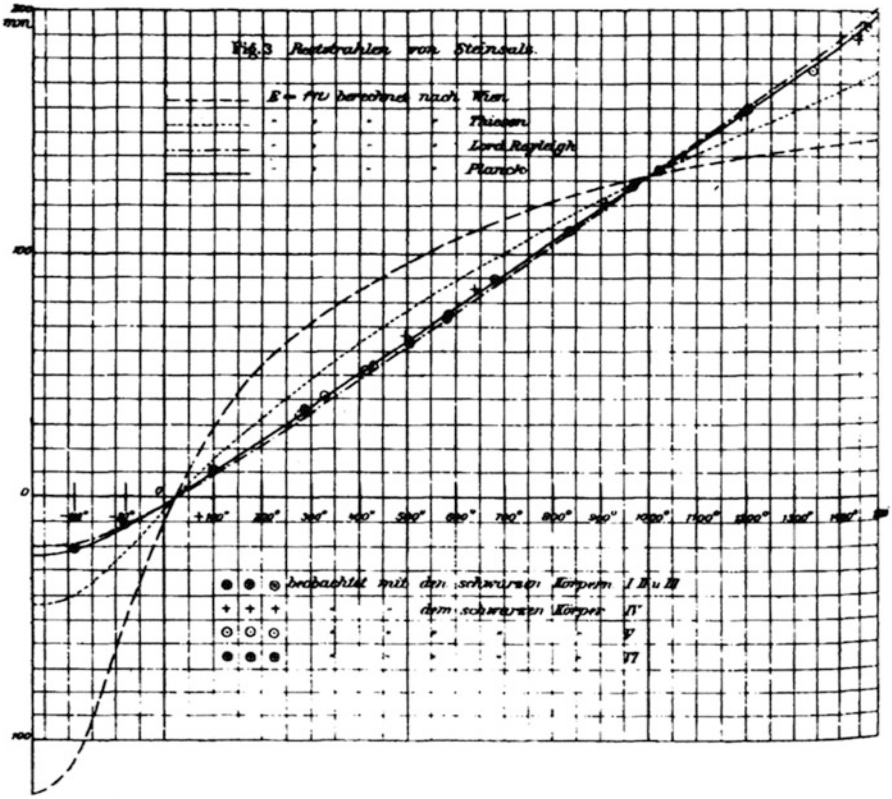

Fig. 15.2 Blackbody radiation curves, 1901. The *solid line* is calculated from Planck's formula. The *circles* and *crosses* are observed values (Reprinted from Heinrich Rubens and Ferdinand Kurlbaum, "Anwendung der Methode der Reststrahlen zur Prüfung des Strahlungsgesetzes," *Ann.* 4 (1901): 649–66, on 660)

brought out a new textbook on heat theory, based on lectures he gave in the winter semester of 1905–6, in which he presented the "entire theory of radiant heat on unified thermodynamic foundations." With these lectures, he explained to his students in Berlin and to his readers elsewhere his general approach to theoretical physics. The resonator, the mediating agency which brings matter and radiation in the blackbody into thermal equilibrium, is an idealization, the simplest conceivable electrical system for the purpose. There was no reason to think that such a system really exists in nature, nor was there any reason for Planck to worry if none did. He had Kirchhoff's radiation law, which is expressed in terms of a "universal" function that requires of the systems it governs only that they conform to the laws of physics, not their reality. Planck built his radiation theory from, and toward, such functions, constants, and laws that are independent of differences between materials, that are *universal*. The law of the normal spectrum of a blackbody is a "universal" law. Planck's starting point, the "universal validity" of the two fundamental laws of thermodynamics, justified his extension of them from matter to radiation. The laws of probability are "universal," too, and Planck reasoned that they should have a close connection with thermodynamics. Entropy and probability are related through the "universal constant of integration," k, which applies to "terrestrial" and to "cosmic" systems alike. Joined to it is the "new universal constant" h, the physical significance of which was yet to be determined. Since the formulas containing k and h have "absolute validity," these two constants together with the speed of light and the gravitational constant provide the basis for a "universal" system of "natural units" for length, time, mass, and temperature, measures of the absolute.[95]

The young physicist Einstein, for the past year a regular reviewer for the *Beiblätter* of the *Annalen der Physik*, admired Planck's lectures on heat for bringing together Kirchhoff's, Wien's, and Planck's own researches into a "wonderfully clear" theory,[96] which was exactly Planck's intention. This did not mean that Planck's readers were left with the idea that the theory of radiation was a closed book. He said that it had not yet been brought to a "fully satisfactory conclusion," since the significance of the constant h, which he proposed calling the "elementary quantum of action," was still undetermined. (The unit of h is that of "action," the product of energy and time.) He recognized that this constant was probably no less significant than the elementary electrical quantum, the electron charge; "naturally," he said, h must receive a "direct electrodynamic significance." He was accurate in anticipating that h would become the center of interest, though he could not have foreseen the far-reaching changes in the foundations of theoretical physics that the incorporation of the new universal constant would bring.[97]

The research we have sketched brought into play several of the developments in our account of German theoretical physics around the turn of the century. It included, first of all, a theoretical derivation of a natural law. The derivation brought together in one theory laws from several branches of physics: thermodynamics, statistical mechanics, and electromagnetism, an illustration of the interconnectedness of physics. The law had great generality, containing all known separate radiation laws: Wien's displacement law, Stefan-Boltzmann's law of total radiation, and Wien's and Rayleigh's laws for the limiting cases of short and

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[95] Max Planck, *Vorlesungen über die Theorie der Wärmestrahlung* (Leipzig: J. A. Barth, 1906) 60, 135, 137, 153, 163–65. This text is discussed in Kuhn, *Black-Body Theory,* 114–34.

[96] Einstein's reviews in the *Beiblätter* in 1905–1907 are discussed in Martin J. Klein and Allan Needell, "Some Unnoticed Publications by Einstein," *Isis* 68 (1977): 601–4.

[97] Planck, *Theorie der Wärmestrahlung*, 153, 179, and 221. In a letter at the time, Planck elaborated on the electrodynamic significance of h. The introduction of the "*finite* quantum of oscillation $\varepsilon = h\nu$ signifies a new hypothesis foreign to the resonance theory," so that a "new element enters the theory that in no case can be deduced in a logical way." Progress might be made by assuming that resonance oscillations are motions of electrons; for it seems that the "existence of an elementary quantum of electricity," the electron charge, provides a "bridge to the existence of an energetic elementary quantum h, especially as h is of the same dimensions and also of the same order of magnitude as e^2/c." Planck to Paul Ehrenfest, 6 July 1905, quoted in full, and in connection with Planck's 1906 lectures, in Kuhn, *Black-Body Theory*, 132, 288–89. In a long correspondence with Lorentz, Planck discussed the physical meaning of h and of the related need for the "electron theory to be enlarged by a new hypothesis" (Planck to Lorentz, 1 April 1908, Lorentz Papers, AR).

long wavelengths. Further, it was a universal law introducing new universal constants. Planck held a new, specialized position in physics: a theorist by preference who worked only in positions for a theorist, he carried out mathematical-theoretical work that was probably beyond the competence of the experimental experts in blackbody radiation. The solution to the blackbody problem, at the place where we leave it, was achieved rapidly through a close interaction of theoretical and experimental physicists, providing an early example of what was to become an increasingly familiar pattern of research in atomic physics. Through meetings of the Physical Society and the German Association and through personal interaction and correspondence, theoretical and experimental specialists came together to give reports and to learn from, and to criticize, one another's work. The research on blackbodies included the perfection of apparatus and the ever more refined precision measurements of physical laws and constants, the experimental groundwork of theoretical physics. The research was carried out in various institutions, one of which, the Reichsanstalt, embodied a related development we have discussed, the rise of technical physics and institutes to further industrial technology. The blackbody problem entered the teaching of theoretical physics, as seen in Planck's published lectures. Finally, it related to the world view of the principal theorist. Blackbody radiation and measurements of it at the Reichsanstalt interested Planck in the first place because of Kirchhoff's law, which says that the spectral energy distribution of a blackbody depends solely on the temperature and not on the nature of the emitting and absorbing body. The law represented an "absolute," something independent of man, and the search for the absolute appeared to Planck as the "most sublime scientific pursuit in life," the reason why he chose a life of science.[98] The first stage of the quantum theory shows that the making of a given theory of physics can call on many of the features of the work of theoretical physics, as discussed at the beginning of this book.

15.5 Relativity

Whereas quantum theory was a response to experimental physics, relativity theory arose in large part from theoretical issues, though experimental results also played a necessary part. In 1905 the *Annalen der Physik* published a paper on the electrodynamics of moving bodies by Albert Einstein.[99] He began it by calling attention to

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[98] Planck, "Scientific Autobiography," 13, 34–35.

[99] Albert Einstein, "Zur Elektrodynamik bewegter Körper," *Ann.* 17 (1905): 891–921. Among the historical studies of Einstein's theory of relativity of 1905 are several papers by Gerald Holton, which are collected in his book *Thematic Origins of Scientific Thought: Kepler to Einstein* (Cambridge, MA: Harvard University Press, 1973), and subsequent papers, especially, "Einstein's Scientific Program: The Formative Years," in *Some Strangeness in the Proportion*, ed. Harry Woolf (Reading, MA: Addison-Wesley, 1980), 49–65; Tetu Hirosige, "The Ether Problem, the Mechanistic Worldview, and the Origins of the Theory of Relativity," *HSPS*, 7 (1976): 3–82; Arthur I. Miller's several papers, which are largely incorporated in his book, *Albert Einstein's Special Theory of Relativity* (New York: Springer, 1980).

a theoretical problem, an asymmetry in “Maxwellian electrodynamics,” which provides two descriptions of a phenomenon where there should be only one: the current induced in a conductor by a magnet depends only on their relative motion, yet the theoretical account depends on whether the magnet or the conductor moves. Einstein next brought up the failure of experiments to detect the Earth’s motion relative to the ether. These and other examples, he said, suggest that there are no phenomena that correspond to the idea of absolute rest, whether they belong to mechanics, where this was established, or to electrodynamics.[100] It had been shown to first-order approximation that the same coordinate systems for which the equations of mechanics hold are also valid for the equations of electrodynamics and optics, a result Einstein raised to the level of a postulate, which he called the “principle of relativity”: the laws of nature are the same for all observers regardless of any uniform motion they may have with respect to one another. To this principle, he added as a second postulate the constancy of the velocity of light to all observers regardless of the motion of its source. Because he had no need of an “absolutely stationary space,” for his purposes the ether was “superfluous.” From the two postulates, he analyzed measurements of space and time, which led him to the relativity of the simultaneity of physical events and of lengths and time intervals. He derived the equations of transformation of the coordinates and the time for observers in uniform relative motion, which are identical with the transformations Lorentz had arrived at by different reasoning. They preserve the form of the “Maxwell-Hertz” equations of the electromagnetic field in empty space:

$$\tau = \frac{t - vx/V^2}{\sqrt{1 - (v/V)^2}}, \xi = \frac{x - vt}{\sqrt{1 - (v/V)^2}}, \eta = y, \zeta = z,$$

where x, y, z are the spatial coordinates and t is the time in one reference system, ξ, η, ζ and τ are the corresponding quantities in a second reference system moving at velocity v with respect to the first, and V is the velocity of light.[101] There are similar equations for motion in the y and z directions. The new “kinematics” provided Einstein with a method for solving problems in the electrodynamics of moving bodies by transforming the electric and magnetic fields to coordinate systems

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[100] Einstein, “Zur Elektrodynamik,” 891.

[101] Einstein, “Zur Elektrodynamik,” 892, 902. Newton accepted absolute space, but it is not accessible to experience. Before Einstein, there were many attempts to found the law of inertia without assuming absolute space, all of them subject to criticism. Newton’s absolute time also cannot be experienced; only the simultaneity of events can. These points are discussed by Ludwig Boltzmann, “On the Fundamental Principles and Basic Equations of Mechanics,” in *Philosophy of Science,* ed. J. J. Kockelmans (New York: Free Press, 1968), 246–60. In the theory of special relativity, absolute time is ruled out because of the impossibility of absolute simultaneity, and absolute space plays no role. According to this theory, the laws of physics are the same in all “inertial frames,” which are space and time coordinate frames that move with constant rectilinear velocity with respect to one another. Lorentz and Poincaré solved the problem of transforming Maxwell’s electromagnetic equations to a moving reference frame, though their approaches made use of the ether.

stationary with respect to the moving bodies. By adding convection currents to the Maxwell-Hertz equations, he arrived at the equations of Lorentz's electron theory, which he showed are compatible with the principle of relativity; he concluded his paper with a set of testable laws of motion of the electron.

Relativity theory made major advances in the connectedness of physics. Although Einstein regarded relativity theory as "simply a systematic development of the electrodynamics of Maxwell and Lorentz," it had consequences beyond the electrodynamic and optical problems his paper expressly treated. By renouncing the idea of absolute simultaneity, the theory provides another argument against action at a distance, simplifying physics by removing the need for a second kind of force in addition to the field. In the kinematics of relativity, space and time are connected, wherever they appear in physics. The velocity of light becomes the maximum possible velocity in nature, not just the maximum velocity of electrical bodies, and the dependence of mass on velocity applies to all ponderable bodies, not just to electrical ones.[102] Based upon statements about light signals, clocks, and rigid measuring rods, the theory requires the invariance of all basic physical laws under the Lorentz transformations.[103] Electrodynamics and mechanics are governed by the the same laws of space, time, and motion. In another paper in 1905, Einstein stated that all energy has mass; 2 years later, he stated the reverse, that all mass has energy, and he wrote the now well-known equation for the energy of a mass at rest, $E = mc^2$. He regarded the equivalence of mass and energy as the most important consequence of relativity theory, introducing another form of connectedness: the "principle of the conservation of mass" loses its independence and becomes "fused with that of the conservation of energy."[104] Because relativity theory applies to any class of phenomena, it belongs to the foundations of physics as a whole.

Einstein's paper on the electrodynamics of moving bodies had a long preparation. His professor at the polytechnic Heinrich Weber told him to read Wien's report on motion and ether at the German Association meeting in 1898, which included Michelson and Morley's experiment and Lorentz's method of incorporating it in the electron theory. Finding the report interesting, Einstein wrote to Wien about his own ideas on the subject. In 1899 he studied Hertz's *Electric Waves*. In a letter that year, he said that electrodynamics in its present form was wrong, and that he had ideas on how to correct it. In 1900, he read Boltzmann's work on gas theory. In 1901, he read Drude's electron theory of metals, with admiration, and in 1903 he set out to make a comprehensive study of the electron theory.[105] He encountered epistemological ideas in physics in his reading of Helmholtz, Hertz, Boltzmann, and Mach.

Einstein's relativity theory was a response to well-known problems of late nineteenth-century electrodynamics. Other physicists at the time were concerned

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[102] Einstein, "Zur Elektrodynamik," 919–20.

[103] Martin J. Klein, "Thermodynamics in Einstein's Thought," *Science* 157 (1967): 509–16, on 515.

[104] Albert Einstein, "What Is the Theory of Relativity?" 1919, in *Ideas and Opinions* (New York: Dell, 1973), 222–27, on 225.

[105] Darrigol, *Electrodynamics,* 374–76.

with the same problems and had some of the same responses. The asymmetry in the theoretical explanation of electromagnetic induction, with which Einstein began his paper in 1905, had been observed before. His rejection of the ether was not original. Emil Cohn presented a well-respected, phenomenological electrodynamics of moving bodies in 1900 and 1904, which avoided the ether and also electrons, atoms, and the Lorentz contraction. Alfred Bucherer retained Lorentz's equations but eliminated the ether in his electron theory in 1902 and 1904, and he also assumed the universal validity of the relativity principle, one of Einstein's postulates. Einstein's main innovation was the connection of time and space. What made his theory stand out was not its individual parts but the way he brought them together resulting in one of the most fruitful theories in the history of physics.[106]

Planck was important in building confidence in the new relativity theory.[107] The "determination and warmth" with which he stood up for it, Einstein said, was responsible "in great part for the attention this theory so quickly received among colleagues." Einstein singled out three contributions. Planck developed the relativistic equations of motion for a material point; he showed that the principle of least action has the same fundamental significance in the new theory as in classical mechanics; and he incorporated thermodynamic energy in the relativistic connection between energy and inertial mass. In a paper delivered at the German Physical Society in 1906, Planck began with the transformation equations from the "principle of relativity," which Lorentz had introduced in 1904 and Einstein, in "still more general wording," in 1905. To see if the theory leads to any absurdities, Planck derived the relativistic equations of motion that take the place of Newton's. Einstein's theory says that each of two coordinate systems in uniform relative motion has the same right to be used to express the fundamental equations of mechanics. Centering a prime coordinate system on a moving body of mass m for which Newton's equations of motion hold, $m\ddot{x}' = X'$, etc., Planck transformed the equations to an unprimed coordinate system moving with respect to the first, resulting in the following equations:

$$\frac{d}{dt}\left[\frac{m\,\dot{x}}{\sqrt{1-q^2/c^2}}\right] = \mathrm{X}, \mathrm{etc.}$$

Here q is the velocity of the mass in the unprimed coordinate system, and the factor $1/\sqrt{(1-q^2/c^2)}$ is a consequence of the space and time transformations of the relativity principle. If q is small compared with the velocity of light c, the equations of motion

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[106] Darrigol, *Electrodynamics*, 366–72, 377–78.

[107] Planck's response to relativity theory is discussed in Stanley Goldberg, "Max Planck's Philosophy of Nature and His Elaboration of the Special Theory of Relativity," *HSPS* 7 (1976): 125–60; "Early Response to Einstein's Theory of Relativity, 1905–1911: A Case Study in National Differences," (Ph.D. diss., Harvard University, 1969). Miller discusses Planck's and others' responses in *Albert Einstein's Special Theory of Relativity*, Planck's especially on 360–62, 365–67.

are almost indistinguishable from Newton's, and the relativistic equations appear as their generalization. By introducing the "kinetic potential," Planck showed that the relativistic equations of motion can be presented in the form of Hamilton's principle of least action.[108] The electron theory had shown that mechanics and electrodynamics could not be completely separate; relativity theory showed exactly how they were joined through the velocity of light, a further step in the connectedness of physics. Relativity theory was gratifying to Planck for the same reason his quantum theory was, as an expression of the absolute; relativity theory gives an absolute energy of a body, for example.[109] Most important, by eliminating the contingencies of individual observers, the vagaries of time, place, and individual intellects, it frees physics of anthropomorphic elements, the goal, he thought, of all genuine scientific understanding.

As Planck came to occupy a public position in German physics similar to Helmholtz's before him, he spoke at various occasions about the increasing unity of the physical world picture, to which his researches, like Helmholtz's, were directed. In a talk at Leiden University in 1908, he singled out two methods that theoretical physics has of reaching this objective: one is to place at the center of the world picture a single concept or principle, as Ostwald's energetics and Hertz's mechanics do; the other method is to admit into the picture only what is confirmed through direct experience, as Kirchhoff's mechanics does. Both methods are indispensable, Planck said.[110] In his lectures at Columbia University in 1909, he referred to the present "conflict between the mechanical and the electro-dynamic views of the world." He thought that the corresponding great division of physics into the physics of matter and the physics of the ether is probably not final, and that a suitably "generalized view of mechanics" is possible, following Helmholtz's train of thought.[111] The world picture of physics has a future, he said, and the reason is "simply the unity of the picture: unity of all separate parts of the picture, unity of space and time, unity of all experimenters, nations, and *kulturs*." The future picture will differ from past ones in that it will not leave out a single phenomenon. In realizing the highest goal of physics, it will bring together all observations into a "unified system, and, if possible, into a single formula."[112]

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[108] Max Planck, "Das Princip der Relativität und die Grundgleichungen der Mechanik." *Verh. Phys. Ges.* 8 (1906): 136–41; repr. in *Phys. Abh.*, vol. 2, 115–20; Einstein, "Planck," 1079.

[109] Max Planck, "From the Relative to the Absolute," in *Where Is Science Going?* trans. J.V. Murphy (New York: W.W. Norton, 1932), 170–200.

[110] Max Planck, "Die Einheit des physikalischen Weltbildes," *Phys. Zs.* 10 (1909): 62–75; repr. in *Phys. Abh.*, vol. 3, 6–29; "The Unity of the Physical Universe," in *A Survey of Physical Theory*, trans. R. Jones and D. H. Williams (New York: Dover, 1960), 1–26. Planck gave this talk in Leiden on 9 December 1908. He speculated that the division of physics in the future would be between reversible and irreversible processes rather than between mechanics and electrodynamics.

[111] Max Planck, *Eight Lectures on Theoretical Physics Delivered at Columbia University in 1909*, trans. A. P. Wills (New York: Columbia University Press, 1915), 9.

[112] Planck, "Unity of the Physical Universe," 1, 20.

15.6 Einstein on the Connectedness of Physics and the Torch of Mathematics

Ohm, the author of the first theory we discuss in this book, spoke of the "torch of mathematics" as his guide in understanding the physical world. To lift the darkness, he made good use of the mathematical methods coming out of France. The mathematics of physics together with its source changed in various ways after Ohm, but the torch proved enduring, as we see when we look again at Einstein, the author of the last theory we discuss.[113]

After earning his diploma at Zürich Polytechnic in 1900, Einstein worked in the Swiss patent office for several years before entering academic life. His teaching career followed Planck's example: between 1908 and 1912, he advanced through the ranks from Privatdocent through extraordinary professor to ordinary professor, in each case in a position designated for theoretical physics. The positions were in Switzerland and Austria. There had been no ordinary professorship for theoretical physics available in Germany during those years. When in 1913 a vacancy occurred in the Prussian Academy of Sciences, Einstein was appointed an ordinary member of the mathematical-physical class, with a salary comparable to that of the highest-paid university physics institute directors, and he was also allowed to lecture at Berlin University as an ordinary professor.

Throughout his life, Einstein held pronounced views on mathematics. Early on, he did not place a high value on advanced mathematics for theoretical physics. This was not owing to any mathematical limitations of his. In the gymnasium he attended in Munich, he was ahead of his classmates in mathematics and apparently in nothing else. Leaving the gymnasium before earning a diploma, he obtained a written statement from his mathematics teacher to the effect that his knowledge of mathematics was uncommon, qualifying him for advanced study in the subject. In the entrance examination for the Zürich Polytechnic, he scored ahead of the other candidates in mathematics, and he did well in physics too, while failing in other subjects.[114] His mathematical performance was sufficiently strong that the director of the polytechnic urged him to attend a cantonal school, earn a diploma there, and return. Although in his final examination at the polytechnic he did well in physical subjects, he again scored highest in mathematics.[115]

In the latter half of the curriculum for teachers of mathematics and physics at the polytechnic, students were expected to take seminars in advanced mathematics, but

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[113] This discussion of Einstein and the relationship of physics and mathematics is based in part on Russell McCormmach, "Editor's Foreword," *HSPS* 7 (1976): xi–xxxv. We thank Princeton University Press for permission to use this material.

[114] Philipp Frank, *Einstein. His Life and Times*, trans. G. Rosen, ed. and rev. S. Kusaka (New York: Knopf, 1947), 16, 18.

[115] Carl Seelig, *Einstein: A Documentary Biography*, trans. M. Savill (London: Staples, 1956), 54.

no one could stir Einstein to attend them.[116] According to Hermann Minkowski, one of the mathematics teachers there, Einstein "never bothered about mathematics at all," which made his later success in theoretical physics puzzling to him.[117] Eventually Einstein came to regret that he had passed up an opportunity of gaining a mathematical education at the polytechnic from outstanding mathematicians such as Minkowski and Adolf Hurwitz.[118]

Writing in 1905, Minkowski said that in the half-century since the death of Dirichlet the development of all parts of mathematics showed his spirit; namely, the desire for "fraternization of the mathematical disciplines, for the unity of our science." David Hilbert, Minkowski's colleague after the latter's move from Zürich to Göttingen, believed in the methodological unity of mathematics: "the question is forced upon us whether mathematics is once to face what other sciences have long ago experienced, namely to fall apart into subdivisions whose representatives are hardly able to understand each other and whose connections for this reason will soon become even looser. I neither believe nor wish this to happen; the science of mathematics as I see it is an indivisible whole, an organism whose ability to survive rests on the connection between its parts." Hermann Weyl, who moved to the Zürich Polytechnic as professor of mathematics in 1913, contrasted physics and mathematics in their respective tendencies toward integration and internal specialization: "Whereas physics in its development since the turn of the century resembles a mighty stream rushing on in one direction, mathematics is more like the Nile delta, its waters fanning out in all directions." He recognized as well that the apparently opposite "tendency of several branches of mathematics to coalesce is another conspicuous feature in the modern development of our science."[119]

For Einstein, mathematics was the Nile delta. In his "Autobiographical Notes," he recalled how the choice of a career in mathematics had looked to him when he was young. He decided against it in part because he was unsure of his way around in the field, lacking an intuitive feeling for what was important as opposed to mere erudition.

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[116] According to Einstein's biographer A. Reiser, cited in Gerald Holton, "Mach, Einstein, and the Search for Reality," *Daedelus* 97 (1968): 636–73, on 638.

[117] Seelig, *Einstein*, 33. It should be noted that the Zurich Polytechnic generally and Minkowski in particular did not attract many students in the advanced mathematics courses. When Einstein was in his second year at the polytechnic, Minkowski observed that there was only one student in the school with more than three semesters of mathematics, that the colloquium was sustained chiefly by assistants, and that in each of his three classes he had only about eight students (Minkowski to Hilbert, 23 November 1897, Hilbert Papers, Göttingen UB, Ms. Dept., 258/65).

[118] Einstein, "Autobiographical Notes," 15.

[119] Hermann Minkowski, "Peter Gustav Lejeune Dirichlet und seine Bedeutung für die heutige Mathematik," in Minkowski, *Gesammelte Abhandlungen*, ed. David Hilbert, 2 vols. (Leipzig and Berlin: B. G. Teubner, 1911), vol. 2, 447–61, on 450. Hilbert quoted in Hermann Weyl, "Obituary: David Hilbert 1862–1943," *Obituary Notices of Fellows of the Royal Society* 4 (1944): 547–53; repr. in Weyl, *Gesammelte Abhandlungen*, ed. K. Chandrasekharan (Berlin: Springer, 1968), vol. 4, 121–29, on 123. Weyl, "A Half-Century of Mathematics," *Amer. Math. Monthly* 58 (1951): 523–53; repr. in Weyl, ibid., 464–94, on 464–65.

He felt like Buridan's ass, unable to decide which specialty within mathematics he should enter, certain only that each specialty could exhaust a lifetime.[120]

He was drawn instead to the specialty of theoretical physics. He recognized that physics, like mathematics, was internally divided into parts, but he soon acquired an intuitive understanding of what was important in physics as he had not in mathematics. From the start, he responded strongly to the unifying potential of theoretical physics. After sending his first paper for publication to the *Annalen der Physik* in 1901, he wrote to his former polytechnic classmate Marcel Grossmann: "It is a magnificent feeling to recognize the unity of a complex of phenomena which appear to be things quite apart from the direct visible truth." As in mathematics, in physics it was possible to spend a lifetime in any one of its specialties, but it was also possible for a physicist to spend it seeking general principles that connect its specialties within a common framework.[121]

Part of the task of connecting the branches of physics was to analyze the mathematics of its fundamental theories. The debates of the 1890s on the foundations and methods of theoretical physics included discussions of the relationship of mathematics to physical concepts. Boltzmann, in his lectures on gas theory in 1895, which Einstein read as a student, argued that the description by differential equations of the inner motions of bodies leads compellingly to the concept of heat as the motion of the smallest particles.[122] Planck, who was allied to Boltzmann in opposition to the claims for mathematical phenomenology, told the German Mathematical Society in 1899 that the main physical idea underlying Maxwell's electromagnetic equations is contiguous action, and that the mathematics appropriate to this idea is fundamentally different than the mathematics of action at a distance.[123] Einstein developed the same point in his paper on light quanta in 1905. There is a "profound formal distinction," he began his paper, between the concepts of the atomic theory of matter and the concepts of Maxwell's electromagnetic theory of light. The emphasis is upon "formal," for he went on to describe the differences in the formalisms by which physicists presented these two branches of physics: in the atomic theory, the energy of a body is given by a discrete "sum" of the energies of a finite number of individual atoms and electrons, whereas in Maxwell's theory the energy is given by a "continuous spatial function." Observing that the continuous functions of Maxwell's theory refer only to time-average values of observations, he suggested that in describing the emission and transformation of light, the "theory of light which operates with continuous spatial functions may lead to contradictions

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[120] Einstein, "Autobiographical Notes," 15.

[121] Ibid. Einstein to Grossmann, 14 April 1901, in Seelig, *Einstein*, English trans., 53.

[122] Ludwig Boltzmann, *Lectures on Gas Theory*, trans. Stephen G. Brush (Berkeley: University of California Press, 1964), 27.

[123] Planck, "Die Maxwell'sche Theorie der Elektricität von der mathematischen Seite betrachtet," 603.

with experience."[124] He thought that physicists would gain a better understanding of such phenomena if they regarded light as behaving like a finite number of spatially localized energy quanta. In this way, he accompanied what he called his "very revolutionary"[125] hypothesis of light quanta with the suggestion that the concepts of matter and light may turn out not to be as formerly different as physicists had believed up to then. In later writings, Einstein characterized the separateness of atomic and field theories by the mathematics used in each: total, a form of ordinary, differential equations in the former and partial differential equations in the latter.[126] In calling attention to the different formalisms in the two theories, he had a similar kind of distinction in mind in 1905.

Given the problems of physical theory in 1905, Einstein was able to carry through analyses of the foundations of physics with modest mathematical tools: algebra, calculus, differential equations, and the probability calculus. The standard problems of electron theory entailed intricate calculations of cases: surface and volume electron charge distributions, rigid and deformable electron structures, slowly and rapidly accelerated electrons, and electron velocities over and under the velocity of light. Einstein showed little interest in these problems and in the associated mathematical investigations that occupied the electron theorists. He was interested in problems of the electron theory, but his attention was drawn to its foundation, which pointed to a fundamental problem of physics. Lorentz's electron theory was built on the dual concepts of discrete particle and continuous field and their respective formalisms, total and partial differential equations. In his paper on light quanta, Einstein reasoned that Maxwell's field equations needed to be revised before the conceptual and mathematical dualism could be resolved.[127]

Einstein told his students at the University of Zürich around 1910 that "in fact with mathematics one can prove anything" and all that mattered was the content. At this time he disagreed with a Swiss friend who believed reasonably enough that the total length of several matches laid end to end is the sum of the lengths of the individual matches; Einstein gave as his reason, "Car moi, je ne crois pas à la mathématique." After meeting Einstein in 1911, the physicist F. A. Lindemann reported that he "says he knows very little mathematics" but added that he "seems to have had a great success with them."[128] Einstein made the same self-deprecating observation in connection with the mathematical text on relativity theory by Max Laue in 1911,

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[124] Albert Einstein, "Einstein's Proposal of the Photon Concept," trans. A. B. Arons and M. B. Peppard, *Am. J. Phys*. 33 (1965): 367–74, on 367–68. This is a translation of Einstein's paper published in *Ann*. 17 (1905): 132–48.

[125] "Very revolutionary" was Einstein's own judgment at the time on his light quantum hypothesis. Einstein to Konrad Habicht, undated [1905], quoted in Seelig, *Einstein*, 89.

[126] Albert Einstein, "Maxwell's Influence on the Evolution of the Idea of Physical Reality" (1931), in *Ideas and Opinions* (New York: Dell, 1973), 259–63, on 261.

[127] McCormmach, "Einstein, Lorentz, and the Electron Theory."

[128] Quoted in F. W. F. Smith, Earl of Birkenhead, *The Professor and the Prime Minister: The Official Life of Professor F. A. Lindemann, Viscount Cherwell* (Boston: Houghton, Mifflin, 1962), 43.

complaining in a half-joking way that he could "hardly understand" it.[129] By then Einstein had begun his sustained work on the general theory of relativity, in the course of which he became aware of mathematical problems he had not confronted before and at the same time of the extent of his own mathematical limitations. In 1912, he wrote to Sommerfeld that "with the aid of a local mathematician who is a friend of mine [Marcel Grossmann] I believe I will now be able to master all difficulties. But one thing is certain that in all my life I have never struggled as hard and that I have become imbued with great respect for mathematics, the subtler parts of which, in my simple-mindedness, I had considered pure luxury up to now! Compared with this problem, the original theory of relativity is child's play."[130] From this time on, Einstein never doubted the physical relevance of certain of the subtler parts of mathematics. His correspondence sometimes dealt with mathematical matters that were now at the center of the physical argument; for example, writing to Lorentz in 1913, he said that mathematicians had not developed group theory sufficiently for his needs.[131] He selected his closest collaborators such as Grossmann in part for their mathematical competence. In his Herbert Spencer Lecture at Oxford in 1933, he went so far as to claim that in theoretical physics the "creative principle resides in mathematics."[132]

In his "Autobiographical Notes," Einstein took pains to clarify his understanding of the relationship of mathematics and theoretical physics. He said that he had abandoned his early attempts to discover by trial and error the field equations for light quanta and electrons. He realized that even if he had hit upon them, he would not have gone deeply into the matter; the equations would have remained arbitrary. He recognized that the development of physics depended on finding principles applicable to all of physics, which took the form of statements about physically permissible equations. Accordingly, the development of physics could be seen as the progressive reduction of the ad hoc or empirical elements in its mathematical foundation. Einstein used his special and general relativity principles as guides in seeking the "simplest" equations for describing the total physical world; even the simplest equations are so involved, he learned, that they "can be found only through the discovery of a logically simple mathematical condition which determines the equations completely or [at least] almost completely."[133] So mathematically demanding was this way of building physical theories that he felt constantly inadequate to the task. He likened his mathematical efforts to "a man struggling to climb a mountain without being able to reach its peak." On his deathbed, he is said to have lamented, "If only I had more mathematics!"[134]

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[129] Quoted in Frank, *Einstein*, 206. Laue said that his presentation drew on the "usual mathematical equipment of the theoretical physicist," the calculus and vector analysis (Max Laue, *Das Relativitätsprinzip* [Braunschweig: F. Vieweg, 1911], vi).

[130] Einstein to Sommerfeld, 29 October 1912, in *Einstein/Sommerfeld Briefwechsel*, ed. Armin Hermann (Basel and Stuttgart: Schwabe, 1968), 26–27, on 26.

[131] Einstein to Lorentz, 14 August 1913, Lorentz Papers, AR.

[132] Albert Einstein, "On the Method of Theoretical Physics," Herbert Spencer Lecture delivered at Oxford, 10 June 1933, in *Ideas and Opinions*, 263–70, on 267–68.

[133] Einstein, "Autobiographical Notes," 37, 53, 69, 89.

[134] Peter Michelmore, *Einstein, Profile of the Man* (New York: Dodd, Mead, 1962), 198, 261.

When Einstein took up physics, the variety of mathematics used by physicists was expanding. Vectors, directed quantities, came into wide use in connection with the concept of a physical "field." The theoretical physicist Abraham wrote in 1899 that vector analysis could yield new laws of physics that would be hard to derive using the customary scalar analysis.[135] Although there were various schools of thought on the subject, for most purposes physicists used the form of vectors introduced by their fellow physicists Oliver Heaviside and Josiah Willard Gibbs rather than the earlier forms introduced by the mathematicians Hermann Grassmann and William Rowan Hamilton.[136] The majority of books introducing vectors were written by physicists,[137] who found vectors useful both in their research and in their presentation of physics in lectures and textbooks.[138] Since students came across both vectors and the older mathematical presentation, physicists might include the main equations in both notations.[139]

With their increasing attention to symmetries, invariances, and transformation properties of physical equations, theoretical physicists and mathematicians recognized a widening field of application of newer mathematical quantities and calculuses. Four-dimensional vectors were applied in the electron and relativity theories. Minkowski applied them in 1908 when exhibiting the Lorentz invariance of the fundamental electrodynamic equations, and he also applied Arthur Caley's matrix

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[135] Heinrich Weber, "Vectoren," in *Die partiellen Differential-Gleichungen*, vol. 1, 207–26; Abraham to Sommerfeld, 7 January 1899, Sommerfeld Correspondence, Ms. Coll., DM.

[136] In preparing the entry dealing with vectors for the *Encyklopädie der mathematischen Wissenschaften* ("Geometrische Grundbegriffe," vol. 4, in 1901), Abraham spoke of the difficult task of uniting the various "schools" of thought on the subject and of striking the right balance, with not too much "mathematical jargon" for the physicists and not too much "modern physics literature" for the mathematicians (Abraham to Sommerfeld, 23 February 1901, Sommerfeld Papers, Ms. Coll., DM). On the form of vectors preferred by physicists: Ludwig Prandtl, "Über die physikalische Richtung in der Vektor-analysis," *Jahresber. d. Deutsch. Math-Vereinigung* 13 (1904): 436–49, on 436.

[137] 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), 242. Some examples of texts on vectors by physicists are: Alfred Bucherer, *Elemente der Vektoranalysis mit Beispielen aus der theoretischen Physik*, 2nd ed. (Leipzig: B. G. Teubner, 1905); Richard Gans, *Einführung in die Vektoranalysis mit Anwendungen auf die mathematische Physik* (Leipzig: B. G. Teubner, 1905); Waldemar von Ignatowski, *Die Vektoranalysis und ihre Anwendung in der theoretischen Physik* (Leipzig and Berlin: B. G. Teubner, 1909–10); Eugen Jahnke, *Vorlesungen über die Vektorenrechnung. Mit Anwendungen auf Geometrie, Mechanik und mathematische Physik* (Leipzig: B. G. Teubner, 1905).

[138] For example: Voigt's 1889 textbook, *Elementare Mechanik*; Emil Budde, *Allgemeine Mechanik der Punkte und starren Systeme. Ein Lehrbuch für Hochschulen*, 2 vols. (Berlin, 1890–1891). A systematic vector presentation of Maxwellian electrodynamics and the electron theory is given in Abraham's revision of Föppl's 1894 textbook, retitled *Theorie der Elektrizität: Einführung in die Maxwellsche Theorie der Elektrizität* (Leipzig: B. G. Teubner, 1904), together with his own companion volume in 1905, *Theorie der Elektrizität: Elektromagnetische Theorie der Strahlung*.

[139] This was the plan of Clemens Schaefer, *Einführung in die theoretische Physik*, vol. 1, *Mechanik materieller Punkte, Mechanik starrer Körper und Mechanik der Continua (Elastizität und Hydrodynamik)* (Leipzig: Veit, 1914), as he explains it on p. iv.

calculus, which he explained at length.[140] Sommerfeld published a study in 1910 of the new algebraic and analytical methods appropriate to the Minkowski "absolute world" and its characteristic four-dimensional vector of space and time.[141] Drawing largely on Sommerfeld's work, Laue devoted a chapter in his book on relativity theory to "world vectors and world tensors" and associated mathematical concepts.[142] Gustav Mie made use of an elaborate apparatus of matrices and higher dimensional vectors in his development of an electromagnetic theory of matter from 1912, as did Max Born and others who investigated Mie's theory.[143] Tensors, which Voigt repeatedly urged physicists to adopt,[144] proved especially useful in the physics of fields to express concisely the equations for energy, momentum, stress, and mass. Abraham emphasized the tensor character of electron mass in his efforts to place physics on electromagnetic foundations.[145] Grossmann devoted the mathematical part of his and Einstein's paper of 1913 to a "general vector analysis," an extension of the vector analysis for four-dimensions recently developed by Minkowski, Sommerfeld, Laue, and others[146]; this paper introduced the compact tensor formulations of Einstein's general theory of relativity and gravitation.[147]

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[140] Hermann Minkowski, "Die Grundgleichungen für die elektromagnetischen Vorgänge in bewegten Körpern," *Gött. Nachr.*, 1908, 53–111, on 78–98. Minkowski's introduction of the four-dimensional space-time element is discussed in Gerald Holton, "The Metaphor of Space-Time Events in Science," *Eranos Jahrbuch* (1965): 33–78, on 68.

[141] Arnold Sommerfeld, "Zur Relativitätstheorie. 1. Vierdimensionale Vektoranalysis," *Ann.* 32 (1910): 749–76, 33 (1910): 649–89.

[142] Laue, *Das Relativitätsprinzip*, chap. 4.

[143] Gustav Mie, "Grundlagen einer Theorie der Materie," *Ann.* 37 (1912): 511–34, 39 (1912): 1–40, and 40 (1913): 1–66. Max Jammer remarks on Mie's use of matrices in *The Conceptual Development of Quantum Mechanics* (New York: McGraw-Hill, 1966), 206. Max Born, "Der Impuls-Energie-Satz in der Elektrodynamik von Gustav Mie," *Gött. Nachr.*, 1914, 23–36. In a footnote Born explained that in matrix multiplication, one multiplies row by column, pointing up the unfamiliarity of physicists with the properties of matrices at this time (33).

[144] Salomon Bochner, "The Significance of Some Basic Mathematical Conceptions for Physics," *Isis* 54 (1963): 179–205, on 193.

[145] Max Abraham, "Dynamik des Electrons," *Gött. Nachr.*, 1902, 20–41, on 28.

[146] Albert Einstein and Marcel Grossmann, "Entwurf einer verallgemeinerten Relativitätstheorie und einer Theorie der Gravitation," *Zs. f. Phys.* 62 (1913): 225–61, on 244.

[147] The increasing compactness of physical mathematics can be traced through the sequence of Einstein's publications. In 1905 Einstein wrote Maxwell's equations as Hertz had, six equations for the two sets of x, y, and z components, to which two additional equations needed to be added, making *eight* altogether. In 1908 he wrote them using three-dimensional vector notation together with the vector operators of "curl" and "divergence"; the resulting equations were *four* in number, half as many. In 1916 he wrote the generalization of Maxwell's equations for the vacuum as *two* four-dimensional tensor equations:

$$F_{\rho\sigma}/\partial x_\tau + \partial F_{\sigma\tau}/\partial x_\rho + \partial F_{\tau\rho}/\partial x_\sigma = 0, \partial F^{\mu\nu}/\partial x_\nu = J^\mu.$$

Here the F components correspond to the electric and magnetic forces and the J to the current and charge (Einstein, "Zur Elektrodynamik bewegter Körper," 907; Albert Einstein and Jakob Laub, "Über die elektromagnetischen Grundgleichungen für bewegte Körper," *Ann.* 26 [1908]: 532–40, on 533; Albert Einstein, "Die Grundlage der allgemeinen Relativitätstheorie," *Ann.* 49 [1916]: 769–822, on 812–13).

The appearance of theoretical physics changed in the early years of the twentieth century. Readers of the theoretical parts of the *Annalen* routinely confronted four- and six-vectors, tensor masses, world-points and world-lines, and the rest of four-dimensional physics. They read about the symmetry properties of 16-term matrices or world-tensors and 10-member groups of motions, and they were directed to the mathematicians' literature on the theory of transformations.[148] In highly mathematical writings on relativity, Einstein might not be mentioned, but Minkowski would be for his representation of the Lorentz transformations as an imaginary rotation in space-time, Voigt for his exposition of vector and tensor products, and Sommerfeld for his extension of vector and tensor analysis to four dimensions.[149]

By the time of the Solvay Congress on the quantum theory in 1911 and of Einstein's completed general theory of relativity a few years later, physicists had a wide range of proven mathematical aids at their disposal. Textbooks and handbooks conveniently organized for their needs the essential materials; for example, differential equations, the series development of arbitrary functions, linear integral equations, linear transformations, the variational calculus, the probability calculus, non-Euclidean and four-dimensional geometries,[150] and "multiple algebras" such as those of complex numbers,[151] dyadics, quaternions, vectors, tensors, matrices, and groups.[152] Moreover, the rapid advance of physics at this time suggested that other branches of mathematics such as the calculus of finite differences might soon become the source of new standard techniques for theoretical physicists.

For all their need of mathematics, physicists did not confuse physics with mathematics. Volkmann put it sharply: "theoretical physics is an independent discipline, which had been enormously served by mathematics, but which will tolerate no mathematical baby halter."[153] Planck, as editor of the *Annalen*,

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[148] Van Alkemade, *Ann.* 38, 1033–40; Frank and Rothe, *Ann.* 34, 825–55, which follows the presentation of group theory in Sophus Lie and Georg Scheffers, *Vorlesungen über kontinuierliche Gruppen* (Leipzig, 1893).

[149] Laue, *Ann.* 35, 524–42; Epstein, *Ann.* 36, 779–95; Mie, *Ann.* 37, 511–34; Frank, *Ann.* 35, 599–606; Herglotz, *Ann.* 36, 493–533.

[150] Early examples are discussed in Ernst Wölffing, "Die vierte Dimension," *Umschau* 1 (1897): 309–14.

[151] Although complex numbers were used in classical physics, including and especially relativity theory, "basic conceptualizations and basic formulations continued to be presented and expressed in real variables only"; it was not until quantum mechanics that the "very basic equations" of the theory displayed the symbol *i* "openly and directly" (Bochner, "The Significance of Some Basic Mathematical Conceptions for Physics," 196).

[152] In his analysis in 1905 of the relativistic composition of velocities, Einstein observed that the transformations of coordinates between parallel, moving coordinate systems form a "group." Einstein, "On the Electrodynamics of Moving Bodies," 51; Henri Poincaré, "Sur la dynamique de l'électron," *Comptes rendus* 140 (1905): 1504–8, and *Palermo, Rend. Circ. Mat.* 21 (1906): 129–75, introduced group theory and four-dimensional vectors into Lorentz's electron theory. This point is discussed in Camillo Cuvaj, "Henri Poincaré's Mathematical Contributions to Relativity and the Poincaré Stresses," *Am. J. Phys.* 36 (1968): 1102–13, especially 1109–11 and 1113.

[153] Volkmann to Sommerfeld, 3 October 1899, Sommerfeld Papers, Ms. Coll., DM.

welcomed any interest shown by mathematicians in physics; he admired, for example, Hilbert's derivations of physical laws from a formal standpoint, but he acknowledged that "physically they bring nothing that is in the least new." Mie explained the difference, as he saw it, between a physicist and a mathematician. The physicist sees a problem as numerical, the "modern mathematician" does not. The physicist and the mathematician do not mean the same thing by terms such as "equal" and "infinity," and so they often misunderstand one another. To the physicist, "equal" means "within the limits of errors," and these limits vary from problem to problem; in one problem, two quantities are equal, in another unequal.[154] For Einstein and for other theoretical physicists of the twentieth century, as for Helmholtz and, for that matter, for Ohm, mathematics was an indispensable tool of their work, but it was not their goal. Physical understanding was their main goal, to which they brought to bear mathematical techniques, which varied with the development of both physics and mathematics. The physical problem of gravitation, which was solved by Einstein using a newer branch of mathematics, the absolute differential calculus, entailed the most far-reaching revision of the physical world picture before the invention of quantum mechanics, with its own characteristic mathematics, in the 1920s.

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[154] Planck to Wien, 4 October 1912, Wien Papers, STPK, 1973.110; Mie to Hilbert, 26 December 1917, Hilbert Papers, Göttingen UB, Ms. Dept.

Chapter 16
Concluding Observations

A reader of our book reminded us that we left out a "Conclusion." We were unsure what a conclusion of the study of the work of physicists would consist of, and we did not see the need for a summary at the end. For the use we anticipated for the book, primarily a reference work, a periodization is called for, and we supplied one. For those readers who have questions, we end this revision with observations that might answer them.

Theoretical Physics as an Empirical Field

From the beginning to the end of our history, German physicists commonly distanced their work from the speculations of nature philosophy, which lacked the necessary empirical support to qualify as science. The case for an empirical field is easy to make for experimental physics, since it is about observations of phenomena, but the one for theoretical physics is not straightforward, and in the early part of our period mathematical and hypothetical methods were regarded with wariness by experimental physicists. Helmholtz, the investigator most concerned with epistemological questions, made the case for theoretical physics as an empirical field in an address in 1871 on his predecessor at Berlin, Gustav Magnus. Nature philosophy was influential around the time Magnus developed as a physicist, and he took the side of experience over speculation. An accurate, methodical experimenter, his skepticism about research that was not experimental extended to the use of mathematical theory, an overreaction, Helmholtz believed. The necessary correction was made, he said, and it was "now understood that mathematical physics is a purely experimental science," based on the same "principles" as experimental physics. He acknowledged that earlier theorists had made use of atomistic hypotheses, and although he did not question the existence of atoms, he did question the hypotheses, since nothing definite was known about the atomic structure of matter. He thought that the empirical character of physical theory was convincingly demonstrated by Gauss, Neumann, and the latter's pupils in Germany, and by Thomson and Maxwell in Britain, using a method that does not depend on atoms. It is to build theories from the simplest knowable parts of bodies, infinitesimal volume elements, which yield

C. Jungnickel, R. McCormmach, *The Second Physicist*, Archimedes 48,
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laws that are "just as much under the control of experience as what are called experimental physics." Helmholtz saw theoretical physics as extending the work of experiment to arrive at "still simpler and still more general laws of phenomena."[1] At the end of our period, when atomism was seen to have a firmer empirical basis, Helmholtz's case for the empirical nature of theoretical physics would have to be updated.

We begin this book with Boltzmann's caution in 1895 that the idea of a theoretical physicist is "not without difficulty." He went on to say that theoretical physics is not mathematics, and other theoretical physicists at the time said the same. Because their work involved mathematics, and because it was done outside the laboratory, they felt the need to affirm what Helmholtz had said in his address to the Prussian Academy a quarter century earlier: theoretical physics belongs to the empirical sciences. It was a reminder that as the division of work progressed, physics remained intact, a science in which experimental and theoretical researchers collaborated in the pursuit of common empirical goals.

The Second Physicist

The division of physics into experimental and theoretical departments recognized a long-standing division of methods, though until the second half of our period there were no purely theoretical specialists. Over time, the division came to be formally recognized in teaching positions for a second physicist in German universities and technical institutes.

This development followed, in a sense, from the introduction of laboratory practices for students, the result of a heartfelt struggle by professors of physics. Their success created a problem for them: the work of heading an institute and conducting laboratory practices was a heavy burden for the lone physics professor, who also needed time for his own research. He welcomed a second physicist to help out, and a junior second position became standard. The second physicist's main teaching area was often mathematical physics, which was recognized as an integral part of physics and an expanding field. The position was sometimes explicitly specified for mathematical physics, and physicists with a decided preference for mathematical physics were considered in filling it. A practical benefit of dividing the teaching this way was that mathematical physics had relatively little need for apparatus, minimizing a potential conflict over the physics institute's collection of apparatus. Near the end of our period, theoretical physics began to be recognized as a career, and in a few instances the second physicist acquired an ordinary professorship complete with an institute for theoretical physics. The second physicist was then, as it were, the second first physicist in standing.

The Complete Physicist

Asked his opinion about physics appointments in 1884, Helmholtz said that it was desirable for a university to have special lectures on mathematical physics, and he

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[1] Hermann von Helmholtz, "Gustav Magnus. In Memoriam," in *Popular Lectures on Scientific Subjects*, trans. E. Atkinson (New York: Appleton, 1881), 1–25, on 16–19.

explained why. Lectures on experimental physics, which were attended by medical students, future government officials, and pharmacists, were not the place to introduce a "complete and rigorous formulation of the laws of nature, which demands a mathematical formulation and must after all be given to future teachers and mathematicians." If a university could not afford two teachers of physics, then one physicist would do if he had "extensive knowledge of the mathematical presentation of physical theories" and was an "experienced experimenter." Helmholtz did not approve of leaving the teaching of physics to mathematicians, since they tended "to treat physical problems only as paradigms for mathematical methods, without concerning themselves further with the relationship of their equations to reality." He did not go so far as to expect every physicist to be capable of doing original research in mathematical theory, but if a candidate for a position in experimental physics was thought to be deficient in his handling, or even in his appreciation, of mathematical methods in physics, his chances could be hurt. Helmholtz objected to a certain candidate because his "theoretical discussions" of experiments were "hardly intelligible and seemed ambiguous and arbitrary," and he wondered if in his teaching he could give the desirable "clear and sharp statement of the conceptual formulation [of] ... the most general laws of phenomena."[2] Experimental physicists held the same expectations as theoretical physicists. In 1866 Knoblauch and Jolly urged Melde's promotion to ordinary professor for experimental physics at Marburg not only for his experimental work but also for his theoretical and mathematical abilities, and in 1900 the faculty recommended candidates for Melde's successor for the same combination of abilities.[3]

The ideal of the complete physicist died hard. In 1906 Drude still maintained that the best physicist was one who did research in both theoretical and experimental physics, as he himself did, but in 1915 Wien observed that the separation of the parts of physics had advanced so far that the young studied either theoretical or experimental physics, not both. Although he recognized that the increasing compass of physics made it difficult to master the entire experimental and theoretical field, he regretted (with some exaggeration) that there were physicists who had never held an apparatus in their hands and were therefore at risk of getting lost in artificial speculations, and that there were experimentalists who neglected the stimulus that theory provides for new directions of research. The only advice he could give future physicists was not to specialize too early.[4]

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[2] Helmholtz to Prussian Ministry official Althoff, 18 May 1884, STPK, Darmst. Coll. F 1 a 1847.

[3] "Separatvotum," 16 February 1866, STA, Marburg, Bestand 305a, 1864/66; Melde. Marburg Phil. Fac. to Curator, 12 November 1900, STA, Marburg, Bestand 310 Acc. 1975/42.

[4] Wilhelm Wien, "Ziele und Methoden der theoretischen Physik," *Jahrbuch der Radioaktivität und Elektronik* 12 (1915): 241–59, on 241.

When Planck set out on his career, he was a “theoretical physicist *sui generis*,” as he put it, one of a kind, a pure theorist.[5] The conceptual innovators in the next stage of theoretical physics, who like Planck advanced in their careers solely through theoretical positions, demonstrated that the formal separation of physics into experimental and theoretical parts was practical, calling for no further justification.

Theoretical Physics, Specialization, and Unity of Physics

Whether theoretical physics was practiced by a physicist who balanced theory and experiment in his work or by a physicist who specialized in theory only, the subject held an enviable place in German universities. This came about owing to a persisting problem in the way that universities worked, which conflicted with their ideal of an overarching unity of knowledge, *Wissenschaft*. Wilhelm von Humboldt, who headed the Prussian Department of Educational Affairs for a time in the early nineteenth century, and who is identified with the ideal of *Wissenschaft*, described the universities’ lofty object as striving “first to derive everything from one original principle . . ., further to mold everything to one ideal,” and “finally to unite this principle and that ideal into one idea.” With the rise of research in the nineteenth century, universities appeared to have renounced this objective, threatening to become mere collections of independent institutes, each preoccupied with its own specialized knowledge. Faculties were disturbed by the trend, but they also knew that research benefited from specialization, a conflict which continued through the century.[6] The recognition of theoretical physics as a specialty would seem to be a furthering of the chronic disunity of learning. But theoretical physics can equally be seen as a gratifying solution to the internal disunity of physics, which by its nature is a collection of largely independent sciences – optics, heat, etc. As a specialization within physics that seeks to unite the sciences that make up physics, theoretical physics was admirably adapted to the ideal of its institutional base, the German university. Externally, theoretical physics can be seen as a bridge between the natural sciences and mathematics, further realizing the ideal of the university.[7]

Importance of University Positions for Theoretical Physics

We have tracked the appearance and spread of positions for theoretical physics in German universities, which raise a basic question. Were the positions important for the development of theoretical physics in Germany? As we have seen, the main justification of new positions was to bring in second physicists to supplement the work of the experimental physics professors. For much of our period, the holders of these positions were not specialists in theoretical physics, and some had little inclination to work in that field. Often they were by preference experimentalists

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[5] Max Planck, *Wissenschaftliche Selbstbiographie* (Leipzig: J. A. Barth, 1948), 16.

[6] R. Steven Turner, “The Growth of Professorial Research in Prussia, 1818–1848–Causes and Context.” *HSPS* 3 (1971): 137–82, on 155–56.

[7] Kiel U. Philosophical Faculty to Prussian Minister of Culture Gossler, 14 December 1882, DZA, Merseburg.

who were more or less knowledgeable in theory, versions of the ideal physicist in a time when specialization was discouraged. Our answer to the question above is a qualified yes; we think that physics overall benefited from the division of positions. Experimental physicists who in early postings were assigned to lecture on mathematical physics strengthened their grasp of the other half of physics, mathematical theory, and positions for second physicists that were designated for mathematical or theoretical physics were a formal recognition of the specialty, drawing attention to it. The positions commonly gave the small but increasing number of physicists whose inclination was theoretical physics a slender livelihood—until late in our period, teaching was about the only paying work for physicists—and an opportunity to bring their teaching in line with their research, doubtless to the benefit of their students and probably of their research, and to any students who shared their inclination, the positions pointed to an emerging career in theoretical physics. By the end of our time, with the establishment of ordinary professorships for theoretical physics in several universities, their incumbants were in principle of equal standing to the professors of experimental physics, doubling the representation of physics in the faculty and insuring that the interests of theoretical physics were heard. In terms of research, theoretical physicists who were not required to devote their time to experimental physics had more opportunity to do what they were good at, working on physical theories. With the growing extent and complexity of physics, the relegation of teaching and research to theoretical and experimental specialties was a practical division of labor. Planck and other theoretical specialists in Germany carried out seminal researches, which earned them a professional and public renown at least equal to that of experimental physicists. Although positions designated for theoretical physics were not necessary for German accomplishments in the field—Helmholtz's position was in experimental physics, for example—they were still important.

Why German Theoretical Physics Was Successful

That work in theoretical physics by German physicists was successful is evident from the names of its representatives: Kirchhoff, Helmholtz, and others we have met, who advanced physics in fundamental ways. The answer to why it succeeded has several parts: universities, teaching, methods of research, and cultural traits. During our period, German universities acquired a habit of regarding research as a qualification of a teacher. Historically, this came about through a combination of the ideal of W*issenschaft*, competition between universities for eminent faculty, and appointment criteria of state ministries.[8] Universities came to teach not only knowledge but also the way to advance knowledge, becoming the basis of their renown. By supplying a well-educated "army of workers, standing under the intellectual generalship of a few great leading minds," they became a self-perpetuating, "powerful organization" for advancing scholarship and the natural sciences.[9] Theoretical physics in Germany in

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[8] Turner, "Growth of Professorial Research in Prussia," passim.

[9] John Theodore Merz, *A History of European Thought in the Nineteenth Century,* 4 vols. (1904–1912; reprint, New York: Dover, 1965), vol. 1, 166–67.

the nineteenth century developed largely within the universities, which proved to be an enduring home: the universities remain a primary location of research in theoretical physics a century and a half later.

Because theoretical physicists carried out their research in their offices and studies, it might seem that it would make little if any difference in which university they found themselves. But in practice the places where they worked often were important. Their colleagues—experimental physicists, mathematicians, chemists, astronomers, and others—might help or collaborate with them. We have given examples of how the proximity of researchers in neighboring fields worked to the advantage of theoretical physics at Göttingen, Heidelberg, and Berlin, but it happened in many locations.

The varied local scientific life of German universities also benefited the teaching of theoretical physics. It was a feature of German universities and polytechnics that students moved freely between them. Following this practice, physics students made contact with established physicists in different locations, broadening their education. We saw how Hertz and Planck moved from Munich to Berlin, where they finished their studies. Helmholtz placed a high value on the opportunity students had of working with their teachers; with reference to his own teacher, Johannes Müller, he wrote: "Anyone who has once come into contact with one or more men of the first rank must have had his whole mental standard altered for the rest of his life."[10]

In the words of a historian of nineteenth-century thought, German universities became identified with the "German man of science," a type of active knowledge-seeker, who had certain traits.[11] First, the German man of science was complete and thorough in his research. We see this criterion at work in the comprehensive series of researches by Weber in electrodynamics, by Clausius in thermodynamics, by Kirchhoff in elasticity, by Helmholtz in mechanics, and by other physicists we treat. Second, he was not isolated; living mostly at his university, he accepted the duties that came with it, which were to define the scope of his science, to see to it that no part of it was left untreated, and to integrate it with existing knowledge. Nearly every one of the physicists we discuss addressed all the major branches of physics in his research; at the same time he worked to bring together the branches to make a coherent science and, in keeping with his belief that physics is basic to all natural science, to connect physics with neighboring fields. Third, he was a teacher, who conveyed the principles of research to the next generation of researchers; he taught them the "right method" for advancing the science, and by surveying "the whole" of the science, he insured that they had the tools to work with. Individual teachers imbued with the scientific spirit were examples to their pupils, who perpetuated the right method. Kirchhoff and Voigt were inspired by their teacher Neumann to pursue theoretical physics as a career, and they continued his ways of research

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[10] Hermann von Helmholtz, "On Academic Freedom in German Universities" (1877), in *Popular Lectures on Scientific Subjects*, 2nd ser., trans. E. Atkinson (London: Longmans, Green, 1908), 237–65, on 251.

[11] The general traits according to Merz, *History of European Thought*, vol. 1, 212–15.

and teaching in their work. Fourth, he was a "philosopher"; in the mathematical and physical sciences, this often meant that he accepted Kant's epistemology. Leaders in theoretical physics such as Helmholtz, Kirchhoff, and Boltzmann showed a strong interest in philosophy as it related to the methods of research. As a German man of science, the theoretical physicist maintained a high level of professional competence. Employed by institutions of higher learning, he was mathematically skilled, physically inventive, and devoted to his work, above all to his research.

The traits of the German man of science cannot fully account for research that rose above the level of professional competence. Einstein had definite ideas on this. As he saw it, the occasional excellence is not owing to talent, which is rather common. To the talented physicist, his work is like a sport, which satisfies a competitive need. He may do fine research, but he could equally be a fine officer or tradesman. Einstein was interested in another type of physicist, one who pursues physics out of a drive to "escape from everyday life" into the "world of objective perception and thought," who replaces the world of experience with a "picture of the world," which becomes the "pivot" of his "emotional life." There are not many such physicists, he believed, but without them physics would not be the same. Planck was this type of physicist, and he would have included himself.[12]

A major reason for the success of German work was that physics was ready to undergo a strong theoretical development. In the eighteenth century, new branches of physics had acquired empirical foundations and several simple laws, and experimental physics in the early nineteenth century established connections between the branches. Building on this foundation, physicists in the middle of the nineteenth century acquired a number of general principles, which made possible a rapid development of the branches of physics and their connections. Helmholtz introduced the universal principle of conservation of energy, arguably his most important work. This principle entered thermodynamics as its first law, to which Clausius added a complementary universal principle, the second law of thermodynamics, unquestionably his most important work. Boltzmann's most important contributions followed from his exploration of the second principle of thermodynamics. Planck used the second principle of thermodynamics as an incisive method of research, as we saw in his work on blackbody radiation leading to the quantum theory. Impressed by the universal laws of thermodynamics, Einstein looked for comparable principles for correcting difficulties in the electrodynamics of moving bodies, guiding him to the theory of relativity. He came to believe that the advancement of physics depends on finding principles applicable to all of physics, reducing the arbitrary element in the formulation of physical laws. Older general principles from mechanics underlay much of Helmholtz's, Kirchhoff's, and Voigt's work. The introduction, investigation, and application of general principles by German physicists were responsible for many of their greatest theoretical

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[12] Albert Einstein, "Principles of Research," 1918, in *Ideas and Opinions* (New York: Dell, 1973), 219–22, on 219–20.

accomplishments. These in turn attracted gifted and motivated individuals to the field of theoretical physics, ensuring a continuity of successful research.

German physics was balanced in ways that contributed to its success. In this book, we single out for study German theoretical physics, separating it (somewhat artificially) from experimental physics. It is important to recognize that German physics as a whole was productive. Because theorists depended on experimenters, and conversely, it could hardly be otherwise; German experimental physicists aided by German instrument makers performed research that compared in quality to research by German theoretical physics. Volkmann thought that his colleagues achieved a balance between deductive and inductive methods in their research, a credible observation.[13] A similar balance is evident in the ideal of the complete physicist, who mastered the methods of both theory and experiment. Physicists who developed theories made the measurements that came with their theories. With the arrival of theoretical specialists who did not do experimental work, a balance was achieved through a close coordination of their work with the work of experimentalists. Individual physicists had favorite methods of theoretical research, but they recognized that physics profited from a balance of methods in the field at large. Another kind of balance obtained between research and teaching; active researchers conveyed their knowledge to students, insuring a supply of young researchers, and through teaching, researchers deepened their understanding of their field and its problems. German physicists made a strong effort to follow the work of foreign physicists, a balance of sources. We see it in their productive use of French and British physics, and we see it in their major journal, which published extensive physical research from abroad. The importance German physicists placed on keeping abreast with foreign work continued through the end of our period.

The generally high standards of other fields in Germany had consequences for theoretical physics. After experimental physics, the field closest to theoretical physics was mathematics, and during our time mathematics in Germany had able practitioners, who in varying degrees supported teaching and research in theoretical physics. Kirchhoff valued his collaboration with his Heidelberg mathematical colleague Leo Königsberger to the extent that he was understood to be unavailable to calls to other universities. When at the end of our period theoretical physics in Germany appeared temporarily to be in decline, with a shortage of takers of strong ability to replace the passing great theoretical physicists, Wilhelm Wien attributed part of the reason to the lately acquired disinterest of mathematicians in physics.[14]

German work in theoretical physics drew support from the wider culture, which prized the same virtues: organization, exactitude, intellect, and unified worldviews. Lacking a central concentration of scientific institutions, German physics was carried out in university towns and cities, calling for organization, a need which

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[13] Paul Volkmann, *Erkenntnistheoretische Grundzüge der Naturwissenschaften und ihre Beziehungen zum Geistesleben der Gegenwart. Allgemein wissenschaftliche Vorträge*, 2nd ed. (Leipzig and Berlin: B. G. Teubner, 1910), 108.

[14] Wien to Sommerfeld, 11 June 1898, Sommerfeld Correspondence, Ms. Coll., DM.

was met from the beginning through a central journal for experimental and mathematical physics. The early physical theorists Neumann and Weber placed a high value on accurate measurements and the control of errors, the experimental complement of the mathematical derivation of physical laws; the value of exactitude was continued by others, becoming a trademark of German physics. Helmholtz's "intellectual mastery of nature," which a life of science offered, rested above all on the intellectual power of theoretical physics. The unified physical world picture that Helmholtz, Planck, and Einstein sought was supported by their world views.

Theoretical Physics and the Goal of Unified Theory

Like every natural science, physics evolves, and because the evolution is cultural, its course is consciously shaped, even as the future is unknowable in detail. Its evolution is guided by goals, one of which is the "unification" of physical theory. To Ohm, to Einstein, to elementary particle physicists and cosmologists, and to my theoretical physics professor William Band, physics has a goal beyond the myriad of researches that contribute to the body of physical knowledge: a unified conceptual grasp of the totality of physical phenomena at the fundamental level.

In the first chapter we discuss various meanings of "unity," as physicists understood the term. Helmholtz said that it is "natural" that at all times men have sought knowledge of the "whole connection of the universe."[15] By the end of our period, major progress had been made in the more modest project of connecting the branches of physics. A tireless pursuer of this object, Planck described the advances and the work still remaining. Physics had produced three major theories, he said, each internally well-developed: mechanics, electrodynamics, and thermodynamics. These theories, which previously were independent, had a number of points of contact, and also of conflict. Relativity theory addressed the problem of mechanics and electrodynamics, modifying mechanics through the incorporation of the universal constant of the velocity of light, bringing it and electrodynamics together under a "unified theory," called "dynamics." An unfinished work was to complete the "fusion" of dynamics and thermodynamics. One day, Planck said, the quantum of action, like the velocity of light, would become part of a general dynamics, for physics could not rest until it became a "unified theory."[16]

The value that German physicists placed on the connectedness of their field had several sources. The main one was intrinsic, it came with the field, as Boltzmann explained: theoretical physics has the job of bringing together experimental results under "unified points of view."[17] Other sources were broadly cultural. The late nineteenth century saw an intense questioning of the foundations of knowledge

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[15] Hermann von Helmholtz, *Vorlesungen über theoretische Physik,* vol. 1, pt. 1, *Einleitung zu den Vorlesungen über theoretische Physik*, ed. A. König and C. Runge (Leipzig: J. A. Barth, 1903), 1.

[16] Max Planck, "Verhältnis der Theorien zueinander," in *Physik,* ed. Emil Warburg (Berlin: Teubner, 1915) , 732–37, on 733–34, 737.

[17] Ludwig Boltzmann, "Josef Stefan. Rede gehalten bei der Enthüllung des Stefan-Denkmals am 8. Dez. 1895," in *Populäre Schriften* (Leipzig: J. A. Barth, 1905), 92–103, on 94.

within and across scholarly and scientific boundaries, accompanied by a strong desire in every field to bring the scattered parts of research into a synthesis.[18] Another source was German philosophy and the related emphasis in German education on the unity of knowledge, the implication of *Wissenschaft*, though with a natural scientific reading of it. Volkmann explained that in general education, there was a phantom of the "whole," which natural science destroyed, and "unity" and "completeness" were likewise illusory ideals of an education directed to the "whole." Wholeness, unity, and completeness were misplaced if they were used as a fantastic ideal realizable only in the distant future, though they were proper if they were used on the path to knowledge, in touch with reality.[19]

The search for connectedness was meaningful at the level of the individual physicist. Helmholtz said that although a scientist needs to specialize, he must hold to the conviction that he adds to the "stupendous whole of Science."[20] Voigt felt dissatisfaction when he compared himself to physicists like Planck and Lorentz, who moved in the "the pure ether of the most general questions," while he dug "like a mole in the earth after small specialties."[21] He trusted that his comprehensive text on crystal physics would reveal that his scattered, seemingly unrelated researches had arisen from a "unified striving."[22] The connectedness of physics was related to individual world views. Late in life, Plank recalled that his choice of physics rather than mathematics owed to his "deep interest in questions of worldview,"[23] which included faith. He believed that "a "unified world picture" demands the identification of the "world order of natural science" with that of the "God of religion." Natural science and religion agree that there is a "rational world order," which cannot be directly known but only indirectly perceived; to make a connection with the world order, religion employs symbols, and natural science employs measurements derived from sense impressions.[24] Just as in science it is necessary to consider the whole system and the connections between its parts, so in intellectual life it is necessary to recognize that the whole is greater than the sum of its parts, and that "a clear-cut between science, religion, and art" is impossible.[25]

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[18] Lewis Pyenson, *The Young Einstein: The Advent of Relativity* (Bristol and Boston: Adam Hilger, 1985), 138, 140.

[19] Volkmann, *Erkenntnistheoretische Grundzüge,* 138, 140.

[20] Hermann von Helmholtz, "The Aim and Progress of Physical Science," 1869, in *Popular Lectures on Scientific Subjects*, trans. E. Atkinson (New York, 1873), 363–97, on 366.

[21] Voigt to Lorentz, 19 May 1911, AHQP.

[22] Carl Runge, "Woldemar Voigt," *Gött. Nachr.*, 1920, 46–52, on 50.

[23] Planck to Josef Strasser, 14 December 1930, quoted in Armin Hermann, *Max Planck in Selbstzeugnissen und Bilddokumenten* (Reinbek b. Hamburg: Rowohlt, 1973), 11.

[24] Max Planck, "Religion and Natural Science," in *Scientific Autobiography and Other Papers*, trans. Frank Gaynor (New York: Greenwood, 1968), 151–87, on 183.

[25] Max Planck, "Physics and World Philosophy," in *Philosophy of Physics,* trans. W. H. Johnston (New York: Norton, 1936), 9–39, on 34.

The goal, as it was worded, of a "unified physical world picture," was a product of its time.[26] Theoretical physicists beginning their careers after the end of our period did not use the same language and may have wondered about their predecessors. But the search for unifying concepts and laws continued. In the form in which Boltzmann expressed the goal, unity of the forces of nature, it has remained a goal of physics. When physicists speak of a "grand unified theory," they refer to a unification of the fundamental forces into a single force. Such unity may turn out to be an illusion arising from a "metaphysical obsession," but to date it continues as a driving impulse in physical research.[27]

If we ask what theoretical physics is for, we have a ready answer. It is the same thing that experimental physics is for. The physicists in this study said it often enough: it is above all to find the laws of the physical world, the assurance of its comprehensibility. But as we have seen, this is not a complete characterization. From the outside, physics looks like a collection of theories, laws, and constants that are backed by experience, but to physicists the collection is not the end, and this would be so even if every single observation were accounted for. There is a hierarchy of theories, constants, and laws, and among the laws of physics, those having universal validity are valued highest. The first law of thermodynamics holds "first place" among dynamical laws, Planck said, and the second law of thermodynamics holds "first place" among statistical laws.[28] They are welcomed for the understanding they bring, for their role in physical research, and for the kind of laws they are, concise expressions of the greatest generality. They foreshadow the law or laws that physicists believe will follow from a total theory.

If the physicists' quest for a total theory should one day succeed, the fundamental law or laws of nature will be known. This will not mean the end of theoretical physics, since there will always be subjects to work on, but the search for foundations will have ended and with it a drive that bound many physicists in our study in a common endeavor, perhaps to be replaced by another goal.

Theoretical Physics as a Field

A common classification of the sciences at the time was "applied mathematics," standing for a group of largely autonomous fields as varied as practical astronomy and fluid mechanics, which had in common only their use of mathematics. The classification made no difference to research in physics, for what mattered there was the outcome of the use of mathematics, a comprehension of the physical world. For most of our period, the work of physics was designated "mathematical physics" and "experimental physics," both expressions being called for, since physics had two main methods. A typical lecture course was described as "experimental

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[26] Neo-humanism, a set of ideas important in nineteenth-century German secondary and higher education, is associated with the ideal of a physical world picture (Pyenson, *Young Einstein*, 176).

[27] Étienne Klein and Marc Lachièze-Rey, *The Quest for Unity*, trans. Axel Reisinger (New York, Oxford: Oxford University Press, 1999), 129–30.

[28] Max Planck, "Dynamical Laws and Statistical Laws," 1914, in *A Survey of Physical Theory,* translated R. Jones and D. H. Williams (New York: Dover Publications, 1960), 56–68, on 66.

physics," not "physics." The title identified the method of teaching, by experimental demonstration, and it acknowledged that "experimental physics" is not all of physics. It was likewise with courses described as "mathematical physics." In a German university, as we have seen, physics was normally represented by one ordinary professor, who was responsible for teaching experimental physics, traditionally considered the more important of the two methods for teaching physics, in part because it did not require students to have a mathematical background. The main mathematical theories of physics were taken up in mathematical physics courses, which were commonly taught by junior physics teachers or mathematicians. When teaching positions for physicists came to include positions for mathematical physics, the university system acknowledged a division in physics that had been there from the start. In research, the two methods came together in the common pursuit of laws of nature.

When we leave our subject, at the turn of the twentieth century, theoretical physics showed some of the characteristics of an independent field. It was a specialized area of research, and it was taught separately. It had problems internal to itself, which often were discussed by theoretical physicists among themselves; each individual work of theoretical physics did not have to address experimental physics directly. But the division of physics was limited in intent, as it had to be, since theoretical and experimental physics were interdependent, each needing the other to advance physics. The division was based on method, not on content; each half covered all of physics, the whole. Theorists and experimenters received the same education, belonged to the same societies, attended the same colloquiums, and published in the same physics journal. Theorists sometimes published their more mathematical papers in journals they shared with mathematicians, which were read by other theorists, while less mathematical versions of the same papers often appeared in a physics journal, where experimentalists read them. By predilection, ability, and sometimes necessity, some physicists specialized in experiment and others in theory, a reality which was acknowledged by positions for first and second physicists. Both experiment and theory had grown in extent and difficulty to the degree that an individual physicist could not be expected lecture on and carry out research in both halves of physics with comparable success and efficiency. The acknowledgment of theoretical physics as a partly autonomous field was accomplished without forfeiting the ultimate goal throughout our period of a unified structure in which theory and experiment each played an essential role and had its proper place.

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Zurich. ETH. See Frey-Wyssling, A.

Index

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