Research and Pedagogy: A History of Quantum Physics through Its Textbooks

Max Planck Research Library for the History and Development of Knowledge

Studies 2

Edition Open Access
2017

Research and Pedagogy: A History of Quantum Physics through Its Textbooks

Massimiliano Badino, Jaume Navarro (eds.)

Edition Open Access
2017

Max Planck Research Library for the History and Development of Knowledge
Studies 2

Communicated by:
Kostas Gavroglu

Edited by:
Massimiliano Badino, Jaume Navarro

Editorial Coordination:
Nina Ruge

Copyedited by:
Jeremiah James with Irene Colantoni, Oksana Kuruts, Jonathan Ludwig, Marius Schneider, Chandhan Srinivasamurthy

Cover image:
Van Vleck between two fans at 1300 Sterling Hall, University of Wisconsin–Madison, ca. 1930 (picture courtesy of John Comstock).

ISBN 978-3-945561-24-9
Published 2017 by Edition Open Access,
Max Planck Institute for the History of Science
http://www.edition-open-access.de
Reprint of the 2013 edition
Printed and distributed by
PRO BUSINESS digital printing Deutschland GmbH, Berlin

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available in the Internet at http://dnb.d-nb.de.

The Max Planck Research Library for the History and Development of Knowledge comprises four subseries, *Studies*, *Proceedings*, *Sources* and *Textbooks*. They present research results and the relevant sources in a new format, combining the advantages of traditional publications and the digital medium. The volumes are available both as printed books and as online open access publications. They present original scientific work submitted under the scholarly responsibility of members of the Scientific Board and their academic peers.

The volumes of the four subseries and their electronic counterparts are directed at scholars and students of various disciplines, as well as at a broader public interested in how science shapes our world. They provide rapid access to knowledge at low cost. Moreover, by combining print with digital publication, the four series offer a new way of publishing research in flux and of studying historical topics or current issues in relation to primary materials that are otherwise not easily available.

The initiative is supported, for the time being, by research departments of three Max Planck Institutes, the MPI for the History of Science, the Fritz Haber Institute of the MPG, and the MPI for Gravitational Physics (Albert Einstein Institute). This is in line with the *Berlin Declaration on Open Access to Knowledge in the Sciences and Humanities*, launched by the Max Planck Society in 2003.

Each volume of the *Studies* series is dedicated to a key subject in the history and development of knowledge, bringing together perspectives from different fields and combining source-based empirical research with theoretically guided approaches. The studies are typically working group volumes presenting integrative approaches to problems ranging from the globalization of knowledge to the nature of spatial thinking.

Each volume of the *Proceedings* series presents the results of a scientific meeting on current issues and supports, at the same time, further cooperation on these issues by offering an electronic platform with further resources and the possibility for comments and interactions.

Each volume of the *Sources* series typically presents a primary source—relevant for the history and development of knowledge—in facsimile, transcription, or translation. The original sources are complemented by an introduction and by commentaries reflecting original scholarly work. The sources reproduced in this series may be rare books, manuscripts, documents or data that are not readily accessible in libraries and archives.

Each volume of the *Textbooks* series presents concise and synthetic information on a wide range of current research topics, both introductory and advanced. They use the new publication channel to offer students affordable access to high-level scientific and scholarly overviews. The textbooks are prepared and updated by experts in the relevant fields and supplemented by additional online materials.

On the basis of scholarly expertise the publication of the four series brings together traditional books produced by print-on-demand techniques with modern information technology. Based on and extending the functionalities of the existing open access repository European Cultural Heritage Online (ECHO), this initiative aims at a model for an unprecedented, Web-based scientific working environment integrating access to information with interactive features.

Contents

Contributors

Massimiliano Badino: Centre d'Història de la Ciència, Facultat de Ciències, Universitat Autònoma de Barcelona, Cerdanyola del Valles, 08193 Bellaterra (Barcelona), Spain
massimiliano.badino@uab.cat

Michael Eckert: Forschungsinstitut Deutsches Museum, Museumsinsel 1, 80538 München, Germany
m.eckert@deutsches-museum.de

Clayton Gearhart: St. John's University, Collegeville, MN 56321, USA
cgearhart@csbsju.edu

Domenico Giulini: Institut für Theoretische Physik, Leibniz Universität Hannover, Appelstraße 2, 30167 Hannover, Germany
giulini@itp.uni-hannover.de

Dieter Hoffmann: Max Planck Institute for the History of Science, Boltzmannstraße 22, 14195 Berlin, Germany
dh@mpiwg-berlin.mpg.de

Don Howard: Department of Philosophy, 100 Malloy Hall, University of Notre Dame, Notre Dame, Indiana 46556, USA
dhoward1@nd.edu

Michel Janssen: Program in the History of Science, Technology, and Medicine, University of Minnesota, Minneapolis, MN 55455, USA
janss011@umn.edu

Marta Jordi Taltavull: Max Planck Institute for the History of Science, Boltzmannstraße 22, 14195 Berlin, Germany
mjordi@mpiwg-berlin.mpg.de

David Kaiser: Program in Science, Technology, & Society and Department of Physics, Room E51–179, Massachusetts Institute of Technology, 77 Massachusetts Avenue, Cambridge, MA 02139, USA
dikaiser@MIT.EDU

Helge Kragh: Centre for Science Studies, Department of Physics and Astronomy, Aarhus University, Building 1520, 8000 Aarhus, Denmark
helge.kragh@ivs.au.dk

Charles Midwinter: Program in the History of Science, Technology, and Medicine, University of Minnesota, Minneapolis, MN 55455, USA
charles.midwinter@gmail.com

Jaume Navarro: University of the Basque Country and Ikerbasque (Basque Research Foundation), D10, Plaza Elhuyar 2, 20018, San Sebastian, Spain
jaume.navarro@ehu.es

Chapter 1
Pedagogy and Research. Notes for a Historical Epistemology of Science Education

Massimiliano Badino and Jaume Navarro

1.1 Transmitting Scientific Knowledge

"Those who can't do teach, and those who can't teach, teach gym." Woody Allen's scornful comment on the role of teaching in *Annie Hall* summarizes fairly well one very popular view. For many, there is a clear-cut distinction between the creative intellectual activity of research and the mere repetition of what someone else has produced to a classroom of students. To be sure, this view affects not only teaching and learning. Rather, it is more or less implicit in any occurrence of the exposition, communication, or transmission of scientific knowledge from the community of experts to the external world.

More importantly, this view is sustained by a certain model of science and its relations with society. The basic tenet of this model—sometimes attributed to Robert K. Merton and therefore called *Mertonian* (Cloitre and Shinn 1985), sometimes more simply called the "classical image of science" (Renn and Hyman 2012b)—is that knowledge produced within the scientific culture is radically different from any of its disseminations to the broader society. More precisely, the classical image of science pictures the scientific community as a highly structured and organized elite of experts, who produce a carefully defined and thoroughly validated—and therefore true—body of knowledge, which is in turn transmitted to an audience (students, informed public, laymen). Finally, this heterogeneous audience is, to various extents, incapable of fully appreciating the products of scientific inquiry without an adequate re-elaboration, and consequently, it is totally unable to feed anything back to the scientific elite.[1]

Although completely discredited by the scholarly work of the last thirty years, this model has maintained its grip on public representations of science. The main reason is that, even though successful in criticizing each of the tenets of the classical image, philosophers, historians, and sociologists of science have not been able to provide an alternative account that is as intuitive and all-embracing. This failure should not be exclusively ascribed to the contemporary tendency of scholars in science studies to insist on the disunity and locality of scientific culture (Galison and Stump 1996). It is also due to the fact that the several branches of specialized work on the transmission of scientific knowledge have grown at different paces. Thus, for example, popularization both aimed at the general public and at fellow scientists belonging to other disciplines received attention as early as the mid-1980s.[2] About the same time, the works of Harry Collins and Bruno Latour, among others, covered

[1] See for example (Whitley 1985; Hilgartener 1990; Olesko 2006).

[2] See the 1985 Yearbook of *Sociology of the Sciences* edited by Terry Shinn and Richard Whitley and especially (Bunders and Whitley 1985).

the analysis of the circulation of knowledge among experts and the transmission of scientific applications to social actors interested in their economic exploitation (Collins 1985; Latour 1987; 1988). By contrast, a systematic investigation of scientific pedagogy has taken off only in the last fifteen years. Instrumental to this general revamping of the image of scientific training has been a re-evalutation of the role of textbooks. Projects such as the volume edited by Anders Lundgren and Bernadette Bensaude-Vincent on the circulation of textbooks on chemistry from the French Revolution to the eve of World War II (Brooke 2000), the 2006 special issue of *Science and Education* on textbooks at the scientific periphery (Bensaude-Vincent 2006; Bertomeu-Sánchez et al. 2006), David Kaiser's edited collection of studies on pedagogy in science (Kaiser 2006), and the *focus section* in *Isis* in 2012 (Vicedo 2012), are just a few of the major steps taken in recent times towards a modernization of analyses of pedagogy and textbooks in science studies.

1.2 Creating Knowers, Creating Facts

However, one should notice that the attitude of scholars towards traditional views of scientific pedagogy has been complex and occasionally ambivalent. It is thus important to reconstruct some lines of development of this attitude.[3] One important line of inquiry many scholars have followed concerns the role of pedagogy and textbooks in producing *knowers*, that is a professionally organized group of people explicitly trained to perpetuate a certain kind of knowledge. It was Thomas Kuhn's deep criticism of the logical positivistic view of science as a purely theoretical activity that first highlighted, for many scholars, the role of training in determining the working style, the self image, and even the ontology of scientists, thus restoring dignity to the learning process (Kuhn 1962). As David Kaiser points out, "scientists are not born, they are made" (Kaiser 2006, 1), and the process of making a scientist has a profound influence on the way in which he or she will conduct future research. What is a good question, what is a satisfactory answer, what counts as a legitimate scientific procedure or a correctly conducted experiment, even what is viewed as a possible object of research is determined, according to the Kuhnian model, during the inculcation of the reigning paradigm, occurring at the training stage (Kuhn 1962, 359; 1963). Pedagogy is not solely a social phase in the formation of the "type" scientist, but is also crucially significant for the broader definition of disciplines and fields of knowledge.

Ironically, as he was giving new philosophical dignity to pedagogy, Kuhn was also playing a key role in keeping textbooks far from the inquisitive examinations of historians. Famously, Kuhn claimed that textbook writing is an activity almost exclusively performed during the peaceful periods he dubbed normal science. In his words, textbooks "are produced only in the aftermath of a scientific revolution […] [t]hey are the bases for a new tradition of normal science" (Kuhn 1962, 144). They "address themselves to an already articulated body of problems, data, and theory, most often to the particular set of paradigms to which the scientific community is committed at the time they are written" (Kuhn 1962, 136). From this point of view, textbooks are only written once a revolutionary process is coming to an end, and their role is basically to transmit the newly-accepted paradigm, never to pose problems for it. Although scientific training does have a critical bearing on scientific culture

[3] Some useful accounts of the role of pedagogy and especially textbooks in science studies are (Myers 1992; Brooke 2000; Olesko 2006; Kaiser and Warwick 2006).

as a whole, for Kuhn it still differs from research in a fundamental manner. This position is clearly stated in his paper "The Function of Dogma in Scientific Research," written only a year after *Structure*:

> Perhaps the most striking feature of scientific education is that, to an extent quite unknown in other creative fields, it is conducted through textbooks, works written especially for students. Until he is ready, or very nearly ready, to begin his own dissertation, the student of chemistry, physics, astronomy, geology, or biology is seldom either asked to attempt trial research projects or exposed to the immediate products of research done by others—to, that is, the professional communications that scientists write for their peers. (Kuhn 1963, 350)

Moreover, textbooks also have a hidden agenda: to erase any trace of crisis, of instability, of change, of historical contingency, and to present the ruling paradigm as an established, consistent whole—as the truth revealed. This trait not only transforms textbooks into repositories of dead doctrines, but it also disqualifies them totally as historiographical tools. Historians should keep away from the image of science conveyed by pedagogical texts. In his later paper "The Essential Tension," Kuhn insists on this view of the roles of textbooks:

> [T]he various textbooks that the student does encounter display different subject matters, rather than, as in many of the social sciences, exemplifying different approaches to a single problem field. Even books that compete for adoption in a single course differ mainly in level and in pedagogic detail, not in substance or conceptual structure. Last, but most important of all, is the characteristic technique of textbook presentation, except in their occasional introductions, science textbooks do not describe the sorts of problems that the professional may be asked to solve and the variety of techniques available for their solution. (Kuhn 1977, 229)

Kuhn seems to extend contemporary Western university education to all times and places when he says that "[t]ypically, undergraduate *and* graduate students of chemistry, physics, astronomy, geology, or biology acquire the substance of their fields from books written especially for students" (Kuhn 1977, 228). Almost certainly Kuhn's view of textbooks is autobiographically motivated, rooted in his own training. Educated in theoretical physics, Kuhn came to see textbooks as a collection of formulas, theorems, and formal techniques; i.e., a set of rules. But rules, Wittgenstein taught us, do not contain the conditions of their own application (Wittgenstein 1953). These conditions are eminently social, partly conventional, and surely cannot be formalized. Textbooks, by extension, would not have a history separate from the practices of their use and, more importantly, they would not be vehicles for history.

Apart from his harsh judgement on the epistemological and historiographical role of textbooks, Kuhn's conception of pedagogy, as functional to the formation of knowers, has been highly influential in several directions of research within science studies. For instance, the Kuhnian emphasis on disciplinary identity as the minimal unity around which knowers organize themselves has led to extensive historical investigations of the effect that pedagogical practices and texts have on the construction of disciplines. Pioneered by Owen Hannaway in the 1970s (Hannaway 1975), this line of research has been developed by, among others, Josep Simon (2011), and explicitly defended by Kostas Gavroglu and Ana Simoes, who argued that "textbooks from an early period in a discipline's history can also be viewed as a genre whose aim was to consolidate a consensus as to the language and practices to be adopted" (Gavroglu and Simoes 2000, 415–416).

Furthermore, Kuhn insisted that pedagogical practices, and therefore knowers, are temporally, spatially, and socially situated. The local aspects of scientific knowledge have encouraged many scholars to look more carefully into the mechanisms for producing national styles in the sciences and into the dynamics of incorporating novel knowledge into the pedagogical routine. Started as demographical studies at the end of the 1970s (Pyenson and Skopp 1977; Pyenson 1979; Jungnickel 1979), these investigations have originated important contributions on the microstructure of the day-to-day exchange between mentors and pupils, both in classes and in special seminars. Major examples are Andrew Warwick's deep study on the meaning of the Cambridge system of Mathematical Tripos for British mathematical physics (Warwick 2003), Karl Hall's account of the role of Landau's and Lifshitz's *Course of Theoretical Physics* in determining the style of physical research in the Soviet Union (Hall 2006), and the discussion of the influence of James J. Sylvester and Felix Klein on the developing American mathematical community pursued by Karen Hunger Parshall and David Rowe (1994).

Pedagogical practices can even lead to the establishment of "research schools" able to imprint a characteristic mark on subsequent research. The pioneering work of Jack Morell, who applied the notion of "research school" to the laboratories of Justus Liebig and Thomas Thomson was the starting point of a tradition that has provided new insights into the relationship between research and pedagogy in the sciences (Morell 1972; Brock 1972; Holmes 1989). Morell showed that Liebig's chemical laboratory owed its success largely to the regime of learning and production that he established in Giessen. From there, the tradition of hands-on training extended to university laboratories throughout modern Europe, encountering sometimes more, sometimes less resistance from those who thought of liberal education as a purely intellectual activity. Kathryn Olesko and, more recently, Suman Seth have extended this tradition to the research schools created around Franz Neumann in Königsberg and Arnold Sommerfeld in Munich, respectively, highlighting the importance of face-to-face interaction between professors and students in close, problem-oriented seminars (Olesko 1991; Seth 2010).

Finally, and more significantly for the purpose of this volume, even the teaching of theoretical physics, which does not need, in principle, the work of laboratories, can be understood to fit within this historiography of hands-on practices, of the transmission of a particular type of craftsmanship, and of specific social values, as shown in the work of historians such as Sharon Traweek, David Kaiser, and Ursula Klein, to cite only a few examples (Traweek 1988; Klein 2003; Kaiser 2005).[4]

Prominent as it was, Kuhn's view was not the only attempt to understand pedagogy in science. Along with the process of producing knowers, historians, philosophers, and sociologists of science have inquired into the effect of training in producing *scientific facts*. Ludwik Fleck wrote some of the most illuminating pages about this social phenomenon. In his 1935 book, which would inspire Kuhn himself many years later, Fleck distinguishes three

[4]This list of topics covered by the study of scientific pedagogy and textbooks does not aim to be exhaustive. Further interesting themes of research, together with a bibliography that includes studies in psychology and other human sciences, can be found in (Vicedo 2012, 85).

elements in scientific education: experience, cognition, and sensation. Through following a pedagogical path, young scientists-to-be are educated to see, feel, and conceptualize the world in a certain manner in order to become part of the established *thought collective* or *thought style*. Partially reshaping the scientific self, this process also reshapes the world around the subject: "a fact always occurs in the context of the history of thought and is always the result of a definite thought style" (Fleck 1979, 95). Fleck also separates sharply popularization from professional training: "in contrast with popular science, whose aim is *vividness*, professional science in its *vademecum* (or handbook) form requires a *critical synopsis in an organized system*" (Fleck 1979, 117–118). The vademecum is the medium of scientific pedagogy, the organized synthesis of what is relevant and worthy in the field. Like Kuhn, Fleck also insists on the difference between research—a creative activity that can even produce contradictory results—and pedagogy, which he represents through the metaphor of a carefully prearranged mosaic:

> The vademecum is therefore not simply the result of either a compilation or a collection of various journal contributions. The former is impossible because such papers often contradict each other. The latter does not yield a closed system, which is the goal of vademecum science. A vademecum is built up from individual contributions through selection and orderly arrangement like a mosaic from many colored stones. The plan according to which selection and arrangement are made will then provide the guidelines for future research. It governs the decisions on what counts as a basic concept, what methods should be accepted, which research decisions appear most promising, which scientists should be selected for prominent positions and which should simply be consigned to oblivion. (Fleck 1979, 119–120)

So far-reaching are the consequences of scientific training. Through the medium of the pedagogical text, both the self, and the world undergo a complete reconfiguration. This crucial insight has suggested to practitioners in science studies to look more carefully into the internal structures of these texts, the economy of their contents, and the communication techniques they deploy.[5] Bruno Latour and Steve Woolgar have provided an impressive analysis of the textual construction of scientific facts through a fivefold categorization of scientific propositions, ranging from *type 1* statements, which qualify the belief as belonging to a certain actor and certain conditions, to *type 5* statements, which black-box the belief as a generally accepted part of common knowledge. Textbooks, Latour and Woolgar conclude, usually do not hedge their claims, but deliver them as the bare truth about nature:

> Scientific textbooks were found to contain a large number of sentences of the stylistic form: "A has a certain relationship with B." [...] Expressions of this sort could be said to be *type 4* statements. Although the relationship presented in this statements appears uncontroversial, it is, by contrast with *type 5* statements, made explicit. This type of statement is often taken as the prototype of scientific assertion. (Latour and Woolgar 1986, 77)

Accordingly, textbooks play an important role in sedimenting concepts, methods, experimental procedures, and orthodox interpretations. This aspect has been investigated by a

[5] An interesting development in this line of thought is the analysis of the rhetoric of science and its bearing on the creation of scientific facts; see for example (Fahnestock 1986; Prelli 1989; Gross 1990).

number of scholars, for example Mary Smyth in her reconstruction of the function of textbooks in creating consensus in psychology (Smyth 2001) or Antonio García-Belmar, Josè Ramon Bertomeu-Sánchez, and Bernadette Bensaude-Vincent, who, in their comprehensive account of French chemistry textbooks, trace the way in which the atomistic hypothesis was received and sustained in the scientific community (García-Belmar, Bertomeu-Sánchez, and Bensaude-Vincent 2006).

1.3 Towards an Epistemological Role for the Pedagogical Text

Reflection on scientific pedagogy and textbooks has hitherto generated an impressive amount of scholarly work, remarkable both in depth and in scope. A prime feature of this work has been the careful reconstruction of the pedagogical practices, the teaching procedures, the social negotiations, and the institutional settings involved in the transmission of knowledge from the scientific elite to those who are supposed to replace it in the near future. However, the fragmentation of this analysis into contingent and situated practices, does not restrict the ambition towards an encompassing model of knowledge transmission able to capture the rich material analyzed in a consistent view, and possibly to enlarge upon it. Quite the contrary, special interest has arisen in recent times in a more epistemological perspective able to illuminate persistent, long-term elements in scientific pedagogy, which tend to remain concealed in more detailed accounts. For this task, besides Kuhn, an obvious source to call upon is Michel Foucault.

In *Discipline and Punish* Foucault showed that, from the eighteenth century onwards, discipline steadily increased its efficiency by means of carefully partitioned spaces, calibrated times, and constrained behaviors (Foucault 1977). Control over the body of the individual is at work in prisons, in hospitals, in military institutions, as well as in schools. To be sure, it is precisely to schools that Foucault dedicates his most stimulating analysis. For Foucault, the pedagogical activity displays itself in three phases: hierarchical observation, normalizing judgement, and examination. The first phase requires the organization of space-time relations between the teacher and the students: the architecture of the classroom, the disposition of the seats, as well as the partition of time for lecturing, exercising, and resting. But it also requires a perpetual gaze from the teacher, which provides the control over posture, gestures, and behaviors. This control is always accompanied by a judgement, whose aim is to normalize the individuals to some preconceived orthodoxy. For this judgement one needs comparison and, more generally, examination, carried out according to rules, procedures, and routines, and evaluated according to normalizing systems.

While Foucault casts these disciplinary settings in terms of social alignments and the power relations established between the controller and the controlled, the master and the pupils, one cannot help but think about the similarities with Kuhn's pages on training. For one thing, the examinations can be really effective and normalizing only if the students have been suitably drilled in the "rules of the game," which is precisely what a paradigm is supposed to do. Docility and the cherishing of tradition are thus the essential ingredients of this approach. An approach whose implicit social alignment had been perceptively anticipated by John Dewey:

> Since the subject-matter as well as standards of proper conduct are handed down from the past, the attitude of pupils must, upon the whole, be one of docility,

> receptivity, and obedience. Books, especially textbooks are the chief representatives of the lore and wisdom of the past, while teachers are the organs through which pupils are brought into effective connection with the material. Teachers are the agents through which knowledge and skills are communicated and rules of conduct enforced. (Dewey 1938, 18)

It is with these similarities in mind that Andrew Warwick and David Kaiser have argued in favor of a "Foukuhnian" position as a possible general framework for the study of scientific pedagogy. In essence, this position boils down to an attempt to further historicize Kuhn's intuition that theoretical knowledge requires routinizing practices through Foucault's view of power as a productive force, acting by means of microscopic forms of social control, and it is developed in two points:

> [F]irst by noting the compatibility of Kuhn's emphasis on skill acquisition with Foucault's insight that power is the form of social relations does not inhibit or conceal knowledge, but is necessary to its production; and, second, by building on Foucault's claim that the minutiae of everyday practices have the power to generate new capabilities in human beings, thereby bringing about significant historical change. (Kaiser and Warwick 2006, 406)

This attempt at putting together the best of two worlds points us toward very interesting perspectives, but it still contains some fundamental difficulties. To begin with, the second point, referring to the production of historical change through everyday practices, seems to beg the question raised by the first. Kaiser and Warwick are certainly right in highlighting the similarity between Kuhn's notion of normal science based on paradigms and Foucault's normalizing regimes relying on disciplinary techniques. However, how these regimes can produce new knowledge, possibly knowledge that challenges the paradigm itself, is a particular sticking point in Kuhn's model and remains so in Foucault's. Occurring during periods of accepted paradigms and developing through normalizing procedures, the Foukuhnian pedagogy seems to leave little room for individual creativity. The strong emphasis that both Kuhn and Foucault put on the one-sidedness of the pedagogical relation between master and student makes it difficult to explain how training can turn a docile and obedient pupil into an independent researcher able, at some point, to metaphorically kill his/her master, that is, to challenge the paradigm itself. Suman Seth puts his finger on this problem when he writes: "disciplining, most specifically, cannot produce people who themselves produce new knowledge and it is the production of novel knowledge that distinguishes the researcher from the student" (Seth 2010, 69).

Furthermore, the daring combination of Kuhnian and Foucaultian insights seems at times to stretch too broadly and thinly the positions of both authors. On the one hand, as we noticed above, Kuhn's discourse on paradigm and scientific pedagogy appears to stem entirely from, and to be applicable especially to, physical sciences such as chemistry or theoretical physics. Foucault, however, famously eschewed entering into the genealogy of the physical sciences:

> [F]or me it was a matter of saying this: if, concerning a science like theoretical physics or organic chemistry, one poses the problem of its relations with the political and economic structures of society, isn't one posing an excessively

> complicated question? Doesn't this set the threshold of possible explanations impossibly high? (Foucault 1980, 109)

On the other hand, and more to the point of this volume, while Kuhn has much to say about textbooks and their relation with the whole body of knowledge—we provided ample textual evidence above—Foucault is almost silent about this topic; he prefers to focus upon the power relations displayed in specific patterns of social control and hands-on acquisition of knowledge.

Finally, their other similarities notwithstanding, it should not be forgotten that Kuhn and Foucault differ in at least one important respect, perceptively remarked upon by Hubert Dreyfus and Paul Rabinow (1983, 199–202). Foucault aims to characterize an interpretative dimension of the microscopic and macroscopic mechanism of society that is totally missing in Kuhn. Reflection on the intersections between power and knowledge inevitably entails an evaluation of the direction these processes take together with an evaluation of our society as a whole. This worry, absent in Kuhn, suggests that we should not underestimate the differences in aims and methods between the two writers.

These considerations lead us to the conclusion that the Foukuhnian approach needs to be complemented by further insights. This complementation should, we believe, derive from an insistence on the "knowledge" horn of the Foucaultian power/knowledge duality. Only in this way can the practice-oriented approach hitherto developed lead to an analysis of scientific pedagogy able to encompass two crucial, and interrelated, requirements. First, textbooks should become legitimate historiographical tools, used to illuminate not only the history of pedagogical practices, but, occasionally, the history of science as a whole. This perspective challenges head-on Kuhn's contention that textbooks provide historically and conceptually misleading perspectives on science making. Moreover, this requirement goes hand in hand with the second one: while both Kuhn and Foucault have insisted on the centrality of pedagogical practices in periods of stability and normal science, it is important to extend our gaze to what happens in times of scientific breakthrough. Theories can be in flux on the written page too, if science is in a period of crises. Thus, if we move our spotlight from the quiet days of normal science to the turmoil of an epoch-making crisis, we realize that textbooks cease to be the neutral repository of truth and enter a dialogue with active research. Through this dialogue pedagogy can offer us an original window on the production and dissemination of scientific knowledge.

Key to this twofold extension are the conceptual resources of historical epistemology and the insights they can provide us on the dynamics of scientific knowledge.[6] To begin with, by focusing upon the exploration of "the dynamics of scientific developments, as they can be extracted from an analysis of scientific texts and practices" (Feest and Sturm 2009, 3), historical epistemology has led to the conclusion that one should ease the Kuhnian distinction between normal science and revolutionary periods. Specifically, the historian of science should be entitled to look at textbooks not only as *products* of scientific change, useful only as tools in training regimes, but also as *active agents* in the creative process of scientific development. A new paradigm is not established overnight, and textbooks appear not only at the end-stages of scientific change.

[6] On the multiple forms that historical epistemology can take in different research contexts, see (Daston 1994; Renn 2006; Feest and Sturm 2009; 2011; Rheinberger 2010).

These thoughts nicely complement Foucault's power/ knowledge duality. "Political power always implied the possession of a certain type of knowledge" (Foucault 2000, 31) and especially true knowledge: "[w]e are subjected to the production of truth through power and we cannot exercise power except through the production of truth" (Foucault 1980, 93). Textbooks, Kuhn points out, are repositories of truths, but to reach that status a process of selection, re-evaluation, and redefinition must be put in place. Textbooks contain previously shared knowledge, which undergoes a process of elaboration and reconfiguration. Truth, historically taken, emerges against the background of inadequate knowledge and the investigation of the struggle for truth is precisely what power/knowledge is about:

> [T]o extend the claims to attention of local, discontinuous, disqualified, illegitimate knowledges against the claims of a unitary body of theory which would filter, hierarchize and order them in the name of some true knowledge and some arbitrary idea of what constitutes a science and its objects. (Foucault 1980, 83)

The studies in this volume aim exactly at de-black-boxing the process of construction of truth in textbooks during a period of crisis.

Second, and more generally, an important tradition of cognitive and epistemological studies on learning has led us to realize that research and pedagogy share the same epistemological fabric. Jean Piaget and, more recently, Peter Damerow highlighted the role of reflection on the resources and the tools of knowledge as a crucial step in learning,[7] but the same epistemological process also guides research, even those leading to revolutionary breakthroughs. Nancy Nersessian went as far as stressing a structural similarity between the learning process and conceptual changes:

> Students learning a scientific representation must also actively construct: they must form new concepts and new relations among existing concepts and integrate the new representation to such an extent that they can make use of it. […] [B]oth the nature of the changes that need to be made in conceptual restructuring and the kinds of reasoning involved in the process of constructing a scientific representation are the same for scientists and students of science. That is, the cognitive dimension of the two processes is fundamentally the same. (Nersessian 1989, 165)

Historical epistemology has internalized the piece-by-piece view of knowledge development that this tradition entails. New revolutionary ideas usually emerge at the boundary between different areas of knowledge as the result of internal tensions present in these areas. But a new idea, however radical, is not yet a scientific revolution or a new paradigm. Precisely because it stems from collisions at the boundaries between different theories, it belongs to none of them. At the beginning, innovative ideas are in 'epistemic isolation.'[8] The transition to a new science can be completed only through the long, intricate, and often tedious process of comparing the novel idea with the established body of knowledge (Renn 2006). This attempt at epistemic integration of novelty and tradition progressively unfolds the revolutionary potential of the new idea and generates the consensus about a new approach that characterizes a paradigm. Paraphrasing Kaiser's catchy sentence quoted above:

[7] See for example (Davis 1990; Damerow 1996).
[8] On the concept of epistemic isolation see (Büttner, Renn, and Schemmel 2003).

"revolutions are not born, they are made." Interestingly, and at this point unsurprisingly, the same epistemological drive can be found in scientific pedagogy during a time of crisis, as the articles in this volume show extensively.

Since textbooks, by necessity, bring into contact tradition and novel approaches, they relentlessly explore the potentialities of older tools and their connection with newer ones. This process, which Kuhn interpreted as concealing the tracks of a revolution, recapitulates in reality the essence of the research process. We can see this dynamic instantiated in the books of Planck, Sackur, Sommerfeld or Reiche discussed in this volume.

Again, this insight adds another dimension to Foucault's and Kuhn's positions. Organization of knowledge occurs at different levels and involves different disciplinary matrixes, leading to heterogeneity and blurring of the boundaries sharply drawn in periods of normal science. This is also a Foucaultian theme, best put by Joseph Rouse:

> Knowledge is established not only in relation to a field of statements, but also to objects, instruments, practices, research programs, skills, social networks, and institutions. Some elements of such an epistemic field reinforce and strengthen one another and are taken up, extended, and reproduced in other contexts; others remain isolated from, or conflict with, these emergent "strategies" and eventually become forgotten curiosities. The configuration of knowledge requires that these heterogeneous elements be adequately adapted to one another and that their mutual alignment be sustained over time. (Rouse 2005, 113)

It is on this complex process of combination of heterogeneous elements, of exclusion/inclusion, and of reconfiguration—a process typical of scientific research—that an investigation of "textbooks in flux" can provide illuminating insights.

1.4 Rethinking the History of Quantum Physics

This volume wants to contribute to the study of textbooks as agents of research by focusing attention on one specific episode in the history of scientific change: the so-called quantum revolution.[9] The emergence of quantum theory, in particular, represents an ideal setting because it is a multidisciplinary, delocalized, and multi-actor phenomenon. The canonical account of this chapter in the history of science starts with the *crisis* of black-body radiation and the solution put forth by Max Planck at the turn of the century. After this, Albert Einstein's 1907 theory of specific heats, Niels Bohr's 1913 model of the hydrogen atom, and the advent of Werner Heisenberg's and Erwin Schrödinger's quantum mechanics in the mid-1920s form the conceptual backbone of a story that, together with the development of relativity, has taken pre-eminence in the history of twentieth-century science. Historians of physics have, for decades, struggled to write a coherent account of a process that eludes simplistic explanations. There are too many, too diverse elements that contribute to the complexity of this particular story: the range of the conceptual changes that took place; the number and diversity of the actors involved; the institutional settings; the networks of power and complicities between scientists, popularizers, and science policy makers; the social and

[9] There are some studies concerning the transmission of knowledge during scientific change, such as the paper by Bernadette Bensaude-Vincent on the emergence of the chemical revolution (Bensaude-Vincent 1990). However, no application of this analysis to the quantum revolution has so far been attempted.

cultural ethos of the times around the two World Wars, not to mention the Manhattan Project, the bombs of Hiroshima and Nagasaki, and the Cold War. Furthermore, no other chapter in the history of recent physics, let alone in the history of science, has the same wealth of material available to the historian, including the gigantic project that produced the Archive for History of Quantum Physics.

The essays collected in this volume bring new light to this massive scholarship by concentrating upon early textbooks on quantum theory. This is one outcome of the large-scale, international project coordinated by the Max Planck Institute for the History of Science and the Fritz Haber Institute in Berlin, on the *History and Foundations of Quantum Physics*, that has worked to emphasize the importance of tradition and the conceptual reservoirs of classical physics in the establishment of the quantum revolution, thereby highlighting the continuous aspects within such a dramatic epistemological shift. The rationale behind this volume is that, since textbooks have seldom been treated either as relevant sources or as actors in the development of the new physics, it was worthwhile exploring the possibilities of treating some of these books as subjects around which to write new stories of the quantum.

A specific emphasis on the epistemological aspects of scientific pedagogy during the emergence of quantum physics can turn textbooks into useful research tools in two different senses. First, the study of how textbooks were conceived, projected, and written can elucidate many of the historical circumstances of the coming of age of the quantum revolution, aspects that remained hidden in the study of research papers. To begin with, it gives us access to the revolution on a different time scale because textbooks have a different life cycle from research articles. Furthermore, contrary to research works, pedagogical texts address a broader scope of topics, ranging from atomic theory to physical chemistry, and a wider audience, thus providing us with a wide-angle snapshot of the community involved in the quantum business.

Secondly, there is a particular character to the way textbooks are understood and composed that makes them especially useful for revealing some elements of the intrinsic dynamics of scientific knowledge. Textbooks, particularly in a moment of scientific turmoil, re-organize the inherited body of knowledge and try to integrate it with the emerging theories. This reflective process, which can involve new hypotheses, concepts, and assumptions, but also new formal techniques, procedures, and methods, is essential in igniting productive thinking. In other words, textbooks offer a privileged example of the systemic quality of knowledge, which seems to be a general feature of the transmission of knowledge in its globalizing dimension (Renn and Hyman 2012a).[10]

As the chapters in this book show, there are many different ways in which a textbook can become the subject in a history of early quantum physics, since the very process of writing a textbook, (i.e., of trying to organize a new doctrine in an accessible way for newcomers), together with its life as an object that is issued, used, changed, and abandoned, embodies the tensions between research and pedagogy developed in the first part of this introduction. Furthermore, the life of these textbooks can also help us better situate other actors in the history of quantum physics, by bringing into the picture the reasons, the context, the research

[10] By the same token, a re-evaluation of the epistemological role of textbooks is also necessary in terms of university policy making. A deep reorganization of the university curricula, essential to meet the challenges of the globalized society, requires a broader approach to how scientific knowledge is accumulated and how novelties have to be included in the pedagogical routine. On this topic see the project *Vom lokalen Universalismus zum globalen Kontextualismus* led by Yehuda Elkana and Jürgen Renn and its theoretical foundation in (Elkana 2012).

agenda, and other aspects that cannot be seen in the publication of research papers or in the abundant correspondence between the main actors involved in the story.

Obviously, the first question to address was how to qualify a book as an early textbook on quantum matters. Contrary to the case of chemistry, where there is a longer tradition of textbook writing, going back to the nineteenth century, some of the instances studied in this volume qualify as textbooks, not because they were formally and explicitly written as such, but mainly because they were used as tools to teach quantum physics in higher education. As David Kaiser has recently pointed out, textbooks possess a peculiar *plasticity* with respect to their collocation, their genre, and their boundaries (Kaiser 2012). During scientific re-alignments this feature becomes even more prominent. Furthermore, the complexities and technicalities of the discussions involved narrow the public to which these books were addressed: only professional physicists and advanced students of physics could have a real interest in and ability to follow the nuances present in these books. We, therefore, exclude popular books. The ten case studies presented here include books from well-known actors in the development of quantum physics, like Max Planck, Arnold Sommerfeld, Max Born or Paul Dirac, as well as names that never appear in extant histories of quantum physics, like Otto Sackur or George Birtwistle, but whose books played an active role in the evolution of the pedagogy of quantum physics.

The elaboration of an exhaustive list of textbooks is not easy, since, especially in the very early years, many books deal with established disciplines and include quantum matters only as solutions to specific problems. This introduces the disciplinary problem that some of the case studies in this volume illustrate. Where should quantum theory be pictured in the disciplinary division of the physical sciences at the beginning of the twentieth century? As is well known, Planck developed his hypothesis in the context of a very abstract theory of black-body radiation. This hypothesis, however, did not take root in an incipient community until the quantum hypothesis was compared with the established statistical mechanics and radiation theory. For this process to happen, it was very important to reconfigure the presentation of traditional disciplines so as to indicate the limitations in the classical approaches, but also its hidden potentialities, and its forgotten riches. Marta Jordi and Massimiliano Badino show us, in their studies of Paul Drude and Otto Sackur, respectively, that the pointing out of such limitations and potentialities was not always a pedagogical tool done a posteriori, with the aim of justifying the need for the new theory, but was, at times, prior to the actual development of the theory. Thus, Drude's *Lehrbuch der Optik* fully reconfigured the presentation of optics, moving away from a purely geometrical optics. Bringing the traditions of optics and electromagnetism together, the student was led into the boundaries at the interface between both fields as central topics for research, and not as marginal issues that one might easily overlook. With it in hand, when the quantum solution eventually appeared, the student of Drude's book was ready to understand the new theory in the context of the shortcomings of the reigning models of the interactions between the ether and matter.

Also, Sackur's 1913 book on thermodynamics and thermochemistry shaped the research agenda of a whole new discipline with a crucial change in emphasis in dealing with the long-standing conundrum of specific heats. Whereas traditional discussions started with the specific heats of gases and then extended the analysis to the specific heats of solids, seen as a still unexplained anomaly, Sackur's book presented the issue in the opposite direction, as a means to consolidate his own particular research agenda in his potential students. After

Einstein's 1907 work that solved the problem of the specific heats of solids using the quantum hypothesis, Sackur's was the first book to take the solid not only as an anomaly, but also as the starting point for a reconfiguration of the field. Thus, the old marginal problem became the first building block for ulterior research.

The examples mentioned above take us to the disciplinary boundaries of the emerging quantum physics. Another boundary seldom explored in the accounts of the quantum revolution is that of its publics. Contrary to the development of relativity, which was largely a one-man work, quantum physics evolved due to the creative interactions of a large number of actors. Even so, traditional historical accounts pay attention only to the community of scientists taking an active role in such developments, forgetting its 'popularization' for those professional physicists interested in the new science, but working in other areas of the discipline. In his interesting study on the popularization of the relativity revolution in France, Michel Biezunski argued that scientists from other disciplines wanted to catch up with the most revolutionary developments in order to maintain the socio-epistemic gap that separated them from the general public (Biezunski 1985). In their analysis of the cases of Fritz Reiche and George Birtwistle, Clayton Gearhart and Jaume Navarro show, in different ways, that, in the 1920s, there was already a market composed of physicists and students of physics interested in developing an introductory but sound, technical, and thoroughly mathematical understanding of quantum theory. In both examples, the pedagogy involved is more conservative, in that it struggles to introduce the new physics within old frameworks. The student is, thus, not led to new research problems but to questions that are, up to that point, broadly accepted. In the specific case of Birtwistle, he was no expert in quantum theory; he was not doing active research; but he had a general understanding that moved him to communicate his knowledge of it to other scientists looking for some introduction to the new physics. By contrast, Fritz Reiche, a PhD student of Planck's, was a first-rank physicist with a direct and profound knowledge of quantum physics. As Clayton Gearhart shows in his article, Reiche's lucid book, *The Quantum Theory*, grew out of a specific demand from other portions of the scientific community to get to know more about the new, exotic, but potentially useful quantum theory.

Better known actors, such as Sommerfeld, Born, Van Vleck, Planck or Dirac present us with other aspects of the various traditions of physics pedagogy. Writing a textbook, or a collection of lectures, has a bearing on the dissemination of a certain kind of knowledge and the prestige deriving from it. Dieter Hoffmann's account of Planck as textbooks author illustrates this point by highlighting the labor Planck devoted to bringing to perfection his books and to propagating, in this manner, his take on the emerging quantum theory. The issue of the research agendas implicit in pedagogical works is an important one. It substantiates a point we made in the first part of this introduction: knowledge is generally a struggle and, in times of crisis, it easily becomes a struggle for the establishment of orthodoxy and the simultaneous exclusion of heterodoxy. As Michael Eckert thoroughly documents, Sommerfeld's *Atombau und Spektrallinien* was not only a prominent advertisement of quantum theory but also a prominent display of *his* quantum theory, which was largely a theory of atomic physics and atomic modeling. By turning his lectures and seminars into a book, Sommerfeld was spreading his research agenda to a public eager to have a first big synthesis of quantum physics. Furthermore, by employing the mathematical techniques of celestial mechanics in his modeling of the atom, Sommerfeld was exposing a large community of astronomers and physicists to his own research agenda. It is not by chance that Sommerfeld toured the United States as well. But the American scientific community was not to remain a passive receiver forever. John Van Vleck was a protagonist in the process of critically recasting the new quantum theory in terms of what was gained and what was lost with respect to earlier traditions. His approach, which Michel Janssen and Charles Midwinter analyze using the concept of Kuhn losses, was beneficial for putting the American physical community on the map of the emerging quantum physics.

The dissemination of a particular perspective on quantum theory opens up the issue of the de-localization of scientific knowledge, that is its supposed universal character as opposed to national differences. Sommerfeld's extensive influence as a teacher both in time (on generations of students) and in space (through his extended trip in the United States) was crucial to the establishment of atomic theory and spectroscopy as *the* main problem of quantum theory in Germany and the world over. Van Vleck was implicitly highlighting this process of de-localization when he complained about the superabundance of attention given to spectroscopy at the expenses of other interesting problems, possibly closer to the American tradition. At the same time, though, some national figures stubbornly resisted the globalization of quantum theory. For instance, Cambridge scholars such as Birtwistle and Dirac insisted on viewing quantum theory from the angle of the British problem-solving approach relying on a substantial use of analytical mechanics.

From a different perspective, Born’s, Dirac’s, and also Pascual Jordan’s efforts to axiomatize and systematize quantum theory as theoretical physics, offer us good examples of how the task of writing a book suitable as a textbook involves more than just the transmission of already published research. In these three examples, unfolded in different fashions by Domenico Giulini, Don Howard, and Helge Kragh, we are introduced to the philosophical background that leads these authors to look for the foundations of the new theory and the logical developments that stem from such foundations. These articles also show another crucial difference between textbooks and research papers. Only the former are suitable sites to muse about the foundations of the field. From this perspective, textbooks provide a seldom-recognized service to the active scientist, and to the historian as well. However unsuccessful one particular axiomatization might have been, as in the case of Born, whose *Vorlesungen über Atommechanik* was published at the same time as Heisenberg was introducing the new quantum mechanics, these efforts can be seen as a way to prioritize the need for immediate research into certain open questions above that into other, less-pressing ones.

Finally, many of the case studies discussed in this volume deal with books that were re-issued in subsequent editions. The evolution we find in these different editions shows the tensions embodied in the task of writing on quantum physics in a time of great change, to the extent that, as Eckert says, the book itself ceases to be one static entity but becomes a process.

1.5 About This Book

This book has a curious story. The idea to start a project on the role of textbooks in quantum theory came to the editors’ minds in early 2009, when they were both working in the History of Quantum Physics Project of the Max Planck Institute for the History of Science (MPIWG) in Berlin. They thought that a good way to begin collecting ideas was to organize a four-speaker panel at the upcoming History of Science Society Conference. So they sent around a call for papers. The enthusiastic reaction of their colleagues surprised and almost overwhelmed the editors, who ended up submitting two special sessions of five speakers each.

The project gained momentum rapidly. To prepare the HSS conference, a workshop was organized between some of the presenters, members of the Quantum Project, colleagues, and visitors at the MPIWG. The workshop took place on 7 October 2009 and produced many exciting discussions. We would like to thank Arianna Borrelli, Jed Buchwald, Diana Kormos Buchwald, Ed Jurkowitz, Shaul Katzir, Christoph Lehner, Jürgen Renn, Arne Schirrmacher, Daniela Schlote and Dieter Suisky for their contributions to that meeting.

The two special sessions on textbooks in quantum physics eventually took place at the HSS Annual Meeting in Phoenix, AZ in late November 2009. On that occasions, talks were delivered by Massimiliano Badino, Michael Eckert, Clayton Gearhart, Don Howard, David Kaiser, Michel Janssen, Marta Jordi, Daniela Monaldi, and Jaume Navarro. Domenico Giulini could not make it for personal reasons. The sessions were a big success and we benefited tremendously from the discussion with the audience. Cathryn Carson and Richard Staley were especially generous in providing productive comments and encouragement to go ahead with our idea.

Back in Europe, we realized that it was time for the next step, that is the organization of our results into the form of an edited book. However, since we wanted more than just a bunch of papers tied together by a loose topic, but rather a new historiographical perspective on quantum physics, we took our time. The History of Quantum Physics Conference in Berlin was coming up and we decided that it was the ideal opportunity to define better our approach and to confront once again the community of historians which was our main intended audience. At the conference, in July 2010, the two editors of this book presented the definitive set-up of the project and discussed more thoroughly the structure of the volume with the authors, all of them in attendance at the conference.

From that moment the book project officially started. And, as any good editor or author knows all too well, it was just the beginning of another journey. Some of the original participants stepped down, some new joined in. In July 2011, we further discussed the structure of the book in a very interesting session devoted to scientific textbooks at the 11th Conference of the International History and Philosophy of Science Teaching Group in Thessaloniki. That experience was important for both of us. The ensuing process of writing, re-writing, re-discussing and negotiating the contributions and this introduction went on for many months. Of course, a series of technical problems cropped up, which were solved with commendable dedication by the editorial team (Irene Colantoni, Oksana Kuruts, Jonathan Ludwig, Marius Schneider, and Chandhan Srinivasamurthy) headed by Nina Ruge. Kai Surendorf took patient care of our requests concerning the fine-tuning of the LaTeX infrastructure and Jeremiah James did wonderful editing work at various stages of the production process.

The History of Quantum Physics Project at the MPIWG has been a stimulating common effort to look at the complex developments of quantum physics from new and sometimes unorthodox angles. For several years we have been discussing and exchanging ideas on a daily basis and it would be futile to isolate individual contributions to the overall setting of this volume. Therefore, we feel that we have to thank all colleagues whose various suggestions permeate this book: Alexander Blum, Arianna Borrelli, Shaul Katzir, Martin Jähnert, Jeremiah James, Christian Joas, Ed Jurkowitz, Christoph Lehner, and Arne Schirrmacher. Jürgen Renn represented an inexhaustible source of inspiration. Many readers will immediately perceive his presence lingering in this introduction. All the rest must be ascribed to (better: blamed on) the editors.

References

Bensaude-Vincent, Bernadette (1990). A View of the Chemical Revolution through Contemporary Textbooks: Lavoisier, Fourcroy and Chaptal. *British Journal for the History of Science* 23: 435–460.

— (2006). Textbooks on the Map of Science Studies. *Science and Education* 15: 667–670.

Bertomeu-Sánchez, José R., Antonio García-Belmar, Anders Lundgren, and Manolis Patiniotis (2006). Introduction: Scientific and Technological Textbooks in the European Periphery. *Science and Education* 15: 657–665.

Biezunski, Michel (1985). Popularization and Scientific Controversy. The Case of the Theory of Relativity in France. In: *Expository Science: Forms and Functions of Popularisation*. Ed. Terry Shinn and Richard Whitley. IX. Sociology of the Sciences. Dordrecht: Reidel. 183–194.

Brock, William H. (1972). Liebig's Laboratory Accounts. *Ambix* 1: 47–58.

Brooke, John H. (2000). Introduction: The Study of Chemical Textbooks. In: *Communicating Chemistry. Textbooks and Their Audiences, 1789-1939*. Ed. Anders Lundgren and Bernadette Bensaude-Vincent. Canton: Science History Publications. 1–18.

Bunders, Joske and Richard Whitley (1985). Popularisation within the Sciences: The Purposes and Consequences of Inter-Specialist Communication. In: *Expository Science: Forms and Functions of Popularisation*. Ed. Terry Shinn and Richard Whitley. IX. Sociology of the Sciences. Dordrecht: Reidel. 61–77.

Büttner, Jochen, Jürgen Renn, and Matthias Schemmel (2003). Exploring the Limits of Classical Physics: Planck, Einstein, and the Structure of a Scientific Revolution. *Studies in History and Philosophy of Modern Physics* 34: 37–59.

Cloitre, Michel and Terry Shinn (1985). Expository Science: Social, Cognitive and Epistemological Linkage. In: *Expository Science: Forms and Functions of Popularisation*. Ed. Terry Shinn and Richard Whitley. IX. Sociology of the Sciences. Dordrecht: Reidel. 31–60.

Collins, Harry M. (1985). *Changing Order. Replication and Induction in Scientific Practice*. Chicago: The University of Chicago Press.

Damerow, Peter (1996). *Abstraction and Representation. Essays on the Cultural Evolution of Thinking*. 175. Boston Studies in the Philosophy of Science. Dordrecht: Kluwer Academic Publishers.

Daston, Lorraine (1994). Historical Epistemology. In: *Questions of Evidence*. Ed. James Chandler, Arnold I. Davidson, and Harry D. Harootunian. Chicago: The University of Chicago Press. 282–289.

Davis, Robert B. (1990). *Learning Mathematics. The Cognitive Science Approach to Mathematics Education*. 3rd ed. New Jersey: Ablex.

Dewey, John (1938). *Experience and Education*. Indianapolis: Kappa Delta Pi Publications.

Dreyfus, Hubert L. and Paul Rabinow (1983). *Michel Foucault: Beyond Structuralism and Hermeneutics*. 2nd ed. Chicago: The University of Chicago Press.

Elkana, Yehuda (2012). The University of the 21st Century: An Aspect of Globalization. In: *The Globalization of Knowledge in History*. Ed. Jürgen Renn. 1. Max Planck Research Library for the History and Development of Knowledge, Studies. Berlin: Edition Open Access. 605–630.

Fahnestock, Jeanne (1986). Accomodating Science: The Rhetorical Life of Scientific Facts. *Written Communication* 3: 275–296.

Feest, Uljana and Thomas Sturm, eds. (2009). *What (Good) is Historical Epistemology?* Berlin: Max-Planck-Institut für Wissenschaftsgeschichte. Preprint 386.

— (2011). What (Good) is Historical Epistemology? Editors' Introduction. *Erkenntnis* 75: 285–302.

Fleck, Ludwik (1979). *Genesis and Development of a Scientific Fact*. Ed. Thaddeus J. Trenn and Robert K. Merton. Chicago: The University of Chicago Press.

Foucault, Michel (1977). *Discipline and Punish*. New York: Pantheon.

— (1980). *Power/Knowledge. Selected Interviews and Other Writings, 1972-1977*. Ed. Colin Gordon. New York: Vintage Books.

— (2000). *Power*. Ed. James D. Faubion. New York: The New Press.

Galison, Peter L. and David J. Stump, eds. (1996). *The Disunity of Science: Boundaries, Contexts, and Power*. Stanford: Stanford University Press.

García-Belmar, Antonio, José R. Bertomeu-Sánchez, and Bernadette Bensaude-Vincent (2006). The Power of Didactic Writings: French Chemistry Textbooks of the Nineteenth Century. In: *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*. Ed. David Kaiser. Cambridge, MA: The MIT Press. 219–251.

Gavroglu, Kostas and Ana Simoes (2000). One Face or Many? The Role of Textbooks in Building the New Discipline of Quantum Chemistry. In: *Communicating Chemistry. Textbooks and Their Audiences, 1789-1939*. Ed. Anders Lundgren and Bernadette Bensaude-Vincent. Canton: Science History Publications. 415–449.

Goldstein, Jan (1984). Foucault among the Sociologists: The "Disciplines" and the History of the Professions. *History and Theory* 23: 170–192.

Gross, Alan G. (1990). *The Rhetoric of Science*. Cambridge, MA: Harvard University Press.

Hall, Karl (2006). 'Think Less about Foundations': A Short Course on the Course of Theoretical Physics of Landau and Lifshitz. In: *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*. Ed. David Kaiser. Cambridge, MA: The MIT Press. 253–286.

Hannaway, Owen (1975). *The Chemists and the World: The Didactic Origins of Chemistry*. Baltimore: Johns Hopkins University Press.

Hilgartener, Stephen (1990). The Dominant View of Popularization: Conceptual Problems, Political Issues. *Social Studies of Science* 20: 519–539.

Holmes, Frederic L. (1989). The Complementarity of Teaching and Research in Liebig's Laboratory. *Osiris* 5: 121–164.

Hughes, Jeff (1998). 'Modernists with a Vengeance': Changing Cultures of Theory in Nuclear Science, 1920-1930. *Studies in History and Philosophy of Science* 29: 339–367.

Hunger Parshall, Karen and David E. Rowe (1994). *The Emergence of the American Mathematical Research Community, 1876-1900: J. J. Sylvester, Felix Klein, and E. H. Moore*. 8. History of Mathematics. Providence: American Mathematical Society.

Jungnickel, Christa (1979). Teaching and Research in the Physical Sciences and Mathematics in Saxony, 1820-1850. *Historical Studies in the Physical Sciences* 10: 3–47.

Kaiser, David (2005). *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics*. Chicago: The University of Chicago Press.
— (2006). Introduction: Moving Pedagogy from the Periphery to the Center. In: *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*. Ed. David Kaiser. Cambridge, MA: The MIT Press. 1–8.
— (2012). A Tale of Two Textbooks. Experiments in Genre. *Isis* 103: 126–138.
Kaiser, David and Andrew Warwick (2006). Kuhn, Foucault, and the Power of Pedagogy. In: *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*. Ed. David Kaiser. Cambridge, MA: The MIT Press. 393–409.
Klein, Ursula (2003). *Experiments, Models, Paper Tools. Cultures of Organic Chemistry in the Nineteenth Century*. Stanford: Stanford University Press.
Kuhn, Thomas S. (1962). *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press.
— (1963). The Function of Dogma in Scientific Research. In: *Scientific Change*. Ed. Alistair C. Crombie. New York: Basic Books and Heineman. 347–369.
— (1977). The Essential Tension: Tradition and Innovation in Scientific Research. In: *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: The University of Chicago Press. 225–239.
Latour, Bruno (1987). *Science in Action: How to Follow Scientists and Engineers through Society*. Cambridge, MA: Harvard University Press.
— (1988). *The Pasteurization of France*. Cambridge, MA: Harvard University Press.
Latour, Bruno and Stephen Woolgar (1986). *Laboratory Life: The Social Construction of Scientific Facts*. 2nd ed. Princeton: Princeton University Press.
Morell, Jack B. (1972). The Chemist Breeder: The Research Schools of Liebig and Thomas Thomson. *Ambix* 1: 1–46.
Myers, Gregory A. (1992). Textbooks and the Sociology of Scientific Knowledge. *English for Specific Purposes* 11: 3–17.
Nersessian, Nancy J. (1989). Conceptual Change in Science and in Science Education. *Synthese* 80: 163–183.
Olesko, Kathryn M. (1991). *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics*. Ithaca: Cornell University Press.
— (2006). Science Pedagogy as a Category of Historical Analysis: Past, Present, and Future. *Science and Education* 15: 863–880.
Prelli, Lawrence J. (1989). *A Rhetoric of Science: Inventing Scientific Discourse*. Columbia: University of South Caroline Press.
Pyenson, Lewis (1979). Mathematics, Education, and the Göttingen Approach to Physical Reality, 1890-1914. *Europa. A Journal of Interdisciplinary Studies* 2: 91–127.
Pyenson, Lewis and Douglas Skopp (1977). Educating Physicists in Germany circa 1900. *Social Studies of Science* 7: 329–366.
Renn, Jürgen (2006). *Auf den Schultern von Riesen und Zwergen*. Weinheim: Wiley-VCH.
Renn, Jürgen and Malcom D. Hyman (2012a). The Globalization of Knowledge in History: An Introduction. In: *The Globalization of Knowledge in History*. Ed. Jürgen Renn. 1. Max Planck Research Library for the History and Development of Knowledge, Studies. Berlin: Edition Open Access. 15–44.

— (2012b). The Globalization of Modern Science. In: *The Globalization of Knowledge in History*. Ed. Jürgen Renn. 1. Max Planck Research Library for the History and Development of Knowledge, Studies. Berlin: Edition Open Access. 561–604.

Rheinberger, Hans-Jörg (2010). *On Historicizing Epistemology*. Stanford: Stanford University Press.

Rouse, Joseph (1987). *Knowledge and Power. Toward a Political Philosophy of Science*. Ithaca: Cornell University Press.

— (2005). Power/Knowledge. In: *The Cambridge Companion to Foucault*. Ed. Gary Gutting. 2nd ed. Cambridge: Cambridge University Press. 95–122.

Seth, Suman (2010). *Crafting the Quantum: Arnold Sommerfeld and the Practice of Theory, 1890–1926*. Cambridge, MA: The MIT Press.

Simon, Josep (2011). *Communicating Physics: The Production, Circulation, and Appropriation of Ganot's Textbooks in France and England, 1851-1887*. London: Pickering and Chatto.

Skaar Jacobsen, Anja (2006). Propagating Dynamical Science in the Periphery of German Naturphilosophie: H. C. Ørsted's Textbooks and Didactics. *Science and Education* 15: 739–760.

Smyth, Mary M. (2001). Fact Making in Psychology: The Voice of the Introductory Textbooks. *Theory and Psychology* 11: 609–636.

Stroud, Barry (2011). Epistemology, the History of Epistemology, Historical Epistemology. *Erkenntnis* 75: 495–503.

Traweek, Sharon (1988). *Beamtimes and Lifetimes: The World of High Energy Physicists*. Cambridge, MA: Harvard University Press.

Vicedo, Marga (2012). Introduction: The Secret Lives of Textbooks. *Isis* 103: 83–87.

Warwick, Andrew (2003). *Masters of Theory: Cambridge and the Rise of Mathematical Physics*. Chicago: The University of Chicago Press.

Whitley, Richard (1985). Knowledge Producers and Knowledge Acquirers. Popularisation as a Relation between Scientific Fields and Their Publics. In: *Expository Science: Forms and Functions of Popularisation*. Ed. Terry Shinn and Richard Whitley. IX. Sociology of the Sciences. Dordrecht: Reidel. 3–28.

Wittgenstein, Ludwig (1953). *Philosophical Investigations*. Oxford: Basil Blackwell.

Chapter 2
Sorting Things Out: Drude and the Foundations of Classical Optics

Marta Jordi Taltavull

2.1 Introduction

> My goal will be reached if these pages will strengthen in the reader the view that optics is not an old, worn-out domain of physics, but that also here a fresh life pulses, the contribution to whose further nourishment should be enticing for anyone.[1] (Drude 1900a, vi)

With these stimulating words Paul Drude aimed at engaging physicists, in 1900, in the reading of his textbook *Lehrbuch der Optik*. More than one hundred years later, in this paper I will try to clarify historically these inspired words: In what sense could optics have been considered old? In which aspects did Drude's new account of optics "pulse with fresh life"? How could the further "nourishment" of optics take place in Drude's view? To what extent did Drude succeed in achieving his goal?

In fact, in 1985 Jed Z. Buchwald already spoke of Drude's *Lehrbuch der Optik* in his thorough account on the complex and gradual transition between the macroscopic outlook of Maxwell's electrodynamics and Drude's microscopic approach to electromagnetic optics (Buchwald 1985). *Lehrbuch der Optik* was Buchwald's finale. I share with Buchwald the viewpoint that *Lehrbuch der Optik* was the first encompassing work in which a microscopic approach to optics was established. For this reason, the book is a particularly interesting subject of study. However, in this paper, instead of analyzing Drude's work against the background of the general history of electrodynamics, I will explore the articulation of the book with other contexts.

First, I understand *Lehrbuch der Optik* not only as a singular point in Drude's career, but as the result of a long process, started in the early 1890s, through which he reflected upon and changed his understanding of what optics should be. Moreover, *Lehrbuch der Optik* not only had an impact on the physics community; the endeavor to write a comprehensive book on optics was also important for Drude as a way to organize his knowledge, strengthen his views on the field, and revamp his career. I will thus follow the story of *Lehrbuch der Optik* through the development of optics, and not electrodynamics. Second, to understand better the distinctiveness of Drude's decisions for a redefinition, both ontological and epistemological, of optics, I will follow Drude in his conversation with his contemporaries, in particular his mentor and dissertation advisor Woldemar Voigt, and in relation to other views about optics conveyed in coetaneous textbooks. Third, after I arrive at Drude's construction of *Lehrbuch der Optik*, I will then go further to ask about the impact the book had in setting the tone for future generations, especially in stimulating further research in the field. To answer

[1] All translations are done by the author.

the above mentioned questions, I have divided the paper into five sections corresponding to different periods of Drude's life and career.

In the first section, I will follow Drude's early career at the University of Göttingen, from 1887 until 1894, under Voigt's supervision. My concern will be to identify those aspects of the Göttingen approach to optics that were unsatisfactory for Drude and, at the same time, to unfold Drude's gradual shaping of an alternative view. A choice between the mechanical and the electromagnetic theory of light, together with the criteria upon which to rely in making such a decision, were both at stake for Drude in this period. His first textbook, published in 1894, *Physik des Aethers*, embodied his decisions on these matters, and thereby marked an important turning point in his life and career.

From 1894 to 1900, Drude served as full professor at the University of Leipzig, where he could develop in depth his own standpoint on optics, hinging completely upon the electromagnetic theory of light. In the years after 1894, Drude managed the incorporation of matter into the electromagnetic picture of light. By 1900 he envisioned a unified theoretical approach for a variety of optical phenomena stemming from different kinds of interactions between light and matter. In his second textbook, *Lehrbuch der Optik*, published in 1900, Drude displayed his own, programmatic view, which merged the electromagnetic theory of light with the dynamical action of the microstructure of matter. I will give an account of Drude's development between 1894 and 1900 in the third section.

Lehrbuch der Optik was not a compendium of well-established knowledge in optics. New optical phenomena were explored experimentally at the end of the nineteenth century, which led to a revitalization of theoretical discussions about the interaction between light and matter. For this reason, many textbooks had become rapidly outdated. Drude's *Lehrbuch der Optik* was very innovative in attempting to encompass both old and new phenomena, through his personal strategy of merging electromagnetism and matter. But Drude's textbook was original also for other reasons. Most European textbooks on optics evinced a special concern for the nature of the ether, because light presumably amounted to the perturbation of that substance. In *Lehrbuch der Optik*, instead, Drude relinquished questions about the constitution of the ether, taking the electromagnetic equations of light as the starting point for his account of optical phenomena. Actually, Drude did not eschew mentioning the ether as the substratum for light, but in starting from the light equations, Drude completed a radical shift in the kind of questions textbooks had addressed so far: from the relation between the nature of the ether and its mathematical expression, to the relation between the microstructure of matter and the modification of the light equations that captured this new dimension of optical phenomena. I will describe the content, organization, and main points of *Lehrbuch der Optik* in the fourth section.

In the fifth section, I examine Drude's work, between 1900 and 1906, concerning the relation between optical phenomena and the microstructure of matter. Important outcomes stemmed from the incorporation of the electron into the previous picture of optics, which allowed Drude to network optics with other fields in science, like chemistry. Such modifications were included in a second edition of his book in 1906.

All in all, Drude not only provided readers with explanations of new phenomena, but also with new questions to ask and a new methodological approach to optics. Advancing into almost virgin terrain, Drude's claims in *Lehrbuch der Optik* had a strong impact, which I will analyze in the epilogue of the paper. Without criticizing directly previous works in optics, Drude redefined them as part of a past that should be overcome. He simply reorganized optical knowledge in such a way that these older traditions were not mentioned or were re-understood through "Drude's lens." *Lehrbuch der Optik* remained influential for years to come, in part through Woldemar Voigt's 1908 *Magneto- und Elektrooptik*, in which he extended Drude's take on optics to the explanation of new features of optical phenomena. Both theoretical and experimental physicists used Drude's and Voigt's accounts as points of reference in the early twentieth century. Later physicists started to juxtapose such approaches with the emerging quantum theory, particularly after Bohr's 1913 quantum model of the atom. Most importantly, what came to be considered classical optics, as opposed to quantum physics, was not simply what came before quantum optics. This is another reason it is important to analyze Drude's *Lehrbuch der Optik*: Drude's selection and reorganization of nineteenth-century optics became the paradigm of "classical optics," against whose backdrop physicists constructed the quantum understanding of optical phenomena.

2.2 Göttingen 1887–1894: From the Optics of Ether to the Electromagnetic Equations

2.2.1 On Voigt's Footsteps

Paul Drude was born on 12 July 1863 in Braunschweig, where he lived until he completed his studies at the local Gymnasium in 1882 (Hoffmann 2006; Goldberg 1990). Thereafter, he studied mathematics, first at Göttingen and then at Freiburg and Berlin. In the sixth semester, he decided to return to Göttingen and devote himself to theoretical physics, under the guidance of Woldemar Voigt, director of the Physics Institute at Göttingen. Drude's early research was very much influenced by Voigt in terms of subject matter, guidelines, and research procedures. Voigt, in turn, was a faithful heir of Franz Ernst Neumann's approach to physics. Thus it is of key importance to trace the scientific genealogy from Neumann to Drude, in order to understand the specific tradition within which Drude grew up.

Neumann, one of the founding fathers of German theoretical physics, was *Privatdozent* for physics and mineralogy starting in 1826 at the University of Königsberg. To supplement his lectures, in 1833, he inaugurated the German mathematical-physical seminar, through which he trained his students, including Voigt, in his particular approach to theoretical physics. Optics was one of the principal topics of interest in Neumann's seminar. At that time, it was commonplace to think that light consisted of elastic perturbations propagated through a transparent substance filling everything, called the ether. An optical theory amounted to the set of differential equations and boundary conditions describing the behavior of the ether, which were to be derived from the application of general mechanical principles to that hypothetical elastic substance. Gaining optical knowledge meant then to obtain the most complete set of equations and boundary conditions, which, on the one side, were supposed to manifest the true properties of the ether, and on the other side, had to describe mathematically optical measurements. The phenomena of reflection and refraction through crystals were the main target of optical theories.

In such a dualist scheme, Neumann pursued a very specific methodology, which Kathryn Olesko dubbed the "ethos of exactitude" (Olesko 1991). The key to Neumann's approach lay in mastery of the relations between the mathematical equations describing the ether and experimental measurements, which required the development of numerical techniques to fit accurate empirical data into theoretical formulas, the improvement of strategies to eliminate the possible experimental sources of error, and the identification of the key parameters for a better comparison between theoretical and observable quantities. As Olesko pointed out, following the ethos of exactitude, experimental reenactment was stimulated only by the desire to check theory, while the creative task of enhancing optical theories was restricted to the addition or modification of differential terms in the mathematical equations describing the ether. No additional hypotheses on the underlying interaction between ether and matter were called for. Thus, no new level of physical explanation beyond the principles of mechanics and the mathematical completion of the ether equations was added. This dynamics of knowledge indeed led to an effective exploration of the limits of the present theories of ether, but not necessarily to new conceptual frameworks.

Voigt completed his dissertation in 1874, expanding upon one of the most frequently recurrent topics in optics tackled at the Königsberg seminar: the behavior of light reflected by or refracted through crystals. On the one hand, Voigt worked with optical constants, which were the measurable quantities that accounted for the behavior of light at the border between ether and matter, i.e. the refractive index and the coefficient of reflectivity of the crystal. Voigt literally spent hours in the laboratory measuring the optical constants of manifold crystal samples. On the other hand, Voigt enhanced Neumann's initial set of differential equations and boundary conditions so that the most satisfactory mathematical expression for the optical constants that fit into his measurements could be derived from them. In general, Voigt aimed to go a step further in the theoretical understanding of optics: the "causal nexus" between the kind of substance explored (different crystals in this case) and the modification of the properties of the ether, represented in a set of differential equations. It is important to notice that, within this framework, the role of matter was solely to *modify* the ether's properties: ether, whether filling the interstices of matter or surrounding it, was considered the only substratum of light propagation. Matter did not play a role in the production of light. From this point of view, the mathematical description of optical phenomena should be a mirror of the ether's behavior.

Voigt brought the Königsberg tradition to the Göttingen Physics Institute in 1883 (Olesko 1991, 412–414). Four years later, Drude finished a dissertation in Göttingen that was a continuation of Voigt's own. Equally driven by the ethos of exactitude, Drude set out to study the optical constants of crystals, although he focused on one very specific class of crystals: those that not only refracted and reflected light, but also partially absorbed it (Drude 1887). For several years after his dissertation, Drude continued to work and publish on this problem, extending his research from crystals to metals. To be sure, Drude's close faithfulness to Voigt's guidelines turned out not to be very beneficial for him. Precisely because he was often regarded, among German theoretical physicists, as unduly dependent on his teacher, from 1887 until 1894 Drude found it very difficult to obtain a job outside of Göttingen (Jungnickel and McCormmach 1986). But his reputation improved in 1894, with the publication of his first textbook *Physik des Aethers*, where he clearly distanced himself from Voigt's agenda and started supporting the electromagnetic theory of light. However, Drude's change of heart did not happen overnight and involved much more than a substitution of one theory of light with another.

2.2.2 Towards a New Way to Optical Knowledge: *Practical Physics*

In 1887–1888, in his laboratory in Karlsruhe, Heinrich Hertz observed that electromagnetic disturbances of the ether exhibited wave-like characteristics and propagated through the ether at the speed of light (Hertz 1888).[2] It is common wisdom that Hertz's experiments provided physicists with a strong argument for the unification of electromagnetism and optics and were a breakthrough for the dissemination of the electromagnetic theory of light in Continental Europe. In 1888–1889, Voigt's students discussed the electromagnetic theory of light in connection with Hertz's experiments in the mathematical-physical seminar in Göttingen (Olesko 1991, 412). From then onwards, Drude divided his interest between the Neumann-Voigt mechanical theory of light and Maxwell's electromagnetic theory. At the beginning, he made no choice between them. In fact, he made up his mind only after a long process of reflection during which he carefully examined and compared both approaches. What was at stake was not simply which theory to choose but, above all, which criteria to use to decide the most satisfactory theory. The conclusions Drude eventually arrived at, in 1894, were significantly different from Voigt's conservationist position at that time.

Drude's first open demonstration of a radical departure from Voigt's standpoint was his provocative 1892 paper (Drude 1892a). There Drude addressed one of the most puzzling consequences of comparing electromagnetic and mechanical theories of light: the various sets of differential equations derived from considering the ether as an elastic substance were mathematically equivalent to Maxwell's electromagnetic equations. In fact, in addition to Neumann's mechanical theory of light, there existed others that also described the phenomena of refraction and reflection of light satisfactorily, most significantly Fresnel's. What differentiated them were specific properties ascribed to the elastic ether. In particular, Neumann considered ether an incompressible substance, while Augustin Fresnel regarded it as a compressible material.[3] Nevertheless, these theories of light all led to equivalent mathematical equations and boundary conditions, including electromagnetic equations. This was, according to Drude, a powerful reason to discount optics altogether:

> Since many different theories, which derive from very different basic assumptions, can account for scores of observable features in the same way without contradictions, the theoretical research on optical phenomena has been discredited to the extent that one tries to understand these phenomena through mathematical and almost philosophical speculations, from which new knowledge about the true properties of nature cannot be extracted, for the same properties are explained differently in the different theories. (Drude 1892a, 366)

What Drude described was an epistemological dilemma. The only criteria the ethos of exactitude offered to evaluate the validity of an optical theory was the precision of the numerical agreement between experimental data and theoretical predictions. Now, given the mathematical equivalence between mechanical and electromagnetic theories of light, it was clear that numerical exactitude could not be the only way to mediate between optical experiments and the physical properties of the ether. How to proceed in this situation? Drude's way-out involved a twofold break with Neumann's and Voigt's tradition.

First of all, Drude endorsed a more radical phenomenological standpoint: theories of light he reduced to just the differential equations and the imposed boundary conditions. He called the combination of these two ingredients an *Erklärungssystem* (explanatory system).

[2]For a general account of the dissemination of the electromagnetic theory of light in Continental Europe, see (Darrigol 2000; Buchwald 1985). More specifically about Hertz's contribution, see (Buchwald 1994).

[3]The story of the various theories of the lumiferous ether in the nineteenth century is rather intricate. A good overview can be found in (Whittaker 1910).

The point of departure for optics was then the *Erklärungssystem*. Questions about the true nature of the ether became irrelevant for the mathematical construction of an optical theory, while the physical system to be studied was reduced to the mathematical parameters making up the *Erklärungssystem*. The choice of an optical theory was thus a choice of language: either density, elasticity, and velocity of ether perturbations, or magnetic permeability, dielectric constant, and magnetic field strength.

Secondly, since the ethos of exactitude had exhausted its potential for revealing new knowledge about the ether, Drude hinted at other possible criteria to help one choose among the different theories of light. More specifically, he claimed that

> the adoption of the electromagnetic theory of light seems to be a significative step in the true understanding of nature, since the velocity of light in vacuum or through air derives directly from electromagnetic features. (Drude 1892a, 366)

In other words, Drude promoted a unification of optical and electromagnetic theories from below. Given that experiments proved that optical and electromagnetic waves propagated at the same velocity, one was inclined to extend this coincidence to the rest of the optical and electromagnetic features. Unification would then mean, according to his radical phenomenological move, the adoption of a single physical language to describe the mathematical equations accounting both for optical and electromagnetic phenomena.

Voigt was also aware of the mathematical equivalence between mechanical and electromagnetic theories. Nevertheless, for years he did not sympathize with this idea of unifying optics and electromagnetism from below. For him, the only way to strive for unification was the determination of the properties of a general ether, from which the equations of optical and electromagnetic phenomena could be derived. Thus, adopting an electromagnetic language would mean, for him, losing generality, and restricting oneself to only one possible nature for the ether. In fact, Voigt had conveyed his own point of view in a paper published just one year before Drude's paper (Voigt 1891). In it, Voigt maintained a "mathematical viewpoint" in the development of optical theories. That is to say, like Drude, he decided to work directly with differential equations. But unlike his disciple, for the sake of generality, Voigt concealed any decision about the physical interpretation of mathematical terms. This eventually implied continuing to rely on the "incontestable principles of mechanics" (Voigt 1891, 411). Voigt upheld such a mathematical viewpoint until the publication of the second volume of his ambitious *Kompendium der Theoretischen Physik* (Voigt 1896).

The break with his master notwithstanding, Drude was not alone in his positivistic move, which he dubbed "practical physics." As he mentioned in his 1892 paper, Drude found inspiration in Hertz's treatment of electrodynamics. In 1890 Hertz had stated that Maxwell's equations contained everything that was essential in Maxwell's theory, so that any attempt to derive them from mechanical models of the electromagnetic ether, as had been done in the past, overshot the mark (Hertz 1890). In his paper, Hertz simply postulated the electromagnetic equations, which he obtained by simplifying Maxwell's formalism and detaching it from any physical assumptions concerning the nature of the electromagnetic forces. The electrical polarizations of the medium were, for him, the only things truly present. To look for their origin in some essential quality of the ether was futile.[4] In fact, such a reformulation of Maxwell's equations became very popular in Europe thanks to its clarity and synthetic

[4]In fact, the British physicist Oliver Heaviside had been working on a similar reformulation of Maxwell's equations since 1885, as Hertz rightly acknowledged in his paper. For more information about the developments of

value. Thus immediately after Hertz, other physicists, like Hermann von Helmholtz and Hendrik Antoon Lorentz, adopted it.

Despite Drude's clear alignment with Hertz's approach, he still pondered for some time the definitive adoption of the electromagnetic theory of light. The theory was troubled by one important difficulty: Maxwell's equations accounted well for the phenomena of reflection and refraction of light, but not for those optical phenomena in which matter was assumed to contribute directly to the generation and absorption of light waves. Ever since the early 1870s, it had become clear to physicists that ether waves were not sufficient to describe optical phenomena, like optical dispersion. In these cases, differential equations referring to the ether had to be combined with differential equations accounting for the action of matter. While there were attempts to interweave the action of ether and matter in the framework of the mechanical theories of light, Maxwell's electromagnetic equations applied only to the ether. To fill this gap, Drude suggested that "in order to fix the facts rightly, also this theory [the electromagnetic one] must be built upon enlarged assumptions [incorporating the action of matter], at the expense of the advantage of its simplicity and evidence" (Drude 1892a, 366). Hence, in the ensuing two years, Drude worked on the possibility of extending the scope of the electromagnetic theory of light to other optical phenomena apart from reflection and refraction. Namely, he wanted to find the *Erklärungssysteme*[5] for these other phenomena, and relate them to Maxwell's *Erklärungssystem* for ether. In particular, he dwelled upon the Kerr effect (Drude 1892b) and optical dispersion (Drude 1893). I will concentrate on the last example, because of its persistence and its special significance to the story.

2.2.3 Optical Dispersion and the "Practical Physics" at Work

Beginning in the seventeenth century, optical dispersion was understood as the continuous spread of white light into different colors when passing through a prismatic medium. The ensuing order of colors was always: red, orange, yellow, green, blue and violet, as observed in rainbows. One parametrized this phenomenon through n, the index of refraction, which referred to the change in the direction of light propagation with respect to the initial beam. Each color corresponded to one frequency of light waves, thus optical dispersion amounted to the dependence of n on the light frequency ν. The continuity of the analytical function $n(\nu)$ stood for the observed order of colors mentioned above.

In the early 1870s, though, a series of circumstances changed radically the understanding of this phenomenon, both from the experimental and the theoretical perspective. On the one hand, it was found that when light passed through certain substances (actually, liquid dyes), the normal succession of colors appeared reversed. The reversal implied that the function n was discontinuous with respect to ν at some point. More interestingly, it was acknowledged that the discontinuity in the behavior of n took place around those colors of the spectrum whose frequency coincided with the frequency at which the liquid dyes typically absorbed light. That is to say, when interacting with these substances, one color component of the light was absorbed, while the others passed through and were dispersed into a spectrum. Absorption and dispersion of light became two complementary features of the same

Heaviside and, in general, the work of the so-called "Maxwellians" (George Francis FitzGerald, Oliver Lodge, Oliver Heaviside) in the 1870s and 1880s, see (Hunt 1991).

[5] The *e* marks a German plural.

light-matter interaction. But if this was the case, matter should not just modify the properties of ether, but should play an active role in the production and absorption of light waves.[6]

On the other hand, almost simultaneous with these experimental findings, a radically new optical theory was put forward, in which both the action of matter and ether were taken into account.[7] It was assumed that hypothetical microscopic matter particles vibrated around fixed positions under the action of elastic forces. When light interacted with them, the particles were set in *Mitschwingungen* (co-vibrations) with ether waves. Only when the frequency of light coincided with the proper frequency of the matter particles did these absorb the light, by resonance, in analogy with a tuning fork. For the other colors, light was transmitted through the material, but with a certain phase delay, whose empirical counterpart was the change of direction of light propagation, parametrized through the index of refraction n. According to the *Mitschwingungen* model, the phase delay depended on the color of the light. Thus the new optical theory accounted for a dispersion of light over the whole spectrum, interrupted at resonance frequencies, which occured when the microscopic particles of matter were assumed to absorb light. If the experimentally determined points of color reversal coincided with the natural frequencies of the hypothetical particles of matter, the *Mitschwingungen* model would explain perfectly the phenomenon of optical dispersion, as complementary to the absorption of light.

Even after the adoption of the electromagnetic theory of light, the *Mitschwingungen* model was considered the most satisfactory account of this phenomenon, and generally, a paradigm for the way in which matter and light should interact at the microscopic level. Yet, the *Mitschwingungen* represented light as consisting of mechanical perturbations of the luminiferous ether and not of electromagnetic fields. How could one account for optical dispersion and for processes of light-matter interaction in general, on the basis of the electromagnetic theory of light? For several theoretical physicists in the early 1890s, most significantly Hermann von Helmholtz, Hendrik Antoon Lorentz, and also Paul Drude, answering this question meant developing an electromagnetic version of the *Mitschwingungen* model.[8]

Von Helmholtz's, Lorentz's, and Drude's approaches to optical dispersion were rather different. The first two physicists sought a mechanical foundation for Maxwell's electromagnetic theory via general principles of mechanics. Once they had given a mechanical form to the electromagnetic equations, both von Helmholtz and Lorentz, almost simultaneously, but independently, incorporated the positions and velocities of the hypothetically vibrating particles of matter, as if they performed *Mitschwingungen* with light. But to form a complete electromagnetic version of the model, one additional hypothesis had to be added: if matter particles are going to respond to the electromagnetic ether, these particles should

[6] Christian Christiansen was the first to measure the discontinuity of light dispersion through fuchsine (Christiansen 1870). Drawing on Christiansen's experiments, August Kundt systematized the anomalous behavior depending on the kind of substances (dyes), the position of the discontinuity and its relation to other properties of the materials, such as the absorption of light (Kundt 1871a; 1871b; 1871c; 1871d). As a result of these observations the term "anomalous dispersion" was coined.

[7] Wolfgang Sellmeier was the theoretician who took the first steps (Sellmeier 1872a; 1872b; 1872c; 1872d). Other physicists, most significantly, Hermann von Helmholtz (1875), Eduard Ketteler (1874), and Eugen von Lommel (1878) subsequently elaborated on Sellmeier's theory.

[8] About von Helmholtz's and Lorentz's electromagnetic theories of optical dispersion, on the basis of the *Mitschwingungen* model, see (Buchwald 1985, 237–239) and (Darrigol 2000, 321–325). About Drude's developments in this direction there is no comprehensive secondary literature.

be electrically charged. In turn, when both ether and charged particles were in co-vibration, the microscopic motions of matter provoked a periodic change in the electrical polarization of the substance. The total electric polarization amounted then to the sum of the ether and matter contributions, the first still being determined by Maxwell's equations.[9] In this way, von Helmholtz and Lorentz reproduced the formalism of the *Mitschwingungen* model, while giving it an electromagnetic meaning: light waves were electromagnetic waves and matter particles were charged particles (von Helmholtz 1892; 1893; 1897; Lorentz 1892). Furthermore, both von Helmholtz and Lorentz identified the hypothetical charged particles with another kind of charged particles deployed in a very different phenomenological domain: the electrolytic ions. Ions had been hitherto understood as the moving electric charges going from one electrode to the other in electrolysis experiments. They were the only sort of moving charged particles postulated in physics at that time, but they had never been attributed optical properties. Thus, the connection between electrolytic ions and the dispersive charged particles pointed at a possible unification of electrical and optical phenomena through the same hypothetical microscopic agents.[10]

Drude's analysis of optical dispersion in 1893 was different from von Helmholtz's and Lorentz's (Drude 1893). He relinquished abstraction, drawing a novel boundary between electromagnetism and mechanics. The reduction of Maxwell's electromagnetic theory to mechanics went far beyond the pure formal analysis required for the practical physics. What was important for Drude was to find the differential terms that complemented Maxwell's equations to give the same functional expression for $n(\nu)$ given by the *Mitschwingungen* model. Any other physical hypotheses on the constitution of the system were unnecessary for the time being, including the mechanical foundation of ether. Moreover, Drude also referred to the microstructure of matter in very different terms from von Helmholtz and Lorentz, without committing himself to the nature of the charged particles.

In his 1893 paper, Drude conducted a formal analysis, comparing the mathematical expression for $n(\nu)$ derived from the *Mitschwingungen* model with the definition of n according to Maxwell's equations. Regarding this comparison, it was important that, in the case of the simplest phenomena of reflection and refraction, when the change of direction of light did not depend on the frequency, the mathematical equivalence of mechanical and electromagnetic theories of light entailed the equivalence of optical and electromagnetic parameters. As in both cases the differential equations referred solely to the behavior of the ether, either luminiferous or electromagnetic, that is $n^2 = \epsilon_0$, ϵ_0 being the dielectric con-

[9] Lorentz published his electromagnetic theory of optical dispersion as early as 1878, but his results remained unnoticed until the mid-1890s. Buchwald and Olivier Darrigol argue that it was most probably because he wrote in Dutch and, at that time, he did not enjoy international connections. In the case of his 1892 theory, however, the situation did not differ much, due to the use of very complicated mathematical tools such as retarded potentials and the ongoing lack of international connections. On the deep differences between Lorentz's account and the rest of European physicists', see (Buchwald 1985, 198–199) and (Darrigol 2000, 322–330). The situation changed when a systematic account in German appeared in 1895 (Lorentz 1895). Also it must be emphasized that, although Lorentz had made use of a mechanical principle in 1892, he was not dogmatic in this respect and soon abandoned general principles, which, on the other hand, were driving von Helmholtz's approach.

[10] In fact, Lorentz first identified charged particles with electrons in the 1895 German translation of his 1892 paper (Lorentz 1895). As Theodore Arabatzis has remarked, the identification of Faraday's ions with charged particles presupposed the conviction that electricity had an atomistic structure, as was the case for von Helmholtz (Arabatzis 2006, 72–73). In this direction, by 1881 von Helmholtz had already suggested a possible connection between the concept of ions in electrolysis and the notion of a moving singular charge in electromagnetic theory (von Helmholtz 1903). Ions acquired optical properties only later. See also (Darrigol 2000, 272–274) for further details on this connection.

stant of the ether. Now, for optical dispersion and other phenomena involving the action of matter, the equality $n^2 = \epsilon_0$ only held in the limit of very low frequencies, i.e. $n_\infty^2 = \epsilon_0$. Drude aimed at determining the differential terms of the *Mitschwingungen* model responsible for the difference between the dispersion formula $n(\nu)$ and its limit n_∞. This would tell him how to complement mathematically Maxwell's equations to fill the gap between $n_\infty^2 = \epsilon_0$ and $n(\nu)$.

His reasoning developed through several steps. The starting point was the mechanical *Erklärungssystem* corresponding to the model of *Mitschwingungen*.[11] By comparing it with Maxwell's equations, Drude arrived at a very interesting conclusion: the difference between n—derived from the *Mitschwingungen Erklärungssystem*—and its limit at very low frequencies n_∞ corresponded to the ratio r of the sum of the masses of all matter particles to the mass of ether contained in the same volume. Therefrom Drude surmised that $n^2 - n_\infty^2 = n^2 - \epsilon_0$ amounted to the contribution of the hypothetical microparticles that composed matter. The problem was that, within the electromagnetic framework, the masses did not relate to the efficacy of those particles in interacting with the ether. To overcome this difficulty, Drude assigned to each particle an electrical polarization χ_h, so that the total electrical polarization of the system, ether-matter, χ, was a sum of the ether and matter contributions $\chi = \chi_0 + \sum \chi_h$, χ_0 being the polarization of the ether. By assuming that χ_h could vibrate at natural frequencies, analogously to the massive particles in mechanical theories of dispersion, one could emulate the formalism of *Mitschwingungen* for χ and therefore pursue a schema of co-vibrations between χ and the electric field of the ether. Using these equations, one obtained χ as a function of the frequency of light, ν, that was obviously the same as $n(\nu)$. In the last step, Drude put forward that, in the same way as the electrical polarization of ether was characterized by the dielectric constant, ϵ_0, the polarization of each matter particle was characterized by a new dielectric constant, ϵ_h. In these terms, the divergence $n^2 - \epsilon_0$ coincided exactly with $\sum \epsilon_h(\nu)$, which played the same role as the ratio r mentioned above.[12]

Through this ingenious method of combining the *Mitschwingungen* model, electromagnetic variables, and new electromagnetic parameters, Drude reached a twofold goal: first, he restored the equivalence between optical and electromagnetic constants, so that $n(\nu)^2 = \epsilon_0 + \sum \epsilon_h(\nu)$; and second, he outlined a procedure to modify Maxwell's equations in cases where the action of matter had to be taken into account, without making general claims about the unification of electromagnetism and mechanics. More specifically, χ_h followed the equations of motion of matter, to wit the *Mitschwingungen*, and in this way contributed to the behavior of the general system. Maxwell's equations remained formally untouched if one considered ϵ instead of ϵ_0. Effectively, Drude had extended to optical dispersion the possibility of switching from the mechanical to the electromagnetic framework through a simple choice of language. Two other assumptions about matter were brought in with the *Mitschwingungen* model: its microstructure and the independence of each particle in giving rise to macroscopic effects, which hence boiled down to the simple sum of indi-

[11] It is remarkable that Drude took von Helmholtz's set of mechanical equations for dispersion, laid down in 1875, as a basis for comparison (von Helmholtz 1875), but not von Helmholtz's 1893 electromagnetic theory. Limiting himself to a formal comparative analysis, Drude chose von Helmholtz's 1875 mechanical theory, "since it supplied the form of the differential equations in the most precise and *anschaulichsten* [clearest, most intuitive] way" (Drude 1893, 537).

[12] In fact, in his 1893 paper, Drude mentioned an inspiring exchange of letters with Hertz.

vidual actions. No hypothesis about the nature of those particles was offered, as for example their identification with electrolytic ions.

2.2.4 *Physik des Aethers* and Drude's Advocacy of the Electromagnetic Theory of Light

In his first textbook *Physik des Aethers* (1894), Drude decisively sided with the electromagnetic theory of light. The book was the result of his lectures on electromagnetism at the University of Göttingen between 1892 and 1894. In the first part of the volume, Drude analyzed the properties of the electromagnetic field. His goal was "to derive, on the basis of fundamental experiments, the strictly necessary formulas for the mathematical characterization of observable features" (Drude 1894, vi). In this way, Drude passed over completely the ethos of exactitude of his masters. He did not aim at the determination of the *Erklärungssysteme* from hypotheses on the ultimate nature of ether, but from below, namely from electromagnetic phenomena. Further, he explicitly restrained himself from any attempt to base electromagnetism on mechanical principles, a task that, according to him, was "only justified [...] as a necessity of the natural philosophers" (Drude 1894, vi).

In the second part of the book, Drude tackled optical phenomena from the point of view of the electromagnetic theory of light. Drude pushed for the adoption of this theory in view of the unification of optical and electromagnetic phenomena through the common *Erklärungssystem* and the coincident manifestations of optical and electromagnetic phenomena in experiments. In particular, Hertz's discovery that electromagnetic waves propagated at the velocity of light was key to Drude's advocacy of such a unification:

> The fact that the equivalence of the properties (of lumiferous and electromagnetic ether) is not serendipitous, but something deeply entrenched in the nature of the thing, was already an idea that Maxwell articulated in 1865, when one was not so far away from possessing the resources, with which Hertz has proved this analogy in such an evident way. (Drude 1894, 482)

Hence Drude's unification of electromagnetism and optics implied that the optical ether was electromagnetic in nature, which, for him, meant that the mathematical terms in optical equations should be interpreted using the language of electromagnetism. Drude's unification of electromagnetism and optics eventually resulted in a divide between electromagnetism and mechanics: the ether was electromagnetic in essence, while mechanics only served to model the dynamics of material objects. If there was nothing beyond equations and the language to describe them, such as mechanical principles, there was no way to unify the two domains of mechanics and electromagnetism. Drude's move thus went precisely in the opposite direction of Voigt's attempt to preserve the generality in optics through the "incontestable principles of mechanics."

After endorsing the electromagnetic theory of light, Drude turned to optical phenomena that called for the combination of electromagnetic waves with the motion of microparticles of matter: optical dispersion and the natural rotation of light. The second phenomenon consisted in the change in the direction of light polarization when light passed through certain transparent media, e.g. quartz. This change of direction, measured by the angle δ, was dependent on the color of the light, hence it showed features similar to dispersion, expressed through the functional relation $\delta(\nu)$. Following his 1893 reasoning, Drude argued that the action of matter particles modified Maxwell's equations through their contribution to the electromagnetic constants. But contra 1893, Drude now related the microdielectric constants, ϵ_h, to electrical microcurrents, u_h, that crossed each matter particle of kind h. Thus, on the one hand, one had the current density, u_0, of the ether, which followed Maxwell's equation, $u_0 = \frac{\epsilon_0}{4\pi c}\frac{dX}{dt}$, X being the external electric field of light. On the other, one had the microcurrents, u_h, which were themselves subject to the particle motions. In the case of optical dispersion, the motion consisted in harmonic oscillations. Hence, under the influence of an external electric field, X, coming from light, the microcurrents followed the equation:

$$u_h + a_h\frac{du_h}{dt} + b_h\frac{d^2u_h}{dt^2} = \frac{\epsilon_h}{4\pi c}\frac{dX}{dt}, \tag{2.1}$$

a_h and b_h being two coefficients related to frictional and vibrational terms, respectively. In the same fashion, to explain the natural rotation of light, Drude assumed that the microcurrents underwent a spiral motion. This other kind of *Mitbewegung* (co-motion) with the ether was also in good agreement with observable facts.[13]

Drude concluded his book with a programmatic section, in which he called attention to the state of the art concerning magneto-optical phenomena. As in the case of optical dispersion and the natural rotation of light, these phenomena stemmed from a process of light-matter interaction, the difference being that an external magnetic field was applied. The Kerr effect, mentioned earlier, and the magneto-rotation of light polarization (nowadays called the Faraday effect) were the two most common instances of magneto-optical phenomena then known. The latter consisted of the rotation of light polarization when light passed through a transparent material under the influence of a magnetic field, and it had been first produced by Michael Faraday (1846). The Scottish physicist John Kerr observed a similar phenomenon (1877): polarized light reflected by a magnetized material experienced a change in the direction of polarization. In fact, as early as 1892, Drude had already dealt with the Kerr effect in the vein of practical physics; he undertook a comparative analysis of the various *Erklärungssysteme* suggested hitherto for the Kerr effect, relinquishing any physical hypothesis regarding the microscopic mechanism that could give rise to the corresponding mathematical description (Drude 1892b). In 1894, however, Drude regarded such a mathematical approach as insufficient. He lamented that

> so far one has only been able to establish one satisfactory *mathematical Erklärungssystem* for the optical features of magnetically active bodies, without being able to give a physical justification of the *Erklärungssystem*, in such a way that it would be possible beforehand to compute theoretically the magneto-optical features on the basis of other physical properties of bodies. (Drude 1894, 585–586)

As a matter of fact, Drude did not attempt to give a physical explanation of magneto-optical phenomena, at least not for the time being. But his programmatic claim epitomized a shift in his heuristic strategy: from ether properties to the behavior of microscopic matter

[13]It must be emphasized that Drude's phenomenological approach entailed an important conceptual difference from Lorentz's and von Helmholtz's electromagnetic constructions of optical dispersion: while for the latter two the electrical force exerted on each particle comprised both the electric field of ether and the electrical polarization of the particles, Drude identified external force with the electric field of the ether. I used the term *Mitbewegung* for brevity's sake.

particles. Also the phenomena of fluorescence and phosphorescence called for an optical theory based on a physical model of the behavior of molecules in his opinion. In *Lehrbuch der Optik*, Drude elaborated at length on this lack of a physical interpretation of optical phenomena. But much was still to come between 1894 and 1900.

2.3 Leipzig 1894–1900: From *Physik des Aethers* to *Lehrbuch der Optik*

2.3.1 *Physik des Aethers* and Drude's Program in Leipzig

Physik des Aethers was very influential in introducing Maxwell's electromagnetism and electromagnetic optics into German Universities (König 1906), and more specifically, in introducing Drude's own view of the field. In comparison to the most prominent books on electromagnetism at the time, especially Ludwig Boltzmann's *Vorlesungen über Maxwells Theorie der Elektrizität und des Lichtes* (1893) and Henri Poincaré's *Electricité et optique* (1890), *Physik des Aethers* (1894), Drude's book, contained a great deal of innovative material, in particular about optics. It presented the first systematic electromagnetic account of optics. Actually, Boltzmann's book was radically different from Drude's. Boltzmann derived electromagnetic equations by classifying the ether as a complicated mechanical system, and he did not deal with optical phenomena beyond refraction and reflection. In contrast, Poincaré's approach was very similar to Drude's, for Poincaré stuck to Maxwell's equations for representing electromagnetic phenomena, and avoided giving them any mechanical foundation. Drude acknowledged Poincaré's influence in the introduction of *Physik des Aethers*. In particular, he followed Poincaré in certain derivations of electromagnetic equations from observable features (e.g. the induction law). However, Poincaré did not explore the consequences of the electromagnetic theory of light in its interplay with the microstructure of matter. He confined himself to accounting for the reflection and refraction of light. In this regard, Drude went further than his French colleague.

Physik des Aethers was also crucial for Drude in promoting his career. In August of the year it was published he was offered an extraordinary professorship for theoretical physics in Leipzig, and it was precisely the appearance of *Physik des Aethers* that tipped the balance in his favor (Jungnickel and McCormmach 1986, 166). Also in 1894, Drude married Emilie Regelsberger, the daughter of a Göttingen jurist. Thus, shortly after the wedding, the couple moved to Leipzig to start a new life, as well as a new chapter in Drude's career.

Drude's inaugural speech, held at the University of Leipzig on 5 December 1894, was a clear, programmatic presentation of the approach to optics he had been forging and wanted to pursue in the ensuing years (Drude 1895). Drude's decision to reduce the physical system to physical language describing the mathematical equations, what he called practical physics in 1892, was reinvigorated and reiterated:

> You see, the goals of the current research on theoretical physics are not so extensive as the goals of the old natural philosophers. Today one does not ask about the so-called true essence of things, which goes beyond what is perceptible, not about the ultimate cause of phenomena. The goal of a theory is simply the description of the phenomenal world.[14] (Drude 1895, 4)

[14]"Phenomenal world" is the usual translation of the German philosophical term *Erscheinungswelt*. Hereafter, the German word will be used.

Drude sided explicitly with Ernst Mach, whom he found an "excellent advocate" of the cause. These words acquired special significance in connection with Drude's description of the ether. According to Drude, "the word 'Ether' does not involve a new hypothesis, but it is only the epitome of space free of matter, which possesses certain physical properties" (Drude 1895, 9), thus it was only the embodiment of Maxwell's equations.

The subsequent shift from ether to matter as the new heuristic around which to develop optical theories was also clearly reflected in Drude's Leipzig speech. On the basis of the phenomenon of dispersion, Drude argued that "the electromagnetic theory of light calls for the concept of molecules in order to describe the optical properties of *matter* in the easiest way. I emphasize: of matter, not ether" (Drude 1895, 12). Here, the molecular structure of matter was by no means to be regarded as an ad hoc hypothesis, but was unequivocally associated with observable phenomena. This hypothesis also led to a re-categorization of optical properties: these were properties of matter, not ether, whose traits were already well-fixed by Maxwell's equations. In light of this, research efforts in optics should focus on the elucidation of the properties of molecules that lead to the proper description of phenomena. Physics of matter and physics of the ether should be combined, without being reducible, one to each other.

No sooner said than done, Drude devoted his research in Leipzig to examining in more detail issues relating to the program he advanced in his inaugural talk, while he persisted in his missionary task to spread his own view of electromagnetism in Germany. His work made significant progress through a 1899 paper in which he produced a consistent and encompassing account of optical dispersion and magneto-optical phenomena (Drude 1899), deriving the *Erklärungssysteme* from physical hypotheses. Two important events led Drude to work in this direction. On one side were novel developments in magneto-optics that changed the field in the period between 1897 and 1899. On the other side was Drude's plan to write a textbook on optics, *Lehrbuch der Optik*, during the elaboration of which he was forced to deal with a broader spectrum of insights into the field. Drude had become one of the most prominent figures in the fields of optics and electromagnetism by that time.

2.3.2 Magneto-optics and the Ion Hypothesis

A new magneto-optical phenomenon had been characterized experimentally, which led to a general revamping of the search for optical theories in magneto-optics. In 1897 Pieter Zeeman, Lorentz's student in Leiden, had detected that the spectral D line at which sodium vapor emitted light split into different components when a magnetic field was applied (Zeeman 1897a; 1897b). Zeeman used the theoretical insights of his master to analyze how the split of spectral lines could be related to the microstructure of sodium atoms. Fundamental in Lorentz's account was the hypothesis that ions emitted light at the frequencies of their mechanical motions, in the same fashion as in the *Mitschwingungen* model. The only difference was that, in the case of dispersion, light was absorbed at specific frequencies, while in the case of spectral lines, it was emitted at specific frequencies. Further, Lorentz put forward that, upon the application of a magnetic field, the initial motion of the ions was decomposed into three components, leading to different frequencies: one linearly polarized oscillation and two circularly polarized in opposite directions. Exploiting this physical model, Zeeman could use the experimental data to calculate the charge-to-mass ratio, e/m, of the ions, which had previously only been assumed. It was indeed the first calculation of ionic properties outside the field of electrolysis.[15]

After 1897, Woldemar Voigt also became very active in discussions of magneto-optics. He had recently "converted" to the electromagnetic theory of light, and his contributions to magneto-optics were well appreciated by Drude, in both the 1899 paper and the 1900 *Lehrbuch der Optik*. Actually, Lorentz, Voigt, and Drude attended the 70th Meeting of German Natural Scientists and Physicians in Düsseldorf, held in 1898, and they could probably have exchanged opinions on the new, fashionable topic of magneto-optics. In 1899 Voigt also laid down a comprehensive theory of magneto-optical phenomena, taking "the system of equations that Drude had developed on the basis of Hertz's work as his starting point" (Voigt 1899, 345). In particular, he built upon Drude's 1893 paper on dispersion and his solution of modifying the electromagnetic constants in order to integrate the actions of matter into optics. Voigt also adopted Drude's language of *Erklärungssystem*. Light equations were complemented by different vectors, P_h, accounting for the action of matter. Nevertheless, Voigt did not interpret the vectors P_h physically, sticking for the time being, to the mathematical form of *Erklärungssysteme*. Drude acknowledged Voigt's *Erklärungssysteme* for the Zeeman and Faraday effects in his own 1899, overarching account of magneto-optical phenomena. But in this paper he went a step further than Voigt: Drude related each magneto-optical *Erklärungssystem* to one hypothetical kind of motion of the microscopic particles in their interaction with the ether, which could explain its specific set of differential equations. The incorporation of a new element, the ion, into Drude's picture of the microstructure of matter was instrumental in achieving the new account. Drude replaced his earlier language of microcurrents with the new language of ions, arguing that "the easiest foundation of the theory of dispersion is embodied in v. Helmholtz's assumption of moving and electrically charged ions" (Drude 1899, 107). Both Drude's maneuver and his theoretical justification deserve close scrutiny. Did Drude's change of mind mean that now he agreed with von Helmholtz's 1892–1893 strategy to reduce electromagnetism to mechanical principles when figuring out an electromagnetic theory of optical dispersion?

Drude borrowed von Helmholtz's ion hypothesis but not von Helmholtz's overall approach. Drude found inspiration for his approach in the work of Richard August Reiff, who, in 1896, had published a book titled *Theorie molekular-elektrischer Vorgänge* (Reiff 1896). The standpoint Reiff developed there was similar to Drude's. For Reiff had

> [a]bove all avoided those developments that only had mathematical interest. I have not used the principle of least action for the derivations of equations, but I have derived the motion equations of ions from molecular considerations, in order to attain the easiest interpretation of the equations. For this purpose I use the language of atomism. (Reiff 1896, vii)

Reiff's procedure consisted in stating first of all the equation of motion of ions of charge density e and mass density m under the influence of the electric field of light X. For example, in the case of dispersion, the ions being presumably subject also to elastic forces, the electrical moment ξ of the ions could be expressed in the following way:

[15] For more information about the interplay between theory and experiment in Zeeman's route to his experiments and analyses, see (Arabatzis 1992; Hentschel 1996; Kox 1997).

$$\frac{1}{4\pi\theta}\xi + r\frac{d\xi}{dt} + \frac{m}{4\pi e^2}\frac{d^2\xi}{dt^2} = X, \tag{2.2}$$

r being the coefficient of the frictional force, and θ the coefficient for the elastic force. By supposing that both X and ξ were wave functions having the frequency of light ν, the latter with a phase delay characterized through the index of refraction n, one easily obtained an expression for n depending on ν identical to the one derived from the *Mitschwingungen* model. Thus the motion of ions in Reiff's picture worked very similarly to the motion of microcurrents in Drude's picture. At any rate, the ether persisted in being an embodiment of Maxwell equations, thus it was only the motion of microparticles of matter which modified optical constants. In this sense, Drude's adoption of the ion essentially implied a replacement of the microcurrents, u_h, in his earlier accounts by ions, and therefore the identification of the motion of the charge with the motion of the mass of the ions.

Apart from their vibrations, which explained optical dispersion, in 1899 Drude assumed that ions underwent two other, different kinds of motion: spiral motions and also translations perpendicular to the magnetic field. Drude related the first kind of behavior to the Kerr effect. Then Drude argued that the *Erklärungsysteme* for the Zeeman and Faraday effects could be obtained by assuming ions performed the second kind of motion. Altogether, with the 1899 paper, Drude laid down the first comprehensive account of magneto-optical phenomena on the basis of a systematic combination of electromagnetic equations and the ion hypothesis. *Lehrbuch der Optik* consisted of a full-fledged expansion of this "ion turn" into book form, from optical dispersion to all varieties of magneto-optical phenomena known at that time, together with fluorescence and phosphorescence. We can say that with *Lehrbuch der Optik* Drude fulfilled the programmatic aims presented in the last chapter of *Physik des Aethers*.

Although his appropriation of the ion hypothesis supplemented nicely Drude's approach and fulfilled his requirements for a physical interpretation of optics, it must be emphasized that the particular adoption of the ion cannot be justified fully from the point of view of practical physics. The molecular structure of matter and the possibility of particles generating time-varying electrical polarizations was enough for that purpose. Polarizations and frequencies were the measurable quantities in optical phenomena. Hence, assigning an active role in optics to ions, which were characterized by a mass, m, and an electrical charge, e, determined through electrolytic experiments, independent of optical phenomena, was a kind of "external hypothesis" to the dynamics of practical physics. While the ether was reduced to the physical language necessary for Maxwell's equations, matter was not reduced to the minimal physical language necessary to give a meaning to the differential terms accounting for the motion of particles. In a way, Drude overstepped the bounds for theoretical physics he himself had established in 1895. Assuming the existence of ions meant asking oneself about the essence of the microparticles of matter, beyond the description of the *Erscheinungswelt*. What could have happened for Drude to make such a decision?

I think that *Lehrbuch der Optik* could have played a catalyzing role. As Drude wrote in the preface, the endeavor to write a book motivated him to sharpen his own view of the field. Indeed he accepted the offer to write a book from the publishing house because, among other reasons, "I wished for myself the development of new ideas through the deepening of my own view of the field, which is compelling in writing a book" (Drude 1900a, iii). Between 1894 and 1900 Drude worked on various topics related to optics, but it was only at the end, in 1899, that he saw in the adoption of the ion hypothesis the possibility of an overarching account of optics. He never mentioned von Helmholtz's ion hypothesis or Reiff's work until 1899, although they were already published in 1892/1893 and 1896, respectively. At that moment, Drude was concentrating on his experimental and theoretical work on electric waves and the nature of actions at a distance. He also published on magneto-optical phenomena, but only from the point of view of the *Erklärungssystem*.[16] So it is plausible to think that Drude's prompt advocacy of the ion hypothesis originated in the reexamination and comparison of different sources necessary for the purpose of writing the textbook, which allowed him to see the ion hypothesis as a means to articulate optical knowledge in a unified way, without giving up his own approach to optics.[17]

Since the advantage of dealing with ions instead of microcurrents was not the possibility of explaining the optical properties of matter, I think that the adoption of the ion offered Drude another kind of advantage: a new heuristics. In fact, the incorporation of the ion into Drude's picture not only had consequences on an ontological level (e.g. the ions as the specific particles that were optically active), but also epistemologically. For the ion hypothesis allowed Drude to explore new ways to gain knowledge in optics, apart from seeking the right mathematical description of phenomena and the unification of ether theories from below (e.g. adoption of the electromagnetic language). As we will see in the following section on *Lehrbuch der Optik*, the ion hypothesis allowed Drude to network different domains of knowledge (optics and beyond) through the specific properties of charge e and mass m of these particles, which should manifest in different phenomena.

2.4 The *Lehrbuch der Optik*

2.4.1 *Lehrbuch der Optik* as a Novel Program in Optics

The year 1900 began very productively for Drude. In January he became the new editor of the most prestigious German scientific journal, *Annalen der Physik*, and he published his second textbook, *Lehrbuch der Optik*, which was soon translated into English and became

[16]More specifically, between 1894 and 1899, Drude's research had been especially fruitful in three directions: electric waves, magneto-optical phenomena and actions at a distance. Concerning the first topic, Drude found experimentally that electric waves, whose frequency lay outside the optical range of the spectrum, also displayed dispersion, and indeed anomalous dispersion through certain liquids. Furthermore, in line with his 1895 claims at his inaugural speech in Leipzig, he immediately related the dispersion behavior to the chemical constitution of the liquids examined (Drude 1896; 1897a; 1898). In 1897 he addressed the Kerr effect again from a theoretical point of view, but without daring to give it a physical interpretation in terms of the properties of matter, as he had himself suggested at the end of the *Physik des Aethers* (Drude 1897c). The paper was the continuation of an old controversy he had been engaged in since the early 1890s with D. Goldhammer, concerning the best *Erklärungssystem* for the Kerr effect and which kind of optical constants best described it. For more details about the controversy, see (Buchwald 1985, 215–232). Eventually, Drude also explored the theoretical possibility of reducing actions at a distance such as gravitation, to local actions, mediated by the electromagnetic ether (Drude 1897b).

[17]It is very remarkable that Drude did not mention Lorentz's account of the Zeeman effect either in 1899 or in 1900; although, he referred to Zeeman's papers and met Lorentz in 1898 at the conference in Düsseldorf. At that time, Lorentz had already published his theoretical account (Lorentz 1897; 1898). In the 1899 paper, Drude indeed resorted to Voigt's 1899 *Erklärungssystem* for the Zeeman effect, but Voigt did not allude to Lorentz either. Darrigol (2000, 331–332) is of the opinion that Drude, and also Voigt, became advocates of the microscopic view of optics abruptly, possibly as a consequence of Lorentz's presence at that meeting. But the absence of Lorentz in Drude's 1899 paper and 1900 book, in the context of magneto-optics, hints at another explanation. Possibly Lorentz had an influence on them, but he was not the only reason for Drude's adoption of the ion. Drude developed, as early as 1893, a microscopic outlook to explain optics, in his contribution to dispersion.

the reference book for optics in many European universities. Drude was asked by the publishing house S. Hirzel to write the textbook "because a modern book covering the whole field was lacking" (Drude 1900a, iii). Since he was considered one of the greatest specialists on optical physics at the time, Hirzel "could really not have contacted any one better than Drude for this request" (König 1906). Woldemar Voigt, Walter König, and Max Planck highlighted precisely this point in their recollections of Drude after his tragic death in 1906 (Voigt 1906; König 1906; Planck 1906).

In fact, *Lehrbuch der Optik* became the first textbook on optics to make use of only the electromagnetic theory of light and to combine it systematically with a microphysical view of matter (in this case the ion hypothesis). Moreover, Drude did not have much concurrence in Germany. At that time, German students of theoretical optics would resort to compendiums on the subject, written by prominent professors at universities. Famous examples were penned by Ketteler (1885), Voigt (1896), and von Helmholtz (1897).

Ketteler's book responded to very different questions than Drude's. Ketteler had been very active, in the 1870s and 1880s, in the further elaboration of optical theories based on the groundbreaking hypothesis that the ether and matter particles were in *Mitschwingungen*. But he obviously did not discuss how to deal with the electromagnetic theory of light in the context of mechanical theories of ether, and he subsequently left magneto-optics out of his analysis altogether. These topics flourished first in the early 1890s, when Ketteler died.

By contrast, Voigt was well aware of the difficulties that had arisen in optics due to the spread of the electromagnetic theory of light in Continental Europe by the time he wrote his *Kompendium*. But he decided to approach optics in the second volume of his *Kompendium der theoretischen Physik* from a mathematical viewpoint, without committing himself to any hypothesis on the nature of the ether, thus leaving the final decision about the nature of the ether to the readers. He just stuck to the mathematical formulation of phenomena, which in turn meant that he took mechanical principles as undisputed bases. Only for optical dispersion did Voigt introduce additional mathematical terms, P_h, into the differential equations for the ether. Magneto-optical phenomena were simply omitted in his account of optics. Ironically, Voigt was the first one to go beyond his own work. From the late 1890s, he advocated the electromagnetic theory of light and devoted himself precisely to the only domain of optics that was absent from his *Kompendium*, namely, magneto-optics, as mentioned earlier. In that field, Voigt soon became an authority.

Von Helmholtz's posthumous collection of lectures thus would have been the most immediate competitor to Drude's *Lehrbuch der Optik* in Germany, for von Helmholtz clearly embraced the electromagnetic theory of light, and he was a pioneer in introducing the concept of ions into optics. However, the hot topic at the end of the 1890s, namely magneto-optics, was also missing in von Helmholtz's book.

Outside Germany, Drude's book was compared with the works of the British physicists Thomas Preston, *The Theory of Light* (1890), and Alfred B. Basset, *A Treatise of Physical Optics* (1892). The style of the English books was rather different from that of the German ones, specifically in their way of approaching and organizing theoretical optics. As Preston expressed well in his book, his goal was to "furnish the student with an accurate and connected account of the most important researches from the earliest times up to the most recent date […] without entering into complicated mathematical theories" (Preston 1890). That was exactly the opposite of what Drude was trying to do in his *Lehrbuch der Optik*. Drude, as well as the above mentioned German authors, normally tried to convey a unified program that could explain all optical problems in a consistent way. The guiding principle of each textbook was expressed in its introduction and was always closely related to the strategy each author chose to describe and manipulate mathematically both optical phenomena and hypothetical physical mechanisms. There was thus not much space for historical accounts in this kind of textbook. Alternatively, both Preston's and Basset's books provided the reader with very precise accounts of optical experiments, the types of instruments used to perform them, and various techniques to manipulate data, without hesitating to stick to diverse European sources and compare different historical accounts from Newton onwards. The reader was conducted smoothly through a manifold of approaches, each one teaching something about the characteristics of optical phenomena and the way in which they were disclosed at specific historical moments, almost chronologically, up to the Faraday and Kerr effects (the Zeeman effect was published later than their books). The electromagnetic theory of light only appeared as the most recent development in optics, in connection with Hertz's experiments, although the authors, especially Preston, had been closely related to the main developers of the electromagnetic theory in Great Britain. In particular, Preston was a junior colleague of George FitzGerald in Dublin. Nevertheless, the point was not to advocate either theory of light, but to give the reader an overview of the state of affairs. In fact, at the moment the books were published, the electromagnetic theory of light did not suffice to account for phenomena stemming from the interaction of light with matter, like optical dispersion or the Kerr effect.

Thus Drude was offered, by the publishing house S. Hirzel, the great opportunity to pioneer the exploration of essentially virgin territory, in a moment when the questions to ask in optics were changing rapidly. In the 1870s and 1880s, optical theoreticians engaged in a honing of the *Mitschwingungen* model. In the early 1890s, the main issue in Germany became how to choose between electromagnetic and mechanical theories of light, and the implications of this choice in relation to the ultimate nature of the ether, alongside the translation of the *Mitschwingungen* model into electromagnetic language. Eventually, in the late 1890s, the field of magneto-optics offered itself as the new boiling pot. Thus Drude arrived at the right moment to take up the most fashionable questions and lay down his own answer.

2.4.2 *Lehrbuch der Optik* Piece by Piece

The textbook was divided into two parts: geometrical optics and physical optics. Geometrical optics dealt with the optical features of light propagation and came down to four essential laws: linear propagation of light (light rays), the geometrical laws of refraction and reflection, and the possibility that a light ray splits at any point of its trajectory. These laws could be expressed by simple geometrical relations and they sufficed to describe the functioning of most optical instruments: lenses, microscopes, telescopes, and prisms. The second part of the textbook was devoted to physical optics, where geometrical laws did not suffice. One had to introduce hypotheses on the nature of light and the physical mechanism of light-matter interaction in order to account for optical phenomena more complex than reflection and refraction. This part was divided into three big sections: the first, about the general properties of light, in which the constitution of matter did not play a role (such as the interference and diffraction of light); the second about the optical properties of bodies measured when light was reflected by or passed through transparent media (principally, optical dispersion and magneto-optical phenomena); and the third about the emission of radiation from matter, which boiled down to the thermodynamics of radiation.

Drude's programmatic claims are to be found in the second part of the book. The first part helpfully provided the reader with the necessary knowledge about how to produce and measure optical phenomena with optical instruments, but it did not contain much new knowledge. In fact, Drude constantly referred the reader to the existing literature on geometrical optics to which he himself had resorted.[18] Drude devoted the longest and most involved section of the introduction to justifying his approach to physical optics in the second part, namely, the adoption of the electromagnetic theory of light and the ion hypothesis.

The first notion from physical optics that Drude introduced in the main text was that of the constant velocity of light in vacuo, c. To support it, Drude put forward a detailed overview of the interferometric experiments to measure c. Thereafter, the phenomenon of interference led Drude to introduce the wave nature of light. The mathematical translation of this physical concept was the wave function, with the velocity of light being one of its parameters. Hence, the second step subsumed the first one. In what followed, Drude explored the different ways in which wave functions could be superposed, thus giving rise to different patterns of interference (such as Newton's rings) and to the diffraction of light (depending on the experimental set up).

Next, to describe the features of the double refraction of light, Drude introduced the concepts of light polarization and transverse waves. With these notions clearly formulated, an indeterminacy in the mathematical formulation appeared. Different conventions concerning the relative orientation of the wave amplitude, its polarization, and the direction of propagation could be adopted. For example, according to Neumann's mechanical theory of light, the wave amplitude coincided with the direction of light polarization, while in Fresnel's mechanical theory the amplitude was perpendicular to the polarization. These differences notwithstanding, in both cases amplitude and polarization were considered to be perpendicular to the direction of wave propagation. From this point of view, the two theories of light were mathematically equivalent.

The electromagnetic theory of light concealed these indeterminacies, for the perpendicularity of the two light vectors (electric and magnetic field) to the direction of propagation was inherent to the formulation of the theory. The adoption of the electromagnetic theory of light led one, therefore, to overcome the conflict, but not to resolve it. But beyond this isolated advantage, Drude's justification of his preference for the electromagnetic theory of light lay in the possibility of unifying optics and electromagnetism through the identification of the velocity of light with the velocity of electromagnetic waves. Because of this, Drude deemed the adoption of the electromagnetic theory of light "one step further in knowledge about nature, since this way two initially parallel domains of knowledge, like optics and electricity, are treated together in a close and measurable relation" (Drude 1900a, 248). With no further explanation, Drude identified the electromagnetic theory of light with Maxwell equations and boundary conditions.

Whereas Maxwell equations accounted well for all the phenomena Drude had described so far, the behavior of light in absorbent media called for additional terms related to the microphysical action of matter. Drude justified this very clearly in his introduction:

---

[18]More specifically, to Winkelmann's *Handbuch der Physik* (1894) and Müller and Pouillets's *Lehrbuch der Physik und Meteorologie* (1897).

> First of all, in this way one succeeds in providing an explanation of the phenomenon of dispersion, since the purely electromagnetic experiments only suggest, I would say, macroscopic properties of bodies. To explain optical dispersion it is necessary to make hypotheses on the microphysical properties of bodies. In this sense I have used the ion hypothesis introduced by v. Helmholtz, because it seems to me the easiest, clearest [*anschaulichste*] and most consequent one to characterize, apart from dispersion, also the absorption and natural rotation of light polarization, and also the magneto-optical properties and properties of bodies in motion. (Drude 1900a, v)

In the following four chapters, Drude developed in detail his programmatic approach to the optical properties of matter. First of all, he tackled the phenomena of optical dispersion and of the rotation of light polarization, both of which he had already dealt with in *Physik des Aethers*, but now, he used the novel perspective of the ion hypothesis. Then he explained the singular features of the reflection of light in metals, and expanded upon the magneto-optical phenomena. Let us look again at the example of dispersion to get a better glimpse of Drude's developments in this direction.

The physical system responsible for the optical properties of matter consisted of the electromagnetic ether, N_1 positive ions, and N_2 negative ions, both kinds being characterized by natural frequencies of vibration. For the case of optical dispersion, when the electromagnetic waves impinged upon matter, these ions were supposed to be set in vibration at the same frequencies as the impinging light. The displacements of the ions from their equilibrium positions, ξ_1 and ξ_2, caused the polarization of the material medium, whereas the ether polarization corresponded to the electric field, X, of the light. Drude's idea for incorporating the polarization of the medium into Maxwell equations was the following: the displacement of the ions gave rise to two electric currents, j_1 and j_2, of opposite sign, which could be expressed as the product of the respective number of ions, N_1 and N_2, their charges, e_1 and e_2, and their velocities, $\frac{d\xi_1}{dt}$ and $\frac{d\xi_2}{dt}$. The total electrical current amounted to the sum of the ion currents and the ether current, $\frac{1}{4\pi}\frac{\partial X}{\partial t}$. By assuming that ions were subject to elastic forces $-\frac{4\pi {e_1}^2}{\theta_1}\xi_1$ and $-\frac{4\pi {e_2}^2}{\theta_2}\xi_2$, while acted upon by electromagnetic light, Drude eventually obtained this expression for the total electric current:[19]

$$j = \frac{1}{4\pi}\frac{\partial X}{\partial t}\left(1 + \frac{\theta_1 N_1}{1-\frac{b_1}{\nu^2}} + \frac{\theta_2 N_2}{1-\frac{b_2}{\nu^2}}\right). \tag{2.3}$$

The factor 1 within the brackets corresponded to the action of the ether, whereas the other two terms referred to the influence of the positive and negative ions. The expression depended on the electric field of light, X, its frequency, ν, two parameters, b_1 and b_2, and the elastic constants of the two types of ions, θ_1 and θ_2. Drude had thus managed a combination, in one mathematical expression, of both electromagnetic parameters (electric fields and frequencies) and mechanical parameters (elastic constants). Subsequently, if one defined the dielectric constant, ϵ', of the joint system of matter and ether as the sum of the dielectric constant of the ether, $\epsilon = 1$, and the two ionic terms written above, one could eventually return to Maxwell's expression for the electric current $j = \frac{\epsilon'}{4\pi}\frac{\partial X}{\partial t}$, with a modi-

[19]For simplicity's sake, I have approximated Drude's formula to the case of no frictional force.

fied dielectric constant. This is the way in which the equations of motion of the ions entered Maxwell equations, and the relation $\epsilon' = n^2$, between optical and electromagnetic constants, remained untouched.

Following the same strategy of modifying constants, Drude continued to account for the other optical properties of matter and magneto-optical phenomena, providing a full derivation of the differential equations for magneto-optics by attributing to each phenomenon a specific kind of ionic motion. While for optical dispersion ions vibrated, for metallic reflection ions were supposed to translate across the metal, and in the phenomenon of rotation of light polarization (through quartz) ions were assumed to combine vibrations and rotations, tracing out helicoidal trajectories. Under the influence of a constant magnetic field, Drude suggested two possible motions for ions, which could modify both the dielectric and the magnetic constants: first, ions could go through proper vibrations and small rotations, resulting in helicoidal motions. Drude argued that the correspondingly modified Maxwell equations led to satisfactory agreement with the features of the "inverse" Zeeman effect through sodium gas.[20] The second possible motion of ions was a superposition of their natural vibrations parallel to the electric field of light and a translation perpendicular both to the electric and magnetic fields (as in the Hall effect). With the differential equations corresponding to these motions, Drude could explain the Zeeman effect in sodium vapor, and the Kerr and Faraday effects, as manifested through the use of nickel, cobalt, and iron vapors.

In the last chapter devoted to the subject of physical optics, Drude eventually referred to the optical properties of bodies in motion. In this case he referred substantially only to Lorentz's *Versuch* (1895). Drude reconsidered his formulation of certain optical phenomena, such as optical dispersion, in light of the possibility that not only could ions move from their equilibrium positions inside molecules, but also the molecules themselves could move. In this case, the frequency of light $\nu = \frac{1}{\tau}$ appearing in the dispersion formula should be replaced, using instead:

$$\frac{1}{\tau'} = \frac{1}{\tau}\left(1 - \frac{p_1 v_x + p_2 v_y + p_3 v_z}{c}\right), \tag{2.4}$$

p_1, p_2, p_3 being the normal components of the direction of wave propagation; v_x, v_y, v_z the components of the molecule's velocity throughout the space and c the light velocity in vacuo. In fact, this was the only occasion in which Drude resorted to Lorentz's theoretical works. It is very remarkable that Drude reduced Lorentz's contribution to the special case in which equilibrium positions of ions inside molecules were not fixed, since Lorentz had founded his whole approach to optical phenomena on the relative motions of ions, making them responsible for the local changes in the properties of the static electromagnetic ether. As Buchwald and Darrigol claimed in their respective books, Lorentz's approach, calling for such complicated mathematical tools as retarded potentials, was almost certainly not well

[20]This effect was observed one year after the Zeeman effect (Macaluso and Corbino 1898), for which Voigt had already provided an *Erklärungsystem* in 1898 and 1899 (Voigt 1899). Damiano Macaluso and his assistant Orso Mario Corbino took a sample of sodium gas and applied a constant magnetic field, as in arrangements that exhibited the Zeeman effect. But in constrast to Zeeman's experiment, Macaluso and Corbino did not examine the light emitted by the gas. They made white light pass through the gas and measured the change of polarization of the transmitted light. They observed a continuos change of the angle of polarization for the whole spectrum, interrupted only by sharp discontinuities at the D spectral lines of sodium.

understood in Germany, in its broader scope (Buchwald 1985; Darrigol 2000). Drude was, in this regard, no exception.

Eventually, in the third part of the book, Drude wrote about the thermodynamics of radiation. It is very significant that Drude devoted more than fifty pages to this matter, for this was the first time that the topic was so thoroughly treated in a textbook on optics. To date, it had been scarcely considered part of optics, since it seemed to have nothing to do with the explanation of light propagation, either through the ether or matter. Thermodynamics of radiation dealt instead with the generation of radiation by matter and the distribution of its energy over different spectral frequencies at different temperatures. The thermodynamics of radiation required, therefore, a very different set of concepts (i.e. distribution of energy, black-body radiation), physical laws (i.e. the second principle of thermodynamics), mathematical procedures (i.e. those of the kinetic theory of gases), and experimental sources (i.e. spectroscopy) than the ones discussed hitherto. In fact, Drude relied on accounts from others, basically Gustav Kirchhoff, Boltzmann, and Wilhelm Wien, to expand upon this issue. However, in the very last section of this part, which was about the emission of radiation by gases and vapors, Drude presented his own insights in a very revealing way. The ions were presumed to be the cause of the light emission in luminescence, fluorescence, and spectral lines, in a way analogous to the way covibrations of ions with light caused optical dispersion and magneto-optical phenomena. The key difference was that now the ions were not supposed to modify the behavior of the already propagating light. They were instead supposed to vibrate and to emit radiation at the same frequency as their vibration. Actually, Drude did not expand at length upon the various manifestations of these phenomena, for which there was, so far, no systematic theory. His description of the phenomenon was rather superficial in comparison with the section on physical optics. Rather, the important point is Drude's extension of the fruitfulness of the ion hypothesis to optical phenomena beyond dispersion and magneto-optics.

All in all, the ion hypothesis seemed to provide Drude with an insightful way to describe mathematically all kinds of optical phenomena involving light-matter interactions from the standpoint of the motion of particles at the microscopic level. Yet, the ion hypothesis also allowed Drude another route to new knowledge.

2.4.3 The Ion Hypothesis as a New Heuristics

Drude justified the adoption of the ion hypothesis in *Lehrbuch der Optik* on the grounds that it provided the most *anschaulich* description of the optical properties of matter, namely, the clearest, most demonstrative, most graphic description. It is true that a moving ion is much easier to picture than a moving particle being crossed simultaneously by an electric microcurrent, which Drude suggested in 1894. But the ion offered much more than a more concrete physical picture; other advantages contributed to its being *anschaulich*.

On the one hand, the ion hypothesis worked well as a heuristic tool, to model the interaction between light and matter in a more approachable way. On the other hand, the ion had an ontological status as the physical agent of optical phenomena, whose specific properties could be measured through different sorts of experiments. Nevertheless, ions could not be directly viewed or detected. Their properties (such as mass and charge) could be calculated from experimental data only by analyzing these data under the previous assumption that measurable phenomena were caused by the hypothetical ions. Through the sharing of ions as both heuristic tools and physical agents in this way, Drude figured out a new way to network different domains of knowledge.

In the second part of his book, Drude placed special emphasis on Zeeman's experiments and the first measurement of the charge-to-mass ratio of ions obtained from them. Drude relied on Zeeman's interpretation of the experimental split of the D lines of sodium under the influence of a magnetic field, as a direct consequence of the decomposition of the natural motion of ions, and he reproduced Zeeman's relation between the frequency difference, $g = D_1 - D_2$, and the mass, m, and charge, e, of the hypothetical ions:

$$g = \nu_1 H \frac{e}{m}, \tag{2.5}$$

H being the magnetic field applied and ν_1 the frequency of the original D line. This approach offered an exceptional opportunity to obtain a numerical value of e/m for the hypothetical ion from optical experiments. But Drude did not stop there. Drude compared Zeeman's value with other values for e/m obtained in other experimental contexts outside of optics, like the production of cathode rays and the process of electrolysis. If it were not for their shared reliance on the ion as a hypothetical cause, it is rather unlikely that such diverse phenomena would have been put together in a textbook on optics:

> It is remarkable that, from the deviation of cathode rays, Kaufmann has derived [...] almost the same value [...] for the relation of the charge to the mass of the accelerated cathode particles. For the ions appearing in electrolysis this relation is much smaller. [...] One can think that either the electrolytic ion contains more positively and negatively charged components that hold together for electrolysis but move freely with light waves and in vacuum, or the electrolytic ion is composed by the bonding of a charge e of mass m (electron) with a bigger neutral mass M. (Drude 1900a, 410–411)

This text appears in a footnote, but this does not mean that it was considered ancillary by Drude. Actually, in the ensuing years, his research focused on the extension of the ion hypothesis to the most varied of fields. Most probably, Drude incorporated these insights into his text in the last moments before delivering it to the publishing house. Maybe he learned about Kaufmann's experiments only in 1899, as Walter Kaufmann gave a talk on the topic in the same session of the 1899 meeting of the German Physical Society as Drude spoke. This would explain why Drude only mentioned Kaufmann in relation to cathode rays. For, in fact, almost simultaneously, Joseph J. Thomson in Cambridge (1897a; 1897b), Kaufmann in Berlin (1898), and Emil Wiechert in Königsberg (1897a; 1897b) performed experiments with cathode rays and measured similar values for e/m. Cathode rays were emitted by the cathode of a vacuum tube during an electrical discharge. If the rays were interpreted as streams of electrically charged particles, then by measuring their deviation in a magnetic field one could obtain a numerical value of their hypothetical e/m.[21]

Some further aspects of this triangular comparison of e/m values should be highlighted. First, only after the same hypothesis of moving charged particles was applied to several different phenomena were ions differentiated into different sorts. Then only the particles

[21] The secondary literature on cathode ray experiments in the late 1890s is huge, especially on Thomson. Representative examples include (Falconer 1987; Robotti 1995), the first four papers of (Buchwald and Warwick 2001), and (Navarro 2012). For the taxonomy of the electron, see (Arabatzis 2006).

responsible for cathode rays seemed to coincide with the ones measured by Zeeman. Remarkably, Drude referred to them as "electrons." This was the first time that Drude made the distinction between electrons and electrolytic ions, a dichotomy that became very fruitful in his research in the ensuing years. Thus the value of e/m obtained in experiments signified that electrolytic ions were much more massive particles than electrons. Second, only after this dichotomy arose could one begin to wonder about how electrons could be combined with electrolytic ions in the constitution of matter, as Drude did. He envisioned two possibilities: either ions were composed of a manifold of positive and negative elementary charges, or a charge e was attached to a bigger mass M. Now, rather than Drude's answer, what is important here is the fact that Drude asked *this* question. The ion turned out to be not only productive in providing him with a clearer physical picture, ions also offered Drude the ability to interrelate different phenomena through the different ways in which they manifested the properties acknowledged as dependent upon ions, and then to speculate on a more general picture of the constitution of matter. That is to say, pieces of information obtained from different experimental contexts, once interpreted as a consequence of the same microscopic agent, could contribute to the disclosure of a new and exciting puzzle: the microstructure of matter.

Drude's appropriation of the ion also impacted the third part of the book, specifically when he dealt with the phenomenon of luminescence. According to Drude, luminescence was produced when ions vibrated and emitted radiation at the same frequency as their vibrations. To support this hypothesis, Drude put forward the following argument: he assumed that the number of vibrating ions coincided with the chemical valence number of the material, and that the charge associated with a valence was a universal constant.[22] Using these hypotheses, together with experimental values for the amount of light energy emitted per second, he calculated a value for the hypothetical amplitude l of the vibrations of the ions, if regarded as ideal Hertz resonators. Finally he pointed out that l was various orders of magnitude smaller than the size of the molecule, which was fully consistent with the general picture of ions as vibrating inside molecules. Thus Drude found support for his initial hypothesis in the plausibility of the results generated by his network of assumptions: ions being Hertz resonators, applicability of the kinetic theory, and valence numbers representing sites for universal charges.

In this manner, exploitation of the ion hypothesis led to a new way to generate knowledge: this was not the ethos of exactitude, nor physical unification through mathematical equivalence, but a networking of different physical domains through a shared hypothesis. In this case, precise numerical agreement between theory and experiments could not help in checking whether the hypothesis was correct. One had to presume the ion in order to calculate e/m from experimental data. The only way to check the ion hypothesis was the consistency of the speculative picture on the microstructure of matter constructed through the network of insights dependent upon it (electrochemistry, spectroscopy, cathode rays, physical optics, heat radiation).

[22]The valence number was related to the number of other atoms that one substance required to form molecules. Hence valence was related to empty positions in the atom, not yet to the number of elementary particles—electrons—to be shared in forming molecules. Drude calculated the charge associated with a valence position from electrolysis, using the kinetic theory of gases, thus it was acknowledged that, in electrolysis, the electrolytes of one substance—ions—always transported the same number of valences from one electrode to the other. Drude happily observed that this charge almost coincided with the charge of the electron measured by Thomson. But Drude did not go further in relating the charge of one electron with the charge of valence.

2.4.4 Content and Narrative in *Lehrbuch der Optik*

The way in which Drude organized his book is inseparable from the way in which he understood the study of optics, the nature of electromagnetic light, and the role attributed to matter in the production of optical phenomena. From the previous two sections it follows that Drude's starting point in accounting for physical optics was always the description of an optical phenomenon. Then he gave a mathematical description of the phenomenon, and at the same time identified each mathematical term with its physical meaning. Since the physical system had been reduced to the physical language to describe the formalism, mathematical and physical accounts of optical phenomena mirrored each other (I call it a *mathematical-physical* approach). Thus there was no space for physical speculation beyond the bounds set out by the formal description of the phenomena. The introduction of phenomena followed a strict order: from those with the simplest mathematical-physical description to those with the most complex, hence from light propagation to magneto-optics. Each step subsumed the former one, both in terms of mathematics (from the parameter of light velocity to complex *Erklärungssysteme*) and in terms of the physical concepts used (from wave propagation to the interactions of electromagnetic light with ions). There were only two detours on this route. First, when Drude claimed the unification of electromagnetism and optics by introducing an external criterium: the experimental coincidence of the velocities of light and electromagnetic waves. Second, when, for the first time, Drude decided to introduce a speculative element: the ion. A clear gap was apparent between the physical description of mathematical terms (dielectric constants, refractive index, electric currents, light velocity, phase delay, light frequencies, and characteristic frequencies of selective absorption) and the supposition that the motions of ions were the cause of the optical properties of matter. Thus, the ion hypothesis enlarged the physical system beyond the interpretation of the mathematical formalism. Drude called this strategy of organizing the book the "synthetic route."

From the perspective of a physicist today, the synthetic route might seem a very obvious way to organize a book. Nevertheless, this was not necessarily the case. Ketteler, for example, articulated his book around the principle that light and matter interacted through *Mitschwingungen*. Thus he organized the chapters according to the type of medium within which the *Mitschwingungen* were performed, from the simplest to the most complicated media (first isotropic media, then anisotropic, then those having natural properties to polarize light). Preston, instead, put the emphasis on how optical phenomena were produced experimentally. He described some fundamental experiments, like Newton's refraction of light, for which he even reproduced passages of Newton's works. Preston thus combined chronology with experimental simplicity in organizing his account.

Drude's particular approach to optics clearly reflected another explicit goal, expressed in the introduction of his book: "To preserve a close contact with experiment, aiming at the simplest characterization possible of the field, I have chosen the synthetic route" (Drude 1900a, iii). Thus Drude's synthetic route aimed at "simplicity," where simplicity meant using the least number of physical hypotheses to support the mathematical formalism. In this sense, Drude's strategy can be regarded as a realization, for the field of optics, of the positivistic move he initiated in 1892. Indeed, it is hard to imagine another strategy, different from the synthetic route, that could articulate the existing knowledge into a unitary and consistent view of optics following the basic simplicity principle. Another option would have been seeking to unify the field around, for example, mechanical principles, as von Helmholtz did, but Drude had already relegated those principles to natural philosophers years before.

The synthetic route implied a very particular kind of trafficking between the simplest account in principle and the older sources. Previous papers entered the narrative for two principle reasons: first, to provide support for Drude's unitary view (in the case of experiments); second, as components naturally incorporated into his overall picture. As a matter of fact, he commented with regard to his own previous works on optical dispersion and the Kerr effect that they were subsumed by the new account in *Lehrbuch der Optik*. The implication was that previous papers could not give us more information about optics, unless we wanted to go beyond the simplest description of phenomena. The synthetic route blocked, in this way, direct contact with the scientific past. This contrasts clearly with Preston's and Basset's books, where older works entered the story by teaching us something about optics, conveying instead an accumulative process of knowledge acquisition.

Through the synthetic route, Drude concealed not only older versions of electromagnetism and the ion hypothesis but also the kind of questions that led to alternative conceptions of these subjects, viz. whether electromagnetism and mechanics related to each other in a manner beyond the system of Maxwell's equations. Simplicity, as characterized above, worked as a self-evident criterion for selecting knowledge and possible ways of generating knowledge in *Lehrbuch der Optik*. But simplicity had not always been such an obvious way to justify choices in physics. As Poincaré argued eloquently in the introduction of his 1891 book on electricity and optics:

> How can we make a decision among all these possible explanations [he is speaking of the various mechanical and electromagnetic theories of light, mathematically equivalent], if the experiments do not help us? […] Our decision can only be founded on considerations in which our personal views play a big role; in that there are solutions that someone can refuse due to their oddity and other solutions that are preferred due to their simplicity. (Poincaré 1891, 6–7)

Thus, what for Poincaré was a matter of personal taste in the selection of theories, for Drude was a distinguishing factor between physics and natural philosophy.

In Drude's synthetic route, the new questions to be posed appeared almost at the end of the narrative, when the introduction of the ion hypothesis produced a gap between the mathematical formalism and its physical interpretation, breaking in this way the simplicity rule. The "fresh life pulsing" in optics was to be found, as Drude prognosticated in the introduction, in the interplay between optical properties of matter, the corresponding *Erklärungssysteme*, the physical hypotheses about how ions behaved, and insights coming from other physical fields.

2.4.5 *Lehrbuch der Optik* as a Modern Book on Optics

Drude's textbook rapidly became influential in Germany, in the rest of Europe, and even in the United States. An English translation by Charles R. Mann and Robert A. Millikan was published in 1902 (Drude 1902), and a second and third edition of the German version appeared in 1906 and 1912 (Drude 1906; 1912).[23]

Reviews of *Lehrbuch der Optik*, both of the 1900 German and 1902 English editions, highlighted the modernity of the book, describing it as an advanced text that contained a lot of novel knowledge, upshots of the fast developments in optics in the preceding decades, and never included in textbooks up to that point. For example, Max Abraham wrote, in a review of 1900: "as a result of the fast progress that optics has made in the last years, the *old* books are obsolete. Drude's *Lehrbuch der Optik*, built on *modern* considerations, is therefore welcome"[24] (Abraham 1900, 415–416). And Albert A. Michelson commented in the preface to the 1902 English edition: "There does not exist to-day in the English language a general advanced text upon Optics which embodies the important advances in both theory and experiment which have been made within the last decade" (Drude 1902, iii). Even in comparison to famous English textbooks on optics, *Lehrbuch der Optik* was highly praised:

> It is a satisfaction to note that there has appeared a translation of this work, which received such instant recognition at the hands of physicists the world over upon its appearance in Germany. […] Descriptively, the book is fully on a par with Preston's *Theory of Light* and mathematically more valuable, as well as more lucid and attractive, than Basset's *Treatise on Physical Optics*. (Kent 1903, 75–76)

The aspect of Drude's account that most satisfied reviewers was his treatment of the electromagnetic theory of light together with the ion hypothesis, well expressed in this English review of the first German edition:

> Textbooks of optics, it is true, are numerous, and the reviewer is apt to think that of the making of many books there is no end. Professor Drude's book, however, contains much that is novel (at any rate, to English books) and the student will find up-to-date information on many points of interest. […] In all this work Prof. Drude has been most successful; the electromagnetic theory, supplemented by the one additional hypothesis of the moving electrons, serves to coordinate in a satisfactory way very many of the phenomena of light. (Anonymous 1900)

Now the question is: in what did the modernity of the book lay? Or put it in another way, what knowledge was left behind as old, once modern knowledge arrived?

In my opinion, the modernity of the book lay not only in the adoption of the electromagnetic theory of light and the introduction of the ion hypothesis, but also in what these decisions implied for the conception of optics as a whole. That is to say, the modernity lay also in the way electromagnetism and the ion hypothesis were redefined in their articulation as part of the synthetic route. Electromagnetic theory was reduced to the Maxwell's equations and optical phenomena were pictured as manifestations of ions, favored for both being *anschaulich* enough to derive optical theories and being embedded in a broader network of experimental practices from which to characterize their properties. Drude's viewpoint on optics was also modern in its way of breaking with the optical debates of the past. Discussions

[23] Moreover, two further editions of the English version appeared in 1959 and 2005. Besides them, there was one French translation in 1912 and another into Russian in 1935 (Cardona and Marx 2006).

[24] Italics are mine.

of the nature of the ether were abandoned for simplicity; the philosophical conflicts about the coincidence of electromagnetic and mechanical *Erklärungssysteme* were out of place. At the same time, *Lehrbuch der Optik* revealed a new domain for speculation in optics regarding the connection between ions and their optical manifestations, and the construction of a consistent microscopic picture of matter through the combination of the properties of ions from different experimental contexts. Drude had conceived a modern book, also because it posed modern questions, which then shaped research in optics in the ensuing years. Drude is the first, best example of the fruitfulness of these new questions. Drude not only wrote a book under their guidance, but the task of writing such a book also opened for him new pathways for further research.

2.5 Giessen 1900–Berlin 1906: Development of *Lehrbuch der Optik*'s Program up to the Second Edition

2.5.1 Ions vs. Electrons: the Sharpening of Drude's New Heuristics

The ion hypothesis turned out to provide great gains for Drude in a short time. On 21 April 1899, Drude presented his work on magneto-optical phenomena in the annual meeting of the German Physical Society, in which he used the ion hypothesis for the first time. On 14 December, Drude submitted a paper on the ion theory of metals, in which he accounted for the optical properties of metals by assuming that there existed a kind of light ions (as opposed to massive ions) that could travel freely across the metal (Drude 1900b).[25] In January 1900 Drude sent off the preface of *Lehrbuch der Optik*, where optics was built upon the ion hypothesis. Finally, in February, he sent off a very long paper to *Annalen der Physik*, in which he extended his optical program to other fields, specifically, to the thermal and electric properties of metals (Drude 1900c; 1900d). From December 1899 to February 1900, in these last papers, the dichotomy between light and massive ions had transformed into a dichotomy between electrons and ions.

In the same span of time, Drude was offered a position as full professor at the University of Giessen and directorship of its Institute of Physics, results of his being "known through his sound and comprehensive works on the field of optics and electric waves" (Lorey 1941, 123). He accepted immediately, and in April 1900 he and his family were already in Giessen, where he spent the five most productive and happy years of his professional life (König 1906; Planck 1906; Voigt 1906; Lorey 1941). He founded the *Physikalisches Kolloquium* and succeeded in creating a very lively research atmosphere among the doctoral students, resident researchers, and visitors. At the forefront of this research were ions and electrons. As one of his doctoral students, Karl Hahn, recalled: "The electron and ion theories were in the foreground in Giessen in that period. Drude himself worked on his electron theory of metals. In second place came everything that was associated with electric waves and radiation" (Lorey 1941, 123).

In terms of effects, the electron was the most significant acquisition of Drude's research in Giessen, and it enhanced the fruitfulness of the electromagnetic theory and ion hypothesis

[25] Drude's inspiration for writing this paper was a work published by Wilhelm Giese in 1889, in which he suggested that electrical conduction through metals was connected to ions (Giese 1889). It must not be just a coincidence that Lorentz, in his 1895 *Versuch*, grounded his adoption of the ion hypothesis on Giese's work, among other things. Drude never mentioned Lorentz's works before *Lehrbuch der Optik*, where he had to read them to provide a comprehensive account of the field.

overall. Analogously to the previous discussion of the various ways in which ions could be productive in *Lehrbuch der Optik*, we also see several different forms of fecundity for the electron in Drude's ways of articulating it. Drude's appropriation of the electron occurred in two steps. First, Drude employed the electron in his electron theory of metals, mentioned previously (Drude 1900c; 1900d). Rather than just the definition of the electron as a universal charged particle, what was useful for Drude was establishing a contrast between the lightness of the electrons and the massiveness of the ions, now relegated to "aggregates of electric cores and ponderable masses that refer to electrolytes" (Drude 1900c, 566). For in this way, Drude could distinguish conceptually between two kinds of microscopic behavior in metals: under the action of an electric field, ions were assumed to be practically at rest, whereas electrons moved freely across the material. This assumption enabled Drude to treat electrons as the particles of an ideal gas, and thereby to make use of the kinetic theory of gases to predict the thermal and electric properties of metals. For a concrete check on his results, he then resorted to Thomson's measurements of the charge, e, of the electron. The value of the mass of electrons, however small it was, played no role in Drude's 1900 theory of metals.

In 1904 Drude turned again to the electron, to give a more precise interpretation of optical phenomena in terms of the microstructure of matter (Drude 1904a; 1904b). In this case, the electrons had both mass and charge. Another post-1900 event also led Drude to reinforce the idea that the electrons and ions had well-differentiated roles in optics. In 1902, another student of Lorentz in Leiden, Lodewijk Siertsema (1902), calculated a value for e/m from his experiments on the Faraday effect in sodium gas, which was almost the same as the value calculated by his colleague Zeeman five years earlier in Leiden, and as the one measured in cathode rays. A sharper dichotomy between electrons and ions was established: electrons were the light charged particles characterized by the universal value e/m, while ions were characterized by the values e'/m' obtained in experiments on electrolysis, which did not all agree with one another, but depended on the substance explored. This dichotomy was very useful for Drude in various senses. First of all, Drude envisioned the vibration of electrons as the origin of natural frequencies in the ultraviolet region of the spectrum, while the motion of ions, being much more massive, caused natural vibrations in the infrared region. This assumption was especially useful in the cases of optical dispersion and the Faraday effect. Let's again follow Drude's reasoning for the example of dispersion.

Given the assumption that macroscopic effects were the sum of the microactions of either ions or electrons, Drude rewrote, in 1904, the formula for dispersion obtained on the basis of the *Mitschwingungen* model, in terms of the ratio of charge to mass of the hypothetical particles. The dependance of the index of refraction on ν of light had the same generic form as in 1872:

$$n^2 = 1 + \sum \frac{K_i {\nu_i}^2}{\nu_i^2 - \nu^2}. \tag{2.6}$$

Now Drude found the following expression for the parameter K_i as a function of the number N_i of electrons/ions with each proper frequency, ν_i, e being their charge and m their mass:

$$K_i = \frac{4\pi e^2 N_i}{m}. \tag{2.7}$$

Applying this expression to already-existing empirical data on dispersion in fluorite, Drude showed the plausibility of his hypothesis. He did it in the following way. He used the two values of K_i obtained by fitting experimental data on dispersion through fluorite into the generic formula, which corresponded to two proper frequencies ν_i (or the inverse of two wavelengths λ_i) at which the hypothetical microscopic particles vibrated. One frequency turned out to be in the ultraviolet region, the other in the infrared. As Drude observed, $\frac{K_{ultrav}}{\lambda_{ultrav}}$ was much larger than $\frac{K_{infrar}}{\lambda_{infrar}}$. By relating this piece of information to the fact that the value e/m of electrons was much smaller than e'/m' of ions, Drude proved the plausibility of his hypothesis that electrons vibrated at ultraviolet and ions at infrared frequencies.

On the whole, the parameters K_i and ν_i of $n(\nu)$, which could be computed by fitting macroscopic observations into the formula, turned out to provide an exceptional window into the invisible microstructure of matter: on the one hand, ν_i were identified directly with the frequency of the natural vibrations of the corresponding charged particles. On the other, by using either the universal value for e/m of the electrons or the varying ones obtained in electrolysis experiments, one could derive, from the empirical value of K_i, the number N_i of charged particles involved in producing optical phenomena. Thus optical dispersion became a means to *count* microscopic particles. In this way, the inner structure of molecules could be further characterized through Drude's interpretation of optical phenomena, while until then this inner structure had remained practically restricted to the domain of chemists.

The electron hypothesis led Drude to suggest another bold connection between optics and an outside scientific domain. In 1904 Drude suggested that the electrons counted in dispersion calculations coincided with valence electrons, to wit the same entities revealed both physical and chemical properties depending on the circumstances. To make this connection between optics and the periodic properties of the elements, Drude relied upon Richard Abegg's recent theory of valence. Just that year Abegg (1904), the theoretical chemist, had laid down the first electron theory of valence, according to which the chemical valence number corresponded to the number of negative electrons loosely bound to the atom that tended to be shared with other atoms in order to form molecules. In the case of fluorite, the identification between dispersion and valence electrons worked, while Drude left the stage open for checking the hypothesis with other substances.

In the end, the electron hypothesis showed itself fecund in two ways: first, through the conceptual differentiation between electrons and ions, grounded in their mass difference, and second, through the possibility of calculating the charge-to-mass ratio, e/m, and number, N_i, from experiments. A network of heterogeneous domains of knowledge was spanned through the connection of the values of e/m, N_i, and chemical valence: cathode rays, Zeeman effect, optical dispersion, chemistry. Thus in these years, Drude deepened his commitment to the new ion heuristics he had introduced in 1900.

2.5.2 Dispersion Electrons and the Second Edition of *Lehrbuch der Optik*

The significance of these novel insights for Drude's program of optics was demonstrated by their incorporation into the second edition of *Lehrbuch der Optik*, as Drude highlighted in the introduction to the 1906 edition. The electron was indeed the essential modification with respect to the first edition of the book:

> In the six years that have elapsed since the publication of the first edition of this book, a fast development of the whole field of physics has taken place through the experimental and theoretical display of the electron theory in a way that is hitherto unique. In optics, this advancement is naturally noticeable in the chapters that, as in the case of dispersion of bodies and their magnetic activity, are built on the ion hypothesis. Basically, the progress consists in replacing the ion hypothesis by the electron hypothesis, namely, by the knowledge that from certain optical phenomena one can derive the same characteristic universal constant as in the case of cathode rays and generally when free electrons appear. (Drude 1906, v)

In 1906 Drude assumed that the charged particles producing optical dispersion, as well as the Zeeman, Kerr, and Faraday effects were indeed electrons. Only when the proper frequencies of the hypothetical charged particles were situated in the infrared, as mentioned earlier, did he work with ions. As a matter of fact, Drude reworked the analytical expressions for the Kerr and Faraday effects in a similar way to his reworking of the dispersion formula. More specifically, he rewrote the variation of the polarization angle, δ, with respect to the frequency of light, ν, in terms of the number, N_i, of electrons, characterized by the natural frequency, ν_i, and the universal constant e/m. Thus, not only from experiments on optical dispersion, but also on the Kerr and Faraday effects, one could calculate the number of dispersion electrons that hypothetically gave rise to measurable features.

In this way the network of phenomena established through the electron hypothesis extended to the whole field of magneto-optics. In the new edition, one question seemed to "pulse fresh life" more than ever: how optical phenomena, taken as a whole, could help us in characterizing the structure of specific chemical substances, once we assumed that electrons were the microscopic causes of their macroscopic features. One important assumption was implicit in Drude's reformulation of optics, which turned out to be problematic a decade later: in order for macroscopic phenomena to result from the arithmetic sum of N_i microscopic resonances, each electron should interact with light independently of all the other electrons.

2.5.3 Last Year in Berlin

As he was writing the preface to the second edition, Drude and his family were living in Berlin. In 1905 he was offered a full professorship in physics and the directorship of the Berlin Physics Institute, replacing Emil Warburg. This was a big promotion, so despite certain reservations about leaving his pleasant life in Giessen, Drude accepted. But soon the situation in Berlin became very demanding. The first year he invested almost all his energies into the reorganization of the Institute, and this kept him away from research. He came to the point where he felt so anxious that, at the end of a talk he gave on 28 June in the Prussian Academy of Sciences, he declared: "I have a feeling of oppression, whether, by the exertion of all my strength, I will be up to the tasks assigned to me"[26] (Lorey 1941, 127).

Just one week after he uttered these dreadful words, Drude committed suicide. Planck, who had worked with him in Berlin, remembered the last weeks of Drude in this way:

[26] "Ich empfinde ein Gefühl der Beklemmung, ob ich durch Anspannung aller meiner Kräfte den an mich gestellten Aufgaben gewachsen sein werde."

> He retained his usual permanently jovial character in his everyday life, inside and outside home, until the last day. While the work at the Institute made its regular progress, he devoted himself with enthusiasm and success again to his scientific activity. He never expressed any idea about drastic relief. On 27 June he wrote the preface to the second edition of his *Lehrbuch der Optik* [...] and on 28 June, just one week before the catastrophe, he gave his inaugural talk at the Academy of Sciences, [...] in which he portrayed his scientific career and immediate plans for the future.
>
> He had already complied with the most important duties of the semester, holidays were close. The application to leave on vacation was already signed, his substitution by the assistants arranged, a tour in the Karwendel mountains with his colleague and friend Willy Wien agreed upon, from the equipment to the last detail of the garderobe prepared for this purpose. (Planck 1906, 628–629)

2.6 Epilogue: Following the Traces of *Lehrbuch der Optik*

2.6.1 *Lehrbuch der Optik* at Universities and in Other Textbooks

Drude's *Lehrbuch der Optik* became one of the reference books for teaching optics at universities, in particular German universities. For instance, Rudolph Minkowski, who worked as assistant to Rudolf Ladenburg, before and after WWI, said, concerning the University of Breslau, in an interview with Thomas Kuhn and John Heilbron for the Archive for History of Quantum Physics:

> Electro-magnetic theory I think the book most widely used would have been Abraham's. To some extent, although that was not translated into German that I know of, Lorentz's theory of electrons. Optics? I think Drude's was probably the commonly used book. Mechanics? I do not quite remember. But this were, in general, lectures tailored after some book. You may read Boltzmann or Kirchhoff....[27]

Further Drude's approach was also influential for other textbooks, in particular Voigt's second textbook, devoted almost exclusively to magneto-optics, titled *Magneto- und Elektro-Optik* (Voigt 1908). Voigt started by describing the simplest Zeeman effect and Lorentz's theoretical account of it, and continued by extending these initial insights to a manifold of new possible varieties of the Zeeman effect and further magneto-optical phenomena, such as the Faraday effect. To do so, Voigt relied upon Drude's strategy of deriving the optical formulas through the modification of constants. Voigt's departure point was actually Drude's formulation of optical dispersion as produced by N_i electrons each vibrating at a proper frequency ν_i. Voigt acknowledged Drude's impact both in the introduction of the book and throughout the text:

> Concerning treatments of magneto-optics in general textbooks on optics the one by P. Drude deserves special mention (Lehrbuch der Optik, Leipzig 1900 and

[27] See page 6 of the interview of Minkowski by Thomas Kuhn and John Heilbron in April 1962, AHQP, APS, M/f No. 1419-04-minkowski-r-002.

> 1906), which has a singular hue due to the strong emphasis on the electron theory. Drude confines himself to the easiest case of the Zeeman effect, namely to the easiest sort of magnetically excitable bodies, and thereby the license for rounding and completing the theory lies in his characterization, which does not correspond completely to the real state. (Voigt 1908, 4)

All in all, Drude's *Lehrbuch der Optik* and Voigt's *Magneto- und Elektro-Optik* had a resounding impact on the ensuing years of optical research in various laboratories in Germany, where experiments on optical phenomena were deemed an appropriate means to explore the microstructure of matter. Most significantly, on the basis of these two works, students of Voigt in Göttingen, Rudolf Ladenburg in Breslau, Christian Füchtbauer in Leipzig, and Dmitri Roschdestwensky in St. Petersburg did a very good job of calculating the number of optical electrons in gaseous substances from measurements of the Faraday effect, anomalous dispersion, and the intensity of dispersed spectral lines.[28] In this context, the electrons that were supposed to be optically active in the production of these phenomena were dubbed "dispersion electrons." Questions about the nature of the ether or the possibility of unifying mechanics and electromagnetism simply disappeared. The strategy of combining matter and ether by means of the electron hypothesis was taken for granted. The main new concern in the measurement of optical phenomena became the analysis of matter, where dispersion electrons acquired a life beyond the specificities of each phenomenon. The Faraday effect, optical dispersion, and the spread of the spectral lines all became a means to calculate the number and properties of dispersion electrons. The heuristics shift from the nature of ether to the nature of matter had been completed. Precision was a precondition of fitting experimental data to the formulas, but knowledge came from the networking of phenomena and different domains of knowledge through the sharing of concepts like dispersion electrons (e.g. cathode rays, optical phenomena, chemistry, and gas theory).[29]

Drude's *Lehrbuch der Optik*, in particular its English edition, was also influential for the American physicist Robert Wood in the writing of his textbook *Physical Optics* (1905). In contradistinction to Drude's and Voigt's books, and more in line with the English tradition, Wood payed more attention to the experimental realization of optical phenomena in their old and new variations, his book being an outstanding reference work from this point of view. However, "no pretence at originality in the mathematical treatment was made" (Wood 1905, 5). Instead, in this regard, Wood relied mostly on Drude's work: "The excellent theoretical treatment, based upon the electro-magnetic theory, given by Drude, has been followed very closely, and it is hoped that this acknowledgment may serve in place of the numerous quotation marks which would otherwise be necessary" (Wood 1905, 5). Mention of the other two English books, by Basset and Preston, Wood omitted. In this way, the audience for Drude grew larger still in English speaking countries.

---

[28]See especially (Geiger 1907; Ladenburg and Loria 1908; Ladenburg 1911; 1912; Füchtbauer and Schell 1913; Füchtbauer and Hofmann 1913; Roschdestwensky 1912).

[29]Lorentz also wrote a very influential book drawing upon the electron theory (Lorentz 1909), containing features on the various domains of physical knowledge that were hypothetically explained through different behaviors of electrons, such as optics and heat. Nevertheless, Lorentz's was not a book on optics, and it lacked a systematic approach to the field, which included the description of instruments and experiments, from the simplest to the most complex phenomena, separate from a theoretical account of the actions of the electrons.

2.6.2 From a Modern Book to Classical Optics

Drude's textbook was also referred to in research papers. Eventually, in the context of the emerging quantum theory, Drude's modern *Lehrbuch der Optik* was redefined as "classical." This transition is particularly well-revealed through Sommerfeld's 1915–1917 attempts to lay down a quantum theory of optical dispersion. In the 1910s, Drude's account of optical dispersion suffered a big theoretical blow. In 1913 Niels Bohr postulated that atoms consisted of a nucleus orbited by electrons and that specific trajectories existed along which electrons did not radiate energy (Bohr 1913a). The crux of the contradiction between Bohr's atom and Drude's dispersion theory was that in the quantum model electrons orbited, not vibrated, and that atoms emitted or absorbed light only through electrons jumping between orbits, not through the mechanical co-vibrations of electrons with light. In this way, Bohr explained the emission process of discrete spectral lines. At the same time, he also attempted to keep Drude's theory of dispersion and the identification between dispersion electrons and valence electrons by allowing orbits to be, under very specific circumstances, perturbed around their stationary states (Bohr 1913b; 1913c).

The theoretical physicist Arnold Sommerfeld singled out this conceptual conflict, and during various years he grappled with the possibility of restoring the correspondence between light frequencies and mechanical vibrations as a way to account for optical dispersion in the context of Bohr's model of the atom (Sommerfeld 1915; 1917). Sommerfeld followed the same direction hinted at previously by Bohr, and he developed at length the analytical problem of stationary orbits being mechanically perturbed under the external influence of the electric field of light. Thus *Mitschwingungen* between light and matter persisted in accounts of optical dispersion, although now electrons vibrated harmonically around their quantum orbits, instead of oscillating around fixed positions. In this way, Sommerfeld recovered the classical theory of optical dispersion, which he directly identified with Drude's *Lehrbuch der Optik*.[30] Eventually, Sommerfeld put forward the following conceptual dichotomy, in order that the whole picture held: the phenomena of spectral lines should be considered quantum in nature, therefore following Bohr's quantum jumps, while optical dispersion persisted in being accounted for by classical physics, namely, *Mitschwingungen* between electromagnetic radiation and matter. Drude's theory of optical dispersion instantiated the classical case (Sommerfeld 1915, 575-577).

In the late 1910s, Sommerfeld's dream faded for various reasons, but the important point here is the redefinition of Drude's previously modern approach as classical, within this context. What made Drude's dispersion theory a classical theory, as opposed to the quantum theory? Was it simply that it was non-quantum? The reason Bohr's atom became problematic in relation to Drude's theory of dispersion in 1913 was in fact a very specific and restricted one: the causal relation between the hypothetical resonance process at the particular mechanical frequencies of electrons and the macroscopic production of optical dispersion as it was observed in the laboratory. But as we have seen, such a causal relation was far from being obvious to Drude in the beginning. It was rather the result of a process extending from 1892 up to 1900, through which Drude, in conversation with his contemporaries, made numerous decisions that affected his and others' conceptions of optics as a whole, both at the epistemological level (from ethos of exactitude to simplicity, and then to networks of insights through the electron hypothesis), and ontological level (from a me-

[30] For more information about this episode, see (Jordi Taltavull 2013).

chanical ether to an electromagnetic ether, irreducible to mechanics). Thus, in the end, it was one of the features that made *Lehrbuch der Optik* a modern book, in the context of its composition, which led it to be considered classical years later: the articulation of vibrating electrons and electromagnetic light through the analysis of optical dispersion as representative of manifold magneto-optical phenomena.

As a matter of fact, it was the experimentalist Ladenburg who, in 1921, first ventured a new interpretation of optical dispersion, which would turn out to be in better agreement with the conceptual requirements of the emerging quantum theory (Ladenburg 1921). Ladenburg's difficulties with Drude's account originated in the counting of dispersion electrons and became apparent in the early 1910s, having, at the beginning, nothing to do with quantum physics. The problem was that the number of dispersion electrons did not coincide with the valence number, and most importantly, the numbers computed from experiments hinted at the possibility that dispersion electrons were actually not independent from each other in their interactions with light. Now, to interpret K_i in terms of N_i, thus $K_i = \frac{4\pi e^2 N_i}{m}$, one had previously to assume that electrons interacted with light, each one independent of the other. In other words, Drude's counting of dispersion electrons led to a dead end, for the results of the counting undermined the conditions for the dispersion electrons to be counted. Thereafter, in 1921, independently from Sommerfeld, Ladenburg suggested a new interpretation of K_i, resorting to new quantum tools and Bohr's atomic model. Ladenburg relinquished the proportion between macroscopic features and microscopic electrons underpinning Drude's theory of dispersion, and suggested a new proportionality: between the measurable parameter K_i and the number of quantum transitions corresponding to the frequency ν_i. In this way, optical dispersion became a quantum phenomenon. But at the same time, the *Mitschwingungen* model persisted in supplying the mathematical expression for $n(\nu)$ used to fit the experimental data and calculate the value of K_i, albeit reduced merely to a formal device. Thus what Ladenburg left behind with his quantum interpretation of optical dispersion was not classical physics altogether, but the identification of the abstract particles resonating with light in the model of *Mitschwingungen* with very specific charged particles, electrons, whose properties could be calculated from a heterogeneous network of phenomena. The *Mitschwingungen* (and *Mitbewegungen* in general) persisted in describing mathematically optical phenomena, though devoid of physical meaning. For the *Mitschwingungen* were conceptually incompatible with quantum transitions. Drude's modern heuristics, developed from 1900 until 1906 to network heterogeneous phenomena through their sharing of the same hypothetical microscopic agent, namely, electrons, was left behind. The new features with physical meaning were quantum transitions, instead of dispersion electrons.

Overall, what was modern in Drude's textbook, including and beyond the blending of the electromagnetic theory of light and electrons, was also what allowed it to become classical, when its differences from the emergent domain of quantum physics were made apparent. Negotiations of the boundary between classical and quantum physics took place precisely at the points of articulation of knowledge that had been ascribed "fresh life" in Drude's account: causal relations between moving, independent electrons and macroscopic features, disclosure of the microstructure of matter through the interrelation of phenomena hypothetically manifesting electron properties. But at the same time, this "modernity" of the book was itself the result of an arduous process that was by no means less challenging or innovative than its later development in relation to quantum physics. Classical physics had been constructed through other distinctions both on the epistemological and ontological levels, which were left behind as optics took on a modern form: mathematical formalism vs. the nature of ether, mechanics vs. electromagnetism. Only a long-term analysis enables us to understand the specific "classical" physics with which physicists grappled in the first decades of the twentieth century, and avoid oversimplifying it to mean all of non-quantum physics. Classical optics, and more specifically Drude's *Lehrbuch der Optik*, established the field of possibilities, the exploration of whose limits, defined the cognitive space within which the boundary with quantum physics was negotiated.

Abbreviations and Archives

AHQP	Archive for History of Quantum Physics. American Philosophical Society, Philadelphia

Acknowledgments

This paper has had a very long conception. It has accompanied me for more than three years, during which it has become part of my dissertation. I have benefited from a grant of the Catalan Government during my initial stay at the Max Planck Institute for the History of Science in Berlin, and then a pre-doctoral grant of the Max Planck Society to continue work on my dissertation there. In this time I have learned a lot from Luis Navarro from the University of Barcelona, and from personal exchanges with all the members of the international project on the History and Foundations of Quantum Physics. Jürgen Renn, my doctoral advisor, deserves special mention. I would also like to address a special thank to Massimiliano Badino and Jaume Navarro for their great idea and their enthusiastic engagement of us in this fascinating project on the History of Quantum Physics through Its Textbooks. And also for their patience in waiting for my paper and reading the various versions of it. Massimiliano is the person who has participated the most in the process of writing this paper, and to whom I am most indebted for the final result. I am also very thankful to Jeremiah James for his copy-editing, which enormously improved the quality of the language. In addition, I would also like to thank the editorial team, led by Nina Ruge, for their steadfast dedication and efforts behind the curtains to make a professional book out of these essays. Sharing an office with Nina I have realized how difficult, important, and, unfortunately, at times invisible, is the task of a book editor.

References

Abegg, Richard (1904). Die Valenz und das periodische System. Versuch einer Theorie der Molekularverbindungen. *Zeitschrift für anorganische Chemie* 38(1): 330–380.
Abraham, Max (1900). Lehrbuch der Optik. *Physikalische Zeitschrift* 1: 415–416.
Anonymous (1900). Lehrbuch der Optik. *Nature* 62: 595–597.
Arabatzis, Theodore (1992). The Discovery of the Zeeman Effect: A Case Study of the Interplay between Theory and Experiment. *Studies in History and Philosophy of Science Part A* 23(3): 365–388.

Arabatzis, Theodore (2006). *Representing Electrons. A Biographical Approach to Theoretical Entities*. Chicago: The University of Chicago Press.
Basset, Alfred B. (1892). *A Treatise on Physical Optics*. Cambridge: Deighton, Bell and Co.
Bohr, Niels (1913a). On the Constitution of Atoms and Molecules. *Philosophical Magazine 6th Series* 26: 1–25.
— (1913b). On the Constitution of Atoms and Molecules. *Philosophical Magazine 6th Series* 26: 476–502.
— (1913c). On the Constitution of Atoms and Molecules. *Philosophical Magazine 6th Series* 26: 857–875.
Boltzmann, Ludwig (1893). *Vorlesungen über Maxwells Theorie der Elektrizität und des Lichts*. Leipzig: Johann Ambrosius Barth.
Buchwald, Jed Z. (1985). *From Maxwell to Microphysics. Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century*. Chicago: The University of Chicago Press.
— (1994). *The Creation of Scientifi Effects. Heinrich Hertz and Electric Waves*. Chicago: The University of Chicago Press.
Buchwald, Jed Z. and Andrew Warwick, eds. (2001). *Histories of the Electron: The Birth of Microphysics*. Cambridge, MA: The MIT Press.
Cardona, Manual and Werner Marx (2006). The Posthumous Impact of Paul Drude. *Annalen der Physik* 15(7-8): 461–468.
Christiansen, Christian (1870). Über die Brechungsverhältnisse einer weingeistigen Lösung des Fuchsin; briefliche Mittheilung. *Annalen der Physik* 217(11): 479–480.
Darrigol, Olivier (2000). *Electrodynamics from Ampère to Einstein*. Oxford: Oxford University Press.
Drude, Paul (1887). Über die Gesetze der Reflexion und Brechung des Lichtes an der Grenze absorbierender Kristalle. *Annalen der Physik* 268(12): 584–625.
— (1892a). In wieweit genügen die bisherigen Lichttheorien den Anforderungen der practischen Physik? *Nachrichten von der Königlichen Gesellschaft der Wissenschaften zu Göttingen* 29(11): 366–412.
— (1892b). Über magnetooptische Erscheinungen. *Annalen der Physik* 282(7): 353–422.
— (1893). Über die Beziehung der Dielectricitätsconstanten zum optischen Brechungsexponenten. *Annalen der Physik* 284(3): 536–545.
— (1894). *Physik des Äthers auf elektro-magnetischer Grundlage*. Stuttgart: F. Enke.
— (1895). *Die Theorie in der Physik. Antrittsvorlesung gehalten am 5. December 1894 an der Universität Leipzig*. Leipzig: S. Hirzel.
— (1896). Anomale electrische Dispersion von Flüssigkeiten. *Annalen der Physik* 294(5): 1–20.
— (1897a). Electrische Anomalie und chemische Constitution. *Annalen der Physik* 296(3): 500–509.
— (1897b). Über Fernewirkungen. *Annalen der Physik* 298(12): 1–49.
— (1897c). Zur Theorie der magneto-optischen Erscheinungen am Eisen, Nickel und Kobalt. *Annalen der Physik* 298(12): 687–692.
— (1898). Zur Theorie der anomalen electrischen Dispersion. *Annalen der Physik* 300(1): 131–158.
— (1899). Zur Theorie der magneto-optischen Erscheinungen. *Verhandlungen der Deutschen Physikalischen Gesellschaft* 1(6): 107–116.

— (1900a). *Lehrbuch der Optik*. 1st ed. Leipzig: S. Hirzel.
— (1900b). Zur Ionentheorie der Metalle. *Physikalische Zeitschrift* 1(14): 161–165.
— (1900c). Zur Elektrontheorie der Metalle. Teil I. *Annalen der Physik* 306(3): 566–613.
— (1900d). Zur Elektrontheorie der Metalle. Teil II. *Annalen der Physik* 308(11): 369–402.
— (1902). *The Theory of Optics*. London: Longmans Green and Company.
— (1904a). Optische Eigenschaften und Elektrontheorie. I. Teil. *Annalen der Physik* 319(9): 677–725.
— (1904b). Optische Eigenschaften und Elektrontheorie. II. Teil. *Annalen der Physik* 319(10): 936–961.
— (1906). *Lehrbuch der Optik*. 2nd ed. Leipzig: S. Hirzel.
— (1912). *Lehrbuch der Optik*. 3rd ed. Leipzig: S. Hirzel.
Falconer, Isobel (1987). Corpuscles, Electrons and Cathode Rays: J. J. Thomson and the 'Discovery of the Electron'. *British Journal for the History of Science* 20(3): 241–276.
Faraday, Michael (1846). On the Magnetization of Light and the Illumination of Magnetic Lines of Force. *Philosophical Transactions of the Royal Society* 136: 1–20.
Füchtbauer, Christian and Walter Hofmann (1913). Über Maximalintensität, Dämpfung und wahre Intensitätsverteilung von Serienlinien in Absorption. *Annalen der Physik* 348(1): 96–134.
Füchtbauer, Christian and Curt Schell (1913). Methoden zur quantitativen Untersuchung von Absorptionslinien, speziell der Natriumlinien. *Physikalische Zeitschrift* 14: 1164–1168.
Geiger, Ludwig (1907). Über die Begleiterscheinung des inversen longitudinalen Zeemaneffektes. *Annalen der Physik* 328(9): 758–794.
Giese, Wilhelm (1889). Grundzüge einer einheitlichen Theorie der Electricitätsleitung. *Annalen der Physik* 273(8): 576–609.
Goldberg, Stanley (1990). Paul Drude. In: *Dictionary of Scientific Bibliography*. Ed. Charles C. Gillispie. New York: Scribner.
von Helmholtz, Hermann (1875). Zur Theorie der anomalen Dispersion. *Annalen der Physik* 230(4): 582–596.
— (1892). Das Princip der kleinsten Wirkung in der Elektrodynamik. *Sitzungsberichte der Königlich Preußischen Akademie der Wissenschaften zu Berlin* 1: 459–475.
— (1893). Electromagnetische Theorie der Farbenzerstreuung. *Annalen der Physik* 284(3): 389–405.
— (1897). Vorlesungen über die elektromagnetische Theorie des Lichts. In: *Vorlesungen über theoretische Physik von Hermann von Helmholtz*. Ed. Arthur König and Carl Runge. 5. Hamburg: Verlag von Leopold Voss.
— (1903). Die neuere Entwicklung von Faradays Ideen über Elektricität. In: *Vorträge und Reden von Hermann von Helmholtz*. 2. Braunschweig: Friedrich Vieweg und Sohn. 249–309.
Hentschel, Klaus (1996). Die Entdeckung des Zeeman-Effekts als Beispiel für das komplexe Wechselspiel von wissenschaftlichen Instrumenten, Experimenten und Theorie. *Physikalische Blätter* 52: 1232–1235.
Hertz, Heinrich R. (1888). Über die Ausbreitungsgeschwindigkeit der electrodynamischen Wirkungen. *Annalen der Physik* 270: 551–569.

Hertz, Heinrich R. (1890). Über die Grundgleichungen der Electrodynamik für ruhende Körper. *Annalen der Physik* 276(8): 577–624.

Hoffmann, Dieter (2006). Paul Drude (1863-1906). *Annalen der Physik* 15(7-8): 449–460.

Hunt, Bruce J. (1991). *The Maxwellians*. Ithaca: Cornell University Press.

Jordi Taltavull, Marta (2013). Challenging the Boundaries between Classical and Quantum Physics: the Case of Optical Dispersion. In: *Traditions and Transformations in the History of Quantum Physics*. Ed. Shaul Katzir, Christoph Lehner, and Jürgen Renn. Proceedings 5: Max Planck Research Library for the History and Development of Knowledge. Berlin: Edition Open Access.

Jungnickel, Christa and Russell McCormmach (1986). *Intellectual Mastery of Nature. Theoretical Physics from Ohm to Einstein*. 2. Chicago: The University of Chicago Press.

Kaufmann, Walter (1898). Die magnetische Ablenkbarkeit electrostatisch beeinflusster Kathodenstrahlen. *Annalen der Physik* 301(6): 431–439.

Kent, N. A. (1903). Review of *The Theory of Optics*, by Paul Drude. *Astrophysical Journal* 18: 75.

Kerr, John (1877). On Rotation of the Plane of Polarization by Reflection of the Pole of a Magnet. *Philosophical Magazine 5th Series* 3: 321–343.

Ketteler, Eduard (1874). Das specifische Gesetz der sogenannten anomalen Dispersion. *Annalen der Physik* Jubelband: 166–182.

— (1885). *Theoretische Optik gegründet auf das Bessel-Sellmeier'sche Princip*. Braunschweig: Friedrich Vieweg und Sohn.

König, Walter (1906). *Zur Erinnerung an Paul Drude. Zwei Ansprachen von F. Richard and W. König*. Giessen: Verlag von Alfred Töpelmann. 9–38.

Kox, Anne J. (1997). The Zeeman Effect. *European Journal of Physics* 18: 139–144.

Kundt, August (1871a). Über die anomale Dispersion der Körper mit Oberflächenfarben. *Annalen der Physik* 219: 163–171.

— (1871b). Ueber anomale Dispersion. *Annalen der Physik* 219: 259–269.

— (1871c). Ueber anomale Dispersion. Dritte Mittheilung. *Annalen der Physik* 220: 128–137.

— (1871d). Ueber anomale Dispersion. Vierte Mittheilung. *Annalen der Physik* 221(1): 67–80.

Ladenburg, Rudolf (1911). Astrophysikalische Bemerkungen im Anschluß an Versuche über Absorption und anomale Dispersion in leuchtendem Wasserstoff. *Physikalische Zeitschrift* 12: 9–12.

— (1912). Über die anomale Dispersion und die magnetische Drehung der Polarisationsebene des leuchtenden Wasserstoffs, sowie die Verbreiterung von Spektrallinien. *Annalen der Physik* 343(7): 249–318.

— (1921). Die quantentheoretische Deutung der Zahl der Dispersionselektronen. *Zeitschrift für Physik* 4: 451–468.

Ladenburg, Rudolf and Stanislaw Loria (1908). Über die Dispersion des leuchtenden Wasserstoffs. *Physikalische Zeitschrift* 9(24): 875–878.

Lommel, Eugen (1878). Theorie der normalen und anomalen Dispersion. *Annalen der Physik* 239(3): 339–356.

Lorentz, Hendrik A. (1892). La théorie électromagnétique de Maxwell et son application aux corps mouvants. *Archives néerlandaises des sciences exactes et naturelles* 25: 363–552.

— (1895). *Versuch einer Theorie der elektrischen und optischen Erscheinungen in bewegten Körpern*. Leiden: E. J. Brill.

— (1897). Über den Einfluss magnetischer Kräfte auf die Emission des Lichtes. *Annalen der Physik* 299(13): 278–284.

— (1898). Optische Verschijnselen die met de lading en de massa der ionen in verband staan. *Verslagen Koninklijke Akademie van Wetenschappen te Amsterdam* 6: 506–519.

— (1909). *The Theory of Electrons and Its Applications to the Phenomena of Light and Radiant Heat*. New York: Columbia University Press.

Lorey, Wilhelm (1941). Die Physik an der Universität Gießen im 19. Jahrhundert. *Nachrichten der Gießener Hochschulgesellschaft* 15: 80–132.

Macaluso, Damiano and Orso M. Corbino (1898). Sur une nouvelle action subie par la lumière traversant certaines vapeurs métalliques dans un champ magnétique. *Comptes rendus hebdomadaires des séances de l'Académie des sciences* 128(16): 548–551.

Müller, Johann H., Claude S. Pouillet, Leopold Pfaundler, and Otto Lummer (1897). *Lehrbuch der Physik und Meteorologie*. 9th ed. Braunschweig: Friedrich Vieweg und Sohn.

Navarro, Jaume (2012). *A History of the Electron: J. J. and G. P. Thomson*. Cambridge: Cambridge University Press.

Olesko, Kathryn M. (1991). *Physics as a Calling. Discipline and Practice in the Königsberg Seminar for Physics*. Ithaca: Cornell University Press.

Planck, Max (1906). Paul Drude. *Verhandlungen der Deutschen Physikalischen Gesellschaft* 8: 599–630.

Poincaré, Henri (1890). *Électricité et optique: cours de physique mathématique*. Paris: Georges Carré.

— (1891). *Elektrizität und Optik. Vorlesungen gehalten von H. Poincaré*. Ed. Wilhelm Jaeger and Ernst Gumlich. Berlin: Julius Springer.

Preston, Thomas (1890). *The Theory of Light*. London: MacMillan and Co.

Reiff, Richard (1896). *Theorie molekular-elektrischer Vorgänge*. Freiburg: Mohr Verlag.

Robotti, Nadia (1995). J. J. Thomson and the Cavendish Laboratory: The History of an Electric Charge Measurement. *Annals of Science* 52: 265–284.

Roschdestwensky, Dmitry (1912). Anomale Dispersion in Natriumdampf. *Annalen der Physik* 344(12): 307–345.

Sellmeier, Wolfgang (1872a). Über die durch die Aetherschwingungen erregten Mitschwingungen der Körpertheilchen und deren Rückwirkung auf die erstern, besonders zur Erklärung der Dispersion und ihrer Anomalien. *Annalen der Physik* 221(3): 399–421.

— (1872b). Über die durch die Aetherschwingungen erregten Mitschwingungen der Körpertheilchen und deren Rückwirkung auf die erstern, besonders zur Erklärung der Dispersion und ihrer Anomalien. *Annalen der Physik* 221(4): 520–549.

— (1872c). Über die durch die Aetherschwingungen erregten Mitschwingungen der Körpertheilchen und deren Rückwirkung auf die erstern, besonders zur Erklärung der Dispersion und ihrer Anomalien. *Annalen der Physik* 223(11): 386–403.

— (1872d). Über die durch die Aetherschwingungen erregten Mitschwingungen der Körpertheilchen und deren Rückwirkung auf die erstern, besonders zur Erklärung der Dispersion und ihrer Anomalien. *Annalen der Physik* 223(12): 525–554.

Siertsema, Lodewijk H. (1902). Berekening van e/m uit de magnetische draaiing van het polarisatievlak, voor stoffen zonder absorptieband in het zichtbare spectrum. *Verslag van de gewone vergaderingen der Afdeeling Natuurkunde*: 499–511.

Sommerfeld, Arnold (1915). Die allgemeine Dispersionsformel nach dem Bohrschen Modell. In: *Festschrift Julius Elster und Hans Geitel*. Ed. Karl Bergwitz. Braunschweig: Friedrich Vieweg und Sohn. 549–584.

— (1917). Die Drudesche Dispersionstheorie vom Standpunkte des Bohrschen Modelles und die Konstitution von H_2, O_2 und N_2. *Annalen der Physik* 358(15): 497–550.

Thomson, Joseph J. (1897a). Cathode Rays. *Proceedings of the Royal Institution* 15: 419–432.

— (1897b). Cathode Rays. *Philosophical Magazine 5th Series* 44: 293–316.

Voigt, Woldemar (1891). Zur Theorie des Lichtes. *Annalen der Physik* 279(6): 410–437.

— (1896). *Kompendium der theoretischen Physik*. Zweiter Band. Elektricität und Magnetismus. Optik. Leipzig: Veit.

— (1899). Zur Theorie der magneto-optischen Erscheinungen. *Annalen der Physik* 303(2): 345–365.

— (1906). Paul Drude. *Physikalische Zeitschrift* 7(14): 481–482.

— (1908). *Magneto- und Elektro-Optik*. Leipzig: B. G. Teubner.

Whittaker, Edmund (1910). *A History of the Theories of Aether and Electricity: From the Age of Descartes to the Close of the Nineteenth Century*. London: Longman, Green and Co.

Wiechert, Emil (1897a). Experimentelles über die Kathodenstrahlen. *Schriften der Physikalisch-Ökonomischen Gesellschaft zu Königsberg* 38: 12–16.

— (1897b). Über das Wesen der Elektrizität. *Schriften der Physikalisch-Ökonomischen Gesellschaft zu Königsberg* 38: 3–12.

Winkelmann, Adolf, ed. (1894). *Handbuch der Physik*. 2. Optik. Breslau: Verlag von Eduard Trewendt.

Wood, Robert Williams (1905). *Physical Optics*. New York: MacMillan and Co.

Zeeman, Pieter (1897a). On the Influence of Magnetism on the Nature of the Light Emitted by a Substance. *Philosophical Magazine 5th Series* 43(262): 226–239.

— (1897b). Doublets and Triplets in the Spectrum Produced by External Magnetic Forces. *Philosophical Magazine 5th Series* 44: 55–60.

Chapter 3
Max Planck as Textbook Author
Dieter Hoffmann

3.1 Planck and Thermodynamics

Not only is Max Planck regarded as one of the outstanding theoretical physicists of his age, who helped establish the field of modern physics with his quantum hypothesis, he was also the author of a five-volume series of textbooks called *Einführung in die theoretische Physik* (1919), which makes him one of the discipline's classic textbook authors. This series emerged from Planck's lectures at the University of Berlin, and the textbook became a standard work on physics the world over. It played a role in contemporary physics training—at least in the interwar period in German-speaking countries—similar to that of the textbook series by Lev Landau and Evgeny Lifshitz, or the "Lectures" series by Richard Feynman. Thus it can be considered a textbook in the traditional sense, and all but the ideal in its fulfillment of a textbook's function, as defined so concisely by Thomas S. Kuhn in *The Structure of Scientific Revolutions*, to "expound the body of accepted theory, illustrate many or all of its successful applications, and compare these applications with exemplary observations and experiments" (Kuhn 1970, 10).

But the story of Planck as a textbook author had already begun decades before this series. In 1897 the publishing house Veit & Company in Leipzig released Planck's one-volume *Vorlesungen über Thermodynamik*. According to Planck, the book originated from suggestions by his colleagues and students that the "treatises [by Planck] that cover the area of thermodynamics be published as a collection or edited into a summary" (Planck 1897, iii). Yet, as Planck explains in his preface, he decided not to follow this advice. Instead he wanted to rid the texts of their character as research results and lend them more of the nature of a textbook

> intended as an introduction to the study of thermodynamics for anyone who has completed a course in beginning physics and chemistry and is familiar with the elements of differential and integral calculus. (Planck 1897, iii)

In this way he could "develop in greater detail some of those general considerations and proofs that were kept a bit short in the terse style of tracts, in order to make them more understandable," and also "expand on the corresponding subject in order to summarize the entire area of thermodynamics in a coherent presentation" (Planck 1897, iii). Despite his best intentions, it must be admitted that the origin of the text in Planck's lectures can still be detected, and in such a way that the book differs considerably from what we accept as a textbook today.

The individual lectures are divided into four sections. In the first section there are three subsections dealing with "basic facts and definitions" like temperature, molecular weight, and thermal energy; the second section covers the first law of thermodynamics, including not only its general formulation, but also its application to homogeneous and inhomogeneous systems; the third section discusses the second law in a similar manner; the subject of the fourth and final section is "applications to special equilibria," which range from homogeneous systems, to systems in various aggregate states, all the way down to dilute solutions.

Although Planck had offered regular courses on thermodynamics and the mechanical theory of heat ever since his stint as a private lecturer in Munich, and in spite of the fact that the title of the book, as well as the purpose Planck claimed in its preface, suggested the character of a textbook, the lectures on thermodynamics published in 1897 resembled less elaborated lessons than an account of Planck's own thermodynamic studies. He attempted a retrospective sketch of these studies in his autobiography of 1947:

> [After] my doctoral dissertation at the University Munich, which I completed in 1879 [I continued] my studies of entropy, which I regarded as, next to energy, the most important property of physical systems. Since its maximum value indicates a state of equilibrium, all the laws of physical and chemical equilibrium follow from a knowledge of entropy. I worked this out in detail during the following years, in a number of different researches. First, in investigations on the changes in physical state, presented in my probationary paper at Munich in 1880, and later in studies on gas mixtures. All my investigations yielded fruitful results. (Planck 1949, 18–20)

These basic experiments on thermodynamic equilibria in physico-chemical systems, which were also the focus of Planck's lectures, came to a provisional conclusion in the mid–1890s. In the following years, Planck then concentrated on radiation equilibria; hence the publication date of his lectures was anything but coincidental. It can be considered a kind of summary of his early work on thermodynamics. On this basis, and because didactic principles play practically no role in its impartation of the latest thermodynamic knowledge, the *Vorlesungen* seem to be more of an encapsulating portrayal and synopsis of contemporary thermodynamic knowledge than a standard textbook. What is interesting is that Planck's *Vorlesungen über Thermodynamik* was reissued and expanded repeatedly—up to the ninth edition in 1930, under the aegis of their original author, and then in its tenth (1954) and eleventh (1963) editions, edited by Max von Laue and Max Päsler, respectively. Meaning these lectures were republished more often than any other book by Planck, which probably reflects less his virtues as a textbook author and more the importance and standing of his works on thermodynamics, which distinguish him as one of the leading thermodynamicists of all time (Ebeling and Hoffmann 2008, vii–xxiv).

3.2 Heat Radiation

As a first-rate expert on thermodynamics, Planck had the means at his disposal to resolve the contradictions prevalent within the theory of radiation at the end of the nineteenth century, contradictions which neither the tools of electrodynamics nor those of optics could resolve. This becomes abundantly clear in Planck's second textbook, his *Theorie der Wärmestrahlung*, subtitled *Vorlesungen von Max Planck*, which was published by Johann Ambrosius Barth's publishing house in Leipzig in spring 1906. According to the preface, the "main contents" of these lectures reflect "the lectures [Planck] held at the University of Berlin in winter semester 1905/06" (Planck 1906, iv). Even though the structure of the book exhibits many similarities to Planck's *Vorlesungen über Thermodynamik*, the lectures on the theory of heat radiation certainly read more like a textbook. Compared to his earlier text on thermodynamics, Planck's work on heat radiation is less focused on his own research, and in addition, it discusses the subject much more comprehensively—in the manner of an "introduction to the study of the entire theory of radiative heat on a uniform thermodynamic foundation" (Planck 1906, iii). With this text Planck provided what has come to be recognized as the first comprehensive account of the theory of radiation.

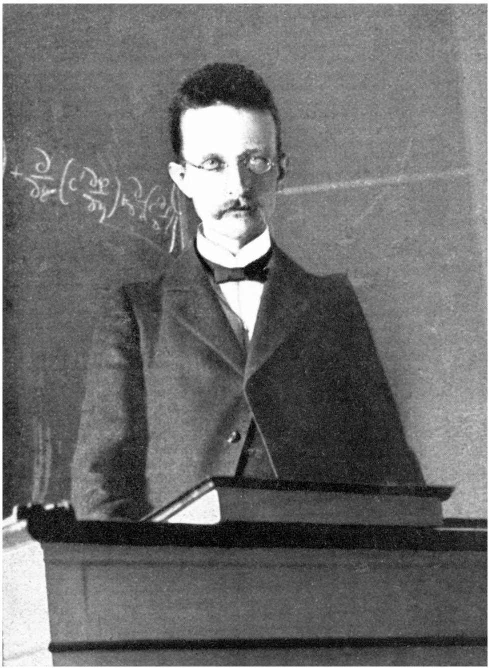


Figure 3.1: Max Planck teaching, late1880s. Archive of the Max Planck Society, Berlin.

The book opens with explanations of the basic principles of (geometric) optics required for the study of heat radiation, especially the laws for the propagation of radiation and the (classical) electrodynamic laws for the emission and absorption of radiation. Then the basic laws of heat radiation are explained: Kirchhoff's radiation law, black-body radiation, the Stefan-Boltzmann law and Wien's displacement law, all generalized so as to apply to radiation with any spectral energy distribution. A whole section is devoted to discussing the linear oscillator as the (simplest) model for emitting and absorbing systems. The central, fourth section of the book investigates the interrelation between entropy and probability, deriving the energy distribution of black-body radiation from a statistical treatment of radiating oscillators, thereby enabling Planck to deduce a universally valid radiation formula. In this statistical treatment Planck's constant h, the quantum of action, appears as a new, universal natural constant, and Planck's law is derived. The book's final section is then devoted to irreversible radiation processes.

Planck's *Vorlesungen über die Theorie der Wärmestrahlung* was a success and carved out a prominent position among contemporary physics textbooks. Indeed, the success of the lectures was probably why the S. Hirzel-Verlag of Leipzig approached physicist and author Arnold Sommerfeld of Munich with a proposal to collaborate with Planck in developing a textbook on theoretical physics (Sommerfeld 2000, vol. 1, 354–368).[1] At the time, these two men were considered to be *the* authoritative representatives of theoretical physics in Germany; they were both also known for giving outstanding lectures. This idea was not realized, however, and at least one reason why can be gleaned from the surviving correspondence; Sommerfeld asserted, "that the consistency of such a work, which is supposed to be its major advantage, is better ensured when a single author does the whole thing"[2] (Sommerfeld 2000, vol. 1, 354–368). Certainly both authors were also aware that their approaches to such a textbook would differ considerably. Planck took great pains to adhere to a systematic, logical structure in his textbooks, and presented the material in a more deductive way, whereas Sommerfeld placed the concrete problems of physics in the foreground, which is particularly and strikingly clear in the textbook he published a decade later, *Atomic Structure and Spectral Lines* (cf. the chapter by Michael Eckert in this volume). In the preface to the first volume of his *Vorlesungen über theoretische Physik*, Sommerfeld himself elaborated on the differences between the two textbooks:

> Compared with the lectures by PLANCK, which are flawless in their systematic structure, I believe that what argues in favor of my lectures are their greater richness of material and freer treatment of the mathematical apparatus. Yet I gladly refer, especially as concerns thermodynamics and statistics, to the more comprehensive and in many ways more thorough account by PLANCK.[3] (Sommerfeld 1943a, vi)

It also speaks to the success of Planck's *Vorlesungen über die Theorie der Wärmestrahlung* that the book ran through six editions—five during Planck's lifetime. The last edition to date was issued by the physicist Hans Falkenhagen of Rostock in 1966, as a reprint of the fifth edition from 1923. The 1966 edition was presumably a response not so much to contemporary teaching needs as the historical interests of the publisher and the physical community of the day; the East German publisher certainly hoped that the distribution of the reprint in the West might also bring in hard currency.

The individual editions differ from each other, sometimes significantly, not least of all because they mirror the development of quantum theory. The greatest differences are found between the first edition (1906) and the second (1912). As Planck explains in his preface to the second edition, the years between 1906 and 1912, considered with respect to the "special theory outlined in this book, especially the hypothesis of the elementary quantum of action, were by and large beneficial. In particular my radiation formula has

[1] Hirzel to Sommerfeld, February 1909; Planck to Sommerfeld, February 1909.

[2] "daß die Einheitlichkeit eines solchen Werkes, die ja gerade sein Hauptvorzug sein soll, besser verbürgt ist, wenn ein Einziger das Ganze macht." All English translations by the author.

[3] "Gegenüber den Vorlesungen von PLANCK, die im systematischen Aufbau einwandfrei sind, glaube ich zu Gunsten meiner Vorlesungen eine größere Reichhaltigkeit an Stoff und eine freiere Handhabung des mathematischen Apparats anführen zu können. Ich verweise aber gern, insbesondere was Thermodynamik und Statistik betrifft, auf die vollständigere und vielfach gründlichere Darstellung von PLANCK."

proven satisfactory so far"[4] (Planck 1912, iv). In addition, during these years, not only did Planck's law find convincing experimental confirmation, but also, and more importantly, the quantum hypothesis was applied successfully by physicists like Albert Einstein, Walther Nernst, Peter Debye, and Johannes Stark—to name but a few—to resolve other, until then intractable physical problems that fell outside the sphere of radiation theory (Hermann 1971). The Solvay Conference of October 1911, whose prominent participants included Planck, had, after all, made clear once and for all with the authority of the leading contemporary physicists that the "quantum problem" was not a special problem for radiation theory, but rather a question fundamental to the development of contemporary physics.

Against the backdrop of contemporary developments in quantum theory, Planck reworked his textbook significantly—for instance, by completely eliminating the section *Emission und Absorption elektromagnetischer Wellen durch einen linearen Oszillator*, which discussed in great detail the oscillator model. In this instance, Planck was reacting to the consequences of the quantum hypothesis placing limits on the validity of the classical laws of electrodynamics; furthermore, the oscillator model turned out to be an oversimplification. Instead, the quantum hypothesis itself, and the assumption of finite energy elements, were more clearly made the focus, in that the leitmotif guiding his reworking of the lectures was to "connect the quantum hypothesis as closely as possible to classical dynamics, and to penetrate the barriers of the latter only where the facts of experience leave no other alternative"[5] (Planck 1912, vi). Characteristic of this conservative approach, Planck presents in the book his second quantum theory, which he had only developed in 1911/12. The new theory presupposes a discontinuous and quantal emission process for radiation, whereas absorption, as before, takes place continuously, in accordance with the classical electrodynamics per Maxwell's equations (Planck 1912, 150–160). The developing quantum theory was quick to gloss over such attempts at a classical reinterpretation of the initial quantum hypothesis; remarks in this vein had disappeared by the fourth and fifth editions of 1921 and 1923, respectively. Instead Planck took into account the latest developments in quantum theory, from the Nernst heat theorem and the application of the quantum hypothesis to the temperature dependence of the specific heats of solids, to the Bohr model of the atom, and all the way to the Stern-Gerlach experiment—albeit presented in all brevity, which certainly must have posed a challenge to the students and readers attempting to understand the material.

The fact that there were no new editions after the fifth edition of 1923—aside from the aforementioned sixth edition of 1966, which should be regarded as a historical reprint rather than a textbook—can be explained primarily by the progressive shift in the focus of quantum theory, ever further away from radiation theory, and ever more toward the problems of atomic and electron theory, which began with the acceptance of Bohr's atomic theory at the latest, and marginalized Planck's thermodynamic approach, transforming heat radiation theory into a peripheral special case.

[4] "in diesem Buche entworfenen speziellen Theorie, insbesondere der Hypothese des elementaren Wirkungsquantums, im großen und ganzen günstig gewesen. Vor allem hat sich meine Strahlungsformel bisher befriedigend bewährt."

[5] "den Anschluß der Quantenhypothese an die klassische Dynamik so eng als möglich zu gestalten, und die Schranken der letzteren erst da zu durchbrechen, wo die Tatsachen der Erfahrung keinen anderen Ausweg mehr übrig lassen."

3.3 The *Introduction to Theoretical Physics*

It may also have played a role in the demise of the textbook that, in the meantime, the same publisher had issued Planck's multi-volume *Einführung in die theoretische Physik*. The second part of the final, fifth volume of this new textbook, which appeared in 1930, is devoted almost entirely to the topics of heat radiation and quantum theory. For reasons of space, more general and synoptic considerations are naturally foregrounded, and concrete applications almost completely eliminated. Hence the contents of the *Vorlesungen über die Theorie der Wärmestrahlung* are by no means fully incorporated in Planck's new volume on the *Theorie der Wärme*; on the contrary—as Planck states himself—"in a sense [they] complement each other reciprocally"[6] (Planck 1932, v).

Planck's *Einführung in die Theorie der Wärme* of 1930 concluded a textbook project that he had begun in 1919. Reference was made above to the context of Planck's lectures and to the S. Hirzel Verlag's idea of publishing a textbook written jointly by Planck and Sommerfeld. In the subsequent years Planck apparently completed the textbook project by himself, and, in the process, initially discouraged Sommerfeld from writing a similar book (cf. the chapter by Michael Eckert in this volume); however, this may also be what prompted Sommerfeld to concentrate fully on writing his epochal *Atomic Structure and Spectral Lines*. As he admitted to Hirzel in June 1924, he had

> often thought about publishing my lectures, but now that Planck's lectures are appearing in your publishing house, I do not consider this urgent. The effort is much greater than you think.[7] (Sommerfeld 2000, vol. 2, 164)

Hence, Sommerfeld's six volumes of lectures on theoretical physics did not appear until the 1940s.

Like the other textbooks by Planck discussed above, his *Einführung in die theoretische Physik* emerged from his lectures at the University of Berlin. He had begun these lectures upon his appointment in the summer semester of 1889, and they were conceived as a five-semester course encompassing the classical areas of theoretical physics—from mechanics to optics—and providing a comprehensive introduction to the field. Interestingly, he modified the sequence of the lectures, closing them not with electricity and optics, which had been customary up to that time, but with the theory of heat. As he writes in his preface to the *Theorie der Wärme*, "[this] was imperative to the functional systematization to which I aspired. For while the theory of heat may build on mechanics and electrodynamics, this is not true the other way around"[8] (Planck 1930, v). This emphasizes, once again, the central role of thermodynamics in Planck's physical thinking. Incidentally, authors of later textbooks would follow Planck in this systematization (Sommerfeld 1943b; Joos 1932).

[6] "ergänzen sie sich in gewissem Sinne wechselseitig."

[7] Sommerfeld to Hirzel, 30 June 1924.

[8] "war [dies] ein zwingendes Gebot der erstrebten sachlichen Systematik. Denn wohl lässt sich die Theorie der Wärme auf der Mechanik und auf der Elektrodynamik aufbauen, nicht aber gilt das umgekehrte."

MAX PLANCK

EINFÜHRUNG IN DIE THEORETISCHE PHYSIK

Zum Gebrauch bei Vorträgen, sowie zum Selbstunterricht

Jeder Band mit einem Verzeichnis der Definitionen und der wichtigsten Sätze

1. BAND: **Einführung in die allgemeine Mechanik.** 4. Auflage. Mit 43 Figuren. VIII u. 226 Seiten. 8⁰. Brosch. RM. 6.—, Leinen RM. 8.—

INHALT: I. Teil. Mechanik eines materiellen Punktes: Bewegung auf einer Geraden. Bewegung im Raume. Zentralkräfte. Potential. Integration der Bewegungsgleichungen. Relative Bewegung. Vorgeschriebene Bedingungen. II. Teil. Mechanik eines Systems materieller Punkte: Statik eines starren Körpers. Statik eines beliebigen Punktsystems. Dynamik eines beliebigen Punktsystems. Dynamik eines starren Körpers.

2. BAND: **Einführung in die Mechanik deformierbarer Körper.** 2. Auflage. Mit 12 Figuren. VI u. 194 Seiten. 8⁰. Brosch. RM. 4.—, Leinen RM. 6.—

INHALT: I. Teil. Allgemeine Bewegungsgesetze eines stetig ausgedehnten Körpers: Kinematische Gesetze. Dynamische Gesetze. II. Teil. Unendlich kleine Deformationen: Feste Körper. Allgemeines. Gleichgewichtszustände fester Körper. Schwingungsvorgänge in festen Körpern. Schwingungsvorgänge in Flüssigkeiten und Gasen. III. Teil. Endliche Deformationen: Allgemeines. Wirbelfreie Bewegungen. Wirbelbewegungen. Reibung.

3. BAND: **Einführung in die Theorie der Elektrizität und des Magnetismus.** 2. Auflage. Mit 12 Figuren. VI u. 210 Seiten. 8⁰. Brosch. RM. 6.—, Leinen RM. 8.—

INHALT: I. Teil. Allgemeine Gleichung des elektromagnetischen Feldes in ruhenden Körpern: Elektrische und magnetische Feldstärke. Gesetze des elektromagnetischen Feldes. II. Teil. Statische und stationäre Zustände: Elektrostatisches Feld ohne Kontaktspannungen. Elektrostatisches Feld mit Kontaktspannungen. Magnetostatisches Feld. Ponderomotorische Wirkungen im statischen Felde. Stationäres elektromagnetisches Feld. Molekulare und ponderomotorische Wirkungen im stationären Felde. III. Teil. Quasistationäre und dynamische Vorgänge: Quasistationäre Vorgänge bei geschlossenen Strömen. Quasistationäre Vorgänge bei ungeschlossenen Strömen. Dynamische Vorgänge in ruhenden Körpern. Dynamische Vorgänge in bewegten Körpern. Grenzen der Maxwell-Hertzschen Elektrodynamik.

4. BAND: **Einführung in die theoretische Optik.** Mit 24 Figuren. VIII u. 184 Seiten. 8⁰. Brosch. RM. 6.—, Leinen RM. 8.—

INHALT: I. Teil. Optik isotroper homogener Körper: Reflexion und Brechung. Spektrale Zerlegung, Interferenz, Polarisation. Geometrische Optik. Beugung. II. Teil. Kristalloptik: Ebene Wellen. Wellenfläche. Senkrechte Inzidenz. Schiefe Inzidenz. III. Teil. Dispersion isotroper Körper: Grundgleichungen. Ebene Wellen. Geometrische Optik inhomogener Körper. Beziehungen zur Quantenmechanik.

5. BAND: **Einführung in die Theorie der Wärme.** Mit 7 Figuren. VIII u. 251 Seiten. 8⁰. Brosch. RM. 8.—, Leinen RM. 10.—

VERLAG VON S. HIRZEL IN LEIPZIG C1

Figure 3.2: Advertisement by the publishing house for Planck's textbook *Einführung in die theoretische Physik* (Planck 1932).

Planck's *Einführung in die theoretische Physik*, several editions of which were published in the interwar period, but which was never republished after World War II, is divided into the following volumes:

- Volume 1: *Einführung in die allgemeine Mechanik* (Introduction to general mechanics); 1st edition 1916; 2nd edition 1920; 3rd edition 1921; 4th edition 1928; 5th edition 1937
- Volume 2: *Einführung in die Mechanik deformierbarer Körper* (Introduction to the mechanics of deformable bodies); 1st edition 1919; 2nd edition 1922; 3rd edition 1931
- Volume 3: *Einführung in die Theorie der Elektrizität und des Magnetismus* (Introduction to the theory of electricity and magnetism); 1st edition 1922; 2nd edition 1928; 3rd edition 1937
- Volume 4: *Einführung in die theoretische Optik* (Introduction to theoretical optics); 1st edition 1927; 2nd edition 1931
- Volume 5: *Einführung in die Theorie der Wärme* (Introduction to the theory of heat); 1st edition 1930

But in addition to this classical textbook Planck published another work that fulfills the criteria for a textbook, and that appeared between the publication of his *Einführung in die Theorie der Wärme* (Planck 1932) and his *Einführung in die theoretische Physik* (Planck 1919). This is the *Eight Lectures on Theoretical Physics*, delivered at Columbia University in 1909, upon which the final section will focus.

3.4 Eight Lectures

In the year 1905 a German-American exchange program for professors was initiated by the influential Prussian ministerial official and science policy maker Friedrich Althoff (vom Brocke 1991, 185–242). The program was funded in large part by a foundation created by the Berlin banker and patron of the sciences Leopold Koppel specifically for this purpose. In 1906 the program was expanded to include a special exchange between the Friedrich-Wilhelms-Universität in Berlin and Columbia University in New York.[9] Max Planck was among the select circle of sponsored visiting professors. In spring 1908 he accepted an invitation from the President of Columbia University to come to New York the following spring to give guest lectures. Planck later reflected upon this invitation in a letter to his colleague Wilhelm Wien in Munich, who was to travel under the aegis of the same program in 1913:

> At the time I nudged myself into accepting the invitation, but I must say that I absolutely did not regret it; on the contrary, after the fact I am very glad that I acquiesced to the idea. I may not have learned much over there in terms of direct science, but I received many suggestions and motivations, and outside science this journey brought me more than any other I have ever made. My trip lasted from early April to late May. During my stay there I had to hold a lecture every Friday and Saturday for four consecutive weeks; apart from that I was free and could use the intervening periods for trips to Washington, Boston, Ithaca, Niagara Falls, etc. Of course it is not possible to give anything conclusive in the

[9] President to Hallock, 30 April 1908. Columbia University Archives NYC, Central Files. Hallock Papers.

> lectures, as the time is much too short for that, and the educational background of the listeners too difficult to control besides. Therefore the only thing one can do is make a suitable selection. But this, too, has its pleasant aspects. For of course, you must take for your lecture precisely those matters at which you are best, for which the least work is needed in preparation, and there is no harm done if the topics of the various lectures are only loosely or not at all connected. In any case it is advisable to complete your preparation as much as possible before departure, for over there one is from the very outset so completely occupied with visits, invitations, sightseeing, and field trips, that there is no longer any opportunity for true collection. Initially I was quite dazed by this hustle and bustle, but after a while I succumbed to a kind of couldn't-care-less attitude, and then everything went fine.[10]

As the overarching topic of his New York lectures Planck selected "The present system of theoretical physics"[11] (Planck 1915). He thereby picked up where his cycle of lectures on theoretical physics in Berlin had left off, since in the summer semester of 1894 this cycle had closed with a one-semester course about the "System der gesamten Physik" (System of the whole of physics). In his eight lectures in New York, Planck attempted "to depict the present state of the system of theoretical physics" and to convey to his audience those principles "which dominate today's physics, the most important hypotheses of which it avails itself, the great thoughts that have penetrated the field especially in recent years"[12] (Planck 1915, 2). Even if Planck did not attempt to impart in his lectures the state of theoretical physics per se, but rather to impart merely a kind of overview of the system of theoretical physics, these lectures can be regarded as a textbook. After all, Planck's purpose was to explain his view of contemporary physics and make its principles comprehensible to his American students and colleagues. The lectures, which were intended for publication from the outset,[13] were on the following topics:

1. *Einleitung. Reversibilität und Irreversibilität* (Introduction. Reversibility and Irreversibility)
2. *Thermodynamische Gleichgewichtszustände in verdünnten Lösungen* (Thermodynamic States of Equilibrium in Dilute Solutions)
3. *Atomistische Theorie der Materie* (Atomic Theory of Matter)
4. *Zustandsgleichung eines einatomigen Gases* (Equation of State for a Monatomic Gas)
5. *Wärmestrahlung. Elektrodynamische Theorie* (Heat Radiation. Electrodynamic Theory)
6. *Wärmestrahlung. Statistische Theorie* (Heat Radiation. Statistical Theory)
7. *Allgemeine Dynamik. Prinzip der kleinsten Wirkung* (General Dynamics. Principle of Least Action)
8. *Allgemeine Dynamik. Prinzip der Relativität* (General Dynamics. Principle of Relativity)

[10] Planck to Wien, Berlin 9 October 1912. Staatsbibliothek zu Berlin, Manuscript Department. Wien Papers.
[11] "Das gegenwärtige System der theoretischen Physik."
[12] "den gegenwärtigen Stand des Systems der theoretischen Physik zu schildern"; "welche die heutige Physik beherrschen, der wichtigsten Hypothesen, deren sie sich bedient, der großen Gedanken, welche gerade in neuerer Zeit in sie eingedrungen sind."
[13] They appeared in 1910, published by S. Hirzel in Leipzig; an American translation was published in 1915 by Columbia University Press, New York.

In his lectures, Planck develops a picture of physics that could fairly be designated, as opposed to the mechanical or the electrodynamic worldview, the thermodynamic worldview. This view is characterized by Planck's belief that the principles of thermodynamics are fundamental for a basic understanding of physics. In this vein, he differentiates all physical processes into reversible and irreversible. This division, along with a comprehensive discussion of the second law of thermodynamics, makes up the content of the first lecture. Planck illustrates the "rich fertility" of this approach in his second lecture, based on the laws of thermodynamic equilibrium. Thermodynamic equilibrium is:

> that the state of a physical configuration which is completely isolated, and in which the entropy of the system possesses an absolute maximum, is necessarily a state of stable equilibrium, since for it no further change is possible. (Planck 1915, 21)

For in nature entropy can only grow; it cannot decrease in closed physical systems. Planck demonstrates how strongly this finding shapes all physical and chemical processes using the example of the theory of dilute solutions, whose individual laws derive from this basic principle. Incidentally, his two introductory lectures adhere very closely to the systematization of his *Vorlesungen über Thermodynamik*. In his third lecture, he then links the concept of entropy with atomistic theory, and discusses at length the relationship Boltzmann established between entropy and probability states. Using this fundamental relationship, in the fourth lecture, he derives the laws applicable to gases in equilibrium—Maxwell's velocity distribution, and Boyle's, Gay-Lussac's and Avogadro's laws. In the fifth and sixth lectures, the problems of radiative equilibria are finally discussed, in the context of thermodynamic observations and with the help of statistical theory. In this way, the lectures highlight Planck's path to the quantum hypothesis and demonstrate the historical derivation of Planck's law. The seventh lecture is devoted to general dynamics or, alternatively, mechanics, with a focus on examining the principle of least action, which dominates "all of mechanics as well as all other physics"[14] (Planck 1915, 122). This is illustrated through applications to mechanics, electrodynamics, and thermodynamics. In the closing lecture, a hymn is sung to the revolutionary importance and far-reaching consequences of Einstein's theory of relativity; in a postcard to Max Laue he even speaks of having made, "propaganda here for the principle of relativity."[15] According to Planck,

> it surpasses in boldness everything previously suggested in speculative natural phenomena and even in the philosophical theories of knowledge: non-euclidian geometry is child's play in comparison […] The revolution introduced by this principle into the physical conceptions of the world is only to be compared in extent and depth with that brought about by the introduction of the Copernican system of the universe. (Planck 1915, 120)

[14]"die gesamte Mechanik ebenso wie die übrige Physik".

[15]Planck to Laue, Washington 28 April 1909. Archive of the Max Planck Society (AMPG), Dept. Va, Rep. 11, No. 566.

3.5 Conclusion

In historical perspective, it seems almost bizarre that Planck's own revolutionary achievement, the quantum hypothesis, received almost passing treatment in his lectures and anything but center stage. Four years later, Wilhelm Wien made up for this, when holding his lectures at Columbia University on the general topic of "Recent problems in theoretical physics"[16] (Wien 1913). In them, Wien not only payed tribute to Planck's merits as the pioneer of quantum theory, but also focused in particular on applications of the quantum hypothesis to the theory of specific heat, to X-rays, and to electron theory that had succeeded in the interim (Wien 1913).

In reviewing Planck's work as a textbook author, it is striking that, despite his manifold activities in this field, he did not write an independent text about quantum theory. This is also true, by the way, of his nearly forty years of lecturing, which included not a single special lecture about quantum theory, and from which all of his textbooks originated to one degree or another. Thus Planck dealt with the (older) quantum theory exclusively in the context of the theory of heat radiation, demonstrating once again his conservative attitude to the development of quantum theory based on his quantum hypothesis.

Abbreviations and Archives

Hallock Papers	Columbia University Archives, Central Files, William Hallock, New York City
Wien Papers	Staatsbibliothek zu Berlin, Stiftung Preußischer Kulturbesitz, Manuscript Department
AMPG	Archive of the Max Planck Society, Berlin

References

vom Brocke, Bernhard (1991). Internationale Wissenschaftsbeziehungen und die Anfänge einer deutschen auswärtigen Kulturpolitik: Der Professorenaustausch mit Nordamerika. In: *Wissenschaftsgeschichte und Wissenschaftspolitik im Industriezeitalter. Das "System Althoff" in historischer Perspektive*. Ed. Bernhard vom Brocke. Hildesheim: LAX. 185–242.

Ebeling, Werner and Dieter Hoffmann (2008). Max Planck und die Thermodynamik. In: *Max Planck. Über thermodynamische Gleichgewichte*. Ed. Werner Ebeling and Dieter Hoffmann. Frankfurt am Main: Harri Deutsch. vii–xxiv.

Hermann, Armin (1971). *The Genesis of Quantum Theory 1899–1913*. Cambridge, MA: The MIT Press. Original German edition: *Frühgeschichte der Quantentheorie*. Physik Verlag, Mosbach, 1969.

Joos, Georg (1932). *Lehrbuch der theoretischen Physik*. Leipzig: Geest & Portig.

Kuhn, Thomas S. (1970). *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press.

[16] "Vorlesungen über neuere Probleme der theoretischen Physik."

Planck, Max (1897). *Vorlesungen über Thermodynamik*. Leipzig: Veit.
— (1906). *Vorlesungen über die Theorie der Wärmestrahlung*. Leipzig: Johann Ambrosius Barth.
— (1910). *Acht Vorlesungen über theoretische Physik*. Leipzig: S. Hirzel.
— (1912). *Vorlesungen über die Theorie der Wärmestrahlung*. 2nd ed. Leipzig: Johann Ambrosius Barth.
— (1915). *Eight Lectures on Theoretical Physics*. New York: Columbia University Press.
— (1919). *Einführung in die theoretische Physik,* 5 vols. Leipzig: S. Hirzel. Published between 1919–1930.
— (1930). *Theorie der Wärme*. Leipzig: S. Hirzel.
— (1932). *Einführung in die Theorie der Wärme*. 2nd ed. Leipzig: S. Hirzel.
— (1949). *Scientific Autobiography and Other Papers*. New York: Philosophical Library.
Sommerfeld, Arnold (1943a). *Mechanik*. Leipzig: S. Hirzel.
— (1943b). *Vorlesungen über theoretische Physik*. Leipzig: S. Hirzel. 9 vols. published between 1943–1952.
— (2000). *Wissenschaftlicher Briefwechsel,* 2 vols. Berlin: GNT–Verlag.
Wien, Wilhelm (1913). *Vorlesungen über neuere Probleme der theoretischen Physik*. Leipzig und Berlin: B.G. Teubner.

Chapter 4
Dissolving the Boundaries between Research and Pedagogy: Otto Sackur's *Lehrbuch der Thermochemie und Thermodynamik*

Massimiliano Badino

4.1 Introduction

Today, the name Otto Sackur hardly appears in historical accounts of quantum physics. Sackur was born in Breslau, in the border region of Silesia in Germany (now Wrocław, Poland) on 28 September 1880.[1] At that time, the University of Breslau was an important center for research in experimental physics, especially spectroscopy and optics. The Chemistry Department, founded by Albert Ladenburg in 1897, was one of the best equipped in the German Reich. Sackur studied physical chemistry in Heidelberg (one semester), in Berlin, and in Breslau under Richard Abegg, who was also his doctoral supervisor. After the doctorate, which he received at a precociously young age on 31 July 1901, he became Abegg's assistant.

However, as was customary in Germany, he left his *alma mater* to enrich his scientific experience. For two years, from October 1902 to October 1904, he worked on the properties of alloys at the *Kaiserliches Gesundheitsamt* in Berlin. The research of this institute, headed by Theodor Paul, focused especially on possible health risks related to the lids of typical German beer mugs, which were made of alloys of lead, zinc, tin, and copper. Then, Sackur moved to London, with a letter of recommendation from Abegg, and worked at William Ramsey's laboratory at University College. In London (October 1904–March 1905), he met Otto Hahn, with whom he worked on radioactivity, particularly on the decay of radium. Sackur's *Wanderjahre* ended with another six-month research stint in Walther Nernst's laboratory in Berlin. After his return to Breslau, on 19 October 1905, he obtained his habilitation and became a *Privatdozent*.

Sackur seemed to be headed for a quiet academic career in Breslau, but these plans were suddenly disrupted by two unexpected events. In 1909, personal problems forced Albert Ladenburg to retire from his position as director of the department, and his successor, Eduard Buchner, was anything but a supporter of physical chemistry. Moreover, in 1910, Sackur's mentor Abegg died in a ballooning accident. All of a sudden, Sackur found himself without an academic guide and in a hostile research environment. His career prospectives appeared to be at a dead end, and during 1909–1913, he had to take on minor teaching assignments to survive. This grim situation changed at the end of 1913, when Fritz Haber invited Sackur to spend a research period at the newly founded *Kaiser-Wilhelm-Institut für Chemie und Elektrochemie*. It seems that Clara Immerwahr, Haber's first wife and herself a student of

[1] Biographical information on Sackur can be found in the entry written by Alexander Kipnis in the *Neue Deutsche Biographie* (Kipnis 2005) and in the obituaries written by personal friends and former colleagues of Sackur's at Breslau after his untimely demise in 1914 (Auerbach 1915; Hertz 1915; Pick 1915).

Abegg in Breslau, played a role in this important call.[2] The following year, Sackur became director of a department, and after the outbreak of the war, he was recruited to study the most effective way to fill the bullets of howitzers. Unfortunately, his career ended abruptly on 17 December 1914, when he was mortally wounded by an unexpected explosion in his own laboratory and died a few hours later.

His contemporaries credited Sackur with a rare gift for explaining the complexity of modern physical chemistry in simple terms. His teaching activities in Breslau included very unconventional courses, like a "Reading group on classics of physical chemistry" (Winter Semester 1906/07), a massive presence of thermodynamics and thermochemistry (Summer Semester 1907, SS 1909, WS 1910/11, WS 1911/12), kinetic theory (SS 1910, SS 1912), and some advanced classes on the "mathematical treatment of chemistry" (WS 1906/07, SS 1908). After Abegg's death, Sackur was forced to take on introductory courses on chemistry (SS 1911, WS 1912/13), a special course on chemistry for dentists (SS 1912, WS 1913/14), and he had to take care of the weekly colloquium on physical chemistry, created by Ladenburg and Abegg some years earlier.

Most probably, it was during this tough period after Abegg's death that Sackur's pedagogical skills were refined and reached the high level later acknowledged by his colleagues. His introductory courses were particularly appreciated, and Sackur developed them into a book (Sackur 1911a) that convinced the publisher Springer to assign a more ambitious project to the young *Privatdozent*, namely a textbook on thermodynamics and thermochemistry, which I will discuss in this paper. One moving recollection concerning Sackur's ability to write textbooks came from Fritz Haber, many years after Sackur's death, on the publication of the second edition of the *Lehrbuch*, edited by Clara von Simson, in 1928. In the foreword to the book, Haber pointed out that, although some of the content was outdated, the clarity of the work was still remarkable. He also added a personal evaluation on Sackur:

> He had in the highest measure those features that are necessary to the composition of a good textbook: a clear understanding of the fundamental concepts, a mastery of the subject matter, practicality and precision in judgement and, finally, lightness and simplicity in his presentation. (Haber 1928)

Apparently, Sackur was not only a born teacher, but also an innovative one. A summary of his courses at the university of Breslau (see the table at the end of the paper) reveals a wide range of interests and a careful balance between experimental and theoretical issues. His ability to master both laboratory techniques and subtleties of differential calculus was underscored by his contemporaries.

There are two lessons that the case of Sackur's textbook can teach us and that I spell out in this paper. First, Sackur's book shows us that, when studied from a historical perspective, a textbook is not only a record of established theories, but it may also reflect internal tensions of the general dynamics of knowledge. A textbook actively selects and organizes its material, a process that is never completely neutral. Research considerations might enter this process and lead to a fundamental reshaping of the pedagogical tradition. This does not happen in every case necessarily: in this paper we will encounter textbooks that separate the pedagogical tradition from up-to-date research. However, it does happen in Sackur's book, particularly in his treatment of specific heat.

[2] Administrative letters concerning Sackur's hiring at the Institute are now collected in Haber's correspondence stored at the Archive of the Max Planck Society, Berlin (AMPG).

Second, a textbook can become a functional vehicle for research, a way to disseminate new methods, concepts, and procedures. More importantly, it can contribute to the formation of a new generation of students able not only to master established techniques, but also to recognize new priorities and new avenues of research. Sackur's discussion of chemical equilibrium and his insistence on the importance of the concept of entropy achieve precisely this goal.

4.2 The Structure of the Book

Sackur's *Lehrbuch der Thermodynamik und Thermochemie* was published by the Berlin publisher Julius Springer in 1912. Apparently, the book was well received. Extant reviews point out its innovative character and its didactical clarity. Above all reviewers remarked, that the book constitutes a useful tool for the chemist eager to find his or her way through the jungle of new developments in physical chemistry (Coehn 1913; Krüger 1914). Although it is difficult to establish with certainty, it seems that the book essentially derived from Sackur's lectures at the university in Breslau. He had been lecturing consistently on thermodynamics and thermochemistry since the summer semester 1907 and especially focused on these topics in the winter semesters 1910/11 and 1911/12, immediately before the publication of the book. Furthermore, in the preface to the first edition, he underscored the clear pedagogical aim of the book: "in the selection and the organization of the material I have been guided especially by the didactical point of view and I have deliberately relinquished completeness" (Sackur 1912a, iii). The book was thus conceived for use in the classroom.

As for content, the book consists of 13 chapters and covers the most important topics of physical chemistry, including electrochemistry and capillarity. There is a section devoted to the theory of radiation and a very long and instructive chapter on Nernst's theorem.[3] The whole edifice of physical chemistry is presented as resting on two main pillars: the first and the second principles of thermodynamics. These two principles are deployed to elaborate the fundamental equations of chemical statics and chemical equilibrium, hence Sackur discusses at length their consequences. The great importance Sackur gives the principles of thermodynamics reveals the influence of Max Planck, who had been emphasizing the foundational role of thermodynamics for physical chemistry since the end of the nineteenth century (Hiebert 1983; Kormos Barkan 1999; cf. also Hoffmann's article in this volume). However, Planck only inspired parts of Sackur's book. For instance, Sackur did not follow Planck in the thorough use of the formalism of thermodynamic potentials. More importantly, contrary to Planck, Sackur leaned more strongly on the experimental side of physical chemistry, and in his book, Sackur reported recent data as well as descriptions of the most advanced experimental techniques. This attention to the empirical basis of the discipline hints at another important source of inspiration, namely Nernst's *Theoretische Chemie* (1909).

More generally, Sackur presented physical chemistry as a discipline in flux. He repeatedly stressed the limitations of classical doctrines as well as the shaky foundations of more recent approaches. Giving shape to feelings that Nernst, Planck, and Einstein voiced at the First Solvay Conference, only one year earlier (Mehra 1975; Marage and Walleborn 1999), he portrayed the theory of matter as a field experiencing deep change, where new,

[3]The book was translated into English by G. E. Gibson and published in 1917 by MacMillan (Sackur 1917). Curiously, the translation has an additional chapter.

interesting, and possibly revolutionary, things were about to happen. Ultimately, Sackur's book is permeated with a sense of transition.

4.3 The Reorganization of Knowledge: The Case of Specific Heats

These observations lead me to the first point of this paper: the impact of new research agendas on the organization of knowledge in a textbook. This creative process turns the textbook into a historiographical tool that allows us to understand a great deal about how ideas about relevant problems, acceptable solutions and, ultimately, the relative importance of different parts of a field of knowledge can change during a period of crisis. I illustrate this general point by considering the way Sackur coped with the issue of specific heats in his book.

The understanding of specific heat underwent a major theoretical change at the turn of the century. The nineteenth century experienced spectacular developments in kinetic models of matter. According to these models, the thermal properties of physical bodies can be traced to the mechanical behavior of the microscopical constituents of matter. In particular, conclusions regarding the specific heats of various substances could be drawn from the so-called equipartition theorem, which states that each degree of freedom, or more precisely, each quadratic term of the total energy, gets the same share of the total energy. Temperature variation is related to changes in the energy of the body, therefore to the "intensity" with which it carries out its motion, but it does not affect the kinds of motion it can perform. Thus, the amount of energy necessary to increase the temperature of the body, say, from 100° to 101° C, must be the same as that required to increase it from 0° to 1° C. In other words, the specific heat of each body must be independent of temperature, a conclusion that fit well with the Dulong-Petit phenomenological law of thermodynamics.

Kinetic models had been constructed especially for gases. The gaseous state was historically the first to be pictured as a collection of tiny particles in random motion, and this led to epoch-making progress in the understanding of the laws of thermodynamics. Extension of the kinetic approach to the solid and liquid states was not as successful, though. Bernhard Weinstein and Gustav Mie made some attempts in this direction around 1900, but it became clear pretty quickly that the solid state involved enormous formal complications and hence gave very little payoff (Weinstein 1901–1903; Mie 1903). In particular, the equipartition theorem was hopelessly at variance with the experimental outcomes of Heinrich F. Weber (1875), who had found a temperature dependence in the specific heats of many solid substances. In kinetic theory, there was no clear idea of how to explain the temperature dependence of the specific heat of a solid.

Many textbooks on thermodynamics and physical chemistry acknowledged this impasse in contemporary research by organizing the topic of specific heat according to a precise didactic scheme. This "kinetic scheme" guided the students' training first through a familiarization with the treatment of a gas as a set of particles in random motion. The application of mechanical arguments was carried out for monoatomic gases, and then more complex models for polyatomic gases were described. Then, strong cohesion forces were introduced to discuss the liquid state. Finally, the student was presented with a model of particles arranged in a lattice, vibrating around equilibrium positions, customarily used to represent a solid. In this schema, an idealized gas was presented as the paradigmatic case, the solved example, while liquids, and above all solids were handled as puzzles. Mastering the treatment of the gas was necessary first, before extending the approach to still-mysterious cases.

This scheme was generally adopted in classic books on thermodynamics used by physical chemists, such as Ludwig Boltzmann's *Vorlesungen über die Gastheorie* (1898) or Planck's *Vorlesungen über Thermodynamik* (1913), as well as in textbooks of physical chemistry that relied mostly on phenomenological thermodynamics, such as George Senter's (1912).

Though, at the beginning of the century, the emerging quantum theory offered an alternative to the kinetic scheme. Planck's theory of radiation, based on the hypothesis that energy depends on the frequency of an oscillation via a universal constant, was in stupendous accord with the experimental data, and its underlying model (electromagnetic resonators in an empty cavity) was intriguingly close to matter. Indeed, the picture of particles vibrating about fixed points of a solid lattice was a natural extension of Planck's original idea of resonators. In 1907, when Einstein applied the quantum hypothesis to a solid lattice, the resulting theory enjoyed marked success, especially because it explained the temperature dependence of specific heats. However, the quantum hypothesis had been conceived and applied primarily to periodic phenomena. It was apparently very difficult to reconcile this hypothesis with the picture of gas molecules in random motion. Thus, in the first decade after 1900, quantum theory turned the research agenda on the structure of matter upside down: while in classical kinetic theory the task had been the extension from an aperiodic to a periodic system, now the priority became the application of a formal procedure devised for periodic behavior to the gas. All of a sudden, the solid had become the paradigmatic case and the gas had become the puzzle.

Sackur's book was the first to change the pedagogical presentation of the topic of specific heat as a result of this radical change in the research agenda. He already introduced the problem of specific heat in the second chapter of his book, just after having defined the concept of temperature. He began with a summary of classical knowledge concerning the thermal behavior of solids, which basically consisted of decades of experimental observations condensed into the law of Dulong-Petit and into Kopp's rule. Both laws, Sackur insisted, have only a very limited range of validity. Next, he abruptly introduced Einstein's quantum theory of specific heats (Sackur 1912a, 26–28). According to Einstein's hypothesis, the atoms in the solid lattice can be interpreted as compounded of three oscillators that perform independent oscillations along each of the coordinate axes. If one assumes that the energy is distributed over the oscillators according to Planck's distribution law, it is easy to find an expression for the specific heat that tends to zero as the temperature decreases. This is a remarkable result, Sackur commented, because it happens to coincide with the predictions of the theorem that Nernst had put forward the previous year (in 1906). Clearly, Einstein's intuition was a step in the right direction, but Sackur was also quick to point out that it was far from being the last word on the subject: "even though Einstein's theory has been qualitatively confirmed by the experience, there can be no doubt that it only gives a rough sketch of the reality" (Sackur 1912a, 28).

Therefore, Sackur discussed techniques and results of the experiments carried out in 1910 and 1911 by Nernst, Arnold Eucken and others: these experiments gradually enlarged the empirical basis of the theory and offered clues about how it could be improved. One attempt in this direction was the Nernst-Lindemann formula, which had two characteristic frequencies that Sackur discussed in the context of these experiments. The Nernst-Lindemann formula was, however, still only a partial result because, as Sackur underscored, "it has not yet found a theoretical interpretation" (Sackur 1912a, 28). Finally, Sackur mentioned the important problem of determining the characteristic frequency, an issue that impacts on the optical and elastic properties of the solid. In particular, he touched on Frederick Lindemann's important argument concerning how to fix the frequency from the temperature of fusion (Lindemann 1910). Overall, Sackur's picture of the theory of the specific heat of a solid is excellently up-to-date and complete: the only missing piece is Peter Debye's theory which would appear at the end of 1912.[4]

The remainder of the chapter deals with specific heats in liquids, solutions, and gases. Here, the treatment becomes very classical and relies solely on thermodynamics and experiments. The kinetic approach to specific heats in gases is mentioned only later, when Sackur introduces the mechanical interpretation of the concept of entropy (Sackur 1912a, 130). In general, specific heat in gases receives much less attention.

4.4 The Quantum in Quarantine

Sackur's account of specific heat was clearly inspired by the results of quantum theory. He did not simply inform the reader that there was a new way to attack the topic: he outright reorganized the material according to the new research agenda. Quantum theory showed that there was a very natural way to handle the specific heat of a solid: Einstein's treatment, albeit incomplete, was the new paradigmatic solution. However, there was no quantum theory of a gas in 1912. The construction of such a theory was the front line of research—research to which Sackur himself was contributing. The apparently well-known gas had transformed into a *terra incognita*. The goal of his reorganization was evidently pedagogical: there was no point in getting the students acquainted with a surpassed scheme. They could contribute more effectively to the advancement of knowledge if they knew from the beginning what the new starting points and the new problems were.

Other textbooks on thermodynamics and physical chemistry had a far less open-minded attitude toward quantum theory. To be sure, almost none of the major textbooks published after 1910 ignored quantum theory altogether. However, at the same time, almost none made an effort to integrate quantum theory into their didactical structure. Instead, the most common strategy consisted of a clear-cut separation between the established, and still reliable, kinetic theory and the new quantum machinery. Usually, the quantum theory was confined to specific chapters, more often than not at the end of the book as a sort of appendix. As a consequence, the kinetic scheme in the organization of the topic of specific heat largely remained dominant: the widespread pedagogical strategy still aimed at consolidating the good old kinetic theory in the minds of students. The quantum theory persisted in a state of *quarantine*, segregated in less prominent places or, as we shall see, in separate volumes.

The fundamental justification for this pedagogical strategy was the "reasonable doubt" argument. After all, the quantum hypothesis was young and imported from radiation theory into the study of matter. By contrast, kinetic theory was more than half a century old, full of glory and not yet completely explored. But the reasonable doubt argument was not merely based on common sense considerations. Research also played a role. A paper published by the authoritative physical chemist Gilbert Lewis in collaboration with Elliot Adams in 1914 made this point clearly (G. N. Lewis and Adams 1914). In that paper, the authors chal-

[4]The foreword to the book was written in April 1912; Debye's paper appeared in November (Debye 1912). It is true that the Born-von Kármán paper was published in March (Born and von Kármán 1912), but it was probably too late to include the results of that paper in Sackur's book.

lenged head-on the necessity of adopting the quantum theory to explain new experimental phenomena connected with the thermal behavior of bodies:

> Now, if there were no other way of explaining the very important facts to which attention has been called by the quantum theory, it would be proper to make such assumptions and to modify the body of physical theory in so far as might be necessary to render it consistent with them. But we believe that no such necessity exists. (G. N. Lewis and Adams 1914, 331)

They went on to argue that the most impressive results of quantum theory, including the temperature dependence of specific heat, might be obtained by supplementing the classical equipartition theorem with a new hypothesis, called the *constraint hypothesis*. According to this hypothesis, the particles in real bodies must be ascribed a smaller value of the partitioned energy because of their mutual interactions. Apparently, the paper by Lewis and Adams was very influential, both in the United States and in Europe, in conveying the message that quantum theory was not yet established enough to justify a revolution in the pedagogical organization of the knowledge in physical chemistry. Lewis and Adams's suggestion was taken up by the vast majority of textbooks on thermodynamics and physical chemistry.

One telling example is a textbook written by Edward W. Washburn in 1915. In his book, Washburn superficially reviewed the quantum developments and Debye's theory. He even began the chapter on specific heats by stating that

> the systematic investigations of specific heats at low temperatures carried out in recent years largely by Nernst and his associates at the University of Berlin have resulted in an extensive modification of former theories concerning heat capacity. (Washburn 1915, 291–292)

However, Washburn maintained a reasonable doubt about this modification. Although Lewis and Adams's hypothesis was a "qualitative interpretation only," Washburn decided to set aside the quantum theory and to base his "presentation […] upon the older principle of equipartion of energy" supplemented by the constraint assumption. Consequently, he organized the topic of specific heat according to the kinetic scheme: he set out the theory of the monoatomic gas and then introduced further complications related to the rotation and vibration of gas particles. Liquids and solids were left in a foggy state. Ironically, in the foreword, Washburn ridiculed the nineteenth-century skepticism concerning kinetic models and took a realist's stance:

> [I]nstead of considering these [models] in a special chapter as interesting but unnecessary hypothetical explanations of observed facts, they are themselves in their most essential features treated as facts already established beyond the possibility of reasonable doubt, and together with thermodynamics, are made to serve as the framework of the development of the whole subject. (Washburn 1915, viii–ix)

Ultimately, Washburn, as well as other authors, used the same "quarantine strategy" against quantum theory that their predecessors had adopted against kinetic models.

But the most significant instance of the persistent quarantine of the quantum theory was William C. Lewis's *System of Physical Chemistry* (1921). The first edition of Lewis's book was printed in 1916, as one installment in a series of textbooks in physical chemistry edited by William Ramsay. In his review of the book, Frederick George Donnan, incidentally the other editor of the series, praised the work as the treatise that would interrupt the British dependence on German texts in physical chemistry. He even went a step further by stating that: "in its arrangement of matter, lucidity of style, and comprehensive unity of design it is destined to become the standard general treatise on the subject of physical chemistry for English-speaking students" (Donnan 1916). Donnan's opinion was by and large confirmed because, for example, in the second edition of his book (1921) Washburn refers the student to Lewis's textbook as the authoritative source for more advanced discussions.

However, the general structure of the work is not revolutionary. An anonymous reviewer of the second edition commented that "the more classical portions are presented to the student in much the same manner as in several of the older text-books (and, it might be added, lecture courses)" (Anonymous 1919, 162). In effect, Lewis consistently adopted the kinetic scheme in his book because, in his pedagogical organization, kinetic theory and thermodynamics are the foundations of the system:

> The scientific treatment of any set of phenomena consists in applying the minimum of general principles or theories which can afford a reasonable explanation of the behavior of matter under given conditions, and predict its behavior under new conditions. The principles referred to as far as physics and chemistry are concerned are the kinetic theory and thermodynamics. (W. C. Lewis 1921)

It is more interesting to see how the attitude toward quantum theory evolved through the three editions of the work. In the first edition, Lewis confined the quantum theory to the end of the second volume, but in the second edition (1919), he felt that "the role which the quantum theory now plays in physical and chemical research makes it imperative for the advanced student to be familiar to some extent" with it. Therefore, he expanded his treatise to three volumes and devoted the last one completely to the quantum. In the prefatory note to the additional volume, Lewis pointed out that, although the quantum theory was born as a theory of radiation, its importance for physical chemistry was related to its application to specific heat:

> [E]ven the success which has attended Planck's treatment of radiation problems would scarcely have sufficed to gain for his views that prominence which they now have, had it not been for the satisfactory explanation which his theory offers at the same time for the heat content of the substances and the variation of the heat content with temperature. (W. C. Lewis 1921)

After this opening statement, in the third volume, Lewis consistently presented the topic of specific heat according to the quantum scheme. First, he accounted for Debye's theory of solid as the best established case and then reported on the attempts at constructing a quantum theory of gas. This structure remained unchanged in the third edition of the book (1921). So, is Lewis's textbook a turning point in attitudes about the quantum? Not quite. The quarantine is still in force: Lewis clearly separated the kinetic approach from the quantum one and highlighted that quantum theory should be reserved for the advanced student only. Also, the reasonable doubt argument was still the main reason for this separation. In the same prefatory note, Lewis wrote:

> In the present volume […] the underlying ideas—especially those involved in the quantum theory—have not yet been fully accepted, at least in their present form. The position of the quantum theory is to a certain extent undefined. The physical significance of what is meant by a quantum of energy or, in stricter sense, the quantum of action is still vague. (W. C. Lewis 1921)

Note an important difference. Sackur had also cautioned his students against premature enthusiasm and highlighted the limitations of Einstein's theory. However, this did not prevent Sackur from grasping the radical innovation of the new approach, and he therefore endorsed the general reorganizaton of knowledge entailed by the quantum theory. Lewis's caveat, instead, undermines the quantum theory as a whole. Quantum theory is a relevant piece of physics, but it has a somewhat inferior pedagogical status. It is something about which the student must be informed, but not formed. By and large, this was the prevailing strategy among the authors of physical chemistry textbooks. The quantum theory did not affect the organization of the didactic material even for those topics, such as specific heat, in which its success was patent and its superiority over the classical theory blatant. Quantum theory remained quarantined in places to which only the most zealous student would have access.

4.5 Research in the Classroom

In contemporary reviews most commentators were struck by the fundamental role that entropy played in Sackur's book. The second lesson that we can draw from his textbook hinges precisely on this concept. Although a key notion in thermodynamics, entropy encountered many difficulties in being accepted by the community of physical chemists (Kragh and Weininger 1996). The reason is that entropy is a very abstract quantity and is usually difficult to measure experimentally. Therefore, physical chemists were more inclined to use notions, such as affinity (what we now call free energy) or maximum work, to express the laws of equilibrium (Hiebert 1982; Laidler 1985). The majority of textbooks on physical chemistry simply ignored entropy altogether (Weininger 1996).

To the contrary, Sackur discussed entropy in detail, not only from a thermodynamical point of view, but also from a statistical-mechanical one. In a section devoted to the "mechanical meaning of the second principle and the concept of entropy," Sackur explicitly followed the leads of Boltzmann and Planck in relating entropy to molecular disorder and the atomic hypothesis (Sackur 1912a, 125–134). He presented Boltzmann's work as the solution to the fundamental puzzle of the compatibility between mechanical reversibility and thermodynamical unidirectionality. The key to the solution, Sackur pointed out, is that it is very easy for a mechanical system to become more and more disordered, while it is very unlikely that it will spontaneously retrieve an ordered disposition. "One understands the difference between ordered and disordered motion," Sackur stated, "by comparing the motion of a regiment of soldiers with that of a swarm of mosquitos" (Sackur 1912a, 127).

Sackur dwelled on the notion of disorder and illustrated it by means of various analogies from the mixture of gases to the behavior of a die. The message he wanted to convey to the students was that Boltzmann's statistical version of entropy was a flexible tool whose scope was not limited to gas theory. In his *Wärmestrahlung*, Planck had shown that the "Boltzmann principle"[5] had a very general validity and followed from the basic properties of entropy and probability themselves (Planck 1906). By repeating the same argument, Sackur was going far beyond the limits of usual textbooks in physical chemistry. His purpose was to present thermodynamics as an area of knowledge intimately intertwined with kinetic theory, probability and, ultimately, quantum theory. The notion of entropy, stubbornly dismissed by physical chemists, was the keystone of this conceptual network. The Boltzmann principle not only provided an effective way to calculate the entropy of a monoatomic gas. Using Planck's work on the quantum theory of radiation, it allowed the assignation of an entropy even to oscillating particles. In the final part of his discussion, Sackur showed that combining Planck's entropy of radiation and Einstein's theory of solid bodies, one can ascribe an entropy to an Einsteinian solid and develop the concept of an "ideal solid body," analogous to the ideal gas. In effect, a large part of this section was taken from Sackur's first research paper on the quantum theory of matter (Sackur 1911b).

In the last part of the book, Sackur insisted on the importance of entropy to the issue of chemical equilibrium. As mentioned above, although there was a clear analogy between thermal and chemical equilibrium, entropy had never enjoyed much success in the community of physical chemists. A telling example is Nernst's *Theoretische Chemie* (1909). The textbook went through many editions and from the beginning (1893) was considered the main transmitter of the most advanced methods in physical chemistry (Eggert 1943). However, Nernst famously hated the concept of entropy, and he hardly mentioned it in the almost 900-page volume. Entropy was not part of the standard theoretical arsenal of a physical chemist at the time.

From this perspective, Sackur's stress on the interrelations between chemical, thermal, and radiation equilibrium is unprecedented. To be sure, Sackur's book was not the only one to make wide use of statistical entropy. During the same period James Riddick Partington, who also made a name for himself as a historian of chemistry, wrote a textbook that contained a long account of Boltzmann's approach to entropy (Partington 1913). In its precision and extensiveness, Partington's account is superior even to Sackur's. However, while Partington's is a fairly complete but rather dry report, Sackur strove to show the relevance of these abstract procedures to practical physical chemistry. The pedagogical strongpoint of his treatment was not confined to making the theoretical interrelations between kinetic theory, physical chemistry, and quantum theory apparent, it went so far as displaying how these interrelations could be turned into workable tools for the physical chemist in the lab.

Sackur discussed the topic of chemical equilibrium in chapters 8 and 13. A chemical reaction is in equilibrium when the transformation of the reagents into the products and the reverse occur at the same rates. The fundamental equations of chemical equilibrium had been established by Jacobus H. Van't Hoff on the basis of general thermodynamics (Van't Hoff 1884). However, his solution was still incomplete, in that the thermodynamic equations could be solved only up to an integration constant. This meant that, to apply these equations to practical problems, one had to know the empirical values of the quantities involved at one temperature in order to calculate the corresponding values at another temperature. Hope for the general solution of the problem of chemical equilibrium lay in the numerical correlation between the various integration constants. In particular, the equilibrium constant was related

[5]The phrase "Boltzmann principle" usually means the proportionality between the entropy of a state and the probability of that state. Boltzmann expresses this equation for a very specific case. It was Planck who, in about 1900, generalized it to the modern form.

to the chemical constant and to the integration constant of the thermodynamic entropy.[6] These numerical relations were never mentioned in physical chemistry textbooks because the entropy constant was unimportant in thermodynamics. To the contrary, Sackur perceived in these relations a way to illuminate the issue of chemical equilibrium:

> Knowing the values of the entropy constants [...], and also the specific heats and their temperature coefficients [...] for all gases, we should be able to calculate the equilibrium constant [...] from the heat of the reaction for all gas reactions at all temperatures. [...] The two laws of thermodynamics alone, however, do not enable us to express the entropy constants [...] in terms of the experimental data. This has only recently been made possible by the discovery of Nernst's heat theorem. (Sackur 1912a, 235)

In the last chapter of the book, Sackur connected this discussion of chemical equilibrium with Nernst's heat theorem and the entropy concept. Sackur explained that thermodynamics allows for the calculation of the state of a system at one temperature if the state at another temperature has been determined. With such a limitation, the problem of chemical equilibrium cannot be solved once and for all because "on the basis of the two principles [of thermodynamics] it is impossible to calculate the affinity or the chemical equilibrium from [...] thermal quantities only; it is always necessary to know the value of affinity at a certain temperature" (Sackur 1912a, 305). Nernst's heat theorem fills this gap using a specific hypothesis about the behavior of affinity and internal energy near absolute zero. In 1906 Nernst had supposed that affinity and internal energy tend to the same value as the temperature decreases (Nernst 1906). This allows for the determination of the integration constant of the so-called Gibbs-Helmholtz equation and the calculation of the affinity for each temperature.

Very soon it was understood that there is a strong connection between Nernst's heat theorem and the quantum hypothesis. As we have seen above, Einstein's theory of specific heats in solids fit surprisingly well with the theorem. Sackur returned to these connections in the last section of his book. In addition, he pointed out the importance of the Boltzmann principle, as a link between old thermodynamics and new developments. Boltzmann's idea of tracing the calculation of entropy back to the calculation of the number of ways of arranging molecules into suitable energy cells provided a representation of the behavior of substances at very low temperatures without introducing kinetic hypotheses. Ultimately, it suggested a possible strategy to derive Nernst's heat theorem:

> This derivation must obviously come out of the features that we ascribe to the absolute zero of temperature. If we now put aside the kinetic theory of heat, then we can characterize the absolute zero as that state in which a body has no heat energy whatever. (Sackur 1912a, 330)

According to the Boltzmann principle, this statement means that there is only one possible allocation of the molecules and the entropy is zero. It is exactly this insight that Sackur would use in his attempt to derive Nernst's heat theorem (Sackur 1911b). More importantly,

[6] The equilibrium constant is the ratio between the rates of the two opposite directions of a chemical reaction and fixes the equilibrium concentrations of the reactants. The chemical constant was introduced by Nernst, and it is formally the integration constant of the Clausius-Clapeyron equation.

the last chapter of the book also anticipates Sackur's crucial work on the chemical constant. The Boltzmann principle and the quantum theory can, together, provide a theoretical expression of the entropy constant and, as a result, a solution to the problem of chemical equilibrium. How all this can be actually done, Sackur would investigate in his research papers (Sackur 1911c; 1912b; 1912c). In the last chapter, he confined himself to outlining the problem and the tools for its solution.

Thus, Sackur was bringing the tensions of advanced research in physical chemistry into the classroom and directly to his students. The foremost problem was the application of quantum physics to gas reactions and chemical equilibrium, a problem that involved a complex conceptual cluster of classical thermodynamics, Nernst's heat theorem, and the new quantum hypothesis. It was precisely this state of intellectual turmoil that Sackur wanted to convey.

4.6 A Pedagogy for Quantum Physics

Sackur's book certainly has a special position in the context of textbooks on thermodynamics and physical chemistry published circa 1912. As our cursory survey of other important textbooks has shown, cutting-edge research, and especially quantum physics, did not easily find a place in the training of students. One might suggest that this was due to the low level of formal sophistication in physical chemistry books: perhaps Sackur was proposing formal methods and procedures that were too difficult for other authors. This explanation only captures a portion of the truth. It is generally correct that quantum physics was not a subject in books that deployed very little mathematics in their approach to physical chemistry. And it is also true that many authors eschewed high mathematics because it was considered unnecessary. In his textbook, S. Lawrence Bigelow efficaciously summarized this attitude:

> An unfortunate impression has got abroad that much mathematics is needed for a comprehension of physical chemistry; unfortunate, as it deters many who want it, and would profit by it, from electing the subject. No attempt has been made to avoid the use of mathematics, but a perusal for this book will show that ordinary arithmetic and elementary algebra are sufficient, except in five or six demonstrations. One unfamiliar with the calculus must take it on faith that steps in the derivations of half a dozen formulae are correct, and that is all. (Bigelow 1912, iii)

Unsurprisingly, Bigelow organized physical chemistry according to the kinetic scheme, without mentioning quantum theory. But the level of the mathematics involved is not a decisive discriminant. Washburn's didactical perspective was, in this respect, almost literally the reverse of Bigelow's position:

> The author is aware that in many elementary textbooks of Physical Chemistry it is customary to avoid the use of the calculus as far as possible, frequently even with the sacrifice of accuracy and at the risk of conveying erroneous impressions concerning some of the most fundamental relationships; and in those cases where the use of the calculus seems to be unavoidable some authors have felt it incumbent upon themselves to assume a somewhat apologetic attitude and to explain that the student must take on faith "these few derivations" but that he

> should not allow this fact to worry him, since with the aid of the accompanying explanations and illustrations he will still be able to understand the relationships and to apply them, even though he is not in a position to appreciate clearly what is involved in their derivation. With this dilettant attitude the writer finds himself entirely out of sympathy. (Washburn 1915, v)

Yet, we have seen that Washburn did not abandon the kinetic scheme. Similarly, Lewis's book was even more advanced than Washburn's, though it carefully separated classical and quantum physics.

Another explanation of the exceptionality of Sackur's book might be based on local considerations. After all, Sackur was working in Germany, the homeland of quantum theory. Furthermore, he was educated in Breslau, one of the centers of German physics, and he was in contact with Planck and Nernst. It was only natural to absorb, so to say, the quantum theory from this environment and to imbue his pedagogical activity with it. However, this local argument looses much of its strength if we compare the case of Sackur with that of other German textbooks used by physical chemists at about the same time.

For instance, nobody could doubt Planck's engagement both in quantum theory and in thermodynamics. At the end of the nineteenth century, Planck wrote important papers on the physical chemistry of dilute solutions, and at the beginning of the twentieth, he initiated the whole quantum business. However, his celebrated book on thermodynamics (Planck 1913) does not give the student the feeling that quantum theory has anything to do with physical chemistry. Planck's book is organized around the two principles of thermodynamics and their application to chemical problems. In this setup, gases have a privileged role on the grounds of van't Hoff's famous analogy between the ideal gas and dilute solutions. Only in the third edition, published in 1911, did Planck add a chapter on Nernst's heat theorem, and only in the fifth, in 1917, did he refer briefly to Debye's theory of the specific heat of solids.

The situation improves slightly looking at Nernst's *Theoretische Chemie*, the first and most respected textbook on physical chemistry. The sixth edition, published in 1909, contains a summary in a few lines of Einstein's theory of specific heat, without any special emphasis on the quantum (Nernst 1909, 700). One has to wait until the seventh edition, in 1913, to find an autonomous chapter entitled "Molecular Theory of the Solid State of Aggregation." The chapter is largely the summary of two research papers Nernst published in 1911 (Nernst 1911a; 1911b). However, the results are basically the same as those presented in Sackur's book, and the level of formal elaboration of the arguments is even lower. Thus, even if deeply interested in the application of the quantum to the theory of matter and to chemistry, two such scientific leaders as Planck and Nernst were reluctant to change the organization of their textbooks because of the new approach.

Albeit special, the position of Sackur's book was not unique. In 1913, less than one year after Sackur, Karl Jellinek published an impressive book (more than 800 pages) which is, to the best of my knowledge, the first textbook in physical chemistry to rely explicitly and consistently on quantum theory (Jellinek 1913). Jellinek's book is divided into two parts: statics and kinetics of gaseous reactions. However, the gaseous state is by no means the only subject of the book. There is a very long chapter on radiation theory—a topic "that is becoming more and more important for physical chemistry" (Jellinek 1913, 194)—and equally long sections on the statistical concept of entropy and on Nernst's heat theorem.

The chapter on specific heats, which is completely devoted to the quantum version of the problem, is particularly interesting. Like Sackur, Jellinek began with the theory of solids which he analyzed both theoretically and experimentally. Then, he continued with the specific heats in gases and mentioned some recent papers on the quantization of the rotation of diatomic molecules. Jellinek's treatment of specific heat is exactly what a quantum account of the topic should be: solid state is the central paradigm. Interestingly, Sackur also contributed to this textbook. In the preface, Jellinek thanked him for a revision of the manuscript before its publication.

Thus, trying to represent Sackur's or Jellinek's books as the result of an idiosyncratic leaning toward abstract concepts or as local phenomena is missing an important historiographical point: it was around 1912 that the necessity gradually emerged to formulate a didactic platform for quantum physics by reorganizing the classical pedagogy. This process, which was eventually taken up by first-rank physicists like Arnold Sommerfeld and Fritz Reiche (see the articles by Michael Eckert and Clayton Gearhart in this book), was initiated by lesser actors who grasped the importance of the new research and tried to implement this research in their teaching activities.

4.7 Conclusion

One might be tempted to read into the story of Sackur's textbook a fundamental contrast between innovative and conservative pedagogy. But again, I think that this reading would conceal a more intriguing point. The two lessons I spell out in the previous sections point more decisively to the interactions between research and pedagogy as a manifestation of the more general dynamics of knowledge. I have used a Kuhnian terminology to illustrate one important novelty in Sackur's book: gas changed its status from paradigm to puzzle, while the solid underwent the opposite change. Though, it is important to stress that this was not a Gestalt switch. On the contrary, it grew out of the steady effort of negotiating the conceptual space of the new quantum theory within classical physics.

Thus, the construction of a new platform for the didactics of quantum physics that Sackur attempted in his book did not occur in a conceptual vacuum: it was developed as *a reorganization of the established platform ideated over time for classical physics*. The determining insights for this reorganization came from research in that field. The platform was ultimately conceived as a translation of new research priorities into new pedagogical priorities; these priorities modified the criteria for the selection and disposition of the material, as the example of specific heat illustrates. These contextual aspects and its fortunate temporal positioning make Sackur's book not just the receptacle of a dead doctrine, but rather a historiographical tool for understanding the transformations brought about by quantum theory.

SS 1906	Messung chemischer Affinitäten, 1 Radioaktivität mit Experimenten, 1
WS 1906/07	Einführung in die mathematische Behandlung der Chemie, 2 Lektüre klassischer Arbeiten der physikalischen Chemie, 1 g
SS 1907	Thermochemie und Thermodynamik, 2 Radioaktivität mit Experimenten, 1 Physikalisch-chemisches Praktikum (Abegg's course), 3
WS 1907/08	Physikalische Chemie technischer Prozesse, 2. Physikalisch-chemische Rechenübungen (with Abegg), 1 g Physikalisch-chemisches Praktikum (Abegg's course), 3
SS 1908	Physikalisch-chemisches Praktikum (Abegg's course), 3 Ausgewählte Kapitel der technischen Elektrochemie, 1 Einführung in die mathematische Behandlung der Chemie, 2
WS 1908/09	Physikalisch-chemisches Praktikum (Abegg's course), 3 Radioaktivität, 1 Ausgewählte Kapitel der technischen Elektrochemie, 1
SS 1909	Physikalisch-chemisches Praktikum (Abegg's course), 3 Übungen zur Thermodynamik (Abegg's course), 1 g Thermochemie und Thermodynamik, 2 Die physikalischen und chemischen Eigenschaften der Metalle und Amalgame, 2
WS 1909/10	Physikalisch-chemisches Praktikum (Abegg's course), 3 Anorganisch-chemische Technologie, 3
SS 1910	Radioaktivität mit Experimenten, 1 Kinetische Theorie der Gase und Flüssigkeiten, 2 Physikalisch-chemisches Praktikum, 3
WS 1910/11	Radioaktivität mit Experimenten, 1 Ausgewählte Kapitel der Thermochemie und Elektrochemie, 2 Physikalisch-chemisches Kolloquium (with Meyer), 1 g Physikalisch-chemisches Praktikum, 3
SS 1911	Physikalisch-chemisches Kolloquium (with Meyer), 1 g Die Beziehung zwischen chemischer Konstitution und physikalischer Eigenschaften, 2 Einführung in die Chemie, 2 Physikalisch-chemisches Praktikum, 3
WS 1911/12	Physikalisch-chemisches Kolloquium (with Meyer), 1 g Physikalisch-chemisches Praktikum, 3 Physikalische Chemie II: Elektrochemie, Thermochemie, Photochemie, 2
SS 1912	Chemische Referate für Vorgeschrittenere (Biltz's course; with von Braun, Meyer) biweekly, 2 g Kinetische und thermodynamische Theorie der Gase und Flüssigkeiten, 2 Einführung in die Chemie für Zahnärzte, 3 Kleines physikalisch-chemisches Praktikum, 3

WS 1912/13	Chemische Referate für Vorgeschrittenere (Biltz's course; with von Braun, Meyer, Koenig and Arndt), biweekly, 2 g Physikalisch-chemisches Kolloquium (with Meyer), 1 g Einführung in die Chemie, 3 Physikalische Chemie II: Elektrochemie und Photochemie, 2 Kleines physikalisch-chemisches Praktikum, 3
SS 1913	Chemische Referate für Vorgeschrittenere (Biltz's course; with von Braun, Meyer, Koenig and Arndt), biweekly, 2 g Radioaktivität mit Experimenten, 1 Bestimmung von Molekulargewicht und Konstitution nach physikalisch-chemischen Methoden, 1 Praktische Übungen, 3
WS 1913/14	Chemische Referate für Vorgeschrittenere (Biltz's course; with von Braun, Meyer, Koenig and Arndt), biweekly, 2 g Einführung in die Chemie für Zahnärzte, 3 Radioaktivität mit Experimenten, 1 Bestimmung von Molekulargewicht und Konstitution nach physikalisch-chemischen Methoden, 1 Praktische Übungen, 3 Physikalisch-chemisches Kolloquium (with Meyer), 1 g

Table 4.1: Sackur's courses at the University of Breslau (the numbers are the weekly hours, "g" means that the lecture was *gratis*, free of charge).

Abbreviations and Archives

AHQP	Archive for History of Quantum Physics. American Philosophical Society, Philadelphia
AMPG	Archive of the Max Planck Society, Berlin

References

Anonymous (1919). Review of the Second Edition of "A System of Physical Chemistry". *Nature* 103(2583): 161–162.

Auerbach, Felix (1915). Otto Sackur - Nekrologe. *Jahresbericht der Schlesischen Gesellschaft für Vaterländische Kultur* 1: 35–37.

Badino, Massimiliano (2009). Das Verfolgen einer Idee: Plancks Theorie idealer Gase. In: *Max Planck und die moderne Physik*. Ed. Dieter Hoffmann. Berlin: Springer. 135–148.

Bartel, Hans-Georg and Rudolf P. Huebener (2007). *Walther Nernst Pioneer of Physics and of Chemistry*. Singapore: World Scientific.

Bigelow, Samuel L. (1912). *Theoretical and Physical Chemistry*. New York: Century.

Boltzmann, Ludwig (1898). *Vorlesungen über Gastheorie*. Leipzig: Johann Ambrosius Barth.

Bonhoeffer, Karl-Friedrich (1953). Fritz Habers wissenschaftliches Werk. *Zeitschrift für Elektrochemie* 57: 2–6.
Born, Max and Theodore von Kármán (1912). Über Schwingungen in Raumgittern. *Physikalische Zeitschrift* 13(8): 297–309.
Castagnetti, Giuseppe and Hubert Goenner (2004). *Einstein and the Kaiser Wilhelm Institute for Physics (1917-1922): Institutional Aims and Scientific Results*. Berlin: Max-Planck-Institut für Wissenschaftsgeschichte. Preprint 261.
Coehn, Alfred (1913). Review of Sackur's Lehrbuch. *Die Naturwissenschaften* 42: 1017–1018.
Debye, Peter (1912). Zur Theorie der spezifischen Wärme. *Annalen der Physik* 39: 789–839.
Donnan, Frederick G. (1916). Review of "A System of Physical Chemistry". *Nature* 98(2446): 25–26.
Eggert, John (1943). Walther Nernsts Lehrbuch. *Die Naturwissenschaften* 31: 412–415.
Eucken, Arnold (1914). Die Entwicklung der Quantentheorie vom Herbst 1911 bis Sommer 1913. In: *Die Theorie der Strahlung und der Quanten. Verhandlungen auf einer von E. Solvay einberufenen Zusammenkunft (30. Oktober bis 3. November 1911)*. Ed. Arnold Eucken. Berlin: Verlag Chemie. 371–405.
Gearhart, Clayton A. (1996). Specific Heats and the Equipartition Law in Introductory Textbooks. *American Journal of Physics* 64(8): 995–1000.
— (2010). "Astonishing Successes" and "Bitter Disappointment": The Specific Heat of Hydrogen in Quantum Theory. *Archive for History of Exact Sciences* 64(2): 113–202.
Haber, Fritz (1905). *Thermodynamik technischer Gasreaktionen*. München: Oldenbourg.
Hertz, Walter (1915). Otto Sackur. *Physikalische Zeitschrift* 16(7): 113–115.
Hiebert, Edwin N. (1982). Developments in Physical Chemistry at the Turn of the Century. In: *Science Technology and Society in the Time of Alfred Nobel*. Ed. C. Gustav Bernhard, Elisabeth Crawford, and Per Sörbom. Oxford: Pergamon. 97–115.
— (1983). Walther Nernst and the Application of Physics to Chemistry. In: *Springs of Scientific Creativity*. Ed. Rutherford Aris, H. Ted Davis, and Roger H. Stuwer. Minneapolis: University of Minnesota Press. 203–231.
Holl, Frank (1996). *Produktion und Distribution wissenschaftlicher Literatur. Der Physiker Max Born und sein Verleger Ferdinand Springer 1913–1970*. Frankfurt am Main: Buchhändler Vereinigung.
Jellinek, Karl (1913). *Physikalische Chemie der homogenen und heterogenen Gasreaktionen, unter besonderer Berücksichtigung der Strahlungs- und Quantenlehre sowie des Nernstschen Theorems*. Leipzig: S. Hirzel.
Jones, Harry C. (1910). *The Elements of Physical Chemistry*. 4th ed. New York: MacMillan and Co.
Kipnis, Alexander (2005). Sackur, Otto. In: *Neue Deutsche Biographie*. 22. Berlin: Dunker und Humblot. 344.
Kirsten, Christa and Hans-Jürgen Treder, eds. (1979). *Albert Einstein in Berlin 1913-1933*. Berlin: Akademie-Verlag.
Klein, Felix (1907). Mathematik und Naturwissenschaft. In: *Universität und Schule*. Ed. Alois Brandl, Adolf Harnack, Felix Klein, and Paul Wendland. Leipzig: B.G. Teubner. 3–9.
Kormos Barkan, Diana (1999). *Walther Nernst and the Transition to Modern Physical Science*. Cambridge: Cambridge University Press.

Kragh, Helge and Steven J. Weininger (1996). Sooner Silence than Confusion: The Tortuous Entry of Entropy into Chemistry. *Historical Studies in the Physical Science* 27(1): 91–130.
Krüger, Friedrich (1914). Review of Sackur's Lehrbuch. *Physikalische Zeitschrift* 15: 679–680.
Laidler, Keith J. (1985). Chemical Kinetics and the Origins of Physical Chemistry. *Archive for History of Exact Sciences* 32: 43–75.
Lewis, Gilbert N. and Elliot Q. Adams (1914). The Distribution of Thermal Energy. *Physical Review* 4: 331–344.
Lewis, William C. (1921). *A System of Physical Chemistry*. 3rd ed. London: Longmans Green and Company.
Lindemann, Frederick A. (1910). Über die Berechnung molekularer Eigenfrequenzen. *Physikalische Zeitschrift* 11(14): 609–612.
Macdougall, Frank H. (1921). *Thermodynamics and Chemistry*. New York: Wiley.
Marage, Pierre and Gregoire Walleborn, eds. (1999). *The Solvay Councils and the Birth of Modern Physics*. Basel: Birkhäuser.
Mehra, Jagdish (1975). *The Solvay Conferences on Physics: Aspects of the Development of Physics since 1911*. Dordrecht: Reidel.
Mewes, Rudolf (1910). *Theorie und Praxis der Großgasindustrie*. Leipzig: Degener.
Mie, Gustav (1903). Zur kinetischen Theorie der einatomigen Körper. *Annalen der Physik* 11: 657–697.
Nernst, Walther (1906). Über die Berechnung chemischer Gleichgewichte aus thermischen Messungen. *Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-Physikalische Klasse* 1: 1–39.
— (1909). *Theoretische Chemie vom Standpunkte der Avogadroschen Regel und der Thermodynamik*. 6th ed. Stuttgart: Enke.
— (1911a). Zur Theorie der spezifischen Wärme und über die Anwendung der Lehre von den Energiequanten auf physikalisch-chemische Fragen überhaupt. *Zeitschrift für Elektrochemie* 17: 265–275.
— (1911b). Der Energieinhalt fester Stoffe. *Annalen der Physik* 36: 395–439.
— (1913). *Theoretische Chemie vom Standpunkte der Avogadroschen Regel und der Thermodynamik*. 7th ed. Stuttgart: Enke.
Partington, James R. (1913). *A Textbook of Thermodynamics*. London: Constable.
Perkins, Henry A. (1912). *An Introduction to General Thermodynamics: An Elementary Treatise on the Fundamental Principles and Their Simpler Applications*. New York: Wiley.
Pick, Hans (1915). Otto Sackur. *Chemiker Zeitung* 39(3): 13.
Planck, Max (1906). *Vorlesungen über die Theorie der Wärmestrahlung*. 1st ed. Leipzig: Johann Ambrosius Barth. 2nd edition: 1913.
— (1913). *Vorlesungen über Thermodynamik*. 4th ed. Leipzig: Veit.
— (1914). Die gegenwärtige Bedeutung der Quantenhypothese für die kinetische Gastheorie. In: *Vorträge über die kinetische Theorie der Materie und der Elektrizität*. Ed. David Hilbert. Göttingen: B.G. Teubner.
Reiche, Fritz (1921). *Die Quantentheorie*. Berlin: Julius Springer.
Sackur, Otto (1911a). *Einführung in die Chemie*. Berlin: Julius Springer.

— (1911b). Zur kinetischen Begründung des Nernstschen Wärmetheorems. *Annalen der Physik* 34: 455–468.
— (1911c). Die Anwendung der kinetischen Theorie der Gase auf chemische Probleme. *Annalen der Physik* 36: 958–980.
— (1912a). *Lehrbuch der Thermochemie und Thermodynamik*. Berlin: Julius Springer.
— (1912b). Die Bedeutung des elementaren Wirkungsquantums für die Gastheorie und die Berechnung der chemischen Konstanten. In: *Nernst Festschrift*. Halle: Knapp. 405–423.
— (1912c). Die universelle Bedeutung des sogenannten elementaren Wirkungsquantums. *Annalen der Physik* 40: 67–86.
— (1917). *A Textbook of Thermo-chemistry and Thermodynamics*. London: MacMillan and Co.
Senter, George (1912). *Outlines of Physical Chemistry*. 3rd ed. London: Methuen.
Haber, Fritz (1928). Foreword. In: *Otto Sackur's Lehrbuch der Thermochemie und Thermodynamik*. Ed. Clara von Simson. Berlin: Julius Springer.
Van't Hoff, Jacobus H. (1884). *Études de dynamique chimique*. Amsterdam: Muller.
Washburn, Edward W. (1915). *An Introduction to the Principles of Physical Chemistry from the Standpoint of Modern Atomistics and Thermodynamics: A Course of Instruction for Students Intending to Enter Physics or Chemistry as a Profession*. New York: McGraw-Hill.
Weber, Heinrich F. (1875). Die specifischen Wärmen der Elemente Kohlenstoff, Bor und Silicium. *Annalen der Physik* 154: 553–582.
Weininger, Steven J. (1996). The Curious Incident of the Dog in the Night-Time or What Lessons Lurk in Old Textbooks. *The Chemical Educator* 1(4): 1–12.
Weinstein, Bruno (1901–1903). *Thermodynamik und Kinetik der Körper*. Braunschweig: Friedrich Vieweg und Sohn.

Chapter 5
Fritz Reiche's 1921 Quantum Theory Textbook

Clayton A. Gearhart

5.1 Introduction

Who reads textbooks? Students, of course. But also their professors and other professionals, including both specialists, who need to keep up with the latest developments, and others who want to maintain a comprehensive picture of their discipline. Not least, textbooks can be a treasure trove for historians: Textbooks give us a snapshot of the state of a discipline in a particular time and place, and from a particular point of view.[1]

Here I will examine Fritz Reiche's 1921 quantum theory textbook (Reiche 1921a).[2] At its publication, barely 20 years had passed since Max Planck had introduced his finite "energy elements" to explain black-body radiation and, in the process, inaugurated quantum theory. Yet, as Reiche's book and its antecedents show, quantum theory had been reaching out beyond the realm of specialists for nearly a decade, both to students and to physicists and physical chemists who wanted to know something of this new theory.

Fritz Reiche was born in 1883 and earned his Ph.D. in 1907, as one of Planck's comparatively small number of research students.[3] He spent the years from 1908 to 1911 in Breslau, working with Otto Lummer in a vain attempt to gain proficiency in experimental physics. There he met Max Born, who was in Breslau for the same reason. Judging from their later recollections, the two vied in producing spectacular floods, explosions, and other catastrophes of the experimental life, leading one to conclude that Lummer was not only an eminent experimentalist, but an eminently patient one. Born recalled learning a great deal about relativity and quantum theory from Reiche (Born 1978, 124).[4]

In 1913, Reiche was back in Berlin and, with Planck's support, qualified as a *Privatdozent* (instructor) at the University of Berlin. He was an assistant to Planck from 1915

[1] The word "textbook" encompasses a multitude of sins. Here I am including under that rubric books written for a professional audience. Since quantum theory was an advanced topic early in the twentieth century, the distinction between books intended specifically for students, and ones aimed at a more general professional audience, was at best hazy.

[2] Remarkably, as of this writing (September 2010), Reiche's book is still in print, in both the German original and the English translation. An electronic copy of the German edition may be found on Google books. The quotations used here draw on the English translation, but often revise it. Other translations in this chapter are my own.

[3] For more comprehensive biographical sketches of Reiche, see (Bederson 2005; Wehefritz 2002). The latter includes a bibliography of Reiche's publications.

[4] See also Thomas S. Kuhn and George E. Uhlenbeck, interview with Fritz Reiche, March and April 1962, Archive for History of Quantum Physics (AHQP). See esp. session 2, pp. 1–2. This episode reminds us that in spite of exceptions like Planck and Einstein, it was still uncommon for physicists to restrict themselves exclusively to theory. It was therefore sensible for both Reiche and Born to seek some background in experimental work. Nevertheless, as their careers demonstrate, their lack of an experimental background was not an insuperable obstacle. See, for example, (Jungnickel and McCormmach 1986).

to 1918, where his duties included correcting students' solutions to the problems Planck assigned, and answering their questions.[5] From 1919 to 1920, he was an advisor to Fritz Haber's Physical Chemistry Institute in Berlin, where, as he later recalled, he became known as the "little oracle"—in contrast to the "big oracle," Albert Einstein.[6] In 1921, the same year in which his book appeared, he was appointed Professor of Physics at the University of Breslau, where he would remain until 1933, when the Nazis forced him out. Reiche's research papers through 1916 were primarily on aspects of electromagnetic theory, including work on diffraction gratings and dispersion theory. But in 1917 his interests turned to quantum theory, and during the late teens and twenties, he published a series of influential and widely cited papers on such topics as dispersion (with Rudolf Ladenburg), molecular spectra, the spectrum of helium (with James Franck), and the specific heat of hydrogen (Gearhart 2010; Duncan and Janssen 2007; Wehefritz 2002).

5.2 Fritz Reiche and *Die Naturwissenschaften*

The story of Reiche's textbook begins in 1913, when Arnold Berliner persuaded Ferdinand Springer to establish a new journal, *Die Naturwissenschaften* (The Natural Sciences), which, like *Nature* in Britain and *Science* in the United States, would report on new developments in all of the natural sciences for all scientists. As Berliner and his co-editor Curt Thesing put it in an editorial in the very first issue,

> The rapidly progressing specialization in all branches of research in the natural sciences [*Naturforschung*] makes it difficult for the individual to become informed about even neighboring domains. It is almost impossible for him to become acquainted with more distant ones […]. "*Die Naturwissenschaften*" is determined to fill this gap.

As his friend Max Born wrote in a 1942 obituary, Berliner

> insisted that every article in *Die Naturwissenschaften* should be written in such a way that his "simple mind" could understand it. How few of the contributions proved up to the high standard which he set, and how lively was the ensuing correspondence. (Born 1942, 285); quoted in (Stöltzner 2003, 172)

Another friend, Paul Peter Ewald, added that

> Berliner addressed himself mainly to the then young generation of men of science. Much of the success of the journal was due to Berliner's vivid personality, his close contact with the majority of young physicists and mathematicians and his initiative in formulating the subject of articles he wanted written for his journal. Thus *Die Naturwissenschaften* became a mirror reflecting the development of science during 1913–30. (Ewald 1942, 284); quoted in (Stöltzner 2003, 171)

[5] See Reiche's description in his AHQP interview (footnote 4), session 1 (30 March 1962), p. 2, and session 2 (4 April 1962), p. 8.

[6] Letter from Reiche to Kuhn, 17 July 1962, in the interview file (footnote 4).

Reiche began writing for *Die Naturwissenschaften* in the first (1913) volume. He may well have come to Berliner's attention via Max Born, who had met Berliner in Breslau about a decade earlier, though by all accounts, Berliner had an extraordinarily wide circle of acquaintances (Born 1978, 79).[7] From the outset, Reiche showed a gift for direct, striking prose, and an ability to present complex material clearly and simply. In December 1913, he opened a five-page article on "lattice phenomena"—a topic related to his early research—by saying,

> If someone, standing at the window of an evening, looked through a fogged or frost-covered window pane by the light of a street lantern, he would see the light surrounded by colored rings, in which he would easily recognize the colors of the rainbow. (Reiche 1913b, 1193)

By stages, he drew his readers into a careful, detailed, but non-technical discussion of wave motion, diffraction, and interference that ended with a description of Max von Laue's work on X-ray diffraction in crystals.

This piece was not Reiche's first contribution. Six months earlier, in June 1913, Reiche published a nine-page, two-part article on quantum theory, intended to summarize the deliberations of the 1911 Solvay conference.[8] It was not the first article on quantum theory to appear in *Die Naturwissenschaften*; that honor fell to Reiche's friend Max Born (Born 1913). But Reiche's piece was by far the most comprehensive. It began by noting that "Greek thinkers were the first to state clearly that all matter consists of small, indivisible particles, atoms." Reiche then reviewed nineteenth-century kinetic theory, Brownian motion, electron theory, and black-body radiation, and went on to give a thorough description of the experimental evidence for and theoretical scope of quantum theory as of 1913, in clear, striking language. He concluded by quoting Marcel Brillouin, from the closing discussion of the Solvay conference:

> It has become necessary to introduce a discontinuity into our physical ideas, an element that can change only in jumps, whose existence we had not suspected until a few years ago. (Reiche 1913a, 572)

With this article, Reiche showed his ability to present at an introductory level what even in 1913 was a complex, many-faceted subject. And although another four years would pass before he began to publish his own research on quantum theory, it is evident that by 1913 he already had an encyclopedic knowledge of the subject. More generally, we see that quantum theory was sufficiently established to merit several articles for non-specialist readers in *Die Naturwissenschaften*.

5.3 Interlude: The Quantum Underground

Between 1913 and 1936, Reiche published some twenty book reviews in *Die Naturwissenschaften*, the majority during the teens and early twenties, and many of them on quantum

[7] For more on Berliner and *Die Naturwissenschaften*, see (Stöltzner 2003).

[8] This conference, named for its sponsor, the Belgian industrialist Ernst Solvay, and organized by the physical chemist Walther Nernst, explored the implications of the new quantum theory and served to introduce it to a wider scientific audience. See for example (Kormos Barkan 1999, chap. 11).

theory. Most of these reviews are unsurprising. For example, he reviewed Arnold Eucken's 1914 German translation of the proceedings of the 1911 Solvay conference, both Planck's and Wilhelm Wien's lectures at Columbia University, and Planck's 1920 Nobel Prize lecture, in which Reiche tells us, "Max Planck gives [...] an overview of the wonderland that he opened up twenty years ago."[9]

But in 1914 and 1915, Reiche also reviewed two books by Siegfried Valentiner and a third by Hermann Sieveking (Reiche 1914; 1915) that throw an unexpected light on early quantum theory, as does a fourth book, which Reiche did not review, by the British physicist Owen W. Richardson. All three authors were physicists. None was deeply involved in quantum theory. And yet, by 1914, all three had written sophisticated and detailed accounts of quantum theory for students and professional audiences. Before continuing with the story of Reiche's book, let us see what these earlier books can tell us about early quantum theory.

Hermann Sieveking was born in 1875, in Hamburg, and earned his Ph.D. in Freiburg in 1899. He was appointed *außerordentlicher Professor* ("extraordinary professor"—often translated, not altogether accurately, as associate professor) at Karlsruhe Technical University (*Technische Hochschule*) in 1910. He died suddenly in 1914; a long obituary appeared in the same issue of *Die Naturwissenschaften* as the last of his several contributions to that journal (Jensen 1914; see also Weinmeister 1926). His research involved such topics as radioactivity, and electrical discharge in gases, and was divided, judging from the obituary, between theoretical and experimental work. He was also interested in airships, on which he lectured at Karlsruhe in 1913.

Sieveking's 1914 book, *Contemporary Problems in Physics*, grew out of a series of lectures that he gave in Mannheim to the local chapter of the Association of German Chemists, who, he tells us in the introduction, wanted to learn about "recent achievements in theoretical chemistry and physics." The book included chapters on electron theory, radioactivity, X-rays, relativity, and a final, thirty-page chapter on "Progress in Thermodynamics"—in fact, quantum theory (Sieveking 1914).

That final chapter had even earlier roots. It had appeared late in 1912, in the *Proceedings* of the Karlsruhe Natural Scientific Society, with the more descriptive title "On the Radiation Law, the Action Quantum, and Nernst's Theorem." Remarkably, a note at the end of this article stated that, even earlier, the article had been prepared to compete for a prize offered by the Eisenlohr foundation (*Eisenlohrstiftung*), on the topic "Explanation [*Darlegung*] of Energy Quanta" (Sieveking and Viefhaus 1911, 134).[10] Karlsruhe was by no means the end of the earth; after all, Heinrich Hertz had done his experiments detecting electromagnetic waves and thus confirming Maxwell's laws there. And Fritz Haber spent the formative years of his career there, leaving in 1911 to become the director of the newly formed Kaiser Wilhelm Institute for Physical Chemistry in Berlin (Stoltzenberg 1994, esp. chaps. 4 and 5). But neither was it a center of activity on quantum theory. Nevertheless, quantum theory had achieved sufficient notoriety to merit a local foundation prize, and subsequent inclusion in a series of lectures to a group of chemists.

Siegfried Valentiner was born in 1876, and earned his Ph.D. in Heidelberg in 1900. He spent several years in Berlin as both an instructor (*Privatdozent*) at the University and a

[9] *Die Naturwissenschaften* 2 (1914), 662–663; 4 (1916), 650; 9 (1921), 18.

[10] The Eisenlohr foundation may be related to Wilhem Eisenlohr, who taught at the Karlsruhe *Technische Hochschule* in the mid-nineteenth century; see (Jungnickel and McCormmach 1986, vol. 2, 85). I have been unable to learn anything more about this prize or the foundation.

staff member of the Physikalisch Technische Reichsanstalt.[11] There he published one paper comparing optical temperature scales using a nitrogen gas thermometer at high temperatures, an example of the practical side of the PTR's work on black-body radiation (Holborn and Valentiner 1907).[12] It is likely enough that he first became acquainted with quantum theory during his years in Berlin. In 1910 he was appointed Professor of Physics at the School of Mines (*Bergakademie*) at Clausthal, where he spent the rest of his career.[13] The bulk of his published research lay in experimental physics and does not seem related to quantum theory. He wrote other textbooks, most notably a book on vector analysis (Valentiner 1907) that remained in print through the 1960s. He died in 1971.[14]

In 1914, Valentiner published two quantum theory textbooks, *Elementary Foundations of Quantum Theory* and *Applications of Quantum Theory in the Kinetic Theory of Solid Bodies and Gases*. More than any of the books discussed in this essay, Valentiner's were clearly intended for students. Valentiner stated in his introduction to the first book that he hoped it would be useful for young physicists and called attention to its elementary mathematical level. His first chapter gave a long historical introduction that carefully stated (but did not prove) the equipartition theorem, and went on to describe, carefully but qualitatively, the implications of quantum theory for such topics as black-body radiation, the specific heats of solids and gases, and Sommerfeld's theory of non-periodic processes. His readers were thus prepared for the more detailed treatment that followed in both books. The two books—in total, about 140 pages—gave a solid and reasonably complete description of the state of quantum theory in 1914.

In that same year, the British physicist Owen W. Richardson published a fourth book in this genre. Entitled *The Electron Theory of Matter* and extending to over 600 pages, it was based on his graduate course at Princeton University. Richardson was an experimentalist who won the Nobel Prize in 1928 for his work on thermionic emission, the emission of electrons from hot bodies. His book is the subject of an excellent essay by Ole Knudsen (2001). Suffice it to say here that although Richardson's research interests were peripheral to quantum theory, he nevertheless found such topics as Planck's radiation law and the photoelectric effect useful for his work, and devoted considerable space to them (Richardson 1914).[15] In his preface, for example, he expressed the hope that "the difficulties which beset the electron theory of metallic conduction […] may be overcome by the application of the ideas underlying Planck's theory of radiation." He even included a brief summary of Bohr's 1913 theory of the hydrogen spectrum—hot off the press in 1914. As far as I know, it represents the first textbook reference to Bohr's theory. Richardson gave a considerably expanded treatment of Bohr's theory in the second (1916) edition.

There are a few themes common to all four of these books:

[11] The PTR, as it is often called, was founded in 1887, and might best be described as a national laboratory concerned with integrating pure science with the technological needs of industry. As such, it served as a model for the National Physical Laboratory in England and the National Bureau of Standards (now the National Institute of Standards and Technology) in the United States, both founded a little over a decade later. See (Cahan 1989).

[12] See also (Cahan 1989). Much of the experimental work on black-body radiation that contributed to Planck's discovery of his radiation law late in 1900 was done at the PTR.

[13] Valentiner served on several occasions as rector at Clausthal during the 1920s and 1930s. This history of the *Bergakademie* in these years does not seem to have been studied in great detail; but see (Müller 2005; 2007).

[14] See (Weinmeister 1926) and later editions of Poggendorff.

[15] Richardson had touched on Planck's radiation law as early as 1911 in an essay in the *Proceedings of the American Philosophical Society* (Richardson 1911). Moreover, Richardson and one of his students at Princeton, Karl T. Compton, conducted experiments on the photoelectric effect; see (Stuewer 1975, 63–64).

1. All three authors were skeptical of Einstein's light quantum hypothesis.
2. All three showed a marked preference for Planck's "second theory," in which oscillators absorbed energy continuously but emitted energy quanta.
3. All three emphasized the photoelectric effect and promoted quantum (albeit not light quantum) explanations. Historians often (and correctly) criticize modern textbooks for emphasizing the photoelectric effect, sometimes at the expense of the more fundamental aspects of Einstein's light quantum hypothesis. Nevertheless, this tradition goes back to the early days of quantum theory textbooks!
4. All three made use of a February 1911 paper by Walther Nernst, published in the *Zeitschrift für Elektrochemie*. This paper presented, in a form suitable for chemists, Einstein's quantum theory of the specific heats of solids and Nernst's extensive low-temperature measurements supporting that theory. Nernst had also pointed out the discrepancies between the equipartition theorem and the measured specific heats of gases, and argued that there too, quantum theory offered a way out. Nernst's paper was widely cited, and seems to have played an important role in acquainting physicists and physical chemists with the new quantum theory. Both Sieveking and Valentiner relied heavily on it. Valentiner, in fact, was lead astray by this paper; he did not notice an error in Nernst's treatment of the rotational specific heat of diatomic gases—an error that Einstein corrected at the Solvay conference late in 1911 (Nernst 1911).[16]

When Nernst began to promote what became the 1911 Solvay conference, Max Planck was concerned that there was not yet sufficient interest in quantum theory to justify such a meeting (Kormos Barkan 1999, chap. 11).[17] These books, together with the articles and reviews in *Die Naturwissenschaften* by Reiche, Born, and others suggest that on the contrary, by 1912 or so, quantum theory was arousing considerable interest outside the realm of specialists.

5.4 Reiche's Textbook and the State of Quantum Theory in 1921

Volume 6, Number 17 of *Die Naturwissenschaften* appeared in April 1918, just as Germany was launching the offensive on the Western Front that it hoped would bring victory in the Great War (Keegan 1998, chap. 10). The issue was devoted to a *Festschrift* celebrating Max Planck's sixtieth birthday, and included articles by such luminaries as Arnold Sommerfeld, Wilhelm Wien, Walther Nernst, and Max von Laue. It also included Reiche's seventeen-page essay, "The Quantum Theory: Its Origin and Development" (Reiche 1918). Like its 1913 predecessor, it was comprehensive in its coverage. This essay became the germ of his 1921 book. The book itself appeared in 1921, and was quickly followed by an English translation (Reiche 1921a). The translators were Henry L. Brose, an Australian physicist, and Henry S. Hatfield, an English chemist. Both were prominent scientific translators; Brose, for example, translated the third edition of Sommerfeld's *Atombau*. Brose and Hatfield may have become

[16] For a discussion, see (Gearhart 2010).

[17] Valentiner and Richardson mentioned the Solvay conference in their books. Sieveking did not, either in his book or his earlier article; he did, however, cite the well known paper of Henri Poincaré that appeared in the aftermath of that conference; see for example (Prentis 1995).

acquainted while both were prisoners in the Ruhleben civilian internment camp near Berlin during the Great War (Stibbe 2008).[18]

The book appears to have sold well. Inexpensive copies are still widely available on the used book market. The English edition may have been especially popular; it went through three printings, the second two in 1924 and 1930. For the 1930 edition, Brose and a colleague, John E. Keyston, contributed a "brief outline of the developments since 1922," as they put it in an introductory note.

As in his earlier *Naturwissenschaften* pieces, Reiche's prose was clear and forceful; I give a few examples later. The level remained introductory. The book is, on the whole, historically accurate; even today, one can get from it a good picture—from Reiche's point of view, to be sure—of the state of quantum theory around 1920.

Reiche's 1914 review of Siegfried Valentiner's books shows that, even then, he was thinking about how to present quantum theory to non-specialists:

> As happy as we are to welcome such an introduction, it nevertheless appears to me difficult to find the correct boundary between a strong mathematical line of argument on the one hand, and arguments that are, as much as possible, mathematics-free and still persuasive on the other. It is a well known difficulty with which all popular accounts must struggle. (Reiche 1915)

By 1921 Reiche had found a solution to this dilemma. His main text was about 160 pages (125 pages in the English translation), but included an additional 25 pages of endnotes, many of them extending and deepening the treatment in the text. Readers seeking only an introduction could confine themselves to the text. Advanced readers would find more detailed and mathematical discussions, as well as citations to the research literature, in the notes.[19]

Reiche's coverage was comprehensive, as one can see from the following chapter outline. Very few topics in quantum theory went unmentioned, and on unsettled questions, Reiche usually gave a careful summary but refrained from taking sides. Note especially the emphasis given in Chapter V to molecular topics—the specific heat of hydrogen and infrared absorption spectra—which, in fact, had provided some of the earliest experimental support for quantum theory (Gearhart 2010). Reiche's treatment reminds us that the scope of quantum theory extended far beyond black-body radiation, the specific heats of solids, and atomic spectra:

- Chapters I, II: Black-body radiation, including experiments; the Stefan-Boltzmann law; Wien's law; Planck's path to his radiation formula; equipartition and the Rayleigh-Jeans law.
- Chapter III: Einstein's light quanta, including derivation; fluctuations and the wave-particle duality; evidence in favor (including phosphorescence [Stokes's law], fluorescence, photoelectric effect, inverse photoelectric effect). But also objections from

[18] See also http://ruhleben.tripod.com/index.html, accessed 31 July 2012. Brose achieved the remarkable distinction of being interned twice: in Germany during the Great War, and in Australia during World War II. See (Jenkin 1993).

[19] Reiche took a different tack in a little-known and considerably shorter (just under 50 pages) book, *From the Worldview of the New Physics*, (Reiche 1921b). This book, intended for a popular audience, included numerous drawings, but no equations and no bibliography or notes. It must have been among the earliest popular accounts of quantum theory.

the success of the wave theory; Planck's "second theory;" Planck's and Sommerfeld's early preference for action quanta over energy quanta.[20]

- Chapter IV: Specific heats of solids, including Einstein's 1907 theory; Nernst's heat theorem and specific heat experiments; the theories of Debye and of Born and von Kármán; relation to infrared absorption and reflection, thermal expansion, thermal conductivity; electron theory of metals.
- Chapter V: Gas theory, including the rotational specific heat of hydrogen; infrared molecular absorption spectra; theories of degenerate gases; chemical (or entropy) constant.
- Chapter VI: Atomic spectra, including the Thomson model; Rutherford scattering and planetary model; the Bohr model; Planck's and Sommerfeld's theories for several degrees of freedom; Sommerfeld's relativistic fine structure; Stark effect; Zeeman effect; selection rules; correspondence principle; intensities; helium atom.
- Chapter VII: Quantum theory of Röntgen spectra (X-rays).
- Chapter VIII: Molecular models; dispersion; further discussion of molecular spectra.

Reiche's treatment of Einstein's light quantum allows considerable insight into his treatment of controversial and unsettled questions, and shows off his striking and insightful prose as well. He set the stage in his discussion of Planck's derivation of black-body radiation, when he said that "this conclusion [Planck's quanta] is a slap in the face to classical electrodynamics" (Reiche 1921a, chap. III, § 1).[21] He went on to say, by way of introducing Einstein's light quanta,

> Here at the entry door to the new land yawns a gulf, which either […] must be bridged by a compromise, or else can be ruthlessly widened by a break with tradition. Einstein felt compelled to take the latter radical step. He proposed the hypothesis that the energy quanta not only played a role in the interaction between radiation and matter […] but that radiation also […] had a quantum structure. (Reiche 1921a, chap. III, § 2)

There followed a several-page, detailed description of Einstein's arguments, including his 1909 proposal of the wave-particle duality based on energy and moment fluctuations, as well as a careful summary of the experimental evidence. And later on, in a discussion of Einstein's 1916 derivation of Planck's radiation law (often referred to today as the A and B coefficients derivation), he added,

> Einstein […] was led to the remarkable conclusion that the radiation of Bohr atoms cannot take place in spherical waves, […] but that the process of emission must have a particular direction, like a shot from a cannon. One cannot fail to recognize that the picture of a quantum structure of radiation is thereby brought within easy reach. (Reiche 1921a, chap. VI, § 11)

So far, it sounds as if Reiche were a strong proponent of light quanta. But immediately, he proceeded to give the opposing view:

---

[20] Planck's constant h has units of "action"—the product of (generalized) momenta and coordinates. Planck's and Sommerfeld's treatments of action were different, but both involved less emphasis on quantized energies.
[21] This striking phrase, first introduced in (Reiche 1913a), did not survive in the English translation.

> With all these successes that the light quantum hypothesis can offer, one must still keep clearly in mind that this radical idea [...] can be brought into agreement with classical theory only with difficulty. But since interference and diffraction phenomena [...] are best reproduced by the wave theory, and the light quantum leads to almost insuperable difficulties, it is understandable that only a few researchers could bring themselves to sanction so drastic a change [...]. M. Planck defended (and defends to this day) this cautious and conservative standpoint, in which he located the significance of the quantum in matter—or at the least, in the interaction between matter and radiation. (Reiche 1921a, chap. III, § 6)

Reiche went on to describe Planck's "second theory." Beginning in 1911, Planck set off in a new direction, motivated by a 1910 calculation of Hendrik Antoon Lorentz (1910), who showed that, especially at short wavelengths, it would take implausibly long times for one of Planck's "resonators" to absorb one energy element from the electromagnetic field. The result was Planck's "second quantum theory," in which he largely abandoned his energy elements. Instead, he assumed that resonators absorb energy continuously but emit quanta of size $h\nu$ when they cross the boundaries of finite cells of size h in the two-dimensional phase space that he had first introduced in his 1906 *Lectures*[22] (Planck 1906, § 150); see also (Kuhn 1978; Gearhart 2002). Planck's second theory was thus an alternative to Einstein's light quanta hypothesis, and it is evident that Reiche took it seriously. At one point in his book, for example, during a discussion of the sharp absorption lines in molecular spectra, he observed that the experimental evidence did not allow one to "decide one of the most fundamental questions of the whole quantum theory, whether, namely, Planck's first or second theory is correct" (Reiche 1921a, chap. V, § 2).

In the end, Reiche left the question open, and in his brief conclusion to the book, pointed to the provisional and downright murky character of the new quantum theory:

> If we now survey the whole structure, as it stands before us, from its foundations to the highest story, we cannot avoid a feeling of admiration: admiration for the few who clear-sightedly recognized the necessity for the new doctrine and fought against tradition, thus laying the foundation for the astonishing successes that have sprung from the quantum theory in so short a time.
>
> Nonetheless, no one who studies the quantum theory will be spared bitter disappointment. For we must admit that, in spite of a comprehensive formulation of quantum rules, we have not come one step nearer to understanding the heart of the matter [...].
>
> The decision has not yet been made, as to whether, as Planck's first theory requires, only quantum-allowed states exist [...], or whether, according to Planck's second version, intermediate states are also possible. We are still completely in the dark about the details of the absorption and emission process, and do not in the least understand why the energy quanta ejected explosively as radiation should form themselves into the trains of waves which we observe far away

[22] Both the first edition (1906) in the original German and a translation of the second edition (1913) are reprinted in *The Theory of Heat Radiation* (Planck 1988). By 1913, Planck's theory had changed substantially; the 1913 translation cannot be used as a guide to the 1906 edition.

> from the atom. Is radiation really propagated in the manner claimed by classical wave theory, or does it also have a quantum character?
>
> Over all these problems there hovers at the present time a mysterious obscurity. (Reiche 1921a, chap. IX)

5.5 Reviews

Reiche's book was widely and positively reviewed. The British journal *Nature* reviewed both the German edition and the English translation (Anonymous 1922; Allen 1923).[23] The first of these reviews, which observed that the book "is an admirable account of the whole field of quantum theory," went on to take a jab at the Germans:

> the literature is very predominantly German, and it is customary in Germany to permit the publication of much more speculative ideas than is usual in other countries. The great merit of the present book is that it brings together all the threads of the argument and criticizes them, so that a just view can be obtained of the whole theory.

The anonymous reviewer went on to suggest that, if anything, Reiche had been too even handed:[24]

> There is little to criticize in such a fair account of the whole theory, but we may venture to say that the author is perhaps inclined to favor Planck's second hypothesis rather more than would the general consensus of present opinion. [… N]either of Planck's hypotheses has yet been made to cover the facts in a really convincing manner.

In the second review (Allen 1923, 280), the spectroscopist H. Stanley Allen sympathized with Reiche's reluctance to take sides in the matter of light quanta, writing that "the extraordinary problem […] has been well put by Sir William Bragg:"

> In many ways, the transference of energy suggests the return to Newton's corpuscular theory. But the wave theory is too firmly established to be displaced from the ground that it occupies. We are obliged to use each theory as occasion demands, and to wait for further knowledge as to how it may be possible that both should be true at the same time.[25] (Bragg 1921, 374)

Bragg famously observed at about the same time that "On Mondays, Wednesdays, and Fridays we use the wave theory; on Tuesdays, Thursdays, and Saturdays we think […] of flying energy quanta or corpuscles" (Bragg 1922, 158). It is often said, with considerable

[23] Note that these favorable British reviews appeared during the early 1920s, when British and German scientists were often at loggerheads in the aftermath of the Great War.

[24] See (Kuhn 1978, chap. X). Indeed, Planck's "second theory" did slowly fall out of favor during the late teens and early 1920s.

[25] In a section not quoted by Stanley, Bragg added, "Toleration of opinions is a recognized virtue. The curiosity of the present situation is that opposite opinions have to be held and used by the same individual in the faith that some day their combined truth may be made plain." Bragg was writing in the context of his experimental and theoretical efforts to understand X-rays; see (Stuewer 1971; 1975; Wheaton 1983).

truth, that many physicists unambiguously rejected Einstein's light quanta for many years, and that only a handful supported their existence. But the reactions summarized here suggest that others—Reiche included—were content merely to withhold judgment and await further developments.[26]

The *Mathematical Gazette* noted the introductory character of the book, saying that "Several experimental physicists have found it to be very instructive" (Piaggio 1923). The *Bulletin of the American Mathematical Society* added:

> The author disclaims any intention of writing a systematic textbook, yet he has produced as systematic a text as exists on the subject, and a very readable one. […] The book should not be used as a substitute for Planck's *Heat Radiation* or Sommerfeld's magnificent *Atombau und Spektrallinien*, but as an introduction […] with which one may physically orient oneself before taking up more complex discussions.[27] (Phillips 1922)

The American physicist Earle Hesse Kennard, who would himself become a prominent textbook author, was more restrained, noting, in 1924, that

> this little book contains a systematic and compact review of the quantum theory […]. Errors of fact or translation are scarce […]. In the absence of a preface one cannot be sure for what class of readers the book was intended by the author. It is quite unsuited for use by a class and would hardly do even as a first introduction for a more experienced reader. It will, however, serve admirably as a good index to the quantum theory as it existed four or five years ago. (Kennard 1924)

Not even historians of science were left out. George Sarton himself, one of the founders of the History of Science Society, reviewed the book for *Isis*, the Society's journal, and observed presciently that

> Reiche's book will be very useful not simply to the student of physics, but also to the historian of modern science […].
>
> The theory of quanta is still full of mystery; suggestive and useful as it is, one can but feel that we have not yet reached the bottom of it. (Sarton 1921)

5.6 Who Read Reiche's Book?

So Reiche's book was favorably reviewed. In all likelihood it sold well. It surely became widely known. But who bought it, and how was it used? It is not so difficult to discover where quantum theory was being taught, and often, who was teaching it.[28] But what textbooks were used in these courses? Or were textbooks used at all? Most early quantum

[26] Richardson (1914, 507–508), took a similar point of view, even though he was skeptical of Einstein's light quanta; see (Knudsen 2001, 244–245).
[27] Phillips was a mathematician at the MIT. Like several other reviews of Reiche's book, this one appeared in a mathematics journal.
[28] See for example (Sopka 1988, 91–95 and 175–193), for the United States.

theory courses were for advanced students, whose instructors often relied on their own lecture notes.[29] Of course, students could and undoubtedly did seek out whatever supplementary material they could lay their hands on. But I have found it difficult to uncover more than anecdotal evidence. For example, the Italian physicist Emilio Segrè remarked in his autobiography that:

> Besides what was taught at school, I studied some physics books [...] on my own. I still have Glazebrook's *Light*, Maxwell's *Theory of Heat*, and above all Reiche's *Die Quantentheorie*, which greatly impressed me [...]. Usually I read these books at school during boring classes. (Segrè 1993, 33)

Similarly, the Japanese theorist Hideki Yukawa tells us:

> My interest in physics gradually deepened, and I became dissatisfied with the physics I learned in school [...]. One day I found a book entitled *Quantum Theory*, written by the German physicist Fritz Reiche and translated into English, and I bought it. With my knowledge of only high school physics, it was hard to understand [...].
>
> Still, I could feel that theoretical physics was in a state of confusion, with discrepancies to be seen everywhere [...].
>
> Never, in my life, have I received greater stimulation or greater encouragement from a single book than I did from that one. (Yukawa 1982, 145–147)

A third example suggests another audience for Reiche's book. One of my own copies of the English translation has the name Lyman J. Briggs inscribed on the flyleaf, with the date 6 June 1925. Briggs is perhaps best known as the head of the 1939 "Uranium Committee," which was charged to look into the prospects for nuclear weapons (Rhodes 1986, 314–317). In 1939 Briggs was the Director of the National Bureau of Standards (now the National Institute of Standards and Technology). His undergraduate degree (in agriculture) was from Michigan Agricultural College (now Michigan State University) in 1893 and his Ph.D. in physics from Johns Hopkins in 1906. From 1896 until 1917, he was in the Physics Laboratory Division of the Department of Agriculture. In 1917 he transferred to the Bureau of Standards and became Director in 1933.

Briggs, whose research interests did not include quantum theory, nevertheless bought his copy in 1925, at the age of 50. Judging from another flyleaf inscription, it appears that he thought enough of it to give his copy to his grandson, Peter Briggs Myers, when the latter was working on a Ph.D. in physics in the late 1940s. Briggs was surely not alone in wanting to keep up with new developments in physics outside his own research interests; Reiche's book would have been ideal for this purpose.[30]

A final example comes from Benjamin Bederson, formerly the Editor-in-Chief of the American Physical Society, who gives the only reminiscence I have found of Reiche as a teacher of physics. Reiche came to the United States in 1941. As a Jew, he had been

[29] See for example the lecture notes of Peter Debye at Göttingen (circa 1914) and Edwin C. Kemble at Harvard (early 1920s), AHQP, reels 24, 55–57. See also the chapter by Midwinter and Janssen in this volume.

[30] For more on Briggs, including lists of publications, see (Myers and Levelt Sengers 1999; Landa and Nimmo 2003).

dismissed from his Breslau Professorship in 1933, and must have been among the last to escape Nazi Germany. He spent much of his remaining career at New York University. Bederson recalled that,

> In 1949 I was completing course work for a Ph.D. degree at New York University (NYU) but needed one additional course in statistical mechanics as a degree requirement. The course was given by a diminutive professor with a slight German accent whose name was Fritz Reiche. This course turned out to be the most memorable one I was ever to take at NYU […]. The clarity, the seeming simplicity of the concepts […] succeeded in transmitting to the listener the impression that he or she was able to follow deeply and with brilliant clarity the true essence of statistical mechanics […].
>
> When reading Reiche's book I discovered, not to my surprise, that it had precisely the same flavor that I recalled from Reiche's lectures at NYU […]. It remains one of the most accessible, and substantive textbooks I have ever read. (Bederson 2005, 453 and 458)

Reiche remained a teacher throughout his life. His papers are on file at the American Institute of Physics, Niels Bohr Library, in College Park, Maryland. There I came across a handwritten manuscript of a modern physics text that seems to date from the mid-1930s (judging from the material on nuclear physics), when Reiche was back in Berlin after being dismissed from his professorship in Breslau. It is a sad document to read. Reiche must have known that it could never have been published in Germany. It serves to remind us, as we study the exciting days of early quantum theory, that our actors were players on a wider stage. It is a side of this history that we do well to keep in mind.

Abbreviations and Archives

AHQP	Archive for History of Quantum Physics. American Philosophical Society, Philadelphia

References

Allen, Herbert S. (1923). The Development of the Quantum Theory. *Nature* 111: 279–281.

Anonymous (1922). Review of *Die Quantentheorie, ihr Ursprung und ihre Entwicklung*, by Fritz Reiche. *Nature* 109: 234–235.

Bederson, Benjamin (2005). Fritz Reiche and the Emergency Committee in Aid of Displaced Foreign Scholars. *Physics in Perspective* 7: 453–472.

Berliner, Arnold and Curt Thesing (1913). Zur Einführung. *Die Naturwissenschaften* 1(1).

Born, Max (1913). Die Theorie der Wärmestrahlung und die Quantenhypothese. *Die Naturwissenschaften* 1: 499–504.

— (1942). Dr. Arnold Berliner. *Nature* 150: 284–285.

— (1978). *My Life: Recollections of a Nobel Laureate*. New York: Scribner.

Bragg, William (1921). Aether Waves and Electrons. *Nature* 107: 374.

Bragg, William (1922). Electrons and Ether Waves. *Scientific Monthly* 14: 153–160.

Cahan, David (1989). *An Institute for an Empire: The Physikalisch-Technische Reichsanstalt, 1871–1918*. Cambridge: Cambridge University Press.

Duncan, Anthony and Michel Janssen (2007). On the Verge of *Umdeutung* in Minnesota: Van Vleck and the Correspondence Principle. Parts One and Two. *Archive for History of Exact Sciences* 61: 553–624, 625–671.

Ewald, Peter P. (1942). Dr. Arnold Berliner. *Nature* 150: 284.

Gearhart, Clayton A. (2002). Planck, the Quantum, and the Historians. *Physics in Perspective* 4: 170–215.

— (2010). "Astonishing Successes" and "Bitter Disappointment": The Specific Heat of Hydrogen in Quantum Theory. *Archive for History of Exact Sciences* 64: 113–202.

Holborn, Ludwig and Siegfried Valentiner (1907). Eine Vergleichung der optischen Temperaturskale mit dem Stickstoffthermometer bis 1600 °. *Annalen der Physik* 22: 1–48.

Jenkin, John (1993). Brose, Henry Herman Leopold Adolph (1890–1965). In: *Australian Dictionary of Biography*. 13. Melbourne: Melbourne University Press. 269–270. http://www.adb.online.anu.edu.au/biogs/A130304b.htm (accessed 01/31/2012).

Jensen, Christian (1914). Hermann Sieveking. *Die Naturwissenschaften* 2: 977–979.

Jungnickel, Christa and Russell McCormmach (1986). *Intellectual Mastery of Nature* 2 Volumes. Chicago: The University of Chicago Press.

Keegan, John (1998). *The First World War*. New York: Knopf.

Kennard, Earle H. (1924). Review of *The Quantum Theory*, by Fritz Reiche. *The American Mathematical Monthly* 31: 450.

Knudsen, Ole (2001). O. W. Richardson and the Electron Theory of Matter, 1901–1916. In: *Histories of the Electron: The Birth of Microphysics*. Ed. Jed Z. Buchwald and Andrew Warwick. Cambridge, MA: The MIT Press. 227–254.

Kormos Barkan, Diana (1999). *Walther Nernst and the Transition to Modern Physical Science*. Cambridge: Cambridge University Press.

Kuhn, Thomas S. (1978). *Black-body Theory and the Quantum Discontinuity, 1894-1912*. New York: Oxford University Press.

Landa, Edward R. and John R. Nimmo (2003). The Life and Scientific Contributions of Lyman J. Briggs. *Soil Science Society of America Journal* 67: 681–693.

Lorentz, Hendrik A. (1910). Alte und neue Fragen der Physik. *Physikalische Zeitschrift* 11: 1234–1257.

Müller, Georg (2005). *Historie der Technischen Universität Clausthal*. http://www.tu-clausthal.de/info/geschichte/Welcome.shtml.en (accessed 02/01/2012).

— (2007). *Technische Universität Clausthal: Abriss ihrer historischen Entwicklung*. Clausthal: Universitätsbibliothek. http://d-nb.info/985782579 (accessed 02/01/2012).

Myers, Peter B. and Johanna Levelt Sengers (1999). Lyman James Briggs, 1874–1963. In: *Biographical Memoirs, National Academy of Sciences*. 77. Washington: National Academy Press. 1–18.

Nernst, Walther (1911). Zur Theorie der spezifischen Wärme und über die Anwendung der Lehre von den Energiequanten auf physikalisch-chemische Fragen überhaupt. *Zeitschrift für Elektrochemie* 17: 265–275.

Phillips, Henry B. (1922). Review of *Die Quantentheorie, ihr Ursprung und ihre Entwicklung*, by Fritz Reiche. *Bulletin of the American Mathematical Society* 28: 69.

Piaggio, Henry T. (1923). Review of *The Quantum Theory*, by Fritz Reiche. *Mathematical Gazette* 11: 276–278.
Planck, Max (1988). *The Theory of Heat Radiation*. New York: American Institute of Physics. With an introduction by Allan A. Needell.
— (1906). *Vorlesungen über die Theorie der Wärmestrahlung*. 1st ed. Leipzig: Johann Ambrosius Barth. 2nd edition: 1913.
Prentis, Jeffrey J. (1995). Poincaré's Proof of the Quantum Discontinuity of Nature. *American Journal of Physics* 63: 339–350.
Reiche, Fritz (1913a). Die Quantentheorie. *Die Naturwissenschaften* 1(23): 549–553, 568–572.
— (1913b). Gittererscheinungen auf verschiedenen Gebieten. *Die Naturwissenschaften* 1: 1193–1197.
— (1914). Sieveking, H., Moderne Probleme der Physik. *Die Naturwissenschaften* 2: 887–888.
— (1915). Valentiner, Siegfried, Die Grundlagen der Quantentheorie in elementarer Darstellung […]. Anwendung der Quantenhypothese in der kinetischen Theorie der festen Körper und der Gase. *Die Naturwissenschaften* 3: 248.
— (1918). Die Quantentheorie. Ihr Ursprung und ihre Entwicklung. *Die Naturwissenschaften* 6: 213–230.
— (1921a). *Die Quantentheorie. Ihr Ursprung und ihre Entwicklung*. Berlin: Julius Springer. Reprinted (Charleston, SC: Bibliolife, 2009); translated in *The Quantum Theory* (New York: E. P. Dutton and Company, 1922; 1924; 1930); translation reprinted by Whitefish: Kessinger Publishing Co., 2008. A Spanish translation also appeared in 1922: *Teoría de los Quanta: su origen y desarrollo*. Madrid: Calpe, 1922.
— (1921b). *Aus dem Weltbild der neuen Physik*. Charlottenburg: Volkshochschulverlag.
Rhodes, Richard (1986). *The Making of the Atomic Bomb*. New York: Simon and Schuster.
Richardson, Owen W. (1911). The Dynamical Effects of Aggregates of Electrons. *Proceedings of the American Philosophical Society* 50: 347–365.
— (1914). *The Electron Theory of Matter*. Cambridge: Cambridge University Press. A second edition appeared in 1916.
Sarton, George (1921). Reiche, Fritz. Die Quantentheorie. Ihr Ursprung und ihre Entwicklung. *Isis* 4: 375–376.
Segrè, Emilio (1993). *A Mind Always in Motion: The Autobiography of Emilio Segrè*. Berkeley: University of California Press.
Sieveking, Hermann (1914). *Moderne Probleme der Physik*. Braunschweig: Friedrich Vieweg und Sohn.
Sieveking, Hermann and E. Viefhaus (1911). Über die Strahlungsgesetze, das Wirkungsquantum und das Nernstsche Theorem. *Verhandlungen des naturwissenschaftlichen Vereins in Karlsruhe* 25: 99–134. The date at the end of the article is December 1912.
Sopka, Katherine R. (1988). *Quantum Physics in America*. New York: American Institute of Physics.
Stibbe, Matthew (2008). *British Civilian Internees in Germany: The Ruhleben Camp, 1914–1918*. Manchester: Manchester University Press.
Stoltzenberg, Dietrich (1994). *Fritz Haber: Chemiker, Nobelpreisträger, Deutscher, Jude*. Weinheim: VCH. Translated in Dietrich Stoltzenberg, Fritz Haber: Chemist, Nobel Laureate, German, Jew. Philadelphia: Chemical Heritage Foundation, 2004.

Stöltzner, Michael (2003). *Causality, Realism and the Two Strands of Boltzmann's Legacy (1896-1936)*. PhD thesis. University of Bielefeld. http://pub.uni-bielefeld.de/publication/2304524 (accessed 07/31/2012).

Stuewer, Roger H. (1971). William H. Bragg's Corpuscular Theory of X-Rays and γ-Rays. *British Journal for the History of Science* 5: 258–281.

— (1975). *The Compton Effect: Turning Point in Physics*. New York: Science History Publications.

Valentiner, Siegfried (1907). *Vektoranalysis*. Leipzig: G. J. Göschen'sche Verlagshandlung.

— (1914a). *Die Grundlagen der Quantentheorie in elementarer Darstellung*. Braunschweig: Friedrich Vieweg und Sohn. A second edition appeared in 1919, and a third in 1920.

— (1914b). *Anwendung der Quantenhypothese in der kinetischen Theorie der festen Körper und der Gase*. Braunschweig/Wiesbaden: Friedrich Vieweg und Sohn. A second edition appeared in 1921.

Wehefritz, Valentin (2002). *Verwehte Spuren: Prof. Dr. phil. Fritz Reiche*. Dortmund: Universitätsbibliothek.

Weinmeister, Paul, ed. (1926). *J. C. Poggendorff's biographisch-literarisches Handwörterbuch für Mathematik, Astronomie, Physik, Chemie und verwandte Wissenschaftsgebiete*. 5. Leipzig: Verlag Chemie.

Wheaton, Bruce R. (1983). *The Tiger and the Shark: Empirical Roots of Wave-Particle Duality*. Cambridge: Cambridge University Press.

Yukawa, Hideki (1982). *"Tabibito" (The Traveler)*. Singapore: World Scientific. Translated by Laurie M. Brown and Rirutaro Yoshida.

Chapter 6
Sommerfeld's *Atombau und Spektrallinien*

Michael Eckert

6.1 Introduction

A textbook is commonly perceived as a didactic tool dedicated to achieving the goals of curricula in teaching institutions. This definition may be regarded in an operational sense, with an eye to the actual uses in practical teaching, or with a focus on the author's intentions (Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2005, 223). In neither regard did Arnold Sommerfeld's *Atombau and Spektrallinien* (1919) start out as a textbook. Its first edition was intended to popularize atomic physics for non-professionals. It was only in the course of its subsequent editions that it eventually transformed into one of the most renowned quantum textbooks in the twentieth century.

The story of *Atombau und Spektrallinien*, therefore, suggests a broader notion of a textbook. Rather than a singular *event* transforming past results of research into didactic lessons, a textbook may be a *process*—subject to change within its environment as much as the research for which it is accounting. *Atombau und Spektrallinien* entails an evolution of intentions, uses, and perceptions. Its author, Sommerfeld, was one of the architects of modern theoretical physics and a charismatic teacher who trained numerous quantum theorists (Eckert 1993; Seth 2010). He involved his talented students, among them prodigies like Wolfgang Pauli and Werner Heisenberg, not only in advanced quantum problems but also in the writing and proofreading of subsequent editions of *Atombau und Spektrallinien*. Thus it became a tool for teaching *and* research. Outside the Munich Sommerfeld school, it was perceived as an authoritative indicator of the current knowledge on atomic physics.

The first four editions of *Atombau und Spektrallinien* (Sommerfeld 1919; 1921; 1922; 1924) and a wave-mechanical supplementary volume (Sommerfeld 1929) mirror the transformation of quantum and atomic physics during this crucial decade after the First World War. In 1931 Sommerfeld published the fifth edition of what he now named volume 1 of *Atombau und Spektrallinien*. The wave-mechanical supplement became volume 2. Like the pre-quantum-mechanical editions of volume 1, the "wave-mechanical" part would be subject to revision, adaptation, and extension. Sommerfeld dedicated a good deal of his energies during the 1930s to this effort. When he finally published the second edition of volume 2 in 1939, its size had more than doubled from 352 to 819 pages. If we ignore the minor changes added in subsequent editions, the process that lay behind *Atombau und Spektrallinien* extended over more than two decades. The results of this process comprise a series of pre- and post-quantum-mechanical editions that stand out as unique within the physics textbook literature (see table 6.1).

Atombau und Spektrallinien		
1st edition	1919	550 pages
2nd edition	1921	583 pages
3rd edition	1922	764 pages
4th edition	1924	826 pages
Atombau und Spektrallinien I		
5th edition	1931	734 pages
6th edition	1944	734 pages
Atombau und Spektrallinien II		
Wellenmechanischer Ergänzungsband	1929	352 pages
2nd edition	1939	819 pages
3rd edition	1944	819 pages

Table 6.1: The chronology of editions of *Atombau und Spektrallinien* (1919–1944).

Despite this ongoing gestation, *Atombau und Spektrallinien* was already praised in 1923 as "the bible of the modern physicist."[1] These and other assessments of the early editions suggest that it was not the final result—the two–volume edition from the 1930s—but the entire process of its development, particularly on the eve of quantum mechanics during the early 1920s, which made *Atombau und Spektrallinien* a classic of modern textbook literature.

This chapter is concerned only with the first four editions of *Atombau und Spektrallinien*, published before the advent of quantum mechanics. The focus is on its conception, birth, growth, and reception, that is, the evolution that characterizes this textbook as the embodiment of a process extending from the First World War until the eve of quantum mechanics. The post-quantum-mechanical phase concerning the further transformation of the fourth into a fifth edition (1931) and the addition of a wave-mechanical supplementary volume (1929–1939) is left to a sequel.

6.2 Popular Lectures

We have to look at Sommerfeld's pedagogy to lay open the roots of *Atombau und Spektrallinien*. Although the later success of Sommerfeld's school tends to glorify its haphazard beginnings (Eckert 1999; Seth 2010, chap. 2), the list of his early disciples bears testimony to flourishing pedagogical activity. (Among these numbered Peter Debye, Paul Epstein, Paul Ewald, Max von Laue, Alfred Landé and Wilhelm Lenz, to list only those who would become famous for their accomplishments in atomic physics). Sommerfeld's advanced lectures covered, for example, quantum theory (summer 1914), the Zeeman effect and spectral lines (winter 1914/15), relativity theory (summer 1915) and quantum theory again (winter 1916/17). During the same period, Sommerfeld's main lecture course dealt with mechanics, continuum mechanics, electrodynamics, optics, thermodynamics, and partial differential equations for mathematical physics—already the same canonical sequence which he would forge into textbooks thirty years later.

[1] Born to Sommerfeld, 13 May 1922, DMA, HS 1977–28/A,34. Unless otherwise indicated, all English translations are by the author.

In addition to the lectures, students in the Munich "nursery of theoretical physics," as Sommerfeld used to call his institute, were trained for future careers in research and teaching through seminars and colloquia. The seminar was, at first, only a forum where students were presented with problems related to the theme of the main lecture.[2] Eventually, the seminar acquired the research orientation about which Sommerfeld's later students reported enthusiastically in their recollections (i.e., Bethe 2000). The pedagogical activity that offered the closest contact with current research themes, before the First World War, was the regular Munich Wednesday Colloquium.[3] Here Sommerfeld's advanced students could present results from their doctoral work and discuss them with advanced students from Wilhelm Röntgen's institute. Occasionally, the Munich theorists invited speakers from other universities to present their most recent papers in an informal environment. On 15 July 1914, for example, Niels Bohr personally introduced the Munich colloquium audience to "Bohr's atomic model, in particular the spectra of helium and hydrogen."[4]

In summer 1916, when Sommerfeld published his extension of Bohr's theory in the *Annalen der Physik*, he had been working for almost two years on what became known as the Bohr-Sommerfeld atomic theory (Eckert and Märker 2000, 431–445). Despite the outbreak of the First World War, Sommerfeld conducted his regular main lecture course four days a week in the morning for one hour, accompanied by a two-hour seminar each Tuesday afternoon; the advanced lectures were scheduled for one or two hours weekly; the colloquium took place on Wednesday evenings, or sometimes on another day of the week—but with few interruptions throughout the war. However, there were changes due to the absence of students who had been drafted for war service. In particular, the Munich professors offered lectures for non-professionals, addressed to colleagues from other faculties. "More recent experimental and theoretical advances in atomistics and electronics (popular, without mathematical developments), Monday, 6–7 pm," was how Sommerfeld announced his first popular lecture in the winter semester 1916/17. Henceforth the popular Monday evening lectures "for attendees from all faculties, without mathematical deduction" became almost routine. In the winter semester 1917/18, they were dedicated to "atomistics," and in the summer of 1918, to "atomic structure and spectral lines."[5]

How little Sommerfeld perceived these popular lectures as the seed for a textbook on theoretical physics is evident from the explicit emphasis on "without mathematical deduction." Although he must have already thought about publishing a book during the course of the first of these public lecture series, in the winter semester of 1916/17, he did not have physicists in mind as his readers. "This semester I held a popular lecture on atomic structure and spectral lines," he wrote to David Hilbert in March 1917.

> The audience was about 80 people, among them 12 colleagues, mainly chemists, medical scientists, and philosophers. I intend to publish it as a book. I had

[2]See the inventory of lectures of Munich University, http://epub.ub.uni-muenchen.de/view/subjects/vlverz_04.html, accessed 18 February 2012.

[3]Interview with Ewald by George Eugene Uhlenbeck and Thomas S. Kuhn, 29 March and 8 May 1962. AHQP, http://www.aip.org/history/ohilist/4523.html, accessed 18 February 2012. According to another recollection, its foundation is due to Peter Paul Koch, who was a *Privatdozent* in Wilhelm Röntgen's institute at that time. Koch to Sommerfeld, 6 August 1944, Nachlass Sommerfeld.

[4]Physikalisches Mittwoch-Colloquium, DMA, 1997–5115. Also in AHQP, P–2/20.

[5]See the inventory of lectures (n. 2), http://epub.ub.uni-muenchen.de/view/subjects/vlverz_04.html, accessed 18 February 2012.

> so much fun that I will try to lecture on relativity in the next semester also popularly, i.e., without mathematics, only conceptually presented.[6]

Sommerfeld also presented popular lectures for soldiers at the western front in January 1918. Unfortunately no records are preserved from these presentations; in a letter to his wife, Johanna (Höpfner) Sommerfeld, he merely revealed that he presented "four speeches about peace physics."[7] Three months later, he lectured before a Red Cross association about "The development of physics in Germany since Heinrich Hertz." Despite the title he also mentioned that "the young Dane physicist Bohr" considered the atom as "a planetary system in miniature" whose characteristics are inscribed in their spectra. "The explanation of the spectra, therefore, will be the acid test for our atomic model" (Sommerfeld 1918). The audience consisted of "about 1,000 people," as he reported home.[8] After this event, Sommerfeld traveled to Belgium for a sequence of popular lectures. "My presentation this morning was very nice and elicited total excitement with the rather small audience," he reported to his wife.[9] Sommerfeld's zeal for popularization was also expressed in other ways. "I am prepared to contribute to the display of the atomic structure with pleasure," he wrote in response to a request by Oskar von Miller, the founder of the Deutsches Museum in Munich. "I would like to do this in collaboration with my colleague Professor Fajans, the expert on radioactivity in our university."[10] Sommerfeld also mentioned the intended readership of his book when he wrote to Albert Einstein in the early summer 1918: "In the last 14 days I am writing a popular book on 'Atombau und Spektrallinien,' in its main part for chemists, in the appendices also for physicists."[11]

To address an audience of non-physicists may have been an exciting challenge when restricted to a few lectures, a museum exhibition, or a speech to soldiers about "peace physics"—but when it came to writing a book it also involved a sacrifice. Sommerfeld could not give as much space to his own recent achievements in atomic theory as he might have wished, had he envisaged theoretical physicists as his readership. Such advanced subjects as the fine structure theory were curtailed for the benefit of a broader exposition of subjects like radioactivity, X-rays, or the periodic system. He was well aware of this self-imposed limitation. "I am now writing a half-popular general presentation of the field and have repressed my own curiosity," he confided in December 1918 to a colleague with whom he otherwise exchanged his most recent results concerning the theory of X-ray spectra.[12]

But he did not entirely abstain from presenting research that had not yet had enough time to be generally accepted—all the more when it originated from his own institute. One recent accomplishment in which he took particular pride was the theoretical derivation of selection rules obtained without recourse to additional assumptions by Adalbert Rubinowicz just a few months earlier. Bohr had arrived at the same result, but by means of the correspondence principle. "The attitude of Rubinowicz is much more satisfying than Bohr's viewpoint in his recent paper," Sommerfeld wrote to a colleague in January 1919. "I will soon write chapter

[6] Sommerfeld to Hilbert, 13 March 1917, SUB, Cod. Ms. D. Hilbert 379A.

[7] Sommerfeld to his wife, Johanna (Höpfner) Sommerfeld, 9 January 1918, private collection, Munich. Also in (Eckert and Märker 2000, doc. 273).

[8] Sommerfeld to his wife, 14 April 1918, private collection, Munich.

[9] Sommerfeld to his wife, 17 April 1918, private collection, Munich.

[10] Von Miller to Sommerfeld, 28 January 1918; Sommerfeld to von Miller, 31 January 1918, DMA, VA 1271.

[11] Sommerfeld to Einstein, undated [June 1918], AEA. Also in (Eckert and Märker 2000, doc. 283).

[12] Sommerfeld to Richard Swinne, 25 December 1918, DMA, HS 1952–3.

VI of my book on atomic structure and spectral lines, where I will review Rubinowicz's ideas with particular fondness."[13] To Bohr he wrote a few days later that he regarded "your formal analogy principle between classical and quantum theory interesting and fruitful" but less satisfying than Rubinowicz's approach. In the same breath, he added that he was now writing "a book *Atombau und Spektrallinien* which should be also understandable for non-physicists."[14] Sommerfeld's pace of writing was quite rapid. Two weeks later, by the end of February 1919, he wrote to his former disciple, Landé, with regard to the interpretation of atomic spectra: "My book is ready except the last chapter."[15]

The writing of *Atombau und Spektrallinien*, therefore, lasted less than a year, from early summer 1918 to the spring of 1919. If we take the first mention of the book at the end of the winter semester 1916/17 as its inception, we may add a two-year stretch of popular lectures as a gestation. Neither the war nor the ensuing revolutionary turmoil seems to have had an impact on the transformation of the popular, wartime lectures into a semi-popular textbook. Sommerfeld, however, like most of his colleagues, was far from untouched by these events. Politically he may be characterized by and large as national-liberal.[16] His lectures at the front involved close contact with leading military officials and chauvinistic cultural propaganda. After his trip to Belgium in January 1918, Sommerfeld praised, in a newspaper article, the transformation of Ghent University into a German university as "the most effective and seminal trait of German politics in Belgium which tackled the problem at its root, the root of the common Germanic culture."[17] After the war, during the short-lived Soviet government in Munich in the spring of 1919 (to which the press often attached the epithet "Jewish"), Sommerfeld wrote in a moment of anger at the Munich revolutionary unrest to the right-wing Wilhelm Wien (who succeeded Röntgen a few months later as Sommerfeld's colleague in the chair for experimental physics) about the publishers he had envisaged for *Atombau und Spektrallinien*.

> It appears at Vieweg. I had also negotiated with Teubner and Springer. Teubner was not at all accommodating and seems to be in economic troubles. Springer was very tempting, but I did not trust his business practices and am becoming more and more anti-semitic in view of the Jewish-political mischief.[18]

Otherwise the turbulent times during which the book project was carried out left no traces. Up to the last moment, Sommerfeld continued to add recent results that seemed pertinent to the proofs.[19] In the preface, dated 2 September 1919, Sommerfeld emphasized once more that his book was an attempt to popularize its subject matter and its inception lay in popular lectures given during the war. That the last two of the six chapters, where he reviewed his fine-structure theory, Rubinowicz's selection rules and the like, might appear,

[13] Sommerfeld to Josef von Geitler, 14 January 1919, private collection, Warsaw. Printed in (Eckert and Märker 2004, doc. 1).
[14] Sommerfeld to Bohr, 5 February 1919, NBA. Printed in (Eckert and Märker 2004, doc. 2). For a comparison of the Sommerfeld-Rubinowicz and Bohr approach, see (Seth 2010, 228–233).
[15] Sommerfeld to Landé, 28 February 1919, Nachlass Landé 70 Sommerfeld.
[16] According to a questionnaire from July 1933, Sommerfeld was a member of the youth organization of the National Liberal Party (NLP) from 1903 to 1906, and for a short period after the war of the German Democratic Party (DDP), the left-wing successor of the NLP which dissolved in 1918, DMA, NL 89, 030, Mappe Hochschulangelegenheiten.
[17] München-Augsburger Abendzeitung, 26 February 1918.
[18] Sommerfeld to Wien, 27 March 1919, DMA, NL 56, 010.
[19] Sommerfeld to Landé, 2 July 1919, Nachlass Landé 70 Sommerfeld.

to a "mathematically untrained" reader, incompatible with this aim, Sommerfeld admitted, but the reader "should be convinced that the major difficulty of these parts is in the nature of things and does not result from the author's hobby" (Sommerfeld 1919, preface).

6.3 First Reactions

By the end of October 1919, the book was printed.[20] The very first reactions signaled that *Atombau und Spektrallinien* would be a success. Carl Runge, who was not only a noted mathematician but also an authority on spectroscopy, called it a "splendiferous book" which would serve "for many as an excellent introduction into the subject." As an expert, Runge particularly liked the final two chapters, which Sommerfeld had discerned to be rather difficult. "In the first chapters you appear to strike a more elementary tone, as if you had intended originally to write in a more popular manner and lost your way in the course of the writing."[21] Pieter Zeeman, the Dutch Nobel laureate, praised the "wonderfully clear, exhaustive, beautiful presentation of the subject" and Sommerfeld's skill as a writer: "Your book reads like a thriller." He considered it most fortunate that this book was authored by someone who had contributed so much of his own research to the field. "The victories of German science," Zeeman alluded to the political situation so shortly after the war, "will finally have to be acknowledged everywhere."[22]

This was not the only political allusion in the flood of positive, and often euphoric, reactions. Walter Kaufmann, for example, framed his praise as a congratulation—not to the author but to the reader of *Atombau und Spektrallinien*: "I commend German physics and all physicists who will have to deal with quantum theory and the like now and in the future to this opus."[23] Adding the attribute "German" reflected the embitterment toward the Entente's science policy, which, for a number of years, further deepened the wartime division of the international scientific community into hostile political camps (Schroeder-Gudehus 1966). Beyond its pedagogical use in teaching atomic physics, Sommerfeld's book served as ideological ammunition for those who considered science "Machtersatz"[24] (Forman 1973). Sommerfeld's renown as a prime authority in the nascent discipline of atomic physics shown forth from his book and decorated it like a glory. "Forsooth, in our science we do not yet notice any indications of the 'decline of the West,'" one admirer praised *Atombau und Spektrallinien* poingnantly—adding another ideological undertone by alluding to Ostwald Spengler's bestseller which had been published in the last year of the war.[25]

Thus the postwar political-ideological climate contributed to the transformation of Sommerfeld's book from a mere exposition of scientific facts into a classic of its time. Of course, there was also a true need for educating physicists on the recent developments in atomic physics. A whole generation of students was returning to the universities, hungry for mental as much as physical nourishment, and eager to absorb the new scientific knowledge about

[20] Sommerfeld to Epstein, 26 October 1919, Epstein Papers.
[21] Runge to Sommerfeld, 12 January 1920, DMA, HS 1977–28/A,298.
[22] Zeeman to Sommerfeld, 16 January 1920, DMA, HS 1977–28/A,380. Also in (Eckert and Märker 2004, doc. 17).
[23] Kaufmann to Sommerfeld, 20 January 1920, DMA, HS 1977–28/A,161.
[24] "Machtersatz" literally translated, means replacement of power.
[25] Beggerow to Sommerfeld, 21 February 1920, DMA, NL 89, 022.

atoms that had been developing so quickly while they were in the trenches.[26] *Atombau und Spektrallinien* offered this knowledge at the right time in a condensed and easily accessible manner. Within a few weeks, the need for a second edition became apparent. "Vieweg informs me today," Sommerfeld wrote to Zeeman by the end of January 1920, "that he has to envisage a new edition."[27] Max Planck found this news "among all the nice reviews the most impressive one."[28]

Given the euphoric response that Sommerfeld received in numerous letters, from his colleagues it is hardly astonishing that the public reception of *Atombau und Spektrallinien* was equally favorable. The reviewer in the *Physikalische Zeitschrift* recommended this "excellent opus" for every physicist interested in atomic physics simply as "indispensable" (Bergwitz 1920, 223–224). In the *Physikalische Berichte*, the review organ of the physics community, it was predicted that *Atombau und Spektrallinien* would exert "the deepest effect as a compendium, tool, and guide to further development" (Kossel 1920, 536–537). Another glowing review appeared in Springer's *Naturwissenschaften*. The book is

> [of] such a pervasive power that every reader with an interest in science must feel swept along and made a docile follower of the author into the new world which was opened up to a large extent by his scientific intuition and that of his disciples. (Franck 1920, 423–424)

The reviewer, James Franck, recommended the book most warmly to all scientists regardless of their specialty.

> Even though many will not follow the guide up to the highest peaks there are enough lookout points within effortless reach from which the sight is rewarding. In particular the first four chapters, which cover more than half of the book, may claim to be broadly understandable. Their reading will be particularly useful for the chemist. (Franck 1920, 423–424)

For the physicist, the book deserved "the greatest interest in all its parts," not the least because the author had the courage to present "here and there theoretical and experimental material which perhaps will not prove sustainable in the course of further research." In this manner he reached "to the farthest outposts of atomic research" (Franck 1920, 423–424).

The praise was not limited to private letters and book reviews. When Hilbert congratulated Sommerfeld for his "magnificent book," which he studied "with daily increasing pleasure," he revealed that "our faculty will offer you a little surprise for your book which will hopefully delight you."[29] The surprise arrived a few weeks later in the form of a check for over 10,000 German marks from the Otto Vahlbruch Foundation, a heritage fund at the disposal of the Philosophical Faculty of Göttingen University for bi-annually honoring "the author of a book written in German which represents the greatest progress in the sciences in these periods."[30]

[26] This view is based on dozens of letters written during the war in which Sommerfeld's students asked for reprints and other communications to learn about progress in their scientific fields. A box of such letters is preserved in DMA, NL 89, 059.

[27] Sommerfeld to Zeeman, 29 January 1920, RANH, Zeeman, inv.nr. 143. Also in (Eckert and Märker 2004, doc. 19).

[28] Planck to Sommerfeld, 15 February 1920, DMA, HS 1977–28/A,263.

[29] Hilbert to Sommerfeld, 21 January 1920, DMA, HS 1977–28/A,141.

[30] The check was dated 30 March 1920; Sommerfeld thanked on 15 April 1920, SUB, UAG II Ph 13i.

The favorable reception of *Atombau und Spektrallinien* was only occasionally accompanied with critical remarks. Max Born found, contrary to James Franck in *Naturwissenschaften*, that Sommerfeld presented:

> [S]ome things in such a way that the layperson must think that everything is in order; but that is often not the case, for example the molecular models of H_2 etc., furthermore the whole theory of X-ray spectra. Landé, at least, has told me recently that everything is in disorder here. Wouldn't it be good to emphasize the doubts a little more?[31]

He also blamed Sommerfeld for being too *lokalpatriotisch*, for example when he gave preference to Rubinowicz regarding the selection rules. "Isn't Bohr's formulation nice too?" But he belittled such criticism when he concluded with a hint to the second edition: "Do not change too much of your book, it is, as it is, wonderful!" Another critical response came from William Wilson, a lecturer at Kings College in London, who had earlier independently formulated the same quantization rule as Sommerfeld. "This should have been mentioned in your book," Wilson complained.[32] Sommerfeld responded that he already had acknowledged Wilson's priority in his publication in the *Annalen der Physik* in 1916, but omitted it in his book because Wilson had not drawn consequences for the theory of spectral lines, "the true subject of my book."[33] Because of "lack of time," as Sommerfeld wrote in September 1920 in the preface of the second edition, he refrained from a thorough revision. The changes concerned mainly the mathematical appendices. "Therefore many things were left (the molecular models, the calculation with co-planar rings of the X-ray spectra) which already appeared questionable to me in the first edition" (Sommerfeld 1921, preface).

6.4 The Second and Third Editions

The second edition was as short-lived as the first. A few months after its appearance, Sommerfeld wrote to Bohr: "I am in the uncomfortable situation that I again have to write a new edition of my book."[34] Bohr had thanked Sommerfeld only four months earlier for the second edition in a similar tone to Planck, arguing that "its fast re-appearance bears the best witness to the great interest that your book has elicited."[35] Within these four months, Bohr and Sommerfeld had exchanged more letters about recent progress in atomic physics. Bohr was at that time developing what was called his "second atomic theory," a concept about the structure of the electronic shells of successive elements in the periodic system. "Your remark," Sommerfeld responded to a letter from Landé in March 1921, "that Bohr has struck like a bomb, is also true for Munich. I received a copy of Bohr's letter to *Nature*. We have to relearn thoroughly."[36] Bohr planned to visit Göttingen that spring but had to cancel his journey because he fell ill. Sommerfeld attributed the illness to the "monstrous thought concentration" which Bohr had expended on his recent discoveries. "I would have visited you in

---

[31] Born to Sommerfeld, 5 March 1920, DMA, HS 1977–28/A,34.
[32] Wilson to Sommerfeld, 7 July 1920, DMA, HS 1977–28/A,371.
[33] Sommerfeld to Wilson, 14 July 1920. Draft, DMA, HS 1977–28/A,371. Sommerfeld added references to Wilson and Jun Ishiwara (another physicist who had formulated the same quantum rules) in the third edition.
[34] Sommerfeld to Bohr, 7 March 1921, NBA, Bohr. Also in (Eckert and Märker 2004, doc. 39).
[35] Bohr to Sommerfeld, 8 November 1920, NBA, Bohr. Also in (Eckert and Märker 2004, doc. 29).
[36] Sommerfeld to Landé, 3 March 1921, Nachlass Landé, Sommerfeld. Also in (Eckert and Märker 2004, doc. 38).

Göttingen and asked about your atomic constructions. Now I have to write my third edition without knowing more details about this decisive turn concerning the electronic orbits."[37]

After releasing the second edition with only minor changes, it was clear to Sommerfeld that he would have to make a considerable effort to adapt the third edition to the current state of atomic knowledge. Besides Bohr's second theory, this effort centered around a number of other specialties in which Sommerfeld saw fit to seek expert advice from colleagues. He asked Lise Meitner, for example, to update his paragraph on nuclear physics.[38] Sommerfeld's own research during the past year resulted in considerable changes with regard to the the spectra of atoms with more than one valence electron. In 1920 Sommerfeld had introduced the concept of an "inner quantum number" to account for certain regularities of these spectra. "You are brooding over the fundamental questions of light quanta," Sommerfeld wrote to Einstein in October 1921:

> I do not have the power to do this and am content with the details of the quantum magic in the spectra. Here there are the 'inner quantum numbers' which interest me. I have no idea what they mean but they unravel the composed triplets (and doublets).[39]

With the "inner quantum numbers" and other ad hoc concepts introduced to explain spectroscopic data, Sommerfeld's atomic theory became more empirical. The new approach seemed particularly appropriate in accounting for the anomalous Zeeman effect (Forman 1970; Seth 2008). "Your effect proves to be more and more an important guide through atomic physics," Sommerfeld wrote to Zeeman in the beginning of the winter semester 1921/22. In the preceding semester, he had dedicated his special lecture to "Magneto- and Electro-Optics," where he presented to his advanced students a "quantum-theoretical re-interpretation" of the classical model (conceived by Woldemar Voigt) of the anomalous Zeeman effect. After this trial run, he thought that the time had come to present "in the new edition of my book on spectral lines all these strange number laws which Landé has found recently and which Paschen has confirmed," he wrote to Zeeman. "Now a very talented disciple of mine even deduced these laws from a model based on simple assumptions."[40] In a letter to Einstein, he revealed that this prodigy student was Heisenberg, then in his third semester. "I have in the meantime convinced myself about wonderful number-laws of line combinations," he enthused about these recent results, "and I have presented them in the third edition of my book."[41]

By that point, in January 1922, he had finished the revisions for the third edition. The excitement about the most recent advances is also manifested in his preface to this edition:

> I attach particular importance to the introduction of the inner quantum numbers (chap. VI, § 5), and to the systematic arrangement of the anomalous Zeeman effects (chap. VI, § 7). The regularities that here obtain throughout are primarily of an empirical nature, but their integral character demands from the outset that

[37] Sommerfeld to Bohr, 25 April 1921, NBA, Bohr. Printed in (Eckert and Märker 2004, doc. 40).
[38] Sommerfeld to Meitner, 21 June 1921, Meitner Papers.
[39] Sommerfeld to Einstein, 17 October 1921, AEA.
[40] Sommerfeld to Zeeman, 2 October 1921, RANH, Zeeman, inv. nr. 910. Also in (Eckert and Märker 2004, doc. 46).
[41] Sommerfeld to Einstein, 11 January 1922, AEA. Printed in (Eckert and Märker 2004, doc. 50).

> they be clothed in the language of quanta. This mode of explanation, just like the regularities themselves, is fully established and is unique. Even at the present early stage it has shown itself in many respects to be fruitful and suggestive.[42]

Thus "Mr. stud. W. Heisenberg" and other Sommerfeld disciples (Adolf Kratzer, Wolfgang Pauli, Gregor Wentzel) found their names immortalized at a rather early stage in their career. Sommerfeld also acknowledged such contributions in doctoral reports.[43] "I have drudged a lot, particularly with the new edition of my book, and am ripe for a holiday now," he wrote to Einstein after the summer semester 1921. "I have made four PhDs (among them Pauli) and one Privatdozent (Kratzer)."[44] Furthermore, Sommerfeld paid tribute to the recent work of Alfred Landé who had turned from a devoted disciple into a rival (Forman 1970; 1968). Compared to a single reference in the second edition, Landé figures as a key player in several parts of the third edition of *Atombau und Spektrallinien*.

In terms of personalities, the indisputable main character of the book was Bohr. Sommerfeld had already praised his Copenhagen colleague in the preface to the first edition, "For all times the theory of the spectra will bear Bohr's name" (Sommerfeld 1919, preface). He had visited Bohr in autumn 1919 and privately compared him to Einstein.[45] With his "second atomic theory" about the build-up principle of the entire periodic system, Bohr's fame was growing further. Yet Bohr was not entirely happy with Sommerfeld's presentation of his ideas in the first and second editions of *Atombau und Spektrallinien*. In particular, Sommerfeld regarded the correspondence principle as merely useful—not as fundamental the way Bohr did. Although the third edition still left something to be desired in this regard, Bohr's response is telling. After expressing his congratulation and admiration, Bohr thanked Sommerfeld

> for the friendly attitude with which you regarded my work and that of my collaborators. During the last years I have often felt scientifically very lonely, under the impression that my tendencies to develop the principles of quantum theory systematically to the best of my ability have been received with very little understanding. For me this is not a matter of a didactic trifle but a sincere effort to obtain an inner connection such that one can hope to create a valid fundament for further construction. I understand very well how little things are yet resolved, and how clumsy I am with expressing my thoughts in an easily accessible manner. All the more I was pleased to see a change of your attitude in the new edition of your book.[46]

By and large, the third edition was praised as a new accomplishment—and a glimpse at a rapidly-evolving subject. "Everywhere one becomes aware about the progress," Planck wrote to Sommerfeld:

[42] The translation is taken from (Sommerfeld 1923; 1922, preface).
[43] Report to the faculty, 8 July 1921, UAM (OC-I-47p).
[44] Sommerfeld to Einstein, 10 August 1921, AEA.
[45] Sommerfeld to Margarethe Sommerfeld (his daughter), 24 September 1919, private collection, Munich.
[46] Bohr to Sommerfeld, 30 April 1922, DMA, HS 1977–28/ A, 28. Also in (Eckert and Märker 2004, doc. 55). See (Seth 2010, 233–237) for an excellent discussion about Bohr's and Sommerfeld's different perceptions of the correspondence principle.

> [A]nd at the same time the systematic rounding of the ideas developed by yourself. Admittedly even now one can not yet speak of an accomplishment of quantum theory as in classical theories. Even the immensely productive correspondence principle does not yet procure the complete connection to the classical theory.[47]

The mathematician Hermann Weyl admired Sommerfeld for his ability to orchestrate such a wealth of "recalcitrant empirical facts" into a well-ordered scheme: "You are in contact here as elsewhere with the reality that is accessible to our senses and sets the registers of your quantum organ. Your book is now my physical bible."[48] Even at technical universities the "bible" was studied. "We have now resolved to read your book together in the physics colloquium," Theodore von Kármán informed Sommerfeld from the Aachen Technical University.[49] At the same time, the third edition was translated into English and French. While these were in the making, however, physicists abroad used the German edition. Within less than a year, more than four thousand copies were sold.[50] However, its character as a "bible" evoked expectations of immortalization that were not always fulfilled to the satisfaction of Sommerfeld's colleagues. Paul Ehrenfest, for example, was "rather depressed," as Sommerfeld learned from Einstein, "because you denied him authorship of the adiabatic hypothesis."[51]

6.5 *Atombau und Spektrallinien* in the United States (1922/23)

In the summer of 1922, Sommerfeld received an invitation from the University of Wisconsin in Madison to lecture there as the Karl Schurz Professor for four months, from September 1922 to January 1923.[52] The invitation of a German so shortly after the war was an event that attracted great attention all over the United States. "German Scientist Coming," the *New York Times* reported the news on 6 August 1922. "The Karl Schurz Memorial Professorship was founded in 1910 as an exchange professorship with the German universities," the newspaper informed its readers. "The appointment of Professor Sommerfeld marks its resumption after the interruption caused by the war." But it was not only this political context—alluding to the pro- and anti-German attitudes taken in the course of US entry into the Great War in 1917—that made the invitation at the University of Wisconsin worth an article in the *New York Times*. The advances in physics achieved in Europe were being watched with great curiosity and had already resulted in invitations of professors from overseas to several American universities, including Einstein, Marie Curie, and Hendrik Antoon Lorentz, see (Sopka 1988, appendix II). "Professor Sommerfeld is expected to give a course on atomic structure, and a second course either on the analysis of wave propagation or in the general theory of relativity," the newspaper further reported.[53]

[47] Planck to Sommerfeld, 28 April 1922, DMA, HS 1977–28/A,263.
[48] Weyl to Sommerfeld, 19 May 1922, DMA, HS 1977–28/A,365. Also in (Eckert and Märker 2004, doc. 57).
[49] Von Kármán to Sommerfeld, 25 May 1922, Theodore von Kármán Papers.
[50] Vieweg to Sommerfeld, 12 January 1923, DMA, NL 89, 019, Mappe 4,1.
[51] Einstein to Sommerfeld, 16 September 1922, DMA, HS 1977–28/A,78. Also in (Eckert and Märker 2004, doc. 58).
[52] Birge to Sommerfeld, 5 July 1922, DMA, NL 89, 019, Mappe 4,1.
[53] *New York Times*, 6 August 1922.

At that time, in autumn 1922, there was still no English translation of *Atombau und Spektrallinien* available. Those attempting to learn about the recent advances in atomic physics used the third German edition, which had just appeared. But the fame of Sommerfeld's book preceded the English translation. The news of Sommerfeld's arrival spread among universities and research laboratories all over the United States, resulting in a flood of invitations to lecture on the subject of his book. "I will be very glad to visit your excellent laboratories at Schenectady and to deliver there a few lectures about Atomic Structure or Spectral Lines," Sommerfeld responded, for example, to an invitation from the Research Laboratory of General Electric.[54] He visibly enjoyed his role as harbinger of a new physics. "Crew is a spectroscopist," Sommerfeld explained in a letter to his wife about an invitation to the Northwestern University in Evanston, "my book was on his desk, I was an oracle for him."[55] At Berkeley his book was so much sought-after "that they cannot keep it," as he reported home. "It has been stolen from the institute's library and had to be purchased again."[56]

Altogether, Sommerfeld lectured at seventeen locations during his six-month sojourn in the United States. While he was based in Madison for his main stay, at the University of Wisconsin from September 1922 to January 1923, he visited Evanston, Milwaukee, Minneapolis, Ann Arbor, and Urbana. In January, he traveled to California, where Robert A. Millikan and Exum P. Lewis invited him to lecture for two weeks each at the California Institute of Technology in Pasadena and the University of California, Berkeley, respectively. On his way west, Sommerfeld included Kansas in his schedule, and on his return Denver and Ames for another couple of lectures. In March 1923, Sommerfeld's main base was at the National Bureau of Standards (NBS) in Washington, D.C., where he collaborated for ten days with the spectroscopy department, headed by William Frederick Meggers. He concluded his American sojourn with a circuit through the eastern states, lecturing in Schenectady, Cambridge (Massachusetts), Ithaca, and New York City.[57]

Although Sommerfeld was regarded by some of his American colleagues as "an oracle" with regard to atomic structure and spectral lines, the knowledge transfer accompanying Sommerfeld's lecture invitations worked both ways. Sommerfeld was particularly interested in "the astrophysical fairyworld of the Mt. Wilson and the first-rate research institution in Pasadena which the energy of Mr. Millikan has created," as he wrote some months before his visit to his former disciple Epstein, whom Millikan had called to Pasadena in 1921 as professor for theoretical physics.[58] His high expectations were not disappointed: "Apart from Millikan's institute the entire staff from Mt. Wilson is attending my lectures, all of them first-rate people," Sommerfeld wrote to his wife during his sojourn at Pasadena. "They offer me their enormous material most readily, including that which is unpublished."[59] Sommerfeld's visit at the NBS, too, brought him in close contact with a wealth of spectroscopic data. "Everyone is eager to present me his stuff," he wrote home.[60]

[54] Sommerfeld to Whitney, 10 October 1922, DMA, NL 89, 019, Mappe 4,1.
[55] Sommerfeld to his wife, 19 November 1922, private collection, Munich.
[56] Sommerfeld to his wife, 16 February 1923, private collection, Munich.
[57] This survey is primarily based on letters of invitation, preserved in Munich, NL 89, and Sommerfeld's correspondence with his wife, private collection, Munich.
[58] Sommerfeld to Epstein, 29 July 1922, Caltech Archives, Epstein 8.3.
[59] Sommerfeld to his wife, 1 February 1923, private collection, Munich.
[60] Sommerfeld to his wife, 9 March 1923, private collection, Munich.

The encounter with empirical spectroscopy in America left visible traces—in Sommerfeld's own research, in his pedagogical practice, and in the forthcoming fourth edition of *Atombau und Spektrallinien*. After his return to Munich, Sommerfeld continued to discuss spectroscopic details with his American colleagues. "May I ask you to send me the Fe-spectrum from Mt. Wilson," Sommerfeld wrote to Meggers in summer 1923. At that time, he was focusing his research on multiplets, families of regularly arranged spectral lines, which had first been identified in 1922 by Miguel Catalan in the spectrum of manganese. Such groups of lines were also observed when atoms were exposed to magnetic fields (Zeeman effect). This feature came to be represented mathematically in terms of different vectorial representations of angular momenta—but the interpretation of such constructions in terms of models remained dubious. "After Landé (*Zeitschrift für Physik*, Bd. 15, S. 189) has explained theoretically-empirically the Zeeman effects for arbitrary multiplets, we have discovered some multiplets in the spectrum of titanium and vanadium (from which we received the Zeeman effects from Pasadena)," Sommerfeld explained to Meggers. "Therefore, the comparison with Fe would be very interesting."[61] By "we" Sommerfeld meant himself and his advanced student Otto Laporte. A year later, Laporte wrote his dissertation on the spectrum of iron, a task which "was still a few years ago considered hopelessly complicated," as Sommerfeld argued in his report to the faculty.[62] In 1925, Laporte spent a one-year fellowship as a theoretical analyst in the spectroscopy department at the NBS; "we nicknamed him our Herr Geheimrat [Privy Councillor]," Meggers mused at the end of Laporte's term.[63]

Besides experimental spectroscopy, the "most interesting" scientific news which Sommerfeld encountered during his American sojourn was "a work by Compton in St. Louis." This was the manner in which he alluded, in a letter to Bohr in January 1923, to the discovery of the Compton effect, about which he had learned sometime after Christmas 1922. He reported no details (Compton's publication appeared in the *Physical Review* only in May 1923) but revealed that:

> it would have the consequence that the wave theory of X-rays has to be definitely abandoned. I am not yet totally convinced whether he is right, and I do not know whether I should already speak about his results. I only would like to point out that we may expect eventually a very fundamental new instruction.[64]

Within a few months, Compton's experimental result was transformed into the "Compton effect," reproduced in other laboratories and interpreted as a manifestation of the particle nature of X-rays (Stuewer 1975).

6.6 The Fourth Edition

Both the Compton effect and the new results about multiplets entered in the fourth edition of *Atombau und Spektrallinien*. Sommerfeld began the tedious effort of revisions and extensions as soon as he returned from America in the summer of 1923. "I am busy in preparing a new German edition in which I will also process my American experiences," he wrote to

[61] Sommerfeld to Meggers, 30 June 1923, Meggers Papers.
[62] Sommerfeld's doctoral report to the Faculty, 26 July 1924, UAM (OC-I-50p).
[63] Meggers to Sommerfeld, 8 July 1926, DMA, HS 1977–28/A,225. Also in (Eckert and Märker 2004, doc. 100).
[64] Sommerfeld to Bohr, 21 January 1923, NBA, Bohr. Also in (Eckert and Märker 2004, doc. 65).

the Madison physicist, Charles Elwood Mendenhall, at a time when the English translation *Atomic Structure and Spectral Lines*, made from the third edition, had just appeared in America.[65] In another letter to his former host at the University of Berkeley, Raymond Thayer Birge, he was more explicit. Besides news about molecular spectra, the fourth edition would cover:

> Bohr's theory of the periodic system, Compton's discovery, much more on inner quanta, magnetons and anomalous Zeeman effects than the third edition. Of course it will contain all we know until now about multiplets. I am glad that my sojourn in Washington bore good fruits in this regard.[66]

With regard to the Compton effect, Sommerfeld confessed that he had clung "as long as possible" to the view of light propagation as a wave phenomenon but that he was "impelled to adopt more and more the position of the extreme light quantum theory." Therefore, he ranked the Compton effect "among the fundamental empirical facts" to be reviewed in the first chapter. He presented the Compton effect as "the most important discovery which has been made in the present state of physics" (Sommerfeld 1924, preface, vii–viii).

The theory of complex spectra (i.e., the spectra of atoms with more than one valence electron) was a particular highlight of this edition. It forced Sommerfeld irrevocably to abstain from model interpretations of spectral lines. The demise of a visual interpretation in terms of electronic orbits seemed already unavoidable in light of the so-called doublet riddle (Forman 1968). Sommerfeld's fine structure theory from 1916 had persuasively shown that the doublets observed in X-ray spectra were caused by a relativistic effect, the same effect which caused the fine-structure split of hydrogen lines, but magnified by a factor proportional to the fourth power of the nuclear charge; on the other hand, the optical doublets, like the yellow sodium lines, were interpreted as a magnetic effect, resulting from different orientations of angular momenta. Thus, the doublets of the optical and X-ray spectra were interpreted in terms of different physical models. In April 1924, ultraviolet spectra measured in Millikan's laboratory in Pasadena indicated that both the optical and the X-ray doublets could be explained relativistically. As Sommerfeld wrote to his rival, Landé, about this news: "The relativity formula, far from being obsolete or refuted, extends its validity into the optical realm." But what reads like a victory of Sommerfeld's relativistic over Landé's magnetic doublet interpretation was not meant to suggest replacing one model by another—but rather the definitive abdication of all model-based explanations. "We have recently learned that arithmetical regularities reach farther than would be expected from models," Sommerfeld argued in view of the multiplets where these regularities were most conspicuous.[67] Sommerfeld also propagated this message in the new edition of *Atombau und Spektrallinien*. "Nowhere does the arithmetical character of quantum theory come to light in a more elementary and beautiful manner than in the complex structure of the series terms," he began the chapter on inner quantum numbers and multiplets, and followed it with the cautionary remark:

> Uncertain, however, is the interpretation in terms of a model […] Like with the X-ray spectra it is at present advisable to leave the model interpretation

[65] Sommerfeld to Mendenhall, 8 September 1923, DMA, NL 89, 003. Also in (Eckert and Märker 2004, doc. 67).
[66] Sommerfeld to Birge, 19 October 1923, Birge Papers, Box 26.
[67] Sommerfeld to Landé, 20 April 1924, Landé Papers, 70 Sommerfeld.

> more or less open and to limit oneself, in the main part, to ascertaining the facts quantum-theoretically. (Sommerfeld 1924, 575)

Apart from the wealth of new empirical material, it was this turn toward a model-free approach in atomic theory that rendered the fourth edition peculiar. Pauli, at least, perceived this message quite clearly, and he regarded as "particularly nice" that Sommerfeld had abstained in the new edition from all model-like explanations:

> The model conceptions are now in a fundamental crisis. I guess it will finally end in a further radical accentuation of the contradiction between classical and quantum theory. As becomes particularly clear from Millikan's and Landé's findings concerning the representation of the optical alkali doublets by relativistic formulae, it will hardly be possible to maintain the notion of definite distinct orbits of electrons in the atom. When we speak in terms of models we use a language that is not sufficiently adequate to the simplicity and beauty of the quantum world. For this reason I found it so nice that your presentation of the complex structure is entirely free of all model prejudices.[68]

Pauli argued from the perspective of a theorist who was primarily interested in the foundations of quantum theory, but experimental spectroscopists also had reasons to welcome the new edition. *Atombau und Spektrallinien* is "the bible of the practical spectroscopists," Friedrich Paschen wrote enthusiastically. "When I recall how I gradually learned about the quantum concepts, in the end it was always your work from which I received clarity." He praised Sommerfeld for offering, through his book, knowledge that "we practitioners could never have appropriated besides our work. I believe it is very similar with the Americans."[69] Exum Percival Lewis from the University of California in Berkeley confirmed this opinion: "The *Atombau* still remains our 'Bible' in its field," he wrote to Munich, "I shall always be very grateful to you for the copy of the last edition which you so kindly sent me."[70] The reaction from the NBS in Washington, D.C., was equally enthusiastic. Although the material "becomes more complex and detailed from the experimental side," Meggers remarked in response to the increase in size by another hundred pages "the beautiful developments in the theory" made it easier to digest. "The remarkable recent progress in the production and interpretation of spectra is tremendously stimulating."[71]

Others regarded the ever-growing empirical material in Sommerfeld's *Atombau* with mixed feelings—and added a dose of sober restraint to the praise. The reviewer in the *Physikalische Zeitschrift* (Georg Joos), for example, found it "regrettable, but apparently unavoidable" that since the appearance of the third edition "all hopes in model calculations were frustrated." But he acknowledged that the gain of "arithmetic laws in the complex structure of the spectra" compensated the abandonment of "atomic mechanical speculations" (Joos 1925, 424).

[68] Pauli to Sommerfeld, 6 December 1924, DMA, HS 1977–28/A,254. Also in (Eckert and Märker 2004, doc. 83). See also (Seth 2007; 2009).

[69] Paschen to Sommerfeld, 27 January 1925, DMA, Nl 89, 012.

[70] E. P. Lewis to Sommerfeld, 26 October 1925, DMA, NL 89, 010.

[71] Meggers to Sommerfeld, 15 December 1924, Meggers Papers.

6.7 Conclusion

Within only five years after the appearance of its first edition, *Atombau und Spektrallinien* experienced manifold transformations. The size increased considerably (from 550 to 862 pages). The popular character receded in favor of a growing exposition of expert knowledge. According to the review quoted above, it assumed "more and more the character of a handbook," although the reviewer considered it "still well readable for the scientifically educated non-professional."

For the professional quantum theorist, the transformations from the first to the fourth editions were even more striking: *Atombau und Spektrallinien* presented an evolving body of knowledge about quanta and atoms, which was used at the same time as an indicator of the forefront of research for those who contributed to this process. Numerous physicists recalled that they experienced their first encounter with quantum theory through *Atombau und Spektrallinien*.[72]

The fourth edition, which saw the explicit demise of "model prejudices," may be regarded as opening the door for Pauli's exclusion principle and the spin concept. Few other textbooks displayed to such an extent their own content as subject to change.

Quantum mechanics did not end this process of transformation. In 1929, Sommerfeld published the *Wellenmechanischer Ergänzungsband* which was subsequently labeled *Atombau und Spektrallinien II*. The story of this second volume is beyond the scope of this chapter, but it is worth mentioning here that it also underwent fundamental transformations. Unlike *Atombau und Spektrallinien I*, it was conceived from the very beginning as a physics textbook, without any aspirations to popularity among non-professionals. But this did not prevent an increase in size between the first and the second edition (published in 1939) from 351 to 820 pages. Furthermore, the second volume shared with the first volume the feature of having been a group effort: Sommerfeld explicitly thanked his disciples Karl Bechert, Walter Franz, Heinrich Welker, August Wilhelm Maue, and Ludwig Waldmann for their collaboration with parts of the book.

Over the years, other textbooks became available which dealt with one or another subfield of quantum physics in a more appropriate manner than the latest available edition of *Atombau und Spektrallinien*. For a physicist of the post-quantum-mechanical era, the historical legacy transmitted through the subsequent editions might appear more of a burden than a virtue. Nonetheless, a good deal of this material was regarded as worth knowing far beyond the initial publication of a new edition, so that later textbooks referred to *Atombau und Spektrallinien* as a basis from which one could embark in a new direction, and to which one could safely return whenever a detail demanded closer inspection. This longevity would be difficult to understand if *Atombau und Spektrallinien* had merely been a depository of settled knowledge from the pre-quantum-mechanical era.

[72] See the oral history interviews with Hans Bethe, Leon Brillouin, Gregor Wentzel and others in AHQP, available at http://www.aip.org/history/ohilist/, accessed 18 February 2012.

Abbreviations and Archives

AEA	Albert Einstein Archives, Hebrew University, Jerusalem
AHQP	Archive for History of Quantum Physics. American Philosophical Society, Philadelphia
AIP-NBL	American Institute of Physics, Niels Bohr Library, College Park, MD
Bancroft Library	University of California, Berkeley
Birge Papers	Bancroft Library
Caltech	Caltech Archives, California Institute of Technology, Pasadena
Churchill Archives Centre	Churchill College, Cambridge
DMA	Deutsches Museum, Archive, Munich
Epstein Papers	Caltech
Meggers Papers	AIP-NBL
Meitner Papers	Churchill Archives Centre
Nachlass Landé	SBPK
Nachlass Sommerfeld	UBM
NBA	Niels Bohr Archive, Copenhagen
NBS	National Bureau of Standards
NLP	National Liberal Party
Private collection, Munich	The owner of this collection wants to remain anonymous
Private collection, Warsaw	The owner of this collection wants to remain anonymous
RANH	Rijksarchief in Noord-Holland, Haarlem
SBPK	Staatsbibliothek Preußischer Kulturbesitz, Berlin
SUB	Staats- und Universitätsbibliothek, Göttingen
Theodore von Kármán Papers	Caltech
UAG	Universitätsarchiv, Göttingen
UAM	Universitätsarchiv, Munich
UBM	Universitätsbibliothek, Munich

References

Bergwitz, Karl (1920). Besprechungen: Prof. Dr. Arnold Sommerfeld: Atombau und Spektrallinien. *Physikalische Zeitschrift* 21: 223–224.

Bertomeu-Sánchez, José R., Antonio Garcia-Belmar, and Bernadette Bensaude-Vincent (2005). The Power of Didactic Writings: French Chemistry Textbooks of the Nineteenth Century. In: *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*. Ed. David Kaiser. Cambridge, MA: The MIT Press. 219–251.

Bethe, Hans A. (2000). Sommerfeld's Seminar. *Physics in Perspective* 2: 3–5.

Eckert, Michael (1993). *Die Atomphysiker. Eine Geschichte der theoretischen Physik am Beispiel der Sommerfeldschule*. Braunschweig: Friedrich Vieweg und Sohn.

Eckert, Michael (1999). Mathematics, Experiments, and Theoretical Physics: The Early Days of the Sommerfeld School. *Physics in Perspective* 1: 238–252.

Eckert, Michael and Karl Märker, eds. (2000). *Arnold Sommerfeld. Wissenschaftlicher Briefwechsel, Band 1: 1892–1918*. München: Deutsches Museum and GNT–Verlag.

— eds. (2004). *Arnold Sommerfeld. Wissenschaftlicher Briefwechsel, Band 2: 1919–1951*. München: Deutsches Museum and GNT–Verlag.

Forman, Paul (1968). The Doublet Riddle and Atomic Physics circa 1924. *Isis* 59: 156–174.

— (1970). Alfred Landé and the Anomalous Zeeman Effect, 1919–1921. *Historical Studies in the Physical Sciences* 2: 153–261.

— (1973). Scientific Internationalism and the Weimar Physicists: The Ideology and Its Manipulation in Germany after World War I. *Isis* 64: 150–180.

Franck, James (1920). Besprechungen. Sommerfeld, Arnold, Atombau und Spektrallinien. *Die Naturwissenschaften* 8: 423–424.

Joos, Georg (1925). A. Sommerfeld, Atombau und Spektrallinien. *Physikalische Zeitschrift* 26: 424.

Kossel, Walter (1920). Arnold Sommerfeld. Atombau und Spektrallinien. *Physikalische Berichte* 1: 536–537.

Nisio, Sigeko (1973). The Formation of the Sommerfeld Quantum Theory of 1916. *Japanese Studies in the History of Science* 12: 39–78.

Schroeder-Gudehus, Brigitte (1966). *Deutsche Wissenschaft und internationale Zusammenarbeit 1914–1928. Ein Beitrag zum Studium kultureller Beziehungen in politischen Krisenzeiten*. Geneva: Dumaret and Golay.

Seth, Suman (2007). Crisis and the Construction of Modern Theoretical Physics. *British Journal for the History of Science* 40: 25–51.

— (2008). Crafting the Quantum: Arnold Sommerfeld and the Older Quantum Theory. *Studies in History and Philosophy of Science* 39: 335–348.

— (2009). Zweideutigkeit about "Zweideutigkeit": Sommerfeld, Pauli, and the Methodological Origins of Quantum Mechanics. *Studies in History and Philosophy of Modern Physics* 40: 303–315.

— (2010). *Crafting the Quantum: Arnold Sommerfeld and the Practice of Theory, 1890–1926*. Cambridge, MA: The MIT Press.

Sommerfeld, Arnold (1918). Die Entwicklung der Physik in Deutschland seit Heinrich Hertz. In: *Arnold Sommerfeld. Gesammelte Schriften*. Ed. Fritz Sauter. IV. Braunschweig: Friedrich Vieweg und Sohn. 520–530.

— (1919). *Atombau und Spektrallinien*. Braunschweig: Friedrich Vieweg und Sohn.

— (1921). *Atombau und Spektrallinien*. 2nd ed. Braunschweig: Friedrich Vieweg und Sohn.

— (1922). *Atombau und Spektrallinien*. 3rd ed. Braunschweig: Friedrich Vieweg und Sohn.

— (1923). *Atomic Structure and Spectral Lines*. London: Methuen. Translated by Henry L. Brose.

— (1924). *Atombau und Spektrallinien*. 4th ed. Braunschweig: Friedrich Vieweg und Sohn.

— (1929). *Atombau und Spektrallinien. Wellenmechanischer Ergänzungsband*. Braunschweig: Friedrich Vieweg und Sohn.

Sopka, Katherine R. (1988). *Quantum Physics in America*. New York: American Institute of Physics.

Stuewer, Roger H. (1975). *The Compton Effect: Turning Point in Physics*. New York: Science History Publications.

Chapter 7
Kuhn Losses Regained: Van Vleck from Spectra to Susceptibilities

Charles Midwinter and Michel Janssen

7.1 Van Vleck's Two Books and the Quantum Revolution

7.1.1 Van Vleck's Trajectory from Spectra to Susceptibilities, 1926–1932

"The chemist is apt to conceive of the physicist as some one who is so entranced in spectral lines that he closes his eyes to other phenomena." This observation was made by the American theoretical physicist John H. Van Vleck (1899–1980) in an article on the new quantum mechanics in *Chemical Reviews* (Van Vleck 1928b, 493). Only a few years earlier, Van Vleck himself would have fit this characterization of a physicist to a tee. Between 1923 and 1926, as a young assistant professor in Minneapolis, he spent much of his time writing a book-length *Bulletin* for the National Research Council (NRC) on the old quantum theory (Van Vleck 1926b). As its title, *Quantum Principles and Line Spectra*, suggests, this book deals almost exclusively with spectroscopy. Only after a seemingly jarring change of focus in his research, a switch to the theory of electric and magnetic susceptibilities in gases, did he come to consider his previous focus myopic. In 1927–28, now a full professor in Minnesota, he published a three-part paper on susceptibilities in *Physical Review* (Van Vleck 1927a; 1927b; 1928a). This became the basis for a second book, *The Theory of Electric and Magnetic Susceptibilities* (Van Vleck 1932b), which he started to write shortly after he moved to Madison, Wisconsin, in the fall of 1928.

By the time he wrote his article in *Chemical Reviews*, Van Vleck had come to recognize that a strong argument against the old and in favor of the new quantum theory could be found in the theory of susceptibilities, a subject of marginal interest during the reign of the old quantum theory. As he wrote in the first sentence of the preface of his 1932 book:

> The new quantum mechanics is perhaps most noted for its triumphs in the field of spectroscopy, but its less heralded successes in the theory of electric and magnetic susceptibilities must be regarded as one of its great achievements. (Van Vleck 1932b, vii)

What especially struck Van Vleck was that, to a large extent, the new quantum mechanics made sense of susceptibilities not by offering new results, but by reinstating classical expressions that the old quantum theory had replaced with erroneous ones. Both in his articles of the late 1920s and in his 1932 book, Van Vleck put great emphasis on this point.

His favorite example was the value of what he labeled C, a constant in the so-called Langevin-Debye formula used for both magnetic and electric susceptibilities (Langevin 1905a; 1905b; Debye 1912). Its classical value in the case of electric susceptibilities is

$1/3$. This turns out to be a remarkably robust result in the classical theory, in the sense that it is largely independent of the model used for molecules with permanent electric dipoles. In the old quantum theory, the value of C was much larger and, more disturbingly, as no experimental data were available to rule out values substantially different from the classical one, extremely sensitive to the choice of model and to the way quantum conditions were imposed. By contrast, the new quantum theory, like the classical theory, under very general conditions gave $C = 1/3$. Van Vleck saw this regained robustness as an example of what he called "spectroscopic stability" (Van Vleck 1927a, 740). New experiments also now began to provide empirical evidence for this value and Van Vleck produced new and better proofs for the generality of the result, both in classical theory and in the new quantum mechanics. From this new vantage point, Van Vleck clearly recognized that the instability of the value for C in the old quantum theory had been a largely unheeded indication of its shortcomings.

The constant C also comes into play if we want to determine the dipole moment μ of a polar molecule such as HCl. Given a gas of these molecules, one can calculate μ using a measurement of the dielectric constant: the greater the value of C, the smaller the value of μ. Because of the instability of the value of C, Van Vleck (1928b) pointed out that, "[t]he electrical moment of the HCl molecule [...] has had quite a history" (494).

Fig. (7.1) shows the table with which Van Vleck illustrated this checkered history. The result for whole quanta was found by Wolfgang Pauli (1921) while finishing his doctorate in Munich at age 21 (Enz 2002, 61). Van Vleck, one year older than Pauli, read this paper as a graduate student at Harvard, but, indicative of the prevailing obsession with spectroscopy of the day, it did not make a big impression on him at that time (Fellows 1985, 136). The entry for half quanta is due to Linus Pauling (1926b), one year younger than Pauli. Although the paper was submitted in February 1926, Pauling was still using the old quantum theory, which is probably why the year is given as 1925 in Van Vleck's table. As the table shows, C increased by a factor of almost 14 between 1912 and 1926, reducing μ to a third of its classical value. "Fortunately [in the new quantum mechanics] the electrical moment of the HCl molecule reverts to its classical 1912 value" (Van Vleck 1928b, 494).

Value of Constant C.	*Form and Year of Theory.*	*Corresponding Value of Electrical Moment μ of HCl Molecule.*
$\frac{1}{3}$	Classical, 1912	$1{\cdot}034 \times 10^{-18}$ e.s.u.
1·54	Whole quanta, 1921	$0{\cdot}481 \times 10^{-18}$
4·57	Half quanta, 1925	$0{\cdot}332 \times 10^{-18}$
$\frac{1}{3}$	New mechanics, 1926	$1{\cdot}034 \times 10^{-18}$

Figure 7.1: The values of the constant C in the Langevin-Debye formula and of the electric moment μ of HCl in classical theory, the old quantum theory, and quantum mechanics (Van Vleck 1928b, 494).

These observations, including the table, are reprised in his book on susceptibilities (Van Vleck 1932b, 107). In fact, these fluctuations in the values of C and μ so impressed Van Vleck that the first two columns of this table can still be found in his 1977 Nobel lecture (Van Vleck 1992b, 356).

Van Vleck's 1932 book on susceptibilities was much more successful than his *Bulletin* on the old quantum theory, which was released just after the quantum revolution of 1925–26. The *Bulletin*, as its author liked to say with characteristic self-deprecation, "*in a sense* was obsolete by the time it was off the press" (Van Vleck 1971, 6, our emphasis). The italicized qualification is important. In the late 1920s and early 1930s, physicists could profitably use the *Bulletin despite* the quantum revolution. The 1932 book, however, became a classic in the field it helped spawn. Interestingly, given that it grew out of work on susceptibilities in gases, the field in question is solid-state physics. In a biographical memoir about Van Vleck for the National Academy of Sciences (NAS), condensed-matter icon Philip W. Anderson, one of Van Vleck's students, wrote that the book "set a standard and a style for American solid-state physics that greatly influenced its development during decades to come—for the better" (Anderson 1987, 524; see also, e.g., Stevens 1995, 1131). This book and the further research it stimulated would eventually earn Van Vleck the informal title, "father of modern magnetism," as well as part of the 1977 Nobel prize, which he shared with Anderson and Sir Nevill Mott.

In this paper we follow Van Vleck's trajectory from his 1926 *Bulletin* on spectra to his 1932 book on susceptibilities. Both books, as we will see, loosely qualify as textbooks. As such, they provide valuable insights about the way pedagogical texts written in the midst (the 1926 *Bulletin*) or the aftermath (the 1932 book) of a scientific revolution reflect such dramatic upheavals.

7.1.2 Kuhn Losses, Textbooks, and Scientific Revolutions

The old quantum theory's trouble with susceptibilities, masked by its success with spectra, is a good example of what is known in the history and philosophy of science literature as a *Kuhn loss*. Roughly, a Kuhn loss is a success, empirical or theoretical, of a prior theory—or *paradigm* as Kuhn would have preferred—that does not carry over to the theory or paradigm that replaced it. As illustrated by the recovery in the new quantum theory of the robust value for the constant C in the Langevin-Debye formula, a feature of the classical theory lost in the old quantum theory, Kuhn losses need not be permanent. As Kuhn himself recognized, they can be regained in subsequent theories or paradigms.

Incidentally, both Thomas S. Kuhn and Philip W. Anderson completed their Ph.D.'s at Harvard in 1949 with Van Vleck as their advisor. In the memoir about Van Vleck mentioned above, Anderson (1987, 524) wrote that "[t]he decision to work with him was one of the wiser choices of my life." By contrast, Kuhn, when asked in an interview in 1995 why he had chosen to work with Van Vleck, answered: "I was quite certain that I was not going to take a career in physics […] Otherwise I would have shot for a chance to work with Julian Schwinger" (Baltas, Gavroglu, and Kindi 2000, 274). This is particularly unkind when one recalls that in 1961, the year before the publication of *The Structure of Scientific Revolutions*, it was Van Vleck who suggested that his student-turned-historian-and-philosopher-of-science be appointed director of the project that led to the establishment of the Archive for History of Quantum Physics (AHQP) (Kuhn et al. 1967, viii; see also Baltas, Gavroglu, and Kindi 2000, 302–303).

In 1963, Kuhn interviewed his former teacher for the AHQP project. Van Vleck once again emphasized the importance of quantum mechanics having regained the Kuhn losses sustained by the old quantum theory in the area of susceptibilities, this time invoking no less an authority than Niels Bohr:

> I showed that the factor one-third [in the Langevin-Debye formula for susceptibilities] got restored in quantum mechanics, whereas in the old quantum theory, it had all kinds of horrible oscillations [...] you got some wonderful nonsense, whereas it made sense with the new quantum mechanics. I think that was one of the strong arguments for quantum mechanics. One always thinks of its effect and successes in connection with spectroscopy, but I remember Niels Bohr saying that one of the great arguments for quantum mechanics was its success in these non-spectroscopic things such as magnetic and electric susceptibilities.[1]

To the best of our knowledge, Kuhn never used the "wonderful nonsense" Van Vleck is referring to here as an example of a Kuhn loss. Still, one can ask whether the example bears out Kuhn's general claims about Kuhn losses. We will find that it does in some respects but not in others. For instance, contrary to claims by Kuhn in *Structure* about how scientific revolutions are papered over in subsequent textbooks, the prehistory of the theory of susceptibilities, including the Kuhn loss the old quantum theory suffered in this area, is dealt with *at length* in Van Vleck's 1932 book. However, we will also see that, in at least one important respect, Van Vleck's version of this prehistory is a little misleading and perhaps even a tad self-serving, which is just what Kuhn would have led us to expect. In general, there is much of value in Kuhn's account, which thus provides a good starting point for our analysis. Ultimately, our goal is not to argue for or against Kuhn but to use the fine structure of the quantum revolution to learn more about the structure of scientific revolutions in general.[2]

7.1.3 Kuhn Losses

The concept of a Kuhn loss, though obviously not the term, is introduced in chap. 9 of *Structure* (Kuhn 1996, 103–110; page numbers refer to the 3rd edition). To underscore that science does not develop cumulatively, Kuhn noted that in going from one paradigm to another there tend to be gains as well as losses. "[P]aradigm debates," he wrote, "always involve the question: Which problems is it more significant to have solved?" (ibid., 110).

In the transition from classical theory to the old quantum theory, gains in spectroscopy apparently outweighed losses in the theory of susceptibilities just as, at least until the early 1920s, they outweighed losses in dispersion theory. The former Kuhn loss was only regained in the new quantum theory,[3] while the latter was recovered in the dispersion theory of Hendrik A. (Hans) Kramers (1924a; 1924b). Kramers's dispersion theory was formulated in the context of the old quantum theory of Bohr and Arnold Sommerfeld but quickly

---

[1] AHQP interview, session 2, 5. See also the opening sentence of the preface of Van Vleck's 1932 book quoted in sec. (7.1.1).

[2] We thus use Kuhn's work in the same spirit as Michael Ruse (1989, 62) in an essay on the plate-tectonics revolution in geology.

[3] As we will see in sec. (7.4), there were four papers published in 1926 all reporting the recovery of $C = 1/3$ in the new quantum theory. As Van Vleck wrote in the conclusion of the one submitted first but published last: "This is a much more satisfactory result than in the older version of the quantum theory, in which both the calculations of Pauli [1921] with whole quanta [...] and of Pauling [1926b] with half quanta yielded results diverging from the classical Langevin theory even at high temperatures" (Van Vleck 1926a, 227).

incorporated into the infamous BKS theory of Bohr, Kramers, and John C. Slater (1924), a short-lived quantum theory of radiation in which a number of fundamental tenets of the Bohr-Sommerfeld theory were abandoned (Duncan and Janssen 2007, secs. 3–4).

Strictly speaking, of course, when we talk about Kuhn losses and their recovery, we should be talking about paradigms rather than theories. Kuhn exegetes, however, will forgive us, we hope, for proceeding on the assumption that a theory can be construed as a key component of a paradigm or a *disciplinary matrix*, the term Kuhn in his 1969 postscript to *Structure* proposed to substitute for the term 'paradigm' when used in the sense in which we need it here (Kuhn 1996, 182). Granted that assumption, we can continue to talk about Kuhn losses in transitions from one *theory* to another.

Although they are both Kuhn losses of the old quantum theory, the one in susceptibility theory is of a different kind than the one in dispersion theory. In the case of dispersion, there was clear experimental evidence all along for the key feature of the classical theory that was lost in the old quantum theory and recovered in the Kramers dispersion theory. In the case of susceptibility theory, as we mentioned above, experimental evidence for the key feature of the classical theory that was lost in the old quantum theory only became available *after* it was recovered in the new quantum theory.

The key feature in the case of dispersion is that anomalous dispersion—the phenomenon that in certain frequency ranges the index of refraction gets smaller rather than larger with increasing frequency[4]—occurs around the absorption frequencies of the dispersive medium. This is in accordance with the classical dispersion theories of Hermann von Helmholtz, Hendrik A. Lorentz, and Paul Drude (Duncan and Janssen 2007, 575–576). However, in the dispersion theories of Sommerfeld (1915; 1917), Peter Debye (1915), and Clinton J. Davisson (1916), based on the Bohr model of the atom, dispersion is anomalous around the orbital frequencies of the electrons, which differ sharply from the absorption frequencies of the atom except in the limit of high quantum numbers. As one would expect in the case of a Kuhn loss, proponents of the Sommerfeld-Debye-Davisson theory had a tendency to close their eyes to this problem. Others, however, including Bohr himself, raised it as serious objection early on. A few years before Kramers (1924a; 1924b), building on work by Rudolf Ladenburg and Fritz Reiche (Ladenburg 1921; Ladenburg and Reiche 1923), eventually solved the problem, Paul S. Epstein sharply criticized the Sommerfeld-Debye-Davisson theory on this score in a paper with the subtitle "Critical comments on dispersion:"

> [T]he positions of maximal dispersion and absorption do not lie at the position of the emission lines of hydrogen but at the position of the mechanical frequencies of the model […] *the conclusion seems unavoidable to us that the foundations of the Debye-Davysson* [sic] *theory are incorrect*. (Epstein 1922, 107–108; emphasis in the original; quoted and discussed by Duncan and Janssen 2007, 580–581)

By contrast, it was only *after* the new quantum theory had restored the classical value $C = 1/3$ in the Langevin-Debye formula for electric susceptibilities that the "horrible oscillations" in the old quantum theory came to be seen as the "wonderful nonsense" Van Vleck made them out to be.

[4]For a brief discussion of this phenomenon and its discovery in the nineteenth century, see (Buchwald 1985, 233, note 1).

When Pauli, for instance, first found a deviation from $C = 1/3$, he did not blink an eye. He just stated matter-of-factly that "the numerical factor in the final formula for the polarization depends on the specific model [...] while in the classical theory the Maxwell distribution and with it the numerical factor $1/3$ hold generally" (Pauli 1921, 325). In the conclusion of his paper, Pauli exhorted experimentalists to measure the temperature-dependence of the dielectric constant of hydrogen halides such as HCl, adding that this "should not pose any particular difficulties" (ibid., 327). Noting that his quantum theory predicted a much smaller value for the electric dipole moment μ of HCl than the classical theory ($\mu_{classical} = 2.1471\, \mu_{quantum}$; cf. the table in fig. 3.2), he suggested that this might provide a way to decide between the two theories. The distance between the two nuclei in, say, a HCl molecule could accurately be determined on the basis of spectroscopic data. This distance, Pauli argued, gives an upper bound on the dipole length $d = \mu/e$ between the charges $+e$ and $-e$ forming the dipole in this case. Hence, he concluded, "*if the classical formula for the dielectric constant gives a dipole length that is greater than the nuclear separation extracted from infrared spectra, the formula must be rejected*" (Pauli 1921, 327, emphasis in the original).

Three years later, the experimentalist Charles T. Zahn (1924) took up Pauli's challenge, but came to the disappointing conclusion that "[t]he upper limit for the moment given by the infrared absorption data for HCl [...] is 6 times the classical value and 13 times the quantum value and hence does not decide between the two theories" (400). Van Vleck's own citations to the experimental literature in his 1932 book strongly suggest that it was only in the period following the quantum revolution of 1925–26 that reliable data in favor of the value $C = 1/3$ became available (Van Vleck 1932b, 61). The Kuhn loss in the theory of susceptibilities emphasized by Van Vleck is thus the loss of a theoretical feature that in hindsight proved to be empirically correct, not, as in the case of the Kuhn loss in dispersion theory, a loss of empirical adequacy in some area. Van Vleck's most persuasive argument against the results of Pauli and Pauling was that they deviated from the classical result even at high temperatures. As he put it in his 1932 book: "the correspondence principle led us to expect usually an asymptotic connection of the classical and quantum results at high temperatures" (Van Vleck 1932b, 107, see also the quotation in note 3).[5]

Kuhn losses come in a variety of forms. In most of Kuhn's own examples, what is lost (and sometimes regained) in successive paradigm shifts are certain types of accounts of

[5] Incidentally, Zahn, who concluded in 1924 that experiment could not decide between the classical formula for the temperature-dependence of electric susceptibilities and Pauli's new quantum formula, is one of the two physicists who showed over a decade later that experiments on the velocity-dependence of the electron mass in the early years of the century could not decide between the theoretical predictions of Albert Einstein's special theory of relativity and Lorentz's ether theory, on the one hand, and Max Abraham's so-called electromagnetic view of nature, on the other (Zahn and Spees 1938). As one of us has argued, the proponents of these competing theories, though paying lip service to the experimental results, especially when they favored their own theories, put much more stock in theoretical arguments (Janssen and Mecklenburg 2007, 105–108). When, for instance, Alfred H. Bucherer presented new data favoring Lorentz and Einstein at the same annual meeting of German Physical Scientists and Physicians in Cologne in 1908 where Hermann Minkowski gave his now famous talk on the geometrical underpinnings of special relativity, Minkowski, while welcoming Bucherer's new data, dismissed Abraham's theory on purely theoretical grounds. He called Abraham's model of a rigid electron, not subject to length contraction, a "monster" and "no working hypothesis, but a working hindrance," and compared Abraham's insertion of this model into classical electrodynamics to attending a concert wearing ear plugs (ibid., 88)! This is reminiscent of how Van Vleck dismissed results derived by the likes of Pauli and Pauling in the old quantum theory as "wonderful nonsense." As we will see in sec. (7.5.4), Van Vleck heaped more scorn on the treatment of susceptibilities in the old quantum theory in his 1932 book (Van Vleck 1932b, chap. V).

phenomena deemed acceptable in a paradigm. In the one example to which he devotes more than a paragraph, Kuhn (1996, 104–106) argues, for instance, that the Newtonian notion of gravity as an innate attraction between particles can be seen as a "reversion (which is not the same as a retrogression)" to the kind of scholastic essences that proponents of the mechanical tradition earlier in the seventeenth century thought they had banished from science for good. Although our examples involve different components of the disciplinary matrix (empirical adequacy, features attractive on theoretical grounds), quantum mechanics can likewise be said to have brought about a reversion but not a retrogression to classical theory in the cases of dispersion and susceptibilities.

7.1.4 Textbooks and Kuhn Losses

Kuhn (1996, chap. 11) famously identified textbooks as the main culprit in rendering the disruption of normal science by scientific revolutions invisible. Textbooks, he argued, by their very nature must present science as a cumulative enterprise. This means that Kuhn losses must be swept under the rug. Textbooks, he wrote,

> address themselves to an already articulated body of problems, data, and theory, most often to the particular set of paradigms[6] to which the scientific community is committed at the time they are written [...] [B]eing pedagogic vehicles for the perpetuation of normal science [...] [they] have to be rewritten in the aftermath of each scientific revolution, and, once rewritten, they inevitably disguise not only the role but the very existence of the revolutions that produced them [...] [thereby] truncating the scientist's sense of his discipline's history. (Kuhn 1996, 136–137)

When he wrote this passage, Kuhn was probably thinking first and foremost of modern science textbooks at both the undergraduate and the graduate level. Given the scope of the general claims in *Structure*, however, his claims about textbooks had better hold up for books used as such in the period and the field we are considering.

The two monographs by Van Vleck examined in this paper would seem to qualify as (graduate) textbooks even though under a strict and narrow definition of the genre they might not. Most of their actual readers may have been research scientists but they were written with the needs of students in mind and both books saw some classroom use. Student notes for a two-semester course on quantum mechanics that Van Vleck offered in Wisconsin in 1930–31 show that, despite the quantum revolution that had supposedly made it obsolete four years earlier, Van Vleck was still using his NRC *Bulletin* as the main reference for almost two-thirds of the first semester.[7] It is unclear whether Van Vleck himself ever used his 1932 book on susceptibilities in his classes. However, one of his colleagues at Wisconsin, Ragnar Rollefson, told Van Vleck's biographer Fred Fellows (1985, 264) that he had occasionally

[6]As we will see below, 'paradigm' is used here in the sense for which Kuhn (1996, 187) later introduced the term 'exemplar'.

[7]These notes, taken by Ralph P. Winch, have been deposited at the Niels Bohr Library & Archives of the American Institute of Physics in College Park, Maryland. Notes for a course in 1927–28 in Minnesota, taken by Robert B. Whitney and not nearly as meticulous as Winch's, also contain numerous references to Van Vleck's *Bulletin*. A full photocopy of these notes was obtained by Roger Stuewer, who kindly made them available to us (accompanying this photocopy is a letter from Barbara Buck to Roger Stuewer, 9 December 1977, detailing its provenance).

used the lengthy chap. VI, "Quantum-mechanical foundations," which includes a thorough discussion of quantum perturbation theory, in his courses on quantum mechanics.[8]

So one can reasonably ask how well Van Vleck's two books fit with Kuhn's seductive picture of how the regrouping of a scientific community in response to a scientific revolution is reflected in the textbooks it produces. It will be helpful to separate two aspects of this picture: how textbooks delineate and orient further work in their (sub-)disciplines, and how, in doing so, they inevitably distort the prehistory of these (sub-)disciplines and paper over Kuhn losses.

Van Vleck's NRC *Bulletin* confirms several of his former student's generalizations about textbooks. The *Bulletin* is organized around the correspondence principle as a strategy for tackling problems mostly in atomic spectroscopy. Van Vleck thus took the approach he, Kramers, Max Born and others at the research frontier of the old quantum theory had adopted around 1924 and fixated that approach in a book meant to initiate others in the field. Putting these correspondence-principle techniques and the problems amenable to them at the center of his presentation and relegating work along different lines or in other areas to the periphery, Van Vleck clearly identified and promoted what he thought was and should be the core pursuit of the old quantum theory.

Those engaged in work that was marginalized in this way predictably took exception. In a review of the *Bulletin*, one such colleague, Adolf Smekal, complained about Van Vleck's organization of the material. Smekal recognized that some organizing principle was needed given the sheer quantity of material to be covered but he did not care for the choices Van Vleck had made:

> Selection of, arrangement of, and space devoted to the offerings is heavily influenced by subjective viewpoints and cannot win every reader's approval everywhere. Instead of the presumably available option of letting all fundamental connections emerge systematically, the author has preferred to put up front what is felt to be the internally most unified part of the quantum theory as it has developed so far, followed by more or less isolated applications to specific problems. (Smekal 1927, 63)

The way in which correspondence-principle techniques take center stage in Van Vleck's book provides a nice example of how textbooks transmit what Kuhn in the postscript to *Structure* called *exemplars*, the "entirely appropriate [meaning] both philologically and autobiographically" of the term 'paradigm' (Kuhn 1996, 186–187). By an exemplar, Kuhn wrote,

> I mean, initially, the concrete problem solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations or at the ends of chapters in science texts. To these shared examples should,

[8] In his review of the 1932 book in *Die Naturwissenschaften*, Pauli (1933) wrote: "One can say that it has the character in part of a handbook and in part of a textbook. The former aspect is expressed in the exhaustive discussion of all questions of detail, the latter in that the foundations of the theory are also presented." In summary, he wrote: "Both for learning the theory of the field covered and for an authoritative introduction to the details the book can be most warmly recommended" (ibid.). Similarly, Pauling (1932, 4121) wrote in his review: "The book is characterized by clear exposition and interesting style, which combined with the sound and reliable treatment, should make it a valuable text for an advanced course, as well as the authoritative reference book in the field."

> however, be added at least some of the technical problem-solutions found in the periodical literature that scientists encounter during their post-educational research careers and that also show them by example how their job is to be done. (Kuhn 1996, 187)

Van Vleck's *Bulletin* presented such "technical problem-solutions found in the periodical literature" in a more didactic text that should help its readers become active contributors to this literature themselves.

Confirming another article of Kuhnian doctrine, the problem with susceptibilities, a Kuhn loss in the transition from classical theory to the old quantum theory, is not mentioned anywhere in the *Bulletin*. Van Vleck may have forgotten about the problem, but there is clear evidence that he had been aware of it earlier. In a term paper of 1921, entitled "Theories of magnetism," for a course he took with Percy W. Bridgman as a graduate student at Harvard, Van Vleck touched on the paper in which Pauli (1921) derived the entry $C = 1.54$ for whole quanta in the table in fig. (3.2) (Fellows 1985, 136).

Whereas the *Bulletin* passes over the Kuhn loss in the theory of susceptibilities in silence, the Kuhn loss in dispersion theory in that same transition is flagged prominently. It is easy to understand why. By the time Van Vleck wrote his *Bulletin*, Kramers (1924a; 1924b) had already recovered that Kuhn loss with his new dispersion formula. Moreover, as we will see in sec. (7.3.2), this formula was one of the striking successes of the correspondence-principle approach central to the book. Van Vleck thus could and did use the recovered Kuhn loss in dispersion theory to promote this approach.

In his 1932 book, as we will see in secs. (7.5.2–7.5.5), Van Vleck made even more elaborate use of the recovered Kuhn loss in susceptibility theory to promote his new quantum-mechanical treatment of susceptibilities. He devoted a whole chapter of the book to the problems of the old quantum theory in this area. Of course, the Kuhn loss in susceptibility theory was regained only after a major theory change. The difference between the two cases, however, is smaller than one might initially think. The BKS theory into which Kramers's dispersion formula was quickly integrated constituted such a radical departure from Bohr's original theory that it might well have been remembered as a completely new theory had it not been so short-lived (Duncan and Janssen 2007, 597–613).

Like the *Bulletin*, the 1932 book provided its readers with all the tools they needed to become researchers in the field it so masterfully mapped out for them. Had the correspondence-principle approach to atomic physics been moribund by the time the *Bulletin* saw print, the approach to electric and magnetic susceptibilities championed in the 1932 book would prove to be remarkably fruitful.

7.1.5 Continuity and Discontinuity in Scientific Revolutions

A couple of Kuhn losses proudly displayed rather than swept under the rug in a pair of books that only broadly qualify as textbooks may not seem like much of a threat to Kuhn's general account of how textbooks make scientific revolutions invisible. But they do point, we believe, to a more serious underlying issue. Van Vleck managed to write two books that equipped their readers with the tools they needed to start doing the kind of research their author envisioned themselves *without* the kind of wholesale distortion and suppression of the prehistory of their subject matter that Kuhn claimed are unavoidable. That is not to say that such distortion and suppression were or could have been completely avoided.

The 1932 book provides the clearest example of this. As mentioned above, Van Vleck devoted an entire chapter to the old quantum theory, putting the problems it ran into with susceptibilities on full display. Yet he conveniently neglected to mention that there had been no clear empirical evidence exposing these problems.

Smekal's review of the NRC *Bulletin* suggests that in 1926 Van Vleck did not completely steer clear of distorting the history of his subject either. Smekal had been championing an alternative dispersion theory, which he complained was "completely misunderstood and distorted" (Smekal 1927, 63) in the one paragraph Van Vleck (1926b, 159) devoted to it. Whether or not this complaint was well-founded, it would have been counterproductive in terms of Van Vleck's pedagogical objectives to cover Smekal's and other competing theories of dispersion to their proponents' satisfaction.

That said, there were many elements in older theories that helped rather than hindered Van Vleck in achieving these objectives. As a result, much of the continuity that can be discerned in the discussions of classical theory and quantum theory in the NRC *Bulletin* is *not*, as Kuhn would have it, an artifact of how history is inevitably rewritten in textbooks, but actually matches the historical record tolerably well. Despite its misleading treatment of the experimental state of affairs in the early 1920s, the same can be said about the 1932 book. The final two clauses of the passage from *Structure* quoted above ("inevitably disguise [...]" and "truncating [...]") are clearly too strong.

On the Kuhnian picture of scientific revolutions as paradigm shifts akin to Gestalt switches, it is hard to understand how a post-revolutionary textbook could make the prehistory of its subject matter look more or less continuous and thereby perfectly suitable to its pedagogical objectives *without* seriously disguising, distorting, and truncating that prehistory. An important part of the explanation, at least in the case of these two books by Van Vleck, is the continuity of mathematical techniques through the conceptual upheavals that mark the transition from classical theory to the old quantum theory, and finally to modern quantum mechanics.

In his recent book, *Crafting the Quantum*, on the Sommerfeld school in theoretical physics, Suman Seth (2010) makes a similar point. He reconciles the continuous and the discontinuous aspects of the development of quantum theory in the 1920s by emphasizing, as we do, the continuity of mathematical techniques. Scientific revolutions, he writes, "are revolutions of conceptual foundations, not of puzzle-solving techniques. Most simply: Science sees revolutions of principles, not of problems" (Seth 2010, 268). To illustrate his point, Seth quotes Arnold Sommerfeld, who wrote in 1929: "The new development does not signify a revolution, but a joyful advancement of what was already in existence, with many fundamental clarifications and sharpenings" (ibid., 266).

Given the radical conceptual changes involved in the transition from classical physics to quantum physics, it is important to keep in mind that there was at the same time great continuity of mathematical structure in this transition. Both the old quantum theory and matrix mechanics, for instance, retain, in a sense, the laws of classical physics. The old quantum theory just put some additional constraints on the motions allowed by Newtonian mechanics. The basic idea of matrix mechanics, as reflected in the term *Umdeutung* (reinterpretation) in the title of the paper with which Werner Heisenberg (1925b) laid the basis for the new theory, was not to *repeal* the laws of mechanics but to *reinterpret* them. Heisenberg took the quantities related by these laws to be arrays of numbers, soon to be recognized as matrices (Duncan and Janssen 2007; 2008). It is this continuity of mathematical structure that undergirds the continued effectiveness of the mathematical tools wielded in the context of these different theories.

In the old quantum theory, techniques from perturbation theory in celestial mechanics were used to analyze electron orbits in atoms classically as a prelude to the translation of the results into quantum formulas under the guidance of the correspondence principle (Duncan and Janssen 2007, 592–593, 627–637). This is the procedure that led Kramers to his dispersion formula. It is also the procedure that Van Vleck (1924b; 1924c) followed in his early research and made central to his exposition of the old quantum theory in the 1926 *Bulletin*. It inspired the closely related perturbation techniques in matrix mechanics developed in the famous *Dreimännerarbeit* of Born, Heisenberg, and Pascual Jordan (1926). In his papers of the late 1920s and in his 1932 book, Van Vleck adapted these perturbation techniques to the treatment of susceptibilities. A reader comparing Van Vleck's books of 1926 and 1932 is probably struck first by the shift from spectra to susceptibilities. Underlying that discontinuity, however, is the continuity in these perturbation techniques, made possible by the survival of much of the structure of classical mechanics in both the old and the new quantum theory. These techniques actually fit Kuhn's definition of an exemplar very nicely, even though they cut across what by Kuhn's reckoning are two major paradigm shifts.

One way to highlight the continuity of Van Vleck's trajectory from spectra to susceptibilities is to note that the derivation of the Kramers dispersion formula, a prime example of Van Vleck's approach in his NRC *Bulletin*, and the derivation of the Langevin-Debye formula for electric susceptibilities, central to his classic of early solid-state physics, both involve applications of canonical perturbation theory in action-angle variables to calculate the electric moment of a multiply-periodic system in an external electric field. The main difference is that in the case of dispersion we are interested in the instantaneous value of the electric moment of individual multiply-periodic systems in response to the periodically changing electric field of an incoming electromagnetic wave, whereas in the case of susceptibilities we are interested in thermal ensemble averages of the electric moments of many such systems averaged over the periods of their motion in response to a constant external field (cf. sec. 7.3.2 and secs. 7.5.3–7.5.5 and note 85).

The remarkable continuity of mathematical structures and techniques in the transitions from classical theory to the old quantum theory, and then to modern quantum mechanics makes it perfectly understandable that Van Vleck could still use his 1926 *Bulletin* in his courses on quantum mechanics in the early 1930s. It also explains how Van Vleck could make such rapid progress once he hit upon the problem of susceptibilities not long after he completed the *Bulletin* and mastered matrix mechanics.

Kuhn had a tendency to see only discontinuity in paradigm shifts. This intense focus on discontinuity is what lies behind his fascination with Kuhn losses. It also made him overly suspicious of the seemingly continuous theoretical developments presented in science textbooks. The analysis of Van Vleck's 1926 and 1932 books and of his trajectory from one to the other provides an important corrective to the discontinuity bias in Kuhn's stimulating and valuable observations about Kuhn losses and textbooks and will thus, we hope, contribute to a more nuanced understanding of the role of the textbooks in shaping and sustaining (sub-)disciplines in science.

Whether one sees continuity or discontinuity in the transition from classical physics to quantum physics depends, to a large extent, on one's perspective. The historian trying to follow the events as they unfolded on the ground, will probably mainly see continuities.

The historian who takes a bird's eye view and compares the landscapes before and after the transition will most likely be struck first and foremost by discontinuities. A final twist in our story about the recovered Kuhn loss in Van Vleck's 1932 book nicely illustrates this difference in perspective.

Van Vleck covered the troublesome recent history of its subject matter in chap. V, "Susceptibilities in the old quantum theory contrasted with the new." This chapter, as we will show in more detail in secs. (7.5.2–7.5.5), allows us to see important elements of continuity in the transition from the old to the new quantum theory. Toward the end of his life, Van Vleck began revising his 1932 classic with the idea of publishing a new edition (Fellows 1985, 258, 262–263, 266).[9] Wanting to add a chapter on modern developments without changing the total number of chapters, he intended to cut chap. V, on the grounds that by then it only had historical value.[10] Even in 1932 he began the chapter apologizing to his readers that "it may seem like unburying the dead to devote a chapter to the old quantum theory" (Van Vleck 1932b, 105). Note also the one reservation Anderson (1987, 509) expressed about the book in his NAS memoir: "It is marked—perhaps even slightly marred, as a modern text for physicists poorly trained in classical mechanics—by careful discussion of the ways in which quantum mechanics, the old quantum theory, and classical physics differ." As it happened, the new edition of the book never saw the light of day, but if it had, it would have been a confirming instance of an amended version of Kuhn's thesis, namely that, going through multiple editions, textbooks *eventually* suppress or at least sanitize the history of their subject matter and paper over Kuhn losses, especially those that turn out to have been only temporary.

7.1.6 Van Vleck as Teacher

Although it will be clear from the preceding subsections that our main focus in this paper is not on Van Vleck's books as pedagogical tools, it seems appropriate to devote a short subsection to Van Vleck as a teacher.

A good place to start is to compare testimony by Anderson and Kuhn, Van Vleck's unlikely pair of graduate students at Harvard in the late 1940s. In his NAS memoir about Van Vleck, Anderson offered the following somewhat back-handed compliment:

> By the 1940s […] his teaching style had become unique, and is remembered with fondness by everyone I spoke to. Most of the material was written in his inimitable scrawl on the board […] Especially in group theory [taught from (Wigner 1931) in the original German], his intuitive feeling for the subject often bewildered us as he scribbled […] in an offhand shorthand to demonstrate what we thought were exceedingly abstruse points. (Anderson 1987, 524)

Anderson's assessment is actually consistent with Kuhn's, even though the latter evidently did not share his fellow student's enthusiasm for the unique style of their advisor: "One of the courses that I then took was group theory with Van Vleck. And I found that

[9]We are grateful to David Huber and Chun Lin at the University of Wisconsin–Madison, two of Van Vleck's students, for providing us with copies of these revisions.

[10]In the never completed manuscript of the revised edition, all of chap. V was "reduced to a single section of four typewritten pages" (Fellows 1985, 263).

somewhat confusing […] Van Vleck was not a terribly good teacher" (Baltas, Gavroglu, and Kindi 2000, 272).

Van Vleck's teaching style must have been less idiosyncratic in his earlier years. As Robert Serber, who studied with Van Vleck in Madison in the early 1930s (cf. fig. 7.2), wrote in the preface to his famous *Los Alamos Primer*:

> John Van Vleck was my professor at Wisconsin. The first year I was there he gave a course in quantum mechanics. No one wanted to take a degree that year. Everyone put it off because it was useless—there weren't any jobs. The next year Van had the same bunch of students, so he gave us advanced quantum mechanics. The year after that he gave us advanced quantum mechanics II. Van was extremely good, a good teacher and an outstanding physicist.[11] (Serber 1992, xxiv)

Anderson offered the following explanation for Van Vleck's effectiveness as a teacher:

> In all of his classes […] he used two basic techniques of the genuinely good teacher. First, he presented a set of carefully chosen problems […] Second, he supplied a "crib" for examination study, which we always thought was practically cheating, saying precisely what could be asked on the exam. It was only after the fact that you realized that it contained every significant idea of the course. (Anderson 1987, 524–525)

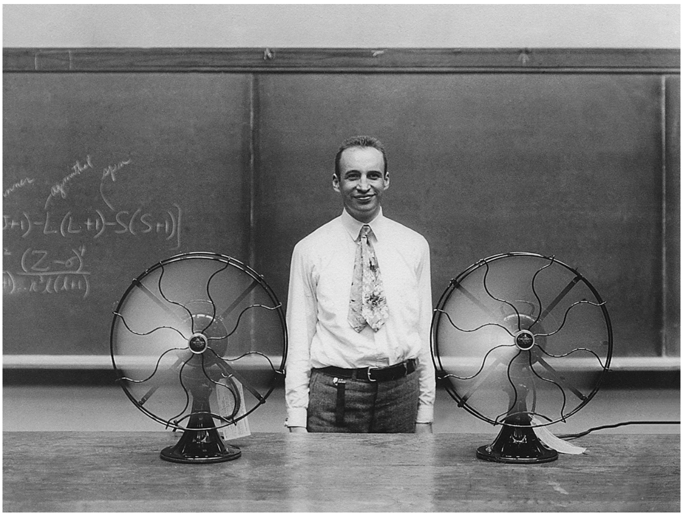


Figure 7.2: Van Vleck between two fans at 1300 Sterling Hall, University of Wisconsin–Madison, ca. 1930 (picture courtesy of John Comstock).

[11] Serber told Charles Weiner and Gloria B. Lubkin the same thing during an interview for the American Institute of Physics, 10 February 1967. As he put it in the interview, it was "always the same gang hanging on" (Fellows 1985, 294). As Van Vleck (1971, 17) noted with obvious relish about Serber: "One now identifies the present President of the American Physical Society with high energy physics, but before he fell under the influence of Oppenheimer at Berkeley, he worked on problems that today would be considered chemical physics."

Even before the Great Depression, students sometimes took Van Vleck's quantum course more than once. Robert B. Whitney, whose notes for the 1927–28 edition of the course in Minnesota (see note 7) support the kinder of Anderson's two assessments of Van Vleck's teaching quoted above, recalled that two advanced graduate students, Edward L. Hill and Vladimir Rojansky, attended the lectures the year he took the course, even though they both had to have taken it before (Fellows 1985, 175–176). Under Van Vleck's supervision, Hill and Rojansky wrote dissertations on topics in molecular and atomic spectroscopy, respectively, using the new quantum mechanics (ibid., 177, 181). Upon completion of his degree Hill went to Harvard as a postdoc to work with Van Vleck's Ph.D. advisor Edwin C. Kemble. Hill co-authored the second part of a review article on quantum mechanics with Kemble (Kemble 1929; Kemble and Hill 1930), which became the basis for the latter's quantum textbook (Kemble 1937). In the preface, Kemble wrote that he was "particularly indebted" to Van Vleck, by then his colleague at Harvard, "for reading the entire manuscript and constant encouragement" (ibid.). Rojansky (1938) wrote a textbook that had gone through eight printings by 1957. In the preface, he acknowledged the influence of Van Vleck's courses.

In his first year at Madison, 1928–29, Van Vleck immediately started supervising two postdocs, Kare Frederick Niessen and Shou Chin Wang, and two graduate students, probably J. V. Atanasoff and Amelia Frank (Fellows 1985, 230).[12] He co-authored papers with several of them, mostly related to his work on susceptibilities. Contributions by all four are acknowledged in his 1932 book. After its publication, Van Vleck continued to pursue research on susceptibilities, often in collaboration with students and postdocs (Van Vleck 1971, 13, 17). In fact, in 1932, ten graduate students (among them Serber and Olaf Jordahl) and three postdocs (Françoise Dony, William Penney, and Robert Schlapp) were working with Van Vleck (Fellows 1985, 294–295).

Physics 212, "Quantum mechanics and atomic structure," was the only lecture course Van Vleck offered during his first few years in Wisconsin (ibid., 230). It was not until 1931–33, the period described by Serber, that Physics 232, "Advanced Quantum Mechanics," and Physics 233, "Continuation of Advanced Quantum Mechanics," were added (ibid., 294). Among the students taking the basic course in 1928–29 was John Bardeen (ibid., 230). Walter H. Brattain had taken the course in Minnesota the year before (ibid., 176). So two of the three men who won the 1956 Nobel Prize for the invention of the transistor, Bardeen and Brattain, as beginning graduate students took quantum mechanics with Van Vleck. The Ph.D. advisor of the third, William B. Shockley, was Slater, Van Vleck's most important fellow graduate student at Harvard. This underscores the importance of the first generation of quantum physicists in the United States for the education of the next.

7.1.7 Structure of Our Paper

The balance of this paper is organized as follows. In sec. (7.2), we sketch Van Vleck's early life against the backdrop of theoretical physics coming of age and maturing in the United States. Our main focus is on his years in Minneapolis leading up to the writing of his NRC *Bulletin* (1923–26). Throughout the paper, but especially in the more biographical secs. (7.2) and (7.4), we make heavy use of the superb dissertation on Van Vleck by Fred

[12] In 1936, Amelia Frank married Eugene Wigner, who had joined the faculty in Madison that year after having been dismissed by Princeton. She died only nine months later. Wigner thereupon accepted a new offer from Princeton, made at the recommendation of Van Vleck, who by this time was at Harvard (Wigner and Szanton 1992, 171–179).

Fellows (1985). In sec. (7.3), we turn to the *Bulletin* itself (Van Vleck 1926b). In sec. (7.3.1), we recount how what had originally been conceived as a review article of average length eventually ballooned into a 300-page book. In sec. (7.3.2) we give an almost entirely qualitative discussion of its contents, focusing on the derivation of Kramers's dispersion formula with the help of the correspondence-principle technique central to the book. For the details of this derivation we refer to Duncan and Janssen (2007, cf. note 28 below). In sec. (7.4), we return to Van Vleck's biography. We describe the years following the *Bulletin*'s publication, his move from Minneapolis to Madison, and the development of his expertise in the theory of susceptibilities. In sec. (7.5), we discuss his book on susceptibilities (Van Vleck 1932b). The structure of sec. (7.5) mirrors that of sec. (7.3). In sec. (7.5.1), we recount how Van Vleck came to write his second book. In secs. (7.5.2–7.5.5), we discuss its content, not just qualitatively in this case but carefully going through various derivations. We focus on the vicissitudes of the Langevin-Debye formula in the transition from classical to quantum theory. In sec. (7.6), we briefly revisit the Kuhnian themes introduced above and summarize our findings.

7.2 Van Vleck's Early Life and Career

John Hasbrouck Van Vleck (1899–1980) was born in Middletown, Connecticut, to Edward Burr Van Vleck and Hester Laurence Van Vleck (*née* Raymond). In 1906 the family moved to Madison, Wisconsin, where his father was appointed professor of mathematics.[13] He had been named after his grandfather, John Monroe Van Vleck, but his mother, not fond of her father-in-law, called him Hasbrouck (Fellows 1985, 6–8). To his colleagues, he would always be Van. A nephew of Van's wife, Abigail June Pearson (1900–1989), recalls that a telegram from Japan congratulating Van Vleck on winning the Nobel prize was addressed to "Professor Van" (John Comstock, private communication).

In 1916 Van Vleck began his undergraduate studies at the University of Wisconsin, where he eventually majored in physics. In the fall of 1920, he enrolled at Harvard as a graduate student in physics.[14] He took Kemble's course on quantum theory and soon found himself working toward a doctorate under Kemble's supervision. In a biographical note accompanying the published version of his Nobel lecture, Van Vleck (1992a, 351) noted that Kemble "was the one person in America at that time qualified to direct purely theoretical research in quantum atomic physics." Indeed, it seems as though his course on quantum mechanics was the only one of its kind in America at the time. The course closely followed

[13] Van Vleck Hall on the University of Wisconsin–Madison campus is named for E. B. Van Vleck.

[14] In addition to three courses in physics, Van Vleck signed up for a course on railway operations in the Harvard Business School (AHQP interview, session 1, 3). As his wife Abigail recalled, Van Vleck abandoned the notion of pursuing a career in railroad management when the instructor asked him point blank whether he or anyone in his family actually owned a railroad (Fellows 1985, 16). Van Vleck, however, retained his fascination with railroads for the rest of his life. His knowledge of train schedules became legendary (Anderson 1987, 503). Years later, now on the faculty at Harvard, he told a colleague, the renowned historian of science I. Bernard Cohen, which trains to take on an upcoming trip. Although the information Van Vleck supplied, apparently off the top of his head, turned out to be perfectly accurate, Cohen was puzzled when he reached his destination and was told by his host that he could have left an hour later, yet arrived an hour earlier, had he taken a different combination of trains. Upon his return to Cambridge, Cohen confronted Van Vleck with this intelligence. Van Vleck was undaunted. "Of course," he is reported to have said, "but wasn't that the best beef lunch you ever had?" (We are grateful to Roger Stuewer for telling us this story, which he heard from I. B. Cohen.)

the "Bible" of the old quantum theory, *Atombau und Spektrallinien* (Sommerfeld 1919).[15] Kemble's 1917 dissertation had been the first predominantly theoretical dissertation in the United States. Even Bohr and Sommerfeld had taken notice of Kemble's work by 1920. When Van Vleck finished his doctorate, just before the summer of 1922, he was solidly grounded in classical physics, especially in advanced techniques of celestial mechanics, but more importantly, he had brought these skills to bear on quantum theory. His dissertation, which was published in the *Philosophical Magazine* (Van Vleck 1922), was on a "crossed-orbit" model of the helium atom, and he had worked with Kemble to calculate the specific heat of hydrogen shortly afterward. Neither of these calculations had agreed well with experiment, but at the time Van Vleck's results were among the best to be found. It would take the advent of matrix mechanics in 1925 before the crossed orbit model was superseded, and before theoretical predictions for the specific heat of hydrogen could be brought into alignment with experiment (Gearhart 2010).[16]

The following year, Van Vleck accepted a position as an instructor in Harvard's physics department. This demanding job left him with little time for his own work. Most of his time was spent preparing for lectures and lab sessions (Fellows 1985, 49). In the midst of this daily grind, the job offer that arrived from the University of Minnesota in early 1923 must have looked especially attractive. As Van Vleck (1992a, 351) would reflect later, it was an "unusual move" for such an institution at that time—indicative, one may add, of the American physics community's growing recognition of the importance of quantum theory—to offer him an assistant professorship "with purely graduate courses to teach."

At first, Van Vleck was hesitant to accept the position.[17] He and Slater had planned to tour Europe together on one of the fellowships then available to talented young American physicists. In the end, however, and partly on the strength of his father's advice, he accepted the Minnesota offer. After a summer in Europe with his parents (during which he managed to meet some of the most visible European theorists), he arrived in Minneapolis, ready for the fall semester in 1923. His teaching load was indeed light. One might expect that he would thus have pursued his own research with a renewed focus. Initially, that is exactly what he did.

In October 1924, after a preliminary report in the *Journal of the Optical Society of America* (Van Vleck 1924a), a two-part paper appeared in *Physical Review* in which Van Vleck (1924b; 1924c) used correspondence-principle techniques to analyze the interaction between matter and radiation in the old quantum theory. Its centerpiece was Van Vleck's own correspondence principle for absorption, but the paper also contains a detailed deriva-

---

[15] Sommerfeld sent a copy of the English translation of the third edition of his book to the University of Minnesota. This copy is still in the university's library. He dedicated it to the graduate students of the University of Minnesota, which had been one of the earlier stops on his 1922–23 tour of American universities (see Michael Eckert's contribution to this volume). The dedication is signed Munich, 16 October 19[23] (the last two digits, unfortunately, have been cut off). By the time this copy of Sommerfeld's book arrived at the University of Minnesota, Van Vleck, as we will see, had joined its faculty.

[16] As Gearhart (2010) concludes, "the story [of the specific heat of hydrogen] reminds us that the history of early quantum theory extends far beyond its better known applications in atomic physics" (193). This underscores the remark by Van Vleck with which we opened our paper about physicists in the early 1920s focusing strongly on spectroscopy. Although, as Gearhart shows, it drew much more attention in the old quantum theory than the problem of susceptibilities, the problem of specific heat is discussed only in passing by Van Vleck (1926b, 101–102) in his NRC *Bulletin*. There actually are some interesting connections between these two non-spectroscopic problems (see note 72).

[17] AHQP interview, session 1, 14.

tion of the Kramers dispersion formula. Although Born (1924) had published a derivation of the formula that August, he and Van Vleck arrived at the result independently of one another (Duncan and Janssen 2007, 590). The quantum part of this paper by Van Vleck (1924b) and the BKS paper (Bohr, Kramers, and Slater 1924) are the only two papers with American authors that are included in a well-known anthology documenting the transition from the old quantum theory to matrix mechanics (Van der Waerden 1968). The breakthrough Heisenberg (1925b) achieved with his *Umdeutung* paper can be seen as a natural extension of the correspondence-principle techniques used by Kramers, Born, and Van Vleck (see sec. 7.3.2 below and Duncan and Janssen 2007).

After his 1924 paper, however, Van Vleck did not push this line of research any further. He had meanwhile been 'invited' to produce the volume to which we now turn our attention. Its completion would occupy nearly all of his available research time for the next two years.

7.3 The NRC *Bulletin*

7.3.1 Writing the *Bulletin*

Later in life, when interviewed by Kuhn for the AHQP, Van Vleck recalled writing his NRC *Bulletin* over the course of about two years:

> I was already writing some chapters on that on rainy days in Switzerland in 1924. I would say I started writing that perhaps beginning in the spring of 1924, and finished it in late 1925. I worked on it very hard that summer [...] I was sort of a "rara avis" at that time. I was a young theoretical physicist presumably with a little more energy than commitments than the older people interested in these subjects, so they asked me if I'd write this thing. I think it was by invitation rather than by my suggestion.[18]

The invitation had come from Paul D. Foote of the U.S. Bureau of Standards, who was the chairman of the NRC Committee on Ionization Potentials and Related Matters. Van Vleck served on this committee in the fall of 1922 (Fellows 1985, 49). These NRC committees, Van Vleck recalled, had been created because "there *was* a feeling among the more sophisticated of the American physicists that we were behind in knowing what was going on in theoretical physics in Europe."[19]

The committees organized the *Bulletins* of the NRC, which existed to present "contributions from the National Research Council [...] for which hitherto no appropriate agencies of publication [had] existed" (Swann et al. 1922, 173–174). This sounds rather vague and overly inclusive, and on reading the motley assortment of topics covered by the *Bulletins* through 1922, one finds that it *was* rather vague and overly inclusive. The *Bulletins* served to disseminate whatever information the myriad committees deemed important. A brief list of topics covered by these publications includes "The national importance of scientific and industrial research," "North American forest research," "The quantum theory," "Intellectual and educational status of the medical profession as represented in the United States Army," and "The scale of the universe" (ibid.). The *Bulletins* tended to be short, averaging about

[18] AHQP interview, session 1, 21.

[19] AHQP interview, session 1, 21, emphasis in the original.

75 pages. Several were even shorter, coming in under 50 pages. The longest at the time Van Vleck was invited to write one on line spectra was a 172-page book, *Electrodynamics of Moving Media* (Swann et al. 1922). It had been written by four authors, including John T. (Jack) Tate, Van Vleck's senior colleague in Minnesota, and W. F. G. Swann, Van Vleck's predecessor in Minnesota.

Given the *Bulletin*'s publication history, Van Vleck was not making an unreasonable commitment when he accepted Foote's invitation. Initially, his contribution was only to be a single paper in a larger volume on "Ionization Potentials and Related Matters."[20] It is unclear exactly how the paper spiraled out of control and became the quagmire of a project that consumed over two years of his available research time, but an interesting story is suggested by his correspondence.

As we saw, Van Vleck later recalled having begun his *Bulletin* in the spring of 1924, but he must have started much earlier than that. In March 1924, Foote returned a draft to Van Vleck along with extensive comments. "This has been read very carefully by Arthur E. Ruark," Foote wrote, "who has prepared a long list of suggestions as enclosed. These are merely suggestions for your consideration. On some of them I do not agree with Ruark but many of his suggestions are of considerable interest."[21] Foote was probably distancing himself from Ruark's remarks not only because of their severity, but also because of their sheer volume. The "suggestions" amounted to 33 pages of typed criticism. Van Vleck's handwritten reactions are recorded in the margins of Ruark's commentary (preserved in the AHQP). Exclamation points and question marks abound, often side by side, punctuating Van Vleck's surprise and confusion. Here and there, he makes an admission when a suggestion seems prudent. For the most part, however, Ruark's suggestions are calls for additional details and clarification, more derivations, in short, a significant broadening of the "article." As one reads on, Van Vleck's annotations become less and less frequent. When they appear at all, they often amount to a single question mark. One gets the impression of a young physicist brow-beaten into submission. This is likely what precipitated the transformation of Van Vleck's *Principles* from review article to full-fledged book.

Perhaps Foote was still expecting a paper, but Van Vleck was producing something much more comprehensive. By November, Foote was becoming impatient. Van Vleck wrote to reassure him:

> Like you I "am wondering" when my paper for the Research Council will ever be ready. I am sorry to be progressing so slowly but I hope you realize that I am devoting to this report practically all of my time not occupied with teaching duties. I still hope to have the manuscript ready by Christmas except for finishing touches.[22]

Van Vleck would blow the Christmas deadline as well. It was not until August that he submitted a new draft:

> I hope the bulletin will be satisfactory, as with the exception of one three-month period it has taken all my available research time for two years.
>
> You wrote me that the bulletin should be "fairly complete." My only fear is

[20] Foote to Van Vleck, 22 March 1924, AHQP.
[21] Foote to Van Vleck, 22 March 1924, AHQP.
[22] Van Vleck to Foote, 21 November 1924, AHQP.

> that it may be too much so. I made sure to include references to practically all the important theoretical papers touching on the subjects covered in the various chapters. Four new chapters have been included since an early draft of the manuscript was sent to you a year ago […]
> You will note that I have used a new title "Quantum Principles and Line-Spectra" as this is much briefer and perhaps more a-propos than "The Fundamental Concepts of the Quantum Theory of Line-Spectra."[23]

It is worth noting the change in title. The old quantum theory was strongly focused on the phenomena of line spectra. Van Vleck's new title conveys at once this focus even as he had significantly broadened the scope of his project.

Even when Foote sent him the galleys for inspection, Van Vleck could not resist making further additions to the *Bulletin*. "I have added 13 pages of manuscript […] in which I have tried to summarize the work of Heisenberg, Pauli, and [Friedrich] Hund," Van Vleck wrote back. "I am sorry to make such an addition," he explained, "but quantum theory progresses extremely rapidly, and I hope the new subject-matter will add materially to the value of the report."[24]

It is clear that however the project began, and whatever Van Vleck's initial expectations, in the end the *Bulletin* was intended by its author as a comprehensive and up-to-date review of quantum theory. This makes it useful not only as a review of the old quantum theory, but also as a window into Van Vleck's own perception and understanding of the field.

Despite some critical notes,[25] the *Bulletin* was "on the whole, well-received" (Fellows 1985, 88). Van Vleck must have read Ruark's review of his *Bulletin* in the *Journal of the Optical Society of America* with special interest, given Ruark's litany of complaints about an early draft of it. Ruark praised the final version as a thorough, clearly written, state-of-the-art survey of a rapidly changing field:[26]

> This excellent bulletin will prove extremely useful to all who are interested in atomic physics […] [T]he fundamental theorems of Hamiltonian dynamics and perturbation methods of quantization are treated in a very readable fashion […] The chapter on the quantization of neutral helium is authoritative […] The author's treatment of the "correspondence principle" is refreshingly clear […] The whole book is surprisingly up-to-date. Even the theory of spinning electrons and matrix dynamics are touched upon. It is to be hoped that this report will run through many revised editions as quantum theory progresses, for it fills a real need. (Ruark 1926)

In fact, Ruark's main complaint was directed not at the author but at the publisher: "Incidentally, many physicists would appreciate the opportunity of buying National Research Bulletins in a more durable binding" (ibid.). Yet, the review also hints at lingering disagreements between author and reviewer. Most importantly, Ruark had his doubts about the Kramers dispersion theory which Van Vleck had used in the *Bulletin* to showcase the power of the correspondence principle:

[23] Van Vleck to Foote, 10 August 1925, AHQP.
[24] Van Vleck to Foote, 2 February 1926, AHQP.
[25] See, e.g., the quotation from Smekal's (1927) review in sec. (7.1.4) above.
[26] Ruark approvingly referred to Van Vleck's *Bulletin* a number of times in a book he wrote with Harold Urey a few years later (Ruark and Urey 1930).

> Many readers will not agree with the author's conclusion that "Kramers's dispersion theory […] furnishes by far the most satisfactory theory of dispersion" [Van Vleck 1926b, 156–157] […] the reviewer believes that a final solution cannot be achieved until we have a much more thorough knowledge of the dispersion curves of monatomic gases and vapors. (Ruark 1926)

Subsequent developments would prove that Van Vleck's confidence in the Kramers dispersion formula was well-placed. It carried over completely intact to the new quantum mechanics (Duncan and Janssen 2007, 655).

7.3.2 The *Bulletin* and the Correspondence Principle

The central element in Van Vleck's presentation of the old quantum theory in his NRC *Bulletin* is the correspondence principle. It forms the basis of 11 out of a total of 13 chapters.[27] As it says in the preface,

> Bohr's correspondence principle is used as a focal point for much of the discussion in Chapters I–X. In order to avoid introducing too much mathematical analysis into the discussion of the physical principles underlying the quantum theory, the proofs of certain theorems are deferred to Chapter XI, in which the dynamical technique useful in the quantum theory is summarized. (Van Vleck 1926b, 3)

The correspondence principle first emerged in the paper in which Bohr (1913) introduced his model for the hydrogen atom (Heilbron and Kuhn 1969, 268, 274). Perhaps the most radical departure from classical theory proposed in Bohr's paper was that the frequency of the radiation emitted or absorbed when an electron jumps from one orbit to another differs from the orbital frequency of the electron in both the initial and the final orbit (Duncan and Janssen 2007, 571–572). However, for high quantum numbers N, the orbital frequencies of the N-th and the $(N-1)$-th orbit and the frequency of the radiation emitted or absorbed when an electron jumps from one to the other approach each other. This is the core of what later came to be called the correspondence principle.

By the early 1920s, the correspondence principle had become a sophisticated scheme used by several researchers for connecting formulas in classical mechanics to formulas in the old quantum theory. The most important result of this approach was the Kramers dispersion formula, which Kramers (1924a; 1924b) first introduced in two short notes in *Nature*. As we mentioned in sec. (7.2), Born (1924) and Van Vleck (1924b; 1924c), independently of one another, published detailed derivations of this result a few months later. Kramers himself would not publish the details of his dispersion theory until early 1925, in a paper co-authored with Heisenberg (Kramers and Heisenberg 1925). This paper has widely been recognized as a decisive step toward Heisenberg's (1925b) *Umdeutung* paper written in the summer of 1925 (Duncan and Janssen 2007, 554).

As a concrete example of the use of the correspondence principle in the old quantum theory in the early 1920s, we sketch Van Vleck's derivation of the Kramers dispersion for-

[27] The remaining chapters deal with "Half quanta and the anomalous Zeeman effect" (chap. XII) and "Light-quants" [sic] (chap. XIII).

mula.[28] This formula and what Van Vleck (1926b, 162) called the "correspondence principle for dispersion" are presented in a section of only two and a half pages in chap. X of the NRC *Bulletin* (ibid., sec. 51, 162–164). The reason that Van Vleck could be so brief at this point is that the various ingredients needed for the derivation of the formula are all introduced elsewhere in the book, especially in chap. XI on mathematical techniques. At 50 pages, this is by far the longest chapter of the *Bulletin*.

Consider some (multiply-)periodic system—anything from a charged simple harmonic oscillator to an electron orbiting a nucleus—struck by an electromagnetic wave of a frequency ν not too close to that system's characteristic frequency ν_0 or frequencies ν_k. The Kramers dispersion formula is the quantum analogue of an expression in classical mechanics for the polarization of such a periodic system resulting from its interaction with the electric field of the incoming electromagnetic wave, multiplied by the number of such systems in the dispersive medium. This expression can easily be converted into an expression for the dependence of the index of refraction on the frequency of the refracted radiation. Optical dispersion, a phenomenon familiar from rainbows and prisms, is described by this frequency dependence of the refractive index (Duncan and Janssen 2007, sec. 3.1, 573–578).

To obtain the Kramers dispersion formula in the old quantum theory, one has to derive an expression for the instantaneous dipole moment, induced by an external electromagnetic wave, of individual (multiply-)periodic systems in classical mechanics, multiply that expression by the number of such systems in the dispersive medium, and then translate the result into an expression in the old quantum theory under the guidance of the correspondence principle.

As with all such derivations in the old quantum theory, the part involving classical mechanics called for advanced techniques borrowed from celestial mechanics. As we mentioned in sec. (7.2), Van Vleck had thoroughly mastered these techniques as a graduate student at Harvard. Decades later, when the Dutch Academy of Sciences awarded him its prestigious Lorentz medal, Van Vleck related an anecdote in his acceptance speech that demonstrates his early mastery of this material:

> In 1924 I was an assistant professor at the University of Minnesota. On an American trip, [Paul] Ehrenfest gave a lecture there [...] [He] said he would like to hear a colloquium by a member of the staff. I was selected to give a talk on my "Correspondence Principle for Absorption" [Van Vleck 1924a, 1924b, 1924c] [...] I remember Ehrenfest being surprised at my being so young a man. The lengthy formulas for perturbed orbits in my publication on the three-body problem of the helium atom [Van Vleck 1922] had given him the image of a venerable astronomer making calculations in celestial mechanics. (Van Vleck 1974, 9; quoted by Duncan and Janssen 2007, 627)

Van Vleck put his expertise in classical mechanics to good use. Using canonical perturbation theory in action-angle variables, he derived an expression in classical mechanics for

[28] For a detailed reconstruction of this derivation, which follows Van Vleck's two-part paper of 1924 rather than his 1926 NRC *Bulletin*, see (Duncan and Janssen 2007): in sec. 3.4 (591–593), an outline of the derivation is given; in secs. 5.1–5.2 (627–637), the result is derived for a simple harmonic oscillator; in sec. 6.2 (648–652), this derivation is generalized to an arbitrary multiply-periodic system; finally, in sec. 7.1 (655–658), it is shown that in modern quantum mechanics the Kramers dispersion formula holds for an even broader class of systems than in the old quantum theory.

the dipole moment of a charged multiply-periodic system hit by an electromagnetic wave of small amplitude, which could then be translated into a quantum-theoretical expression.

In general coordinates and their conjugate momenta (q_k, p_k) (where $k = 1, \ldots, f$, with f the number of degrees of freedom), Hamilton's equations are:

$$\dot{q}_k = \frac{\partial H}{\partial p_k}, \quad \dot{p}_k = -\frac{\partial H}{\partial q_k}, \tag{7.1}$$

where $H(q_k, p_k)$ is the Hamiltonian and dots indicate time derivatives. Given the Hamiltonian of some multiply-periodic system, one can often find special coordinates, (w_k, J_k), called *action-angle variables*, such that the Hamiltonian in the new coordinates only depends on the new momenta, the action variables J_k, and not on the new coordinates, the angle variables w_k. In that case, Hamilton's equations take on the simple form:

$$\dot{w}_k = \frac{\partial H}{\partial J_k} = \nu_k, \quad \dot{J}_k = -\frac{\partial H}{\partial w_k} = 0. \tag{7.2}$$

The first of these equations shows what makes the use of action-angle variables so attractive in celestial mechanics. It makes it possible to extract the characteristic periods of the system from the Hamiltonian without having to know the details of the orbit.

Action-angle variables played a central role in the old quantum theory. They are used to formulate the Sommerfeld-Wilson(-Ishiwara) quantum conditions (Van Vleck 1926b, 39–40), which select the orbits allowed by the old quantum theory from all classically allowed ones. The relation between the new momenta J_k and the original position and momentum variables q_k and p_k is: $J_k = \oint p_k dq_k$, where the integral is over one period of the motion. The Sommerfeld-Wilson quantum conditions restrict the classically allowed orbits to those satisfying

$$J_k = \oint p_k dq_k = n_k h, \tag{7.3}$$

where h is Planck's constant and the n_k's are integers.

For orbits with high values for all quantum numbers, there is only a small difference between the values of the Hamiltonian for $J_l = N_l h$ and for $J_l = (N_l \pm 1)h$ (with the values of all J_m's with $m \neq l$ fixed). The differential quotients $\partial H / \partial J_l = \nu_l$ in the first equation in eq. (7.2) can then be approximated by difference quotients:

$$\nu_l \approx \frac{H(J_1, \ldots, J_l = (N_l + 1)h, \ldots, J_f) - H(J_1, \ldots, J_l = N_l h, \ldots, J_f)}{h}. \tag{7.4}$$

The two values of the Hamiltonian in the numerator give the energies E_{N_l} and E_{N_l+1} of two orbits, close to each other, with high values for all quantum numbers (all, except for the l-th one, equal for the two orbits). Eq. (7.4) is thus of the form:

$$h\nu_l = E_{N_l+1} - E_{N_l}. \tag{7.5}$$

In the limit of high quantum numbers, this equation for the orbital frequency ν_l of the electron—and thereby, according to classical electrodynamics, the frequency of the radiation emitted because of the electron's acceleration in that orbit—coincides with Bohr's rule,

$$h\nu_{i\rightarrow f} = E_{n_i} - E_{n_f}, \tag{7.6}$$

for the frequency $\nu_{i\rightarrow f}$ of the radiation emitted when an electron jumps from an initial orbit (quantum number n_i) to a final orbit (quantum number n_f). This asymptotic connection between this classical formula for the orbital frequencies ν_l and Bohr's quantum formula for the radiation frequencies $\nu_{i\rightarrow f}$ is what Van Vleck (1926b, sec. 11, 23–24) called "the correspondence theorem for frequencies."

Such asymptotic connections can be used in two ways, either to *check* that a given quantum formula reduces to its classical counterpart in the limit of high quantum numbers, or to make an educated *guess* on the basis of the classical formula assumed to be valid for high quantum numbers as to what its quantum-theoretical counterpart, valid for all quantum numbers, might be. While Born (1924; 1925) emphasized the latter *constructive* use, Van Vleck (1924b; 1924c; 1926b) preferred the former *corroborative* use (Duncan and Janssen 2007, 638–640). The correspondence theorem for frequencies is a good example of the corroborative use of correspondence-principle arguments, the Kramers dispersion formula is the prime example of their constructive use.[29]

To derive a formula for the classical dipole moment from which its counterpart in the old quantum theory can be constructed (or against which it can be checked), one treats the electric field of the electromagnetic wave striking the periodic system under consideration as a small perturbation of the system in the absence of such disturbances. The full Hamiltonian H is then written as the sum of an unperturbed part H^0 and a small perturbation $H^{int} \ll H^0$ (where 'int' stands for 'interaction'). Using action-angle variables in such perturbative calculations, one can derive the formula for the classical dipole moment without having to know anything about the dynamics of the unperturbed system other than that it is solvable in these variables.[30]

Once again, Born (1924; 1925) and Van Vleck (1924b; 1924c; 1926b) proceeded in slightly different ways. Born tried to find action-angle variables (w, J) for the full Hamiltonian H, Van Vleck continued to use action-angle variables (w_k^0, J_k^0) for the unperturbed Hamiltonian H^0 even when dealing with the full Hamiltonian H. As Van Vleck (1926b, 200) explicitly noted, H will in general depend on both the J_k^0's and the w_k^0's, so (w_k^0, J_k^0) are *not* action-angle variables for H, but one can still use them to describe the behavior of the full system with interaction.[31] As we will see in sec. (7.5.3), Van Vleck (1932b, 38) like-

[29]Ruark (1926) picked up on this distinction in his review of the *Bulletin*. Elaborating on his praise for the "refreshingly clear" treatment of the correspondence principle (see the quotation at the end of sec. 7.3.1), he explained that Van Vleck "takes pains to point out that certain asymptotic connections between quantum theory and classical dynamics can be definitely proved, while other connections are only postulated. Thus he distinguishes carefully the correspondence *theorem* for frequencies, and the correspondence *postulates* for intensities and polarization." Cf. (Ruark and Urey 1930, chap. VI, sec. 2, "The correspondence theorem and the correspondence principles").

[30]The calculation of the effect of external fields on spectra, such as the Stark and Zeeman effects in atoms with one electron in external electric and magnetic fields, respectively, proceeds along similar lines (Van Vleck 1926b, chap. V).

[31]This choice of variables is analogous to the one made in the Dirac interaction picture in time-dependent perturbation theory in modern quantum mechanics (Duncan and Janssen 2007, 655, note 204).

wise used action-angle variables for the unperturbed Hamiltonian in his later calculations of susceptibilities.[32]

The classical formula Van Vleck eventually arrived at for the dipole moment of a multiply-periodic system has the form of a derivative with respect to the action variables J_k^0 of an expression involving squares of the amplitudes of the Fourier components and the characteristic frequencies $\nu_k^0 = \dot{w}_k^0$ of the motion of the unperturbed system. The correspondence principle, as it was understood by Kramers, Born, Van Vleck, and others in the early 1920s, amounted to the prescription to make three substitutions in this classical formula to turn it into a formula in the old quantum theory that is guaranteed to merge with the classical formula in the limit of high quantum numbers (Duncan and Janssen 2007, 635):

1. Replace the characteristic frequencies ν_k^0, the orbital frequencies of the motion in the unperturbed multiply-periodic systems under consideration, by the frequencies $\nu_{i \to f}$ of the radiation emitted in the transition from the n_i-th to the n_f-th orbit.

2. Replace squares of the amplitudes of the Fourier components of this motion by transition probabilities given by the A coefficients for spontaneous emission in the quantum theory of radiation proposed by Einstein (1917).

3. Replace the derivatives with respect to the action variables J_k^0 by difference quotients as in eq. (7.4). This last substitution is often attributed to Born but it was almost certainly discovered independently by Born, Kramers, and Van Vleck (Duncan and Janssen 2007, 637–638, 668).

Although this construction guarantees that the quantum formula merges with the classical formula for high quantum numbers, it still took a leap of faith to assert that the quantum formula would continue to hold all the way down to small quantum numbers. In the case of the Kramers dispersion formula, however, there were other considerations, besides this correspondence-principle argument for it, that inspired confidence in the result.

As mentioned in sec. (7.1.3), the Kramers dispersion formula amounted to the recovery of a Kuhn loss. Experiments clearly showed that the frequency ranges where dispersion becomes anomalous (i.e., where the index of refraction gets smaller rather than larger with increasing frequency) are around the absorption frequencies of the dispersive material. The classical dispersion theory of von Helmholtz, Lorentz, and Drude of the late-nineteenth century was designed to capture this feature. The theory posited the existence of small charged harmonic oscillators inside matter with characteristic frequencies corresponding to the material's absorption frequencies. Instead of such harmonically bound electrons, the Bohr model of the atom had electrons orbit a nucleus as in a miniature solar system. When Sommerfeld (1915; 1917), Debye (1915), and Davisson (1916) adapted the classical dispersion theory to this new model of matter, they were inexorably led to the conclusion that dispersion should be anomalous in frequency ranges around the orbital frequencies ν_k^0 of the Bohr atom, which, as noted above, differ sharply from the absorption and transition frequencies $\nu_{i \to f}$, at least for small quantum numbers. This is the Kuhn loss in dispersion theory mentioned in sec. (7.1.3). As long as the old quantum theory could boast of successes in spectroscopy, the problem with dispersion could be ignored. In the early 1920s, however, physicists started to take it more seriously (see, e.g., the comments by Epstein quoted in sec. 7.1.3). Ladenburg (1921)

[32]Those calculations, however, involve time-*independent* perturbation theory (cf. note 31).

and Ladenburg and Reiche (1923) proposed a new quantum dispersion theory in which they simply assumed that dispersion would be anomalous in frequency intervals around the transition frequencies $\nu_{i\to f}$ rather than the orbital frequencies ν_k^0. Kramers (1924a; 1924b), in effect, generalized the formula that Ladenburg (1921) had proposed and, in the process, provided it with the theoretical underpinnings it had been lacking before. And thus was the Kuhn loss in dispersion theory recovered.

The recovery of this Kuhn loss is a good example of what Kuhn (1996, 105) described as a "reversion (which is not the same as a retrogression)" to an older theory or paradigm (cf. sec. 7.1.3). Both in the classical theory and in the quantum theory, the dispersion formula contains a set of parameters, one for every absorption frequency, called the "oscillator strengths." These parameters are adjusted to give the best fit with the experimental data. In the classical theory, the "oscillator strength" for a given absorption frequency was interpreted as the number of harmonically-bound charges per atom with a resonance frequency equal to that absorption frequency. Unfortunately, this interpretation was strongly at odds with the experimental results. It was not uncommon to find values as low as 1 "dispersion electron," as these charged oscillators were called, per 200 or even per 50,000 atoms! In quantum theory, as Ladenburg (1921) first realized, the "oscillator strength" for a given absorption frequency can be interpreted as the number of transitions with transition frequencies $\nu_{i\to f}$ equal to that absorption frequency. The low values of these parameters then simply reflect that, for many frequencies $\nu_{i\to f}$, there will only be a small number of atoms in the initial excited state (Duncan and Janssen 2007, 582–583).

The correspondence-principle translation scheme outlined above was central to the research in the early 1920s of both Van Vleck (1924b; 1924c) and Born (1924). In fact, their approaches were so similar that the two men had a testy correspondence about the proper assignment of credit for various results and insights (Duncan and Janssen 2007, 569–571, 638–639). Moreover, both Born (1925) and Van Vleck (1926b) wrote a book on the old quantum theory in which they organized the material covered around the correspondence principle as they had come to understand and use it in their research.[33]

Both Born and Van Vleck missed the next step, which was to use the correspondence-principle translation scheme for the basic laws of classical mechanics rather than for individual formulas. That step would be taken by Heisenberg (1925b) in his *Umdeutung* paper (Duncan and Janssen 2007, sec. 3.5, 593–596; sec. 8, 668). In doing so, Heisenberg abandoned electron orbits altogether and formulated his theory entirely in terms of quantum transitions, accepting, for the time being, that there was nothing in the theory to represent the states between which such transitions were supposed to take place. Born and Jordan (1925) first recognized that the two-index quantities thus introduced (referring to initial and final states of a transition) were nothing but matrices.

Since the Sommerfeld-Wilson quantum condition refers to individual orbits, Heisenberg had to find a new quantum condition. Taking the difference in the values of $\oint pdq$ of two neighboring orbits and translating this using his *Umdeutung* scheme, he arrived at a corollary of the Kramers dispersion formula that had inspired his reinterpretation scheme. This corollary had been found independently by Werner Kuhn (1925) and Willy Thomas (1925) and is known as the Thomas-Kuhn sum rule (which thus has nothing to do with Thomas S. Kuhn). Born and Jordan (1925) showed that the Thomas-Kuhn sum rule is

[33] Born's book is analyzed in Domenico Giulini's contribution to this volume.

equivalent to the diagonal elements of the basic commutation relations, $[p, q] = \hbar/i$, for position and momentum in matrix mechanics (Duncan and Janssen 2007, 659–660). The Thomas-Kuhn sum rule, as Van Vleck (1926b) noted ruefully in his NRC *Bulletin*, "appears to have first been incidentally suggested by the writer" (152). It can be found in a footnote in the classical part of his two-part paper on his correspondence principle for absorption (Van Vleck 1924c, 359–360; cf. Duncan and Janssen 2007, 595–596, 668). By 1924, Van Vleck thus had the two key physical ingredients of Heisenberg's *Umdeutung* paper, the Kramers dispersion formula and the Thomas-Kuhn sum rule. In a very real sense, he had been on the verge of *Umdeutung* (Duncan and Janssen 2007).

Van Vleck apparently told his former student Kuhn in the early 1960s that, had he been "a little more perceptive," he could have done what Heisenberg did. When Kuhn reminded him of that boast during the official interview for the AHQP in 1963, Van Vleck backed off and told his interviewer: "Perhaps I should say *considerably* more perceptive."[34]

Born was not that modest. In the preface to the 1927 English translation of his 1924 book, he claimed that "discussions with my collaborators Heisenberg, Jordan, and Hund which attended the writing of this book have prepared the way for the critical step which we owe to Heisenberg" (Born 1927, xi–xii). Even though it is not clear how much Heisenberg's *Umdeutung* paper owes to these discussions with Born, there is no doubt that Born already recognized the limitations and the provisional character of the old quantum theory when he turned his lectures on 'atomic mechanics'[35] of 1923/1924 into a book. In the preface, dated November 1924, he wrote:

> [T]he work is deliberately conceived as an attempt […] to ascertain the limit within which the present principles of atomic and quantum theory are valid and […] to explore the ways by which we may hope to proceed […] [T]o make this program clear in the title, I have called the present book "Vol. I;" the second volume is to contain a closer approximation to the "final" atomic mechanics […] The second volume may, in consequence, remain for many years unwritten. In the meantime let its virtual existence serve to make clear the aim and spirit of this book. (Born 1925, v)

By the time the English translation of Born's book was ready to be sent to press two years later, both matrix mechanics and wave mechanics had arrived on the scene. In the preface to the translation, dated January 1927, Born addressed the question whether, given these developments, "the appearance of an English translation is justified" (Born 1927, xi). He believed it was, on three grounds:

> [I]t seems to me that the time is [sic] not yet arrived when the new mechanics can be built up on its own foundations, without any connection with classical theory […] Further, I can state with a certain satisfaction that there is practically nothing in the book which I wish to withdraw. The difficulties are always openly acknowledged […] Lastly, I believe that this book itself has contributed in some

[34] AHQP interview, session 1, 24, quoted by Duncan and Janssen (2007, 555–556).

[35] The term 'atomic mechanics' (*Atommechanik*) was chosen in analogy with the term 'celestial mechanics' (*Himmelsmechanik*) (Born 1925, preface). For the English translation, the title was rendered as *Mechanics of the Atom*, but in the text "the clumsier expression atomic mechanics has often been employed" (Born 1927, v, note).

> small measure to the promotion of the new theories, particularly those parts which have been worked out here in Göttingen.[36] (Born 1927, xi)

Quantum mechanics continued to develop rapidly in the late 1920s (Duncan and Janssen 2013). Only three years after the English translation of his 1924 book, the sequel Born had promised in the preface to the original German edition appeared. The book, co-authored with his former student Jordan, who had meanwhile emerged as one of the leading young quantum theorists, is entitled *Elementary Quantum Mechanics*: *Lectures on Atomic Mechanics,* Vol. 2. In the preface, Born and Jordan explained that

> [t]his book is the continuation of the "Lectures on atomic mechanics" published in 1925; it is the "second volume" that was announced in the preface, of which "the virtual existence should serve to make clear the aim and spirit of this book." The hope that the veil that was still hanging over the real structure of the laws of the atom would soon be parted has been realized in a surprisingly fast and thorough fashion. (Born and Jordan 1930, v)

The authors then warned their readers that they had made a conscious effort to see how much could be done with "elementary, i.e., predominantly algebraic means" (ibid., vi). In other words, elementary quantum mechanics, for Born and Jordan, was essentially matrix mechanics. They relegated wave-theoretical methods to a future book they promised to write "as soon as time and energy permit" (ibid).

In his review of *Elementary Quantum Mechanics* in *Die Naturwissenschaften*, Pauli took Born and Jordan to task for their decision to restrict themselves to matrix mechanics, adding pointedly that "one cannot reproach the reviewer on the grounds that he finds the grapes sour because they are hanging too high for him" (Pauli 1930). Pauli, after all, had solved the hydrogen atom in matrix mechanics before wave mechanics was available. The authors' promise of a future volume on wave mechanics (which never saw the light of day) provided Pauli with the perfect opening line for his review: "The book is the second volume in a series in which goal and purpose of the n-th volume is always made clear through the virtual existence of the $(n+1)$-th volume" (ibid.). Pauli's review famously ends with the observation that "the production of the book in terms of print and paper is excellent" (ibid.). Born was angry enough about this scathing review to complain about it to Pauli's teacher Sommerfeld (Duncan and Janssen 2008, 641). Pauli's negative verdict on Born and Jordan's 1930 effort stands in marked contrast to his high and unqualified praise for Van Vleck's 1932 book in the same journal a few years later.[37]

Contrary to Born, Van Vleck only seems to have realized how serious the problems facing the old quantum theory were after its demise. Talking to Kuhn in 1963, he claimed that, as early as 1924, he had a clear premonition that a drastic conceptual change was imminent: "I certainly realized, and that must have been in 1924 or possibly 1925, certainly before the academic year 1925–26—more likely 1924–25—that there was something rotten in the state of Denmark as regards the old classical quantum theory."[38] This is not the impression one gets if one looks at the text of the NRC *Bulletin*. It is true that Van Vleck was perfectly candid about the theory's failures and short-comings. He devoted an entire chapter

[36] The next sentence is the one referring to Born's discussions with Heisenberg and others quoted above.
[37] See the quotations in note 8 in sec. (7.1.4) and at the end of sec. (7.5.1).
[38] AHQP interview, session 2, 2–3.

(chap. VIII) to the problems one ran into as soon as one considered atoms with more than one electron. Van Vleck, however, remained optimistic that these problems could be solved without abandoning the basic conceptual framework of the old quantum theory.

In one of the sections of chap. VIII, sec. 35, entitled "Standard Quantum Conditions and Correspondence Theorem for Frequencies Remain Valid Even if Classical Mechanics Break [sic] Down," he wrote:

> [T]o escape from the difficulties thus encountered [in the preceding section] it appeared necessary to assume that the classical mechanics do [sic] not govern the motions of the electrons in the stationary states of atoms with more than one electron. It might seem that this *bold proposal* would invalidate the considerable degree of success already sometimes attained in complicated atoms [...] Such successful applications, however, need not be forfeited if only we assume that the Bohr frequency condition and the standard quantum conditions retain their validity, even though the motions quantized by the latter are not in accord with ordinary dynamics in atoms with more than one electron. (Van Vleck 1926b, 108, our emphasis)

As bold as Van Vleck may have thought his proposal was, by the time his *Bulletin* was in print, Heisenberg's *Umdeutung* paper had already made it clear that much more radical measures were called for, even though, as we formulated it in sec. (7.1.5), *Umdeutung* meant that the laws of mechanics were not repealed but reinterpreted. Working on his *Bulletin* in relative isolation in Minnesota, Van Vleck had not been privy to the skepticism with which electron orbits had increasingly been viewed by his European colleagues. Heisenberg and others were prepared to abandon orbits altogether. Van Vleck, by contrast, remained convinced that the old quantum theory was essentially right, and only in error concerning the specific details of the orbits.

By the time he wrote the article about the new quantum theory in the *Chemical Reviews* from which we quoted at the beginning of this paper, Van Vleck had certainly understood that the transition from the old to the new quantum theory required much more radical steps than the ones he had contemplated in his NRC *Bulletin*. As he explained to his colleagues in chemistry,

> one cannot use a meter stick to measure the diameter of an atom, or an alarm clock to record when an electron is at the perihelion of its orbit. Consequently we must not be surprised [...] that models cannot be constructed with the same kind of mechanics as Henry Ford uses in designing an automobile. (Van Vleck 1928b, 468, quoted and discussed by Duncan and Janssen 2007, 666)

In the years following the *Bulletin*'s publication, Van Vleck's perceptions of the old quantum theory would change a great deal. Specifically, he would come to see its shortcomings through the lens of his subsequent work on susceptibilities and his own accomplishments in this area as providing powerful arguments against the old and in favor of the new quantum theory.

7.4 New Research and the Move to Wisconsin

Only after the *Bulletin* was sent to press was Van Vleck able to confront matrix mechanics. By late March of 1926, he had no doubt caught up with current developments, in part through his own reading and in part through direct contact with Born, who lectured in Madison that month (Fellows 1985, 102). In January of 1926, Jack Tate, Van Vleck's senior colleague in Minnesota, had become the new editor-in-chief of the *Physical Review* (Van Vleck 1971, 7). Van Vleck joined the editorial board and assumed the responsibilities of associate editor (Fellows 1985, 105).[39] He suddenly had access to the papers of his American colleagues on fellowships overseas before they were published.

In April 1926, Van Vleck read a paper submitted to the *Physical Review* by Pauling (1926b) with a calculation of the electric susceptibility of HCl gas in the old quantum theory (Fellows 1985, 106).[40] New experimental evidence indicated that the rotation of polar molecules like HCl ought to be quantized with half quanta rather than, as Pauli (1921) had done, with whole quanta. Pauling closely followed Pauli's calculation otherwise, using the old quantum theory to quantize the angular momentum of a rotating dumbbell or rigid rotator, the model used for the diatomic molecules under consideration.[41] The results of both calculations can be found in the table in fig. (3.2). They deviated sharply from the classical value of 1/3 for the constant C in the Langevin-Debye formula.

About a month after Van Vleck read Pauling's paper, a paper by David M. Dennison (1926) came across his desk. It would be the first involving matrix mechanics to be published in the *Physical Review*. As Van Vleck recalled decades later:

> I remember in particular [Tate] showing me an article by Dennison written in Copenhagen [while on an International Education Board (IEB) fellowship] which had the matrix elements for the symmetrical top. I realized this was just what was needed to compute the dielectric constant of a simple diatomic molecule. I requested Dennison's permission to use them in advance of their appearing in print, and remember his wiring me permission to do so. I found that they made the factor C in the Debye formula [...] for the susceptibility reacquire the classical value $1/3$, replacing the nonsensical values yielded by the old quantum theory.[42] (Van Vleck 1971, 8)

Van Vleck's calculation was analogous to Pauli's and Pauling's but, relying on Dennison's results, he now quantized the angular momentum of diatomic molecules using matrix mechanics rather than the old quantum theory. He sent a quick note to *Nature* to secure priority, and in June set off for Europe, where his parents were vacationing. The summer would bring disappointment, though. In July he received a letter from the editors of *Nature*, who were "rather wary of publications by comparatively unknown authors" (Van Vleck 1968, 1235) and requested a significant reduction in the length of his note (Fellows 1985, 109). Van Vleck complied but the delay cost him his priority in publishing the result. He still vividly remembered his disappointment in 1963:

[39] See also AHQP interview, session 2, 5.

[40] Van Vleck wrote to Pauling in his capacity as associate editor to correct an error in the manuscript (Van Vleck to Pauling, April 27, 1926, AHQP; see note 76).

[41] We will discuss these calculations by Pauli (1921) and Pauling (1926b) in detail in sec. (7.5.4) (see also Fellows 1985, 141–142).

[42] Cf. the passage from the AHQP interview with Van Vleck quoted in sec. (7.1.2).

> I must confess that that rather burned me up because I felt it was quite a significant achievement in quantum theory. When I mentioned it to Bohr he said "you should have got me to endorse it, it would have gone through quicker" [see also Van Vleck (1968, 1235)]. As it was, I think [Lucy] Mensing and Pauli beat me to it on being the first to publish that factor one-third. It was essentially a triple tie, though [Ralph de Laer] Kronig had it too, all three of us.[43,44]

Van Vleck (1971) later called it a "quadruple tie" (7), adding a paper by Charles Manneback (1926). The latter, however, actually cited Mensing and Pauli (1926) and claimed priority only for having derived the result in wave rather than matrix mechanics (Manneback 1926, 564).[45] Still, the papers by Mensing and Pauli (1926), Kronig (1926a) and Manneback (1926) all made it into print before the note by Van Vleck (1926a) finally appeared in the issue of *Nature* of 14 August.[46] However, as Pauli (1933) would concede in his review of Van Vleck's 1932 book, it would fall to Van Vleck (1927a; 1932b) to show in full generality that the new quantum mechanics restored the value $1/3$ for the factor C. These 1926 papers only dealt with the special case in which the rigid rotator was used to model the gas molecules.[47]

While crossing the Atlantic in June 1926, Van Vleck finished another calculation in quantum mechanics only to discover upon reaching Copenhagen that he had been scooped by Heisenberg. He thereupon extended this perturbative calculation to higher order but was scooped again, this time by Ivar Waller (see Van Vleck 1968, 1235, and Fellows 1985, 108). For the remainder of the summer, Van Vleck worked primarily on calculating the specific heat of hydrogen, another ill-fated endeavor. Dennison would solve that puzzle (Gearhart 2010, sec. 12, 183–188). On a train from Switzerland to Paris, Van Vleck happened to run into Pauling, whom he had not met in person before (Van Vleck 1968, 1236). Pauling told Van Vleck that he had become interested in calculating electric susceptibilities for molecules of new shapes, specifically symmetrical tops. They resolved to write a joint paper on the subject, but Kronig (1926b) once again beat them to the punch (Fellows 1985, 111–112, 114–115, 148–150). Despite this losing streak, Van Vleck's work during this period did sow the seeds of further research. His 1926 note briefly mentions an application of his approach to paramagnetism (Van Vleck 1926a, 227, discussed by Fellows 1985, 152). Ultimately, a general treatment of susceptibilities, both electric and magnetic, would cement his reputation as a theorist.

When Van Vleck returned to the United States, he found that quantum theorists were in high demand and that the publication of his NRC *Bulletin* had earned him a reputation

[43] Drawing the veil of charity over his subject's 1921 paper on the topic, Pauli's biographer Charles Enz (2002) concluded: "Thus Mensing and Pauli's paper brought a long and confusing development to a close and helped establish faith in the new quantum theory" (63). Enz does not mention Van Vleck or Kronig. We will discuss the paper by Mensing and Pauli (1926) in sec. (7.5.4). This whole episode is also discussed briefly by Mehra and Rechenberg (1982b, 266–267).

[44] AHQP interview, session 2, 5.

[45] Manneback (1926, 567) acknowledged Debye's interest and encouragement in this effort to recover the formula first published by Debye (1912).

[46] Referring to the 1926 note in his 1932 book, he described it as an "abstract only" (Van Vleck 1932b, 147). See (Fellows 1985, 143–148) for detailed discussion of Van Vleck's note and a reconstruction of some of the derivations he suppressed for brevity.

[47] In sec. (7.5.5), we will give the flavor of Van Vleck's general derivation but we will only cover the details of his derivation for this special case.

as one of the few in the United States who had a grasp of the theory. He had also found time that summer to write a short report on the new quantum mechanics for the Progress Committee of the Optical Society of America (Van Vleck 1928c). Leonard R. Ingersoll at the University of Wisconsin called it "the only readable synopsis of the present situation in this difficult subject" (Fellows 1985, 162).

As Van Vleck's fame increased, he found himself wooed more and more doggedly by other universities. From the fall of 1926 through the spring of 1928, he declined offers from the University of Chicago, Princeton, and the Mellon Institute. Many of these he rejected out of a sense of loyalty to the University of Minnesota, which had been so generous to him. The department continued to recognize Van Vleck's value, following up with raises and promotions. In June 1926 he had become an associate professor, and only a year later he became a full professor. By the summer of 1927, having married Abigail June Pearson, a native Minnesotan, he had established family ties to the state as well. It took an offer from his *alma mater* to win him over, and even then he vacillated for over a year before accepting a position at the University of Wisconsin (Fellows 1985, 169–175). He arrived at Madison in time for the fall semester of 1928.

Over the same period, Van Vleck had been busy pursuing the line of inquiry that would secure him fame as an expert in magnetism. He published a three-part paper that advanced a general theory of susceptibilities (Van Vleck 1927a; 1927b; 1928a). This trilogy would form the basis for *The Theory of Electric and Magnetic Susceptibilities* (Van Vleck 1932b).

Figure 7.3: Van Vleck receiving the National Medal of Science in 1966 from President Lyndon B. Johnson with Lady Bird Johnson looking on (picture courtesy of John Comstock).

Before turning to that volume in the next section, we wrap up this section with some brief comments about Van Vleck's career after he left the Midwest. In early 1934, Van Vleck was offered an associate professorship at Harvard to replace Slater, who had left Harvard for MIT (Fellows 1985, 343). Harvard offered conditions Wisconsin could not match, not the least important of which was the renewed proximity to Kemble and Slater. Although it initially bothered him that he was not offered a full professorship right away, Van Vleck was satisfied by Harvard's assurances that he would quickly be promoted, so he and Abigail moved to Cambridge in the fall of 1934 (ibid., 350). Within a year he was made a full professor.

During World War II, Van Vleck was the head of the theory group at Harvard's Radio Research Laboratory, thinking about ways to jam enemy radar, and a consultant to MIT's much bigger Radiation Laboratory (Anderson 1987, 514). From 1945 to 1949 he was chair of Harvard's physics department (ibid., 519). In 1951, he succeeded Bridgman in the Hollis Chair of Mathematics and Natural Philosophy, a position he held until his retirement at the age of seventy in 1969. From 1951 to 1957, he served as Dean of Engineering and Applied Physics, and in 1952–53, he served a term as President of the American Physical Society (ibid.), something the more temperamental Slater never did, even though the two men were of comparable stature in the postwar American physics community. Of the many honors bestowed upon Van Vleck, we have already mentioned his share in the 1977 Nobel Prize and the Lorentz medal and will add only the National Medal of Honor, which he received out of the hands of President Lyndon B. Johnson in 1966 (see fig. 7.3).

Even though Van Vleck spent the better part of his career at Harvard, he always retained a soft spot for Minnesota and Wisconsin. Together with Roger Stuewer (University of Minnesota) and Chun Lin (University of Wisconsin–Madison), he wrote an article on the origin of the popular fight songs "On Wisconsin" and "The Minnesota Rouser." This article, which Van Vleck when talking to his co-authors would affectionally call "our *magnum opus*" (Roger Stuewer, private communication), appeared in slightly different versions in the alumni magazines of both universities (Lin, Stuewer, and Van Vleck 1977; 1980). As an undergraduate, Van Vleck had been in the Wisconsin band, probably playing the flute (Anderson 1987, 503). As a young boy, he had attended the game in Madison in November 1909 that saw the premiere of "On Wisconsin." Unfortunately for young Van Vleck, the Badgers lost that game to the visiting Gophers (Lin, Stuewer, and Van Vleck 1977, 4). When many decades later he won the Nobel Prize, Stuewer sent him a one-word telegram: "SKI-U-MAH." This is a Minnesota football cheer, which supposedly, as Stuewer had explained to Van Vleck earlier, is an old Native American war cry meaning "victory." Van Vleck wrote back that of all the congratulatory messages he had received this one was "the briefest and most to the point."[48]

7.5 The Theory of Electric and Magnetic Susceptibilities

7.5.1 Writing the 1932 Book

In 1928 Van Vleck had been thinking about writing his own book on quantum mechanics, but he became interested that fall when Ralph H. Fowler suggested that he write a book about susceptibilities for Oxford's International Series of Monographs on Physics instead. This is the same series in which Dirac's famous book on quantum mechanics appeared (Dirac 1930). The idea of expanding his 1927–28 trilogy on susceptibilities (Van Vleck 1927a; 1927b; 1928a) into a book appealed to him. As he wrote to Fowler: "These papers would,

[48] Stuewer to Van Vleck, 11 October 1977 (telegram); Van Vleck to Stuewer, 16 November 1977. We are grateful to Roger Stuewer for providing us with copies of this correspondence.

in fact, be in a certain sense the backbone of what I would have to say." Fowler and the other editors of the international monographs series were eager to accept a volume on any theoretical subject Van Vleck might "care to write about, and allow [them] to publish." Van Vleck liked the idea, but, the drawn-out process of writing the *Bulletin* still fresh in his mind, warned Fowler of the "adiabatic speed" at which he wrote.[49] The caveat was well warranted. It would take Van Vleck over three years to complete *The Theory of Electric and Magnetic Susceptibilities*.

The delays were of a different nature than the trials and tribulations that had prevented a slightly younger Van Vleck from publishing his completed "article" in the NRC *Bulletin*. This time, he made his own original research a higher priority. He also accepted several invitations to give talks in Iowa, Minneapolis, and New York. This, and supervising the research of his graduate students and postdocs, took up most of his time during the 1928–29 school-year. He did manage to squeeze in one chapter, however. "I have actually, mirab[i]le dictu, completed one chapter of my book," he wrote to Fowler in June 1929. "At such a rate, you can calculate how long it will take me to write eleven more" (Fellows 1985, 238). Clearly, Van Vleck was going to miss his original Spring 1930 deadline.

After spending the summer on research, he devoted all of his free time in the fall to the book and completed another chapter. The following spring, 1930, he negotiated a sabbatical leave in which he received half of his salary from Wisconsin, and made up the rest with a Guggenheim fellowship. He and Abigail went to Europe, making stops in England, Holland, and Germany. Finally, Van Vleck went to Switzerland while Abigail joined his parents for a tour of Italy. Unfortunately, when Van Vleck arrived at the *Eidgenössische Technische Hochschule* (ETH) in Zurich, he discovered that Pauli and other faculty were away on lengthy spring vacations (ibid, 240–241). Van Vleck turned this to his advantage:

> The janitor at the ETH, fortunately, was very friendly and arranged for me to have the use of the library. I lived comfortably at the Hotel Waldhaus Dolder, and with a portable typewriter and no distractions by colloquia, social life or sight-seeing, I probably wrote more pages of my 'Theory of Electric and Magnetic Susceptibilities' in my first month at Zurich than in any other comparable time interval. (Van Vleck 1968, 1236, quoted by Fellows 1985, 242)

When Pauli returned from vacation and heard what Van Vleck had been up to, he was dismissive. "I don't republish my papers as a book," he said (Van Vleck 1968, 1237, quoted by Fellows 1985, 242). Partly in response to this criticism, Van Vleck resolved to include more original research (Fellows 1985, 242).

In June 1930, Van Vleck received an invitation to the Sixth Solvay Congress, devoted to magnetism. In his contribution, Van Vleck (1932a) derived formulas for magnetic susceptibilities, using the same techniques he had used in his 1927–28 trilogy and would use again in his book. He did not mention the failures of the old quantum theory with one word. It is possible that this was simply because he was talking about magnetic rather than electric susceptibilities, but it may have been, at least in part, because he did not want to incur the wrath of Pauli, who was present at the meeting and whose 1921 paper, after all, was a prime example of the "wonderful nonsense" the old quantum theory had produced on the subject.

[49]The quotations are from Fowler to Van Vleck, 26 November 1928, and Van Vleck to Fowler, 28 November 1928 (Fellows 1985, 233–234).

After receiving permission from Wisconsin, he extended his trip into the fall, finally returning in October with the book almost complete. Bob Serber had already begun as a graduate student. Van Vleck started him on a research problem immediately, not realizing he was only a first-year student. The following spring, 1931, he enlisted the help of Serber and Amelia Frank (cf. note 12) in proof-reading the galleys of the book (Van Vleck 1932b, viii). True to form, Van Vleck continued to add material and make myriad corrections during these final phases. Finally, in September, the publisher wrote to him, warning that he would be billed personally if he continued to ignore the usual limit of twenty corrections per proof-sheet. He completed the corrections in December. The book was published in April 1932 (Fellows 1985, 247–248).

Reviewers immediately recognized its importance.[50] Even Pauli, whose caustic remarks about Born and Jordan's *Elementary Quantum Mechanics* we quoted in sec. (7.3.2), had nothing but praise for the volume that he had originally dismissed as a rehash of old papers. This is all the more remarkable given that Van Vleck sharply criticized Pauli's (1921) own early contribution to the subject. Pauli (1933) called Van Vleck's book "a careful and complete overview of the entire field [...] of the dielectric constant and the magnetic susceptibility" (see also the quotations in note 8). He recognized that many of the results reported in the book had first been found by Van Vleck himself, such as "the general proof for the occurrence of the numerical factor $1/3$ in the Langevin-Debye formula" (ibid.). Pauling, who shared the responsibility for the "wonderful nonsense" about susceptibilities in the old quantum theory with his near namesake, also wrote a glowing review, calling Van Vleck's book an "excellent treatise [...] written by the world's leading authority in the field" (Pauling 1932, cf. note 8). Unlike the NRC *Bulletin* (recall Ruark's complaint quoted at the end of sec. 7.3.1), the 1932 book came in a durable binding, as Pauling noted in the last line of his review: "The volume is handsomely printed, with pleasing typography and binding" (ibid.). It is tempting to read this as a tongue-in-cheek reference to the last line of Pauli's review of Born and Jordan's book (quoted in sec. 7.3.2), though Pauling's variation on this theme has none of the venom of Pauli's original.

7.5.2 The 1932 Book and Spectroscopic Stability

Van Vleck's *The Theory of Electric and Magnetic Susceptibilities* is remarkable both for the wide range of concepts it covers and techniques it assembles, and for the amount of discussion devoted to the historical development of the theories under consideration. Even though the main focus of the book is on gases, it ended up, as we mentioned in the introduction, setting "a standard and a style for American solid-state physics" (Anderson 1987, 524). As Van Vleck explained in the preface:

> At the outset I intended to include only gaseous media, but the number of paramagnetic gases is so very limited that any treatment of magnetism not applicable to solids would be rather unfruitful. (Van Vleck 1932b, vii)

In the book, Van Vleck clearly demonstrated how his general Langevin-Debye formula for susceptibilities in gases can be adapted to the study of magnetism in crystalline solids, sketching out the research program that would occupy him and his students for years to come.

[50]For discussion of the book's reception, see (Fellows 1985, 282–284).

The book can be roughly divided into two parts, separated by an interstitial aside concerning the defects and demise of the old quantum theory. Chaps. I–IV constitute the first part. Here Van Vleck surveyed the classical theories of electric and magnetic susceptibilities. In addition to marshaling resources that will be drawn from in later chapters, Van Vleck carefully examined the failings of the classical theories, motivating the quantum-mechanical approach that is developed in the book's second half. Chap. V is the interstitial aside, which we will discuss in more detail in sec. (7.5.4). Chap. VI begins the book's second half, which develops a quantum-theoretical approach to electric and magnetic susceptibilities. Like chap. XI of the NRC *Bulletin* on mathematical techniques, this chapter on "Quantum-Mechanical Foundations," is by far the longest of the book. It takes up 59 pages (chap. XI of the *Bulletin* ran to 50 pages). It is so complete that, as we mentioned in sec. (7.1.4), it was sometimes used by itself as an introductory text in courses on the new theory. Although Van Vleck's work had largely been in the tradition of matrix mechanics, his general exposition of quantum mechanics, in his book as well as in his lectures (as evidenced by the lecture notes mentioned in note 7), has none of the "Göttingen parochialism" (Duncan and Janssen 2008, 641) of Born and Jordan's (1930) *Elementary Quantum Mechanics*. As Van Vleck wrote about Chapter VI in the preface:

> I have tried to correlate and intermingle the use of wave functions and of matrices, rather than relying exclusively on the one or the other, as is too often done. It is hoped that this chapter may be helpful as a presentation of the perturbation machinery of quantum mechanics, quite irrespective of the magnetic applications.[51] (Van Vleck 1932b, viii)

Chaps. VII–XII interrogate and extend Van Vleck's general Langevin-Debye formula, sometimes with impressive numerical accuracy, as in the case of paramagnetism, where Van Vleck had made one of his most famous contributions to the field by 1932, and sometimes qualitatively with suggestions for future lines of research, as in the case of fields within crystals and ferromagnetism. Chap. XIII, finally, is devoted to some related optical phenomena. The first section of this chapter (sec. 82, 361–365) is devoted to the Kramers dispersion formula.

The book does exactly what a good textbook ought to do according to Kuhn (1996, see, e.g., the passages on 136–137 and 187 quoted in sec. 7.1.4).[52] It not only set much of the agenda for the research program envisioned by its author, it did so in the form of a pedagogically carefully constructed text, in which all the relevant theoretical and experimental literature is reviewed and all the required mathematical techniques are introduced, along with their canonical applications, all with the aim, ultimately, of preparing its readers to become active contributors to this research program themselves.

The book reflects Van Vleck's own trajectory, from his early work in the old quantum theory to the line of work in the new quantum theory that won him his reputation as one of the pioneering theorists of solid-state physics in the United States (cf. the remark by Anderson quoted above). Although he changed fields in the process, Van Vleck's journey from spectra to susceptibilities shows a remarkable continuity. To highlight this continuity, we already

[51] As one of the reviewers of the book noted, "particular attention [is] being paid to the relation between the wave and matrix methods, a combination of which, in Van Vleck's hands, has proved a powerful weapon in dealing with the problems under consideration" (Stoner 1932, 490).

[52] Cf. the characterizations of Van Vleck's book in the reviews by Pauli and Pauling quoted in note 8.

drew attention (see sec. 7.1.5) to the connection between the derivation of the Kramers dispersion formula in his early work (Van Vleck 1924b; 1924c; 1926b) and the derivation of the Langevin-Debye formula for electric susceptibilities, which played a central role in Van Vleck's work in quantum mechanics that began in 1926 and reached a milestone with his second book (Van Vleck 1926a; 1927a; 1927b; 1928a; 1932b). As we emphasized in sec. (7.1.5), it is the perturbation theory used in both derivations that provides the continuity in the transition from classical theory to the old quantum theory to modern quantum mechanics, and in Van Vleck's career move from spectra to susceptibilities. As we will see in secs. (7.5.4) and (7.5.5), it is the old quantum theory's problems with the quantization of specific periodic systems that is responsible for the discontinuity and the Kuhn loss in the area of susceptibilities, and it is the new quantum theory's systematic solution to the problem of how to quantize such systems that is behind the recovery of that Kuhn loss.

The Langevin-Debye formula for the electric susceptibility χ of some gas is

$$\chi = N\left(\alpha + \frac{\mu^2}{3kT}\right), \tag{7.7}$$

where N is the number of molecules, α is a constant, μ is the permanent electric moment of the molecule under consideration, k is Boltzmann's constant, and T is the temperature (Van Vleck 1927a, 727; 1932b, 28).[53] The first term comes from the induced moment of the molecule, resulting from the deformation of the molecule by the external electric field. The second term comes from the alignment of the permanent moment of the molecule with the field. Thermal motion will frustrate this alignment, which is expressed in the inverse proportionality to the temperature T. As Van Vleck noted when he introduced the formula in his book:

> The idea of induced polarization is an old one [...] The suggestion that part of the electric susceptibility might be due to alinement [sic] of permanent moments, resisted by temperature agitation, does not appear to have been made until 1912 by Debye [1912]. A magnetic susceptibility due entirely to the orientation of permanent moments was suggested some time previously, in 1905, by [Paul] Langevin [1905a, 1905b], and the second term of [eq. 7.7] is thus an adaptation to the electric case of Langevin's magnetic formula. (In the electric case, a formula such as [7.7] is commonly called just the Debye formula, but we use the compound title Langevin-Debye in order to emphasize that the mathematical methods which we use to derive the second term of [eq. 7.7] apply equally well to magnetic or electric dipoles). (Van Vleck 1932b, 30)

It is this temperature-dependent second term that Van Vleck was most interested in. We can write this term as

$$\frac{NC\mu^2}{kT}. \tag{7.8}$$

Both classical theory and quantum mechanics correctly predict that, under very general conditions, $C = 1/3$. The two theories agree except at very low temperatures, where the classical theory breaks down and where quantum mechanics gives deviations from $1/3$ (Van

[53] The electric susceptibility χ is related to the dielectric constant ε of the gas via: $\chi = (3/4\pi)(\varepsilon - 1)/(\varepsilon + 2)$ (see, e.g., Pauli 1921, 319; Pauling 1926b, 568; Van Vleck 1927b, 32; 1932b, 28).

Vleck 1932b, 185, 197). Other than that, the factor $1/3$ is a remarkably robust prediction of both theories. It is true for a wide range of models (e.g., dumbbell, symmetrical top) and it is independent of the choice of a z-axis for the quantization of the z-component of the angular momentum in these models. The latter feature is an example of what Van Vleck called "spectroscopic stability." As he put it in Part I of the trilogy that provided the backbone for his 1932 book:

> [T]he high spectroscopic stability characteristic of the new quantum mechanics is the cardinal principle underlying the continued validity of the Langevin-Debye formula. We shall not attempt a precise definition of the term "spectroscopic stability."[54] It means roughly that the effect of orientation or of degeneracy in general is no greater than in the classical theory, and this usually implies that summing over a discrete succession of quantum-allowed orientations gives the same result as a classical average over a continuous distribution.[55] (Van Vleck 1927a, 740)

The old quantum theory gave values for C much greater than $1/3$, as Pauli (1921) and Pauling (1926b) discovered using the rigid rotator as their model for the gas molecules (see the table in fig. 3.2). Redoing the calculation in matrix mechanics, Mensing and Pauli (1926) recovered the value $C = 1/3$ for this special case, as did Kronig (1926a), Manneback (1926) and Van Vleck (1926a) (see sec. 7.4). Van Vleck, however, was the only one who stated explicitly that this result is independent of the choice of the axis of quantization of the rigid rotator's angular momentum: "in the matrix theory the susceptibility is the same with spacial[56] quantization relative to the applied field as with random orientations" (ibid., 227; see Fellows 1985, 144). Van Vleck managed to salvage a plausibility argument for this claim when he had to shorten his note for *Nature* (see sec. 7.5.4). In subsequent publications, he gave the full proof, not just for the rigid rotator but for a broad class of models (Van Vleck 1927a; 1932b).

That the susceptibility of a gas of rigid rotators does not depend on the axis of quantization is an example of spectroscopic stability. In his book, Van Vleck devoted considerable space to the "principle" or the "theorem" of spectroscopic stability (Van Vleck 1932b, 111, 139). Before giving a mathematical proof (ibid., sec. 35, 137–143), he explained the situation qualitatively in the chapter on the old quantum theory (ibid., sec. 30, 111–113). After conceding that the term, which he took from Bohr (1918, 85), "is not a particularly happy one" (ibid., 111),[57] he wrote:

[54] Yet another illustration of the continuity of Van Vleck's research across the quantum revolution of 1925–26 is that the footnote inserted at this point refers to the subsection, "The Hypothesis of Spectroscopic Stability," of sec. 54, "The Polarization of Resonance Radiation," of his NRC *Bulletin* (Van Vleck 1926b, 171–173).

[55] On the next page, before giving his general proof of spectroscopic stability, Van Vleck noted that a similar result for a special case had already been established in the *Dreimännerarbeit* (Born, Heisenberg, and Jordan 1926, 590) and that he was "informed that the more general result has also been obtained independently by Born (unpublished)" (Van Vleck 1927a, 741). So, as in the old quantum theory (see sec. 7.3.2), Born and Van Vleck were pursuing similar lines of research in matrix mechanics. In the discussion of electric susceptibilities in their book, Born and Jordan (1930, sec. 42, 212–225) followed Van Vleck, citing (ibid., 219) his note in *Nature* and the trilogy in *Physical Review* (Van Vleck 1926a; 1927a; 1927b; 1928a). Born and Jordan did not use the term 'spectroscopic stability' in this context (see note 57).

[56] This is how Van Vleck consistently spelled 'spatial'.

[57] Bohr introduced the term in the context of the Zeeman effect: "from a consideration of the necessary "stability" of spectral phenomena, it follows that the total radiation of the components, in which a spectral line, which orig-

> [I]t can for our purposes be considered identical with the idea that the susceptibility is invariant of the type of quantization, or in the special case of spacial quantization, that summing over the various quantized orientations is equivalent, as far as results are concerned, to a classical integration over a random orientation of orbit. It is indeed remarkable that a discrete quantum summation gives exactly the same answers as a continuous integration. This was not at all true in the old quantum theory. (Van Vleck 1932b, 111)

In the three subsections that follow, we present derivations of the formula for the electric susceptibility in gases in classical theory (sec. 7.5.3), the old quantum theory (sec. 7.5.4), and quantum mechanics (sec. 7.5.5). In the quantum theory, old and new, we focus on the special case in which the gas molecules are modeled as rigid rotators. We will see how the robustness of the value $C = 1/3$ was established, lost, and regained. In secs. (7.5.3) and (7.5.5), we follow Van Vleck (1932b). In sec. (7.5.4), we follow Pauli (1921), Pauling (1926b; 1927) and Mensing and Pauli (1926), though we will also quote liberally from chap. V of Van Vleck's 1932 book. In this chapter, "Susceptibilities in the old quantum theory contrasted with the new," the author used some uncharacteristically strong language to describe the shortcomings of the old quantum theory in this area.

7.5.3 Susceptibilities in Classical Theory

The susceptibility of a gas, χ, is a measure of how the gas responds to external fields. We will consider the electric susceptibility in particular. The field, $\mathbf{E}$, and polarization, $\mathbf{P}$, are assumed to be parallel, and the medium is assumed to be both isotropic and homogenous. Predictions of χ require one to deal with the motions of the systems used as models for the gas molecules and their constituent atoms: the specific behavior of these systems in response to the external field will determine their electric moments, and in turn, the polarization of the medium.

Consider a small volume of a gas of molecules with permanent dipole moments, such as HCl. When an electric field is applied, say in the z-direction of the coordinate system we are using, the molecules experience a torque that tends to align them with the field. In addition, the charges in each molecule will rearrange themselves in response. If the field is too weak to cause ionization, the charges will settle into equilibrium with the field, creating a temporary induced electric moment. Both of these effects contribute to a molecule's electric moment $\mathbf{p}$. Following Van Vleck, we largely focus on the first of these effects, which, as mentioned above, is responsible for the temperature-dependent term in the Langevin-Debye formula (see eqs. 7.7–7.8).

To find the polarization, $\mathbf{P}$, we need to take two averages over the component of these electric moments in the direction of the field $\mathbf{E}$, in this case the p_z component. First, we need to average p_z over the period(s) of the motion of the molecule (or in the case of quantum

inally is unpolarized, is split up in the presence of a small external field, cannot show characteristic polarisation with respect to any direction" (Bohr 1918, 85). Born and Jordan (1930, 13, 106, 161) attributed the term 'spectroscopic stability' to Heisenberg, citing a paper submitted in November 1924 on the polarization of fluorescent light (Heisenberg 1925a). Van Vleck also emphasized the connection between spectroscopic stability and the polarization of resonance radiation (Van Vleck 1926b, 171, see note 54; 1927a, 730; 1932b, 111). The first example Born and Jordan (1930, 12–13) gave of spectroscopic stability is the Thomas-Kuhn sum rule (see sec. 7.3.2). The "(optical) stability principle" is also mentioned prominently in a later book by Jordan (1936, 46–47, 169).

theory, over the stationary state). This is indicated by a single overbar: $\overline{p_z}$. Second, we need to average this time-average $\overline{p_z}$ over a thermal ensemble of a large number N of such molecules. This is indicated by a double overbar: $\overline{\overline{p_z}}$. All derivations of expressions for the susceptibility call for this two-step averaging procedure.[58]

The strength P of the polarization is given by:

$$P = N\overline{\overline{p_z}}. \tag{7.9}$$

The electric susceptibility, χ, is defined as the ratio of the strengths of the polarization and the external field:

$$\chi \equiv \frac{P}{E} = \frac{N}{E}\overline{\overline{p_z}}. \tag{7.10}$$

When it comes to the derivation of expressions for χ, the various theories differ only in how $\overline{p_z}$ and $\overline{\overline{p_z}}$ are obtained.

We first go through the calculation in the classical theory, covered elegantly in chap. II of Van Vleck's book, "Classical Theory of the Langevin-Debye Formula" (Van Vleck 1932b, 27–41). Consider a multiply-periodic system with f degrees of freedom, which, in its unperturbed state, is described by the Hamiltonian H^0, and which is subjected to a small perturbation coming from an external electric field **E** in the z-direction. The Hamiltonian for the perturbed system can then be written as the sum $H^0 + H^{int}$, where $H^{int} \ll H^0$. In this case, the full Hamiltonian is given by:

$$H = H^0 - Ep_z. \tag{7.11}$$

As in his NRC *Bulletin*, Van Vleck (1932b, 38) used action-angle variables (w_k^0, J_k^0) $(k = 1, \ldots, f)$ for the *unperturbed* Hamiltonian H^0, even when dealing with the full Hamiltonian (see also Van Vleck 1927b, 50; cf. our discussion in sec. 7.3.2).

The z-component of the polarization of the system, p_z, can be written as a Fourier expansion. For a system with only one degree of freedom the expansion is given by:[59]

$$p_z(w^0, J^0) = \sum_{\tau=0,\pm1,\pm2,\ldots}^{\infty} (p_z)_\tau(J^0)\, e^{2\pi i \tau w^0}. \tag{7.12}$$

Essentially the same Fourier expansion is the starting point both for the derivation of the Kramers dispersion formula discussed in sec. (7.3.2) and for Heisenberg's (1925b) *Umdeutung* paper (Duncan and Janssen 2007, 592–594).[60]

58 Pauling (1926b, 568) gives a particularly clear statement of this procedure: "[double bar] is the average value [...] for all molecules in the gas, and [single bar] is the time average [...] for one molecule in a given state of motion."

59 Van Vleck (1932b, 38) writes $p_\tau^{(z)}$ for the complex amplitudes $(p_z)_\tau$ and suppresses the argument J^0 in his notation.

60 As Dennison put it in the introduction of the paper that Van Vleck (1926a) used for his note in *Nature* on the susceptibility of a gas of rigid rotators (see sec. 7.4): "According to [matrix mechanics] the coordinates of a multiply periodic system which may be expressed classically by means of multiple Fourier series in the time, are to be replaced by infinite matrices of the Hermite type of which each member is a harmonic component in time" (Dennison 1926, 318).

To ensure that p_z in eq. (7.12) is real, the complex amplitudes $(p_z)_\tau$ must satisfy $(p_z)_\tau = (p_z)^*_{-\tau}$. Eq. (7.12) also gives the expansion for a system with f degrees of freedom, if, following Van Vleck, we introduce the abbreviations $J^0 \equiv J^0_1 \dots J^0_f$, $w^0 \equiv w^0_1 \dots w^0_f$, $\tau \equiv \tau_1 \dots \tau_f$, and $\tau w^0 = \sum_{k=1}^{f} \tau_k w^0_k$ (Van Vleck 1932b, 38). Through p_z, the full Hamiltonian, H, in eq. (7.11) depends on w^0, so the action-angle variables (w^0, J^0) are *not* action-angle variables for H. The phase space element, however, is invariant under the transformation from action-angle variables for H to action-angle variables for H^0, i.e., $dJ^0 dw^0 = dJ dw$ (ibid., 39).

Using the standard formula for the canonical ensemble average, we find for $\overline{\overline{p_z}}$ (ibid., 38):[61]

$$\chi = \frac{N}{E} \frac{\iint p_z \, e^{-H/kT} \, dJ^0 dw^0}{\iint e^{-H/kT} \, dJ^0 dw^0}. \tag{7.13}$$

To first order in the field E, the Boltzmann factor is given by:

$$e^{-H/kT} \approx e^{-H^0/kT} \left(1 + \frac{E p_z}{kT}\right). \tag{7.14}$$

Assuming there is no residual polarization in the absence of an external field (which is true for gases if not always for solids), i.e., $\overline{\overline{p_z}} = 0$ for $E = 0$, we have

$$\iint p_z \, e^{-H^0/kT} \, dJ^0 dw^0 = 0. \tag{7.15}$$

Using eqs. (7.14) and (7.15), we can rewrite eq. (7.13) as (ibid., 39)

$$\chi = \frac{N}{kT} \frac{\iint p_z^2 \, e^{-H^0/kT} \, dJ^0 dw^0}{\iint e^{-H^0/kT} \, dJ^0 dw^0}. \tag{7.16}$$

For p_z^2 we insert its Fourier expansion

$$p_z^2(w^0, J^0) = \sum_{\tau=0,\pm1,\pm2,\dots}^{\infty} (p_z^2)_\tau (J^0) \, e^{2\pi i \tau w^0}. \tag{7.17}$$

Only the $\tau = 0$ terms on the right-hand side will contribute to the integral of p_z^2 over w^0 in eq. (7.16). All $\tau \neq 0$ terms are periodic functions of w^0, which vanish when integrated over a full period of these functions. Hence,

$$\int p_z^2 \, dw^0 = (p_z^2)_0. \tag{7.18}$$

[61] One can think of the integration of p_z over one period of the angle variable w_0 for a fixed value of J_0 as giving $\overline{p_z}$ and of the subsequent integration over J_0 as turning $\overline{p_z}$ into $\overline{\overline{p_z}}$ (ibid., note 11). In this case, averaging over a thermal ensemble of identical systems is replaced by taking a weighted average over different states of one system, where the weight factor is given by the usual Boltzmann factor $e^{-H/kT}$, in which $H \approx H_0(J_0)$.

In other words, $(p_z^2)_0$ is the time average $\overline{p_z^2}$ of p_z^2. It follows from eq. (7.18) that the integrals over w^0 in numerator and denominator of eq. (7.16) cancel. Eq. (7.16) thus reduces to (ibid., 39–40):

$$\chi = \frac{N}{kT} \frac{\int \overline{p_z^2}\, e^{-H^0/kT}\, dJ^0}{\int e^{-H^0/kT}\, dJ^0} = \frac{N}{kT} \overline{\overline{p_z^2}} = \frac{N}{3kT} \overline{\overline{p^2}}, \tag{7.19}$$

where in the last step we used that

$$\overline{\overline{p_z^2}} = \tfrac{1}{3} \overline{\overline{p^2}}. \tag{7.20}$$

This relation holds both in the classical theory and in quantum mechanics. That it does not hold in the old quantum theory is central, as we will see, to that theory's failure to reproduce the Langevin-Debye formula. Van Vleck thus took great care in explaining this relation:

> $\overline{\overline{p_z^2}}$ denotes the statistical mean square of p_z^2 in the absence of the field E, i.e. the average over only the J^0 part of the phase space, weighted according to the Boltzmann factor, of the time average value of p_z^2 [in our notation: $\overline{p_z^2}$] for a molecule having given values of the J^0's [recall that J^0 short-hand for $J_1^0 \dots J_f^0$]. Now if the applied electric field E is the only external field, all spacial orientations will be equally probable when $E = 0$, and the mean squares of the x, y, and z components of moment will be equal [i.e., $\overline{\overline{p_x^2}} = \overline{\overline{p_y^2}} = \overline{\overline{p_z^2}}$]. This will also be true even when there are other external fields (e.g. a magnetic field) besides the given electric field[,] provided, as is usually the case, these other fields do not greatly affect the spacial distribution. We may hence replace $\overline{\overline{p_z^2}}$ by one-third the statistical mean square of the vector momentum $\mathbf{p}$ of the molecule. (Van Vleck 1932b, 39–40)

In the old quantum theory, as pointed out by Pauling (1927), the susceptibility is sensitive to the presence of a magnetic field (see sec. 7.5.4). In classical theory and in quantum mechanics it is not. This is undoubtedly why Van Vleck emphasized this feature.

Van Vleck (1932b) called eq. (7.19) "a sort of generalized Langevin-Debye formula" (40). No particular atomic model need be assumed for its derivation. To obtain the familiar Langevin-Debye formula (7.7) with terms corresponding to permanent and induced electric moments, we need to adopt a model for the molecule of the gas similar to that underlying the classical dispersion theory of von Helmholtz, Lorentz, and Drude, involving harmonically-bound charges (see sec. 7.3.2). As Van Vleck (1932b) wrote: "This naïve depicture of an atom or molecule as a collection of harmonic oscillators is not in agreement with modern views of atomic structure as exemplified in the Rutherford atom, but yields surprisingly fruitful results" (30).[62] Let s be the number of degrees of freedom with which these bound charges can vibrate, then with a set of normal coordinates $\xi_1, \xi_2, \dots, \xi_s$, we can write the component of the electric moment $\mathbf{p}$ along the principal axis of inertia, labeled x, as a linear function of these normal coordinates (ibid., 33):

$$p_x = \mu_x + \sum_{i=1}^{s} c_{xi} \xi_i, \tag{7.21}$$

[62] The same can be said about classical dispersion theory (Duncan and Janssen 2007, 576–577).

where μ_x is the x-component of the permanent electric dipole moment of the molecule, and where the coefficients c_{xi} are real positive numbers. Similar expressions obtain for the y- and z-components of $\mathbf{p}$.

Since positive and negative displacements will cancel during the averaging process, $\overline{\overline{\xi_i \xi_j}} = 0$ for $i \neq j$ (ibid., 40). If we associate a 'spring constant' a_i with the linear force binding the i-th charge, then, by the equipartition theorem, we get: $\frac{1}{2} a_i \overline{\overline{\xi_i^2}} = \frac{1}{2} kT$. Inserting eq. (7.21) for p_x and similar equations for p_y and p_z for the components of $\mathbf{p}$ in eq. (7.19) and using the relations for $\overline{\overline{\xi_i \xi_j}}$ and $\overline{\overline{\xi_i^2}}$, we find (ibid., 37):

$$\chi = \frac{N\mu^2}{3kT} + \frac{N}{3} \sum_i \frac{c_{xi}^2 + c_{yi}^2 + c_{zi}^2}{a_i}. \tag{7.22}$$

As desired, the first term gives us the contribution of the permanent moment with a factor of $1/3$, and the second is of the form $N\alpha$, where α is independent of temperature.

Unfortunately, the assumption that electrons can be thought of as harmonically-bound charges in the atom had to be discarded as the old quantum theory began to shed light on atomic structure. This is the same development that was responsible for the old quantum theory's Kuhn loss in dispersion theory (see sec. 7.3.2). Expanding on the comment quoted above, Van Vleck concluded chap. II by emphasizing the limitations of the classical theory:

> A model such as we have used, in which the electronic motions are represented by harmonic oscillators, is not compatible with modern knowledge of atomic structure […] Inasmuch as we have deduced a generalized Langevin-Debye formula for any multiply periodic system, the question naturally arises whether [eq. 7.19] cannot be specialized in a fashion appropriate to a real Rutherford atom instead of to a fictitious system of oscillators mounted on a rigid rotating framework. This, however, is not possible. (Van Vleck 1932b, 41)

The reason Van Vleck gave for this is that, in the Rutherford(-Bohr) atom, the energy of the electron ranges from 0 to $-\infty$ causing the Boltzmann factors $e^{-H/kT}$ to diverge. Hence, he concluded, "the practical advantages of the [general formula 7.19] are somewhat restricted because of the inherent limitations in classical theory" (ibid.).

7.5.4 Susceptibilities in the Old Quantum Theory

Attempts to derive a formula for susceptibility in the old quantum theory, similar to the one in classical theory given above, ran afoul of some of the old quantum theory's most striking yet little-known inconsistencies. The old quantum theory was at its best when physicists could be agnostic about the details of the multiply-periodic motion in atoms or molecules (as in the case of the Kramers dispersion formula, see sec. 7.3.2). As soon as they were forced to take these details seriously, new problems emerged that could not easily be dealt with. Van Vleck had run into such problems in his work on helium. Similar problems arose in molecular physics, where the details of rotational and vibrational motion of specific models for various molecules had to be taken into account. The derivation of a formula for susceptibility hinges on detailed consideration of rotational motion, in particular on the question of how to quantize angular momentum. Unlike modern quantum mechanics, the old quantum theory did not provide clear instructions on how to do this. As a result, as Van Vleck wrote in Part I of his 1927–28 trilogy,

> the old quantum theory replaced the factor $1/3$ [in the Langevin-Debye formula 7.7] by a constant C whose numerical value depended rather chaotically on the type of model employed, whether whole or half quanta were used, whether there was "weak" or "strong" spacial quantization, etc.[63] This replacement of $1/3$ by C caused an unreasonable discrepancy with the classical theory at high temperatures, and in some instances the constant C even had the wrong sign. (Van Vleck 1927a, 728)

The issue of 'weak' or 'strong' quantization mentioned in this passage has to do with the question of how to quantize the *unperturbed* motion in the old quantum theory. Consider a rotating molecule. If a strong enough electric field is present, it makes sense to quantize the molecule's rotation with respect to the direction of the field. But how to quantize in the absence of an external field? In that case, there is no reason to assume a preferred direction in space, and it seems arbitrary to preclude entire classes of rotational states. Yet one had to proceed somehow. Two different kinds of quantization could be assumed (Van Vleck 1932b, 106). In the first, called 'strong spatial quantization,' rotation was assumed to be quantized with respect to the field *even when there was, as yet, no field*. In the other, called 'weak spatial quantization,' molecules were assumed to be in some intermediate state between 'strong quantization' and a classical distribution of rotational states.[64] Van Vleck highlighted this conceptual conundrum:

> Spacial quantization cannot be effective unless it has some axis of reference. In the calculation of Pauli and Pauling [...] the direction of the electric field is taken as such an axis [...] [I]n the absence of all external fields [...] there is no reason for choosing one direction in space rather than another for the axis of spacial quantization. (Van Vleck 1932b, 108)

We need to take a closer look at these calculations by Pauli (1921) and Pauling (1926b). They considered the special case in which the rigid rotator is used to model diatomic molecules such as HCl. Its rotational states are specified by two angular coordinates, the polar coordinate ϑ and the azimuthal coordinate φ, and their conjugate angular momenta p_ϑ and p_φ. The angle ϑ is measured from the z-axis chosen in the direction of the external field $\mathbf{E}$. The Hamiltonian for the system in this field is:

$$H = \frac{1}{2I}\left(p_\vartheta^2 + \frac{p_\varphi^2}{\sin^2\vartheta}\right) - \mu E \cos\vartheta, \tag{7.23}$$

where I is the molecule's moment of inertia (Pauli 1921, 321).[65]

Implicitly assuming strong spatial quantization, Pauli (ibid., 324) quantized the angular momentum of the rigid rotator with respect to the direction of the field, even when the

[63] At this point a footnote is inserted with references to Pauli (1921) and Pauling (1926b).

[64] In Part II of his 1927–28 trilogy, Van Vleck (1927b, 37) referred to his NRC *Bulletin* for a discussion of 'weak' versus 'strong' quantization (Van Vleck 1926b, 165). In the *Bulletin* the same distinction is also made in terms of 'diffuse' versus 'sharp' quantization (ibid., 171–178).

[65] Pauli used A and F and Pauling used I and F for what in our notation are I and E, respectively.

field had not been switched on yet. Keep in mind that Pauli wrote this paper the year before Otto Stern and Walther Gerlach published what appeared to be strong evidence for spatial quantization (Gerlach and Stern 1922). When, in early 1926, Pauling redid Pauli's calculation with half rather than whole quanta, he likewise assumed strong spatial quantization, but, unlike Pauli, was quite explicit about it and devoted the final subsection of his paper to a discussion of the issue of strong versus weak spatial quantization (Pauling 1926b, 576).

Pauli (1921, 321, 324) introduced the quantities K and J, defined as (a sum of) action variables subject to Sommerfeld-Wilson-type quantum conditions (cf. eq. 7.3 in sec. 7.3.2):

$$K \equiv \oint p_\vartheta d\vartheta + \oint p_\varphi d\varphi = lh, \quad J \equiv \oint p_\varphi d\varphi = 2\pi p_\varphi = mh. \tag{7.24}$$

Pauling (1926b, 570) did not use the designations K and J for these quantities and changed the first condition to[66]

$$\oint p_\vartheta d\vartheta + \left|\oint p_\varphi d\varphi\right| = lh. \tag{7.25}$$

Both Pauli and Pauling actually used m instead of l and n instead of m. We use l and m because it turns out that these quantum conditions boil down to setting the norm and the z-component of the angular momentum **L**, both averaged over periods of ϑ and φ, equal to $l\hbar$ and $m\hbar$, respectively ($\hbar \equiv h/2\pi$). The reason Pauling modified Pauli's first quantum condition was probably because he realized that L could never be smaller than L_z. For the purposes of reconstructing the calculation (cf. note 68), the quantum conditions (7.24–7.25) can be replaced by:

$$L = l\hbar, \quad L_z = m\hbar. \tag{7.26}$$

Pauli used integer quantum numbers, which means that $l = 1, 2, 3, \ldots$; Pauling used half-integers, which means that $l = \frac{1}{2}, \frac{3}{2}, \frac{5}{2}, \ldots$. In both cases, m runs from $-l$ to l. The state $l = m = 0$ was forbidden in the old quantum theory.

The equations on the blackboard behind Van Vleck in the picture in fig. (7.2) may serve as a reminder that even this sanitized version (7.26) of the quantum conditions (7.24–7.25) is *not* how angular momentum is quantized in modern quantum mechanics.[67] This modern treatment of angular momentum underlies the calculations of susceptibilities by Mensing and Pauli (1926), Van Vleck (1926a) and others in the new quantum theory. It is precisely because of the dubious way in which it quantized angular momentum—the conditions (7.26) in conjunction with spatial quantization—that the old quantum theory came to grief in its treatment of susceptibilities.

To find the susceptibility of a gas of rigid rotators in the old quantum theory, Pauli and Pauling first calculated the time average (indicated by the single overbar, cf. sec. 7.5.3) of the component of the electric moment in the direction of **E** in a particular state of the rigid rotator characterized by the quantum numbers l and m:

$$\mu\overline{\cos\vartheta} = \frac{\mu}{T}\int_0^T \cos\vartheta \, dt, \tag{7.27}$$

[66]In a preliminary report on his results, Pauling (1926a, 33) did not take the absolute value of $\oint p_\varphi d\varphi$ in this quantum condition.

[67]For a concise modern discussion of angular momentum in quantum mechanics, see, e.g., (Baym 1969, chap. 6).

where T is the period of rotation. Substituting the classical equation $p_\vartheta = I(d\vartheta/dt)$ into the Hamiltonian in eq. (7.23) and using eq. (7.24) to set $p_\varphi = m\hbar$, Pauli and Pauling derived an equation relating dt to $d\vartheta$:

$$dt = \frac{2\pi I d\vartheta}{\sqrt{8\pi^2 IW - \dfrac{m^2h^2}{\sin^2\vartheta} + 8\pi^2 I\mu E\cos\vartheta}}, \tag{7.28}$$

where W, the value of H, is the total energy of the molecule (Pauli 1921, 322; Pauling 1926b, 570). Using eq. (7.28), Pauli and Pauling could replace integration over t in eq. (7.27) by integration over ϑ at the cost of a rather more complicated expression.

In the evaluation of $\overline{\cos\vartheta}$, a distinction needs to be made between two energy regimes (Pauli 1921, 322). In the first, the molecules have energies W much smaller than μE, the energy of the interaction between the electric moment and the field. In the second, W is much larger than μE. The calculations of Pauli and Pauling only apply to the latter $W \gg \mu E$ regime. In that case, we can take μE to be a small perturbation of a purely rotational Hamiltonian and expand the denominator on the right-hand side of eq. (7.28) in the small dimensionless parameter $\mu E/W$, keeping only first-order terms.

Pauli (1921, 324) and Pauling (1926b, 570) eventually arrived at:

$$\overline{\cos\vartheta} = \frac{\mu EI}{2\hbar^2 l^2}\left(\frac{3m^2}{l^2} - 1\right). \tag{7.29}$$

The ratio (m^2/l^2) on the right-hand side corresponds to the *time* average $\overline{(L_z/L)^2} = \overline{\cos^2\vartheta}$ for the *unperturbed* system. In the *classical* theory,[68] but *not* in the old quantum theory, the *ensemble* average, $\overline{\overline{\cos^2\vartheta}}$, of this time average, $\overline{\cos^2\vartheta}$ (both for the *unperturbed* system) is equal to $1/3$. This is the same point that Van Vleck (1932b, 39–40) made in one of the passages we quoted in sec. (7.5.3): $\overline{\overline{p_z^2}} = \frac{1}{3}\overline{\overline{p^2}}$ (see eq. 7.20). It thus follows from the classical counterpart of eq. (7.29) (see note 68) that the ensemble average, $\overline{\overline{\cos\vartheta}}$, of the time average, $\overline{\cos\vartheta}$ (now both for the *perturbed* system) vanishes.

According to the classical theory, in other words, there is no contribution to the susceptibility *at all* from molecules in the energy regime $W \gg \mu E$ for which the classical counterpart of eq. (7.29) (see note 68) was derived. As Pauli (1921, 324) noted, this fits with the conclusion drawn earlier by Alexandrow (1921), that it is only the molecules in the lowest energy states that contribute to the susceptibility. Pauli also noted, however, that the lowest energy state in the old quantum theory ($l = m = 0$) is forbidden. In the old quantum theory, we thus have the paradoxical situation that there are "*only such orbits present that according to the classical theory do not give a sizable contribution to the electrical polarization*" (Pauli 1921, 325; emphasis in the original).

[68]Pauli (1921, sec. 4, 322–324), in fact, first showed that, in classical theory, $\overline{\cos\vartheta} = (\mu EI/2\overline{L^2})(3\overline{L_z^2/L^2} - 1)$ (in our notation, where the overbars on the left- and the right-hand sides refer to time averages for the *perturbed* and the *unperturbed* system, respectively). He then set $\overline{L} = l\hbar$ and $\overline{L_z} = m\hbar$ (cf. eq. 7.26) to turn this classical equation into eq. (7.29) in the old quantum theory.

Pauli went on to show that, contrary to the situation in the classical theory, the ensemble average, $\overline{\overline{\cos\vartheta}}$, of the time average, $\overline{\cos\vartheta}$, given by eq. (7.29) does *not* vanish in the old quantum theory (where both averages are for the perturbed system). Hence, he concluded, in the old quantum theory the susceptibility does not come from molecules in the low energy states but from those in the high energy states of the $W \gg \mu E$ regime in which eq. (7.29) holds. It therefore should not surprise us, Pauli argued, that the old quantum theory does not reproduce the factor $1/3$ of the Langevin-Debye formula (Pauli 1921, 325).

Before calculating $\overline{\overline{\cos\vartheta}}$, Pauli rewrote the factor multiplying the expression in parentheses in eq. (7.29). Elementary Newtonian mechanics and the quantum condition, $L^2 = \hbar^2 l^2$, tell us that the energy W_0 of the molecule in the absence of the field is given by:[69]

$$W_0 = \frac{\hbar^2 l^2}{2I}. \tag{7.30}$$

This energy, in turn, can be expressed in terms of a new quantity σ (Pauli 1921, 326):

$$\sigma \equiv \frac{\hbar^2}{2IkT} = \frac{\Theta}{T}, \tag{7.31}$$

where Θ is a "temperature characteristic for the quantum drop in specific heat associated with the rotational degree of freedom" (ibid.). Combining eqs. (7.30) and (7.31), we see that

$$W_0 = \sigma kTl^2. \tag{7.32}$$

From eqs. (7.30) and (7.32), it follows that $I/(2\hbar^2 l^2) = 1/(4W_0) = 1/(4\sigma kTl^2)$. Using this relation, we can rewrite eq. (7.29) as:

$$\overline{\cos\vartheta} = \frac{\mu E}{4\sigma kTl^2}\left(\frac{3m^2}{l^2} - 1\right). \tag{7.33}$$

The ensemble average of $\overline{\cos\vartheta}$ is given by (Pauli 1921, 325):

$$\overline{\overline{\cos\vartheta}} = \frac{\sum_{l>0}\sum_m \overline{\cos\vartheta}\, e^{-W_0/kT}}{\sum_{l>0}\sum_m e^{-W_0/kT}}, \tag{7.34}$$

where we used that, in the $W \gg \mu E$ regime, W can be replaced by W_0 in the Boltzmann factors. Inserting eq. (7.33) for $\overline{\cos\vartheta}$ and using eq. (7.32) for W_0, we arrive at:

$$\overline{\overline{\cos\vartheta}} = \frac{\mu E}{4\sigma kT}\,\frac{\sum_{l>0}\sum_m \frac{1}{l^2}\left(\frac{3m^2}{l^2} - 1\right)e^{-\sigma l^2}}{\sum_{l>0}\sum_m e^{-\sigma l^2}} \tag{7.35}$$

(Pauli 1921, 326; Pauling 1926b, 571). Evaluating these sums for integer and half-integer quantum numbers, respectively, and multiplying by $N\mu$, both Pauli (1921, 327) and Pauling (1926b, 571–572) arrived at an expression of the general form $C(N\mu/kT)$ for the temperature-

[69] Using that $L = I\omega$ (with ω the angular frequency), we can rewrite the rotational energy $W_0 = \frac{1}{2}I\omega^2$ as $W_0 = L^2/2I$.

dependent term in the formula for electric susceptibilities. Using whole quanta, Pauli found $C = 1.5367$, which is 4.6 times the classical value of $1/3$. Half-quanta—first introduced, at Einstein's suggestion, by Reiche in 1920 (Gearhart 2010, 158)—typically led to better agreement with the data in the old quantum theory. In this case, however, it did not help matters at all (see note 75 for an explanation). Pauling found even more troubling departures from $C = 1/3$ with half-quanta than Pauli had with whole quanta. For low temperatures ($T \approx \Theta$), Pauling calculated C to be 1.578. In his theory, however, C increases with temperature and in the limit of $T \gg \Theta$ (a limit in which his calculation should have been entirely valid) takes on the value 4.570, over 13 times the classical value. As we saw in sec. (7.1.3), reliable experimental data to rule out values other than $C = 1/3$ only became available after Pauling's paper was published, but it certainly was odd that C would increase with temperature in this way.

As we mentioned in sec. (7.4), Pauli revisited the problem of the susceptibility in diatomic dipole gases, such as HCl, shortly after the advent of matrix mechanics in a paper he co-authored with Lucy Mensing. Mensing had just obtained her doctorate in Hamburg, where Wilhelm Lenz and Pauli had been her advisors. She was now working as a postdoc with Born and Jordan in Göttingen (Mehra and Rechenberg 1982a, 188). In an earlier paper, Mensing (1926) had already applied the new matrix mechanics to the rigid rotator, taking the treatment of angular momentum in the *Dreimännerarbeit* (Born, Heisenberg, and Jordan 1926, chap. 4, sec. 1) as her point of departure.[70] Instead of the ad hoc quantization conditions in eqs. (7.24–7.25) that Pauli and Pauling had used earlier, Mensing and Pauli (1926) based their calculation on quantum conditions for the angular momentum of the rigid rotator systematically derived from the fundamental principles of the new theory. Van Vleck (1926a) did the same in his note on susceptibilities in *Nature* (see sec. 7.4), citing both Mensing (1926) and Dennison (1926) for the "matrices of the rotating dipole" (227).

The new theory replaced eqs. (7.24–7.25) for the quantization of the rigid rotator's angular momentum from the old quantum theory with relations familiar to the modern reader:[71]

$$L^2 = l(l+1)\hbar^2, \quad L_z = m\hbar, \tag{7.36}$$

where $l = 0, 1, \ldots$ and $-l \leq m \leq l$ (see, e.g., Mensing 1926, 814). Eq. (7.30) for the molecule's rotational energy W_0 in the absence of a field accordingly changes to (Mensing and Pauli 1926, 510):[72]

$$W_0 = \frac{\hbar^2}{2I}\, l(l+1) = \frac{\hbar^2}{2I}\left[(l+\tfrac{1}{2})^2 - \tfrac{1}{4}\right]. \tag{7.37}$$

Hence, up to an additive constant, the energy is given by squares of half-integers rather than integers, as Pauli had assumed in 1921. In this respect, matrix mechanics thus vindicated Pauling's use of half-quanta (ibid., 511).

[70] See (Cassidy 2007) for discussion of this paper.

[71] We will continue to use the letter l even though Mensing (1926), Mensing and Pauli (1926) and Van Vleck (1932b, sec. 37, 147–152; see sec. 7.5.5 below) all used j instead. To a modern reader, the letter j may suggest a combination of orbital angular momentum and spin, whereas in the case of the rigid rotator we only have the former, $\mathbf{L} = \mathbf{x} \times \mathbf{p}$.

[72] Gearhart (2010, 166) discusses this same formula in the context of work on the specific heat of hydrogen and work on molecular spectra in the early 1920s, which likewise involved rotating dumbbells and half-quanta (cf. note 16).

Mensing and Pauli now considered the average value $\overline{\mu_z} = \mu\,\overline{\cos\vartheta}$ of the component of the dipole moment of the molecule in the direction of the field (cf. eq. 7.27). They wrote this in the form

$$\overline{\mu_z} = \alpha(l,m)\,E. \tag{7.38}$$

In the old quantum theory, $\alpha(l,m)$ would be given by (μ/E) times the expression on the right-hand side of eq. (7.29) for $\overline{\cos\vartheta}$. In the new quantum theory, $\alpha(l,m)$ is given by

$$\frac{2\mu^2 I}{3\hbar^2}, \qquad \frac{2\mu^2 I}{\hbar^2}\frac{1}{(2l-1)(2l+3)}\left\{\frac{3m^2}{l(l+1)} - 1\right\}, \tag{7.39}$$

for $l = 0$ and $l \neq 0$, respectively (ibid., 512).[73]

These results can be used to calculate the ensemble average $\overline{\overline{\mu_z}}$ (cf. eqs. 7.34–7.35 for $\overline{\overline{\cos\vartheta}}$ in the old quantum theory). Setting $W = W_0$ in the Boltzmann factors, as in Pauli's earlier calculation (see eq. 7.34),[74] we find

$$\overline{\overline{\mu_z}} = \frac{\sum_l \sum_m \overline{\mu_z}\, e^{-W_0/kT}}{\sum_l \sum_m e^{-W_0/kT}} = E\frac{\sum_l \sum_m \alpha(l,m)\, e^{-\sigma l(l+1)}}{\sum_l (2l+1)\, e^{-\sigma l(l+1)}}, \tag{7.40}$$

where, in the second step, we used the relation $W_0 = \sigma kTl(l+1)$, the analogue in the new theory of the relation $W_0 = \sigma kTl^2$ in the old one (see eq. 7.32), and evaluated the sum over m in the denominator (ibid., 510).

When eq. (7.39) for $\alpha(l,m)$ is substituted into eq. (7.40) we find that only the $(l = 0)$-term in the sum over l in the numerator contributes to $\overline{\overline{\mu_z}}$ (ibid., 512).[75] The contributions coming from $l \neq 0$ can be written as:

$$\overline{\overline{\mu_z}} = \frac{2E\mu^2 I/\hbar^2}{\sum_l (2l+1)\, e^{-\sigma l(l+1)}} \sum_{l\neq 0}\left(\frac{e^{-\sigma l(l+1)}}{(2l-1)(2l+3)} \sum_m \left\{\frac{3m^2}{l(l+1)} - 1\right\}\right). \tag{7.41}$$

The well-known sum-of-squares formula tells us that

$$3\sum_{m=-l}^{l} m^2 = 6\sum_{m=1}^{l} m^2 = l(l+1)(2l+1). \tag{7.42}$$

[73]Note that, for $l \gg 1$, the second expression in eq. (7.39) reduces to (μ/E) times on the right-hand side of eq. (7.29), the corresponding expression in the old quantum theory. In sec. (7.5.5), we will cover the corresponding step in Van Vleck's (1932b, 151–152) calculation for the rigid rotator in more detail.

[74]As we will see in sec. (7.5.5), Van Vleck (1932b, 182) was more careful with these Boltzmann factors.

[75]Whereas the sum in eq. (7.41) for the new quantum theory vanishes, the corresponding sum in eq. (7.35) for the old quantum theory (in which $l = 0$ is forbidden) does not. It is because of this key difference between the calculation based on the modern quantum conditions (7.36) and the calculation based on the old quantum conditions (7.26) that the switch from whole to half quanta did nothing to bring the value for the electric susceptibility closer to what we now know to be the empirically correct one.

Using this formula to evaluate the sum over m in eq. (7.41) for any fixed non-zero value of l, we find:[76]

$$\sum_m \left\{ \frac{3m^2}{l(l+1)} - 1 \right\} = \frac{3\sum_m m^2}{l(l+1)} - (2l+1) = 0. \tag{7.43}$$

This shows that none of the $(l \neq 0)$-terms in the sum over l in the numerator of eq. (7.40) contribute to $\overline{\overline{\mu_z}}$. As Mensing and Pauli (1926) commented with obvious relief: "*Only the molecules in the lowest state* $[l = 0]$ *will therefore give a contribution to the temperature-dependent part of the dielectric constant*" (512; emphasis in the original). The new quantum theory thus reverted to the classical theory in this respect.[77]

Substituting eq. (7.39) for $\alpha(0, m)$ into eq. (7.40), we find that

$$\overline{\overline{\mu_z}} = \frac{2\mu^2 IE}{3\hbar^2} \frac{1}{\sum_l (2l+1)\, e^{-\sigma l(l+1)}}. \tag{7.44}$$

Using the relation $\chi = (N/E)\overline{\overline{\mu_z}}$ (see eq. 7.10) in combination with the expression $NC\mu^2/kT$ for the temperature-dependent term in χ (see eq. 7.8), we can write C as:

$$C = \frac{kT}{\mu^2 E} \overline{\overline{\mu_z}}. \tag{7.45}$$

Inserting eq. (7.44) for $\overline{\overline{\mu_z}}$ and using that $\sigma \equiv \hbar^2/2IkT$ (see eq. 7.31), we find:

$$C = \frac{1}{3\sigma \sum_l (2l+1)\, e^{-\sigma l(l+1)}}. \tag{7.46}$$

For sufficiently high temperatures, $l \approx l + 1$ in most terms of the sum over the l in the denominator, and the sum can be replaced by an integral:

$$\sum_l (2l+1)\, e^{-\sigma l(l+1)} \approx \int_0^\infty 2l\, e^{-\sigma l^2}\, dl = \frac{1}{\sigma}, \tag{7.47}$$

in which case $C = 1/3$. Mensing and Pauli concluded:

---

[76]The error in the paper by Pauling (1926b) pointed out by Van Vleck (see note 40) occurred in a summation just like the one in eq. (7.43). Pauling considered the sum

$$\sum_{n=-m+\frac{1}{2}}^{n=m-\frac{1}{2}} \left(\frac{3n^2}{2m^2} - \frac{1}{2} \right).$$

In the letter cited in note 40, Van Vleck commented: "I think the error resulted from counting the term $-\frac{1}{2}$ only once instead of $2r - 1$ times in the summation."

[77]In his note on susceptibilities in *Nature*, Van Vleck (1926a, 227) made the same point: "The remarkable result is obtained that only molecules in the state $[l = 0]$ of lowest rotational energy make a contribution to the polarisation. This corresponds very beautifully to the fact that in the classical theory only molecules with energy less than $[\mu E]$ contribute to the polarisation." Like Pauli (1921, 324), Van Vleck (1926a) cited Alexandrow (1921) for this result in the classical theory. So did Kronig (1926a, 491), who also drew attention to this analogy between classical theory and quantum mechanics.

> This result is completely opposite to the results that were obtained on the basis of the earlier quantum theory of periodic systems according to which the coefficient C [...] should have a numerical value substantially different from $1/3$ even in the limiting case of high temperatures.[78] This shows that here, as in many other cases, the new quantum mechanics follows classical mechanics more closely than the earlier quantum theory when it comes to statistical averages. (Mensing and Pauli 1926, 512)

And thus Mensing and Pauli recovered the Kuhn loss in susceptibility theory, at least for the special case of a gas consisting of rotating dumbbells. These authors, however, did not explain how the new calculation gets around the choice of a preferred axis of quantization. Mensing and Pauli, in other words, avoided the thorny problem of spatial quantization. As we will see in sec. (7.5.5), the solution to that problem boils down to the proof that the sum $\sum_m m^2$, and thereby the vanishing of eq. (7.41), does not depend on the choice of the z-axis for the quantization of L_z. Van Vleck already indicated this in his brief note in *Nature* in 1926. Translated into our notation, he wrote:

> The average value of L_z^2 is then
>
> $$\frac{1}{2l+1}\sum_{m=-l}^{l} m^2\hbar^2 = \tfrac{1}{3}l(l+1)\hbar^2 = \tfrac{1}{3}L^2,$$
>
> which is obviously the same result as with random orientations.[79] (Van Vleck 1926a, 227)

Note that this relation does not hold if the quantum-mechanical relation $L^2 = l(l+1)\hbar^2$ is replaced by the relation $L^2 = l^2\hbar^2$ of the old quantum theory (see eq. 7.26). This is one way to understand the difficulties the old quantum theory ran into with susceptibilities. In quantum mechanics, $\overline{L_z^2/L^2} = 3m^2/l(l+1)$. In that case, the sum-of-squares formula tells us that the ensemble average $\overline{\overline{L_z^2/L^2}} = 1/3$ (see eqs. 7.39–7.43). In the old quantum theory, $\overline{L_z^2/L^2} = 3m^2/l^2$ and $\overline{\overline{L_z^2/L^2}} \neq 1/3$ (see eqs. 7.33–7.35).

In subsequent publications, Van Vleck (1927a; 1932b) explained in more detail, and with greater generality, how the new quantum theory dispensed with the need for spatial quantization. This is precisely what is provided by the elusive notion of "spectroscopic stability" (cf. the quotations in sec. 7.5.2). Because of this general property of quantum mechanics, Van Vleck showed, it is true for a broad class of models and regardless of the axis along which one chooses to quantize that the only contribution to the susceptibility comes from the lowest energy states (the term $l = 0$ in eq. (7.40) for the special case of the rigid rotator). This was true in classical theory as well, but not in the old quantum theory. As he explained in *The Theory of Electric and Magnetic Susceptibilities*:

---

[78] Here the authors cite Pauli (1921) and Pauling (1926b).

[79] Feynman, Leighton, and Sands (1964, Vol. 2, chap. 34, 11) used this same relation as an argument for why one should set $L^2 = l(l+1)\hbar^2$, if one sets $L_z = m\hbar$ with $m = 0, \pm 1, \pm 2, \ldots, \pm l$. It is only natural to demand that the average value of L^2 be three times the average value of L_z^2. The average value of L^2 is then given by $3\hbar^2(\sum_m m^2)/(2l+1)$, which the sum-of-squares formula tells us is equal to $l(l+1)\hbar^2$.

> [C]lassically the susceptibility arises entirely from molecules which possess so little energy that they would oscillate rather than rotate through complete circles […] As the temperature is increased, the fraction of molecules which are located in the 'lazy' states that contribute to the susceptibility will steadily diminish, and hence we can see qualitatively why the susceptibility due to permanent dipoles decreases with increasing temperature […] In the old quantum theory the susceptibility did not arise uniquely from the lowest rotational state […] and this is perhaps one reason why the old theory gave such nonsensical results on the dielectric constants.[80] (Van Vleck 1932b, 184)

In blaming the "nonsensical results" of the old quantum theory on this unusual feature, Van Vleck ignored that, without it, the temperature-dependent term of the susceptibility could not be derived *at all*. In the case of the rigid rotator, the state $l = 0$ was forbidden in the old quantum theory. The susceptibility thus had to come from the $l \neq 0$ states. The preferred direction introduced by spatial quantization ensured that the sum over $l \neq 0$ in eqs. (7.34–7.35) for $\overline{\overline{\cos\vartheta}}$ does *not* vanish, thus producing a non-zero contribution to the susceptibility. Without spatial quantization, all orientations would be equiprobable and the average moment in the direction of the field would be zero. We would then be stuck with the absurd conclusion that a permanent electric moment contributes nothing to the susceptibility! This is why, at the end of his paper, Pauling (1926b, 577) suggested that 'strong spatial quantization' itself was the mechanism responsible for polarization.

While spatial quantization thus offered make-shift solutions to some problems in the old quantum theory, it also introduced new ones. If one took it seriously, one was faced with a question about the quantization process itself. If it was somehow caused by the presence of a field, did it happen all at once or gradually as the field was applied? Either way, there would be physical consequences. Indeed, the experimentalist August Glaser claimed to have observed such an effect, a transition from 'weak' to 'strong' spatial quantization as the strength of the field was increased. Van Vleck was not fond of the "unphysical […] bugbear of weak and strong spacial quantization" (Van Vleck 1932b, 110). He had already expressed his displeasure about this "bugbear" in Part I of his 1927–28 trilogy (Van Vleck 1927a, 37). In the 1932 book, he ends his discussion of it on a reassuring note:

> If the reader has felt that our presentation of weak and strong quantization in the old quantum theory was somewhat mystifying (as indeed it had to be, as physicists themselves were hazy on the details of the passage from one type of quantization to another), he need now no longer feel alarmed, as the new mechanics gives no susceptibility effects without some analogue in classical theory. (Van Vleck 1932b, 111)

Spatial quantization also led to problems in the old quantum theory's treatment of the effect of magnetic fields on the dielectric constant. It was Pauling who drew attention to that problem. As he explained in the abstract of a paper submitted in September 1926:

> The investigation of the motion of a diatomic dipole molecule in crossed magnetic and electric fields shows that according to the old quantum theory there

[80] That the only contribution to the susceptibility comes from the lowest state is a special feature of the rigid rotator. It is true much more generally, however, that the bulk of the susceptibility comes from the lower energy states (ibid.).

> will be spatial quantization […] with respect to the magnetic field […] As a result of this the old quantum theory definitely requires that the application of a strong magnetic field to a gas such as hydrogen chloride produce a very large change in the dielectric constant of the gas. […] [T]he new quantum theory, on the other hand, requires the dielectric constant not to depend upon the direction characterizing the spatial quantization, so that no effect of a magnetic field would be predicted. The effect is found experimentally not to exist; so that it provides an instance of an apparently unescapable and yet definitely incorrect prediction of the old quantum theory. (Pauling 1927)

By late 1926, as this passage shows, Pauling had come to recognize the "wonderful nonsense" of the old quantum theory for what it was.[81] Pauli had recognized this even earlier. This makes it understandable how both of them could be so magnanimous in their reviews of Van Vleck's 1932 book (see the quotations at the end of sec. 7.5.1), even though the author pounced on their earlier work.

Van Vleck devoted a section of chap. V of his book to the issue raised by Pauling (Van Vleck 1932b, sec. 31, "Effect of a Magnetic Field on the Dielectric Constant"). As with the anomalous values for C found by Pauli (1921) and Pauling (1926b) (cf. our discussion in sec. 7.1.3), Van Vleck left the reader with the impression that physicists had been well aware of the discrepancy between the old quantum theory's prediction of the effect and reliable experimental data. If we look more carefully, we see that Van Vleck (1932b, 114) credited Pauling (1927) with having been the first to derive the prediction and that, like Pauling, he only cited papers published in 1926 or later for its experimental refutation. The way in which Van Vleck used this spurious effect to lambast the theory that predicted it makes it easy to forget that the prediction was not made, let alone tested, until after the theory's demise:

> The influence of a magnetic field on the dielectric constant […] was ludicrously large in the old quantum theory because of spacial quantization […] a crossed magnetic field would make the constant C in [eq. 7.8] negative, an absurdity. Only a comparatively feeble magnetic field would be required […] An innocent little magnetic field of only a few gauss should thus in the old quantum theory change the sign of the temperature coefficient of the dielectric constant and make the electric susceptibility negative in so far as the orientation rather than induced polarization is concerned. This is what one might term extreme spectroscopic instability. Needless to say, such a cataclysmic influence of a magnetic field on the dielectric constant is not found experimentally […] In the new quantum mechanics the choice of the axis of spacial quantization is no longer of importance, and so a magnetic field should be almost without effect on the dielectric constant, in agreement with the experiments. (Van Vleck 1932b, 113–115)

In light of all this, it is no mystery that Van Vleck was so impressed by the way in which quantum mechanics dispensed with spatial quantization and, in the process, restored

[81] In his interview for the AHQP, Pauling said: "I had already been especially struck by the fact that the Debye equation for the dielectric constant and the Langevin equation for paramagnetism are valid in quantum mechanics and that $\cos^2\vartheta$ averages one third in quantum mechanics for all states if one interprets the total angular momentum vector as a square root of j times j plus one. I emphasized this strongly in the book." (session 2, 12) The last sentence refers to a lengthy footnote in a book by Pauling and Samuel Goudsmit (1930, 231–232).

the factor of $1/3$ in the Langevin-Debye formula in full generality. "The new mechanics," he wrote, "always yield [sic] $C = 1/3$ without the necessity of specifying the details of the model, and the generality of this value of C is one of the most satisfying features of the new theory" (Van Vleck 1932b, 107–108). This then is one of the "less heralded successes" and "great achievements" of the new quantum theory that Van Vleck was referring to in the preface of his book (see the quotation in sec. 7.1.1). The following subsection explores this achievement in greater detail.

7.5.5 Susceptibilities in the New Quantum Mechanics

In this subsection, we present Van Vleck's derivation, from his 1932 book, of the electric susceptibility of a diatomic gas, such as HCl, with the rigid rotator as the model for its molecules. The most important difference between this derivation and the one by Mensing and Pauli (1926) discussed in sec. (7.5.4) is that Van Vleck starts from a much more general approach to the calculation of the susceptibility in gases, one that he used in calculations for a variety of models for the gas molecules. The first, more general steps of this derivation run in parallel to the classical calculation we outlined in sec. (7.5.3). While we will not go into the details of the general derivation, at the end of this subsection we will comment on one of its crucial components—Van Vleck's proof of spectroscopic stability and the elimination of spatial quantization.

The Langevin-Debye formula holds under very general conditions in quantum mechanics. One assumption identified by Van Vleck is that "the medium is sufficiently rarefied so that one may use the Boltzmann instead of the Fermi statistics" (181).[82] This assumption becomes critical only when Van Vleck tried to extend his approach from gases to solids. For gases (in weak fields), we only need two assumptions (187): first, that the constituent molecules have a permanent dipole moment; second, that all possible transitions are such that the energy jumps $h\nu_{i\to f}$ are either much greater or much smaller than kT. Quantum mechanics thus solves the problem one runs into in the classical theory that the Langevin-Debye formula only obtains for unrealistic models of matter (see the quotation at the end of sec. 7.5.3). In other words, Van Vleck's quantum-mechanical theory of susceptibilities can be seen as another instance of what Kuhn (1996, 105) described as a "reversion (which is not the same as a retrogression)" to an older theory.

Van Vleck gave the general quantum-mechanical derivation of the Langevin-Debye formula for the electric susceptibility in gases in chap. VII of his book (secs. 44–47, 181–202). In this chapter, he used several results of chap. VI, "Quantum-Mechanical Foundations" (secs. 32–43, 122–180), especially from the sections on perturbation theory (secs. 34–36, 131–147).[83] Moreover, in sec. 37, he had already derived the susceptibility for the special case of the rigid rotator (147–152). He briefly revisited this special case in chap. VII (sec. 45, 183–185). Our discussion combines elements from these sections of chaps. VI and VII.

[82] Unless noted otherwise, all page references in sec. (7.5.5) are to the book by Van Vleck (1932b).

[83] A footnote appended to the title of sec. 34, "Perturbation Theory," acknowledges that perturbation theory in quantum mechanics was first developed in the *Dreimännerarbeit* (Born, Heisenberg, and Jordan 1926) and in the third communication on wave mechanics by Erwin Schrödinger (1926).

Following Van Vleck, we first derive an expression for the susceptibility of a gas without assuming a special model for its molecules. Let

$$H = H^0 - E p_E, \tag{7.48}$$

be the Hamiltonian for a gas molecule, represented by some multiply-periodic system, in an external electric field **E**. H^0 is the Hamiltonian of the unperturbed system, p_E the electric moment of the system in the direction of the field. The quantities H, H^0, and p_E are now operators; E is still just a real number. The electric moment p_E can be extracted from the Hamiltonian by taking the derivative with respect to the field strength:

$$p_E = -\frac{\partial H}{\partial E}. \tag{7.49}$$

This relation is crucial for the calculation of the matrix elements of p_E (143, 181).

In general, Van Vleck wrote the Hamiltonian of a system subject to a small perturbation as $H = H^0 + \lambda H^{(1)} + \lambda^2 H^{(2)} + \ldots$, with the parameter $\lambda \ll 1$ (132). For the Hamiltonian in eq. (7.48), $\lambda = E$ and $\lambda H^{(1)}$ is the only term in the expansion. Perturbation theory allowed Van Vleck to compute the energy of the perturbed system as a series of corrections to the energy of the unperturbed system, each term corresponding to a different power of the expansion parameter:

$$W_n = W_n^0 + E W_n^{(1)} + E^2 W_n^{(2)} + \mathcal{O}(E^3). \tag{7.50}$$

To second order, we have (133):

$$W_n^{(1)} = \langle n^0 | H^{(1)} | n^0 \rangle, \qquad W_n^{(2)} = \sum_{n' \neq n} \frac{|\langle n'^0 | H^{(1)} | n^0 \rangle|^2}{W_n^0 - W_{n'}^0}, \tag{7.51}$$

where the $|n^0\rangle$'s are the eigenvectors of the unperturbed Hamiltonian.[84] Combining eqs. (7.49) and (7.50), we obtain an expression for the matrix elements of the electric moment in eigenstates of the full Hamiltonian with eigenvectors $|n\rangle$ (ibid., 144):

$$\langle n | p_E | n \rangle = \langle n | \left(-\frac{\partial H}{\partial E} \right) | n \rangle = -\frac{\partial}{\partial E} \langle n | H | n \rangle = -W_n^{(1)} - 2E W_n^{(2)} + \mathcal{O}(E^2). \tag{7.52}$$

Inserting the expressions in eq. (7.51) for $W_n^{(1)}$ and $W_n^{(2)}$, and using $E H^{(1)} = -E p_E$, we find, to first order in E (144):

$$\langle n | p_E | n \rangle = \langle n^0 | p_E | n^0 \rangle - 2E \sum_{n' \neq n} \frac{|\langle n'^0 | p_E | n^0 \rangle|^2}{W_n^0 - W_{n'}^0}. \tag{7.53}$$

[84] We use modern Dirac notation both because it is more familiar to the modern reader and because it is the notation Van Vleck adopted when he began revising his 1932 book for a second edition (cf. sec. 7.1.5). In the 1932 book, he wrote what in our notation would be $\langle n|H|n' \rangle$ as $H(n; n')$. He also typically used two or three quantum numbers to label the (degenerate) energy eigenstates, writing, for instance, $H(nm; n'm')$ or $H(njm; n'j'm')$. We will follow his example in the case of the rigid rotator (see eq. 7.65).

Van Vleck used the Bohr frequency condition to write $W_n^0 - W_{n'}^0 = -h\nu_{n'\to n}$ (133).[85]

To find an expression for the susceptibility, χ, we need to take two averages (cf. the discussion leading up to eq. 7.9 in sec. 7.5.3): (1) the expectation value $\overline{p_E} = \langle n|p_E|n\rangle$ of the electric moment of an individual molecule in a given state; (2) the average $\overline{\overline{p_E}}$ of this expectation value over a thermal ensemble of N such molecules. Both steps are captured in the following formula (181; cf. eqs. 7.10, 7.13, and 7.19 in sec. 7.5.3):[86]

$$\chi = \frac{N}{E}\,\overline{\overline{p_E}} = \frac{N}{E}\,\frac{\sum_n \langle n|p_E|n\rangle\, e^{-W_n/kT}}{\sum_n e^{-W_n/kT}}. \tag{7.54}$$

The Langevin-Debye formula is applicable only in regimes for which we can neglect saturation effects, which means that the susceptibility must be independent of the field strength E. Accordingly, we will assume the numerator in eq. (7.54) to be linear in E, and the denominator to be independent of E.

To first order, the Boltzmann factors in eq. (7.54) are given by (182; cf. eq. 7.14):

$$e^{-W_n/kT} = e^{-W_n^0/kT} e^{-EW_n^{(1)}/kT} = e^{-W_n^0/kT}\left(1 + \frac{E}{kT}\langle n^0|p_E|n^0\rangle\right), \tag{7.55}$$

where in the last step we used eq. (7.51) for $W_n^{(1)}$ (with $H^{(1)} = -p_E$). We now substitute eqs. (7.53) and (7.55) into eq. (7.54), keeping only terms to first order in the numerator and terms of zeroth order in the denominator. This gives us:

$$\chi = \frac{B}{E}\sum_n \left(\langle n^0|p_E|n^0\rangle + \frac{E}{kT}\langle n^0|p_E|n^0\rangle^2 - 2E\sum_{n'\neq n}\frac{|\langle n'^0|p_E|n^0\rangle|^2}{W_n^0 - W_{n'}^0}\right) e^{-W_n^0/kT}, \tag{7.56}$$

where $B \equiv N/\sum_n e^{-W_n^0/kT}$ (190). The first term, $B\sum_n \langle n^0|p_E|n^0\rangle\, e^{-W_n^0/kT}$, represents the average electric moment in the absence of an external field. This kind of 'hard'

[85] When he first published this formula, Van Vleck commented: "This is, of course, the same result as given by extrapolation of the Kramers dispersion formula to infinitely long impressed wavelengths" (Van Vleck 1927a, 734). Mensing and Pauli (1926, 511) and Kronig (1926a, 490) had made that same connection. In chap. XIII of his book, Van Vleck gave a formula for the index of refraction n of some transparent material as an ensemble average of the polarization of its constituents, given by the Kramers dispersion formula (361):

$$n^2 - 1 = \frac{8\pi N}{\sum_l e^{-W_l^0/kT}}\sum_{l,l'}\frac{\nu_{l'\to l}|\langle l'|p_E|l\rangle|^2}{h(\nu_{l'\to l}^2 - \nu^2)}e^{-W_l^0/kT},$$

where ν is the frequency of the incident light wave and $\nu_{l'\to l} = W_{l'}^0 - W_l^0$ (cf. Duncan and Janssen 2007, 658). For $\nu = 0$, the sums over l' for fixed l have the same form (modulo the Boltzmann factor) as the second term on the right-hand side of eq. (7.53). This underscores the relation between dispersion and susceptibility that we drew attention to in sec. (7.1.5) and at the beginning of sec. (7.5.2).

[86] Even though in the modern view, the expectation value cannot be viewed as a time average, in 1932 Van Vleck considered it to be something very similar: "A diagonal Heisenberg matrix element $\langle n|\, f\,|n\rangle$ has the physical significance of being the average value of f over all the phases of motion in a given stationary state" (Van Vleck 1932b, 129).

polarization is nonexistent in gases, so the term must be zero (182). We are then left with (189):

$$\chi = B \sum_{n} \left(\frac{\langle n^0 | p_E | n^0 \rangle^2}{kT} - 2 \sum_{n' \neq n} \frac{| \langle n'^0 | p_E | n^0 \rangle |^2}{W_n^0 - W_{n'}^0} \right) e^{-W_n^0/kT}, \tag{7.57}$$

or equivalently, in terms of the energy corrections (182):

$$\chi = B \sum_{n} \left(\frac{{W_n^{(1)}}^2}{kT} - 2W_n^{(2)} \right) e^{-W_n^0/kT}. \tag{7.58}$$

Eqs. (7.57–7.58) hold for any model of the constituent molecules of a gas. Van Vleck used it as a starting point for all of his electric susceptibility calculations, including the most general derivation of the Langevin-Debye formula. However, from this point onward, we will focus on the special case of the rigid rotator (sec. 37, 147–152). In sec. (7.5.4), we covered the calculations for this special case by Pauli (1921) and Pauling (1926b) in the old quantum theory and by Mensing and Pauli (1926) in the new quantum theory.[87]

The Hamiltonian for a rigid rotator in an external electric field **E** is given by (cf. eq. 7.23 in sec. 7.5.4):

$$H = \frac{L^2}{2I} - \mu E \cos \vartheta, \tag{7.59}$$

where **L** is the angular momentum and I is the moment of inertia (cf. note 69). Consider the vectors $|l, m\rangle$, which are simultaneous eigenvectors of L^2 and L_z:

$$L^2 \, |l, m\rangle = \hbar^2 l(l+1) \, |l, m\rangle, \qquad L_z \, |l, m\rangle = \hbar m \, |l, m\rangle, \tag{7.60}$$

with $l = 0, 1, \ldots$ and $-l \leq m \leq l$. Since $H^0 = L^2/2I$, these are also eigenvectors of the unperturbed Hamiltonian

$$H^0 \, |l, m\rangle = W_l^0 \, |l, m\rangle, \tag{7.61}$$

with ($(2l+1)$-fold degenerate) eigenvalues:

$$W_l^0 = \frac{\hbar^2}{2I} \, l(l+1) \tag{7.62}$$

(cf. eq. 7.37 in sec. 7.5.4). The vector $|l, m\rangle$ corresponds to the wave functions $\psi_{lm}^0(\vartheta, \varphi) \equiv \langle \vartheta, \varphi | l, m \rangle$ (Baym 1969, 160) given by (sec. 37, 149):

$$\psi_{lm}^0(\vartheta, \varphi) = \sqrt{\frac{(2l+1)(l-m)!}{4\pi(l+m)!}} P_l^m(\cos \vartheta) e^{im\varphi}, \tag{7.63}$$

where the $P_l^m(x)$ are associated Legendre functions.

[87] Van Vleck (1932b) cited Mensing and Pauli (1926), Kronig (1926a), Manneback (1926) and Van Vleck (1926a) at the beginning of sec. 37 (147) and mentioned them again at the beginning of sec. 45 (183). Cf. sec. (7.4) and note 46.

The susceptibility for a gas of rigid rotators is given by (182):

$$\chi = \frac{N}{\sum_l \sum_m e^{-W_l^0/kT}} \sum_l \sum_m \left(\frac{W_{lm}^{(1)^2}}{kT} - 2W_{lm}^{(2)} \right) e^{-W_l^0/kT}, \tag{7.64}$$

which is just the general eq. (7.58) for χ derived above with l and m rather than n labeling the (degenerate) energy eigenstates. To find χ, we need to find the first- and second-order energy corrections, $W_{lm}^{(1)}$ and $W_{lm}^{(2)}$. Replacing subscripts n by lm and vectors $|n^0\rangle$ by $|l,m\rangle$ in eq. (7.51) and substituting $H^{(1)} = -\mu \cos\vartheta$, we find (152):

$$W_{lm}^{(1)} = -\mu \langle l,m| \cos\vartheta |l,m\rangle, \qquad W_{lm}^{(2)} = \mu^2 \sum_{l'm' \neq lm} \frac{|\langle l',m'| \cos\vartheta |l,m\rangle|^2}{W_l^0 - W_{l'}^0}. \tag{7.65}$$

These expressions can be evaluated with the help of the following characteristic recursion formula for associated Legendre functions (151):

$$(2l+1)\cos\vartheta P_l^m(\cos\vartheta) = (l+m)P_{l-1}^m(\cos\vartheta) + (l-m+1)P_{l+1}^m(\cos\vartheta). \tag{7.66}$$

Combining this recursion formula with eq. (7.63), we find (ibid.)

$$\cos\vartheta\, \psi_{lm}^0(\vartheta,\varphi) = A_{l-1,m}\, \psi_{l-1,m}^0(\vartheta,\varphi) + B_{l+1,m}\, \psi_{l+1,m}^0(\vartheta,\varphi), \tag{7.67}$$

where we introduced the abbreviations:

$$A_{l-1,m} \equiv \sqrt{\frac{l^2 - m^2}{(2l-1)(2l+1)}}, \qquad B_{l+1,m} \equiv \sqrt{\frac{(l+1)^2 - m^2}{(2l+3)(2l+1)}}. \tag{7.68}$$

For $l = 0$, only the $B_{l+1,m}$ term is present. In terms of the corresponding state vectors, eq. (7.67) expresses that the vector obtained by letting the operator $\cos\vartheta$ act on $|l,m\rangle$ can be written as a linear combination of $|l-1,m\rangle$ and $|l+1,m\rangle$:[88]

$$\cos\vartheta\ |l,m\rangle = A_{l-1,m}\ |l-1,m\rangle + B_{l+1,m}\ |l+1,m\rangle. \tag{7.69}$$

Since $|l',m'\rangle$ is orthogonal to $|l,m\rangle$ as soon as $l' \neq l$ or $m' \neq m$, it follows immediately from eq. (7.69) that $W_{lm}^{(1)}$ in eq. (7.65) vanishes, and that the only contributions to $W_{lm}^{(2)}$ come from terms with $(l' = l-1, m' = m)$ and $(l' = l+1, m' = m)$, for which we have:

$$\langle l-1,m| \cos\vartheta |l,m\rangle = A_{l-1,m}, \qquad \langle l+1,m| \cos\vartheta |l,m\rangle = B_{l+1,m}. \tag{7.70}$$

[88] Taking the inner product with an arbitary vector $|l',m'\rangle$ on both sides of eq. (7.69), we find:

$$\langle l',m'| \cos\vartheta\, |l,m\rangle = A_{l-1,m}\, \langle l',m'|l-1,m\rangle + B_{l+1,m}\, \langle l',m'|l+1,m\rangle.$$

In coordinate space, these inner products turn into integrals:

$$\int d\omega\, \psi_{l'm'}^{0\,*} \cos\vartheta\, \psi_{lm}^0 = A_{l-1,m} \int d\omega\, \psi_{l'm'}^{0\,*} \psi_{l-1\,m}^0 + B_{l+1,m} \int d\omega\, \psi_{l'm'}^{0\,*} \psi_{l+1\,m}^0$$

where $d\omega \equiv d\vartheta d\varphi$ and where we suppressed the argument (ϑ,φ) of the various ψ functions. Since $\psi_{l'm'}^0$ is arbitrary, this last relation implies eq. (7.67), the form in which Van Vleck gave eq. (7.69).

For $l > 0$, the expression for $W_{lm}^{(2)}$ in eq. (7.65) thus reduces to:

$$W_{lm}^{(2)} = \mu^2 \left(\frac{A_{l-1,m}^2}{W_l^0 - W_{l-1}^0} + \frac{B_{l+1,m}^2}{W_l^0 - W_{l+1}^0} \right). \tag{7.71}$$

For $l = m = 0$, only the second term is present. Eq. (7.68) tells us that $B_{0+1,0}^2 = 1/3$ and eq. (7.62) that $W_0{}^0 - W_1{}^0 = -\hbar^2/I$, which means that, for $l = m = 0$, eq. (7.71) gives (152, 183):

$$W_{00}^{(2)} = \mu^2 \frac{B_{0+1,0}^2}{W_0^0 - W_1^0} = -\frac{I\mu^2}{3\hbar^2}. \tag{7.72}$$

Using eq. (7.68) for $A_{l-1,m}$ and $B_{l+1,m}$ and eq. (7.62) for $W_l{}^0$, Van Vleck showed that, for arbitrary non-zero values of l and m, eq. (7.71) becomes (ibid.):

$$W_{lm}^{(2)} = \frac{I\mu^2}{\hbar^2} \frac{l(l+1) - 3m^2}{l(l+1)(2l-1)(2l+3)}. \tag{7.73}$$

We now substitute these results for the energy corrections into eq. (7.64) for χ. Since $W_{lm}^{(1)} = 0$, the equation reduces to:

$$\chi = \frac{-2N \sum_l \sum_m W_{lm}^{(2)} e^{-W_l^0/kT}}{\sum_l \sum_m e^{-W_l^0/kT}}. \tag{7.74}$$

Carrying out the sum over m in the denominator, we can rewrite this as (183):

$$\chi = \frac{-2N \sum_l e^{-W_l^0/kT} \sum_m W_{lm}^{(2)}}{\sum_l (2l+1) e^{-W_l^0/kT}}. \tag{7.75}$$

As we already saw in sec. (7.5.4), where we covered Mensing and Pauli's (1926) calculation for the rigid rotator (see eqs. 7.41–7.44), only the $l = 0$ term in the summation over l in the numerator gives a contribution to χ. The terms for all other values of l vanish. To verify this, we insert eq. (7.73) for $W_{lm}^{(2)}$ ($l \neq 0$) in the sum over m in eq. (7.75):

$$\sum_m W_{lm}^{(2)} = \frac{I\mu^2}{\hbar^2} \frac{(2l+1)l(l+1) - 3\sum_m m^2}{l(l+1)(2l-1)(2l+3)}. \tag{7.76}$$

As Van Vleck noted (183), the numerator in this last expression vanishes on account of the formula $3\sum_m m^2 = l(l+1)(2l+1)$ (152; cf. the sum-of-squares formula 7.42). The entire susceptibility thus comes from the $l = 0$ term. This fits with the classical theory, for which Alexandrow (1921) had already shown that the susceptibility is due entirely to molecules with energies less than μE. Mensing and Pauli (1926), Kronig (1926a), and Van Vleck (1926a) had all noted with satisfaction earlier that the new quantum theory reverted to the classical theory in this respect (cf. sec. 7.5.4, especially note 77).

Eq. (7.75) thus reduces to the $(l = 0)$-term (184):

$$\chi = \frac{-2N\, e^{-W_0^0/kT}\, W_{00}^{(2)}}{\sum_l (2l+1) e^{-W_l^0/kT}} = \frac{2NI\mu^2}{3\hbar^2} \frac{e^{-W_0^0/kT}}{\sum_l (2l+1) e^{-W_l^0/kT}}, \tag{7.77}$$

where, in the last step, we used eq. (7.72) for $W_{00}^{(2)}$. Since $kT \gg W_0^0$ at the temperatures of interest, the Boltzmann factor in the numerator in this expression can be replaced by 1. In the denominator, we use eq. (7.62) for W_l^0. At sufficiently high temperatures $l \approx l + 1$ for most terms in the sum, which can then be replaced by an integral (185; cf. eq. 7.47):

$$\sum_l (2l+1) e^{-l(l+1)\hbar^2/2IkT} \approx \int_0^\infty 2l\, e^{-l^2\hbar^2/2IkT}\, dl = \frac{2IkT}{\hbar^2}. \tag{7.78}$$

With these approximations, eq. (7.77) becomes (185):

$$\chi = \frac{N\mu^2}{3kT}, \tag{7.79}$$

which is just the temperature-dependent term in the Langevin-Debye formula of the classical theory (see eqs. 7.7–7.8). Though the derivation above is for the special case of a gas of rigid rotators, Van Vleck (1927a; 1932b) showed that the result holds under very general conditions in the new quantum theory *and does not involve spatial quantization*. And thus was the Kuhn loss in susceptibility theory recovered.[89]

Van Vleck did not bother to show explicitly that, despite appearances to the contrary, this derivation of the susceptibility of a gas of rigid rotators does not involve the choice of a preferred z-axis for the quantization of L_z. For Van Vleck, this was just an instance of his general theorem of spectroscopic stability (137–143). To bring out the role of this theorem in this specific case, we prove that the susceptibility of a gas of rigid rotators is indeed independent of our choice of a z-axis. In the calculation above, we used the orthonormal basis $\{|l,m\rangle\}_{m=-l}^{l}$ to span the $(2l+1)$-dimensional subspace corresponding to the $(2l+1)$-fold degenerate energy eigenvalue W_l^0 (see eq. 7.62). The number m labels the different values of L_z with respect to a z-axis chosen in the direction of the applied field **E**. We can span that same subspace with a different orthonormal basis $\{|l,r\rangle\}_{r=-l}^{l}$, where r labels the different values of L_z with respect to a z-axis in some arbitrary direction. The vectors in the old basis can be written in terms of the new one:

$$|l,m\rangle = \sum_{r=-l}^{l} |l,r\rangle\langle l,r|l,m\rangle. \tag{7.80}$$

What we need to show is that the derivation of eq. (7.79) for the susceptibility of a gas of rigid rotators does not depend on whether we use m or r to label the degeneracy. More

[89] As Born and Jordan (1930, 222–223) noted in their textbook, this "was one of the first "practical" successes of the new quantum mechanics. The methods of the old quantum theory [here a footnote is inserted citing Pauli (1921)], in which a "directional quantization" of the axes of the molecules had to be imposed, lead to a wrong numerical factor at high temperatures."

specifically, we need to check whether $\sum_m W^{(2)}_{lm}$ in eq. (7.75) is invariant under rotation of the z-axis, i.e., under switching from the orthonormal basis $\{|l,m\rangle\}^{l}_{m=-l}$ to the orthonormal basis $\{|l,r\rangle\}^{l}_{r=-l}$. Using eq. (7.65), we can write:

$$\sum_m W^{(2)}_{lm} = \sum_{l'\neq l} \frac{\mu^2}{W^0_l - W^0_{l'}} \left(\sum_{m,m'} |\langle l',m'|\cos\vartheta|l,m\rangle|^2 \right). \tag{7.81}$$

It is easy to show that m and m' in the expression in parentheses can be replaced by r and r':[90]

$$\sum_{m,m'} |\langle l',m'|\cos\vartheta|l,m\rangle|^2 = \sum_{r,r'} |\langle l',r'|\cos\vartheta|l,r\rangle|^2. \tag{7.82}$$

The derivation of the susceptibility, thus, does not depend on how the degeneracy in the energy levels W^0_l is resolved.

To conclude our discussion of Van Vleck's work in this area, we consider some features of his more general derivation of the Langevin-Debye formula and how they relate to the hated "bugbear" of spatial quantization. First, recall eq. (7.19), what Van Vleck called a "sort of generalized Langevin-Debye formula" (40). The last step in obtaining this formula is the assumption that $\overline{\overline{p_z^2}} = \frac{1}{3}\overline{\overline{p^2}}$ (eq. 7.20), i.e., the mean square average of the unperturbed electric moment in the z-direction (the direction of the field *even when the field is turned off*) is $1/3$ the mean square average of the total moment. In the classical theory, this is exactly what one would expect. When the field is turned off, there should be equal contributions to the mean square of the moment for each spatial dimension. This is exactly the feature, however, that was *eliminated* by spatial quantization in the old quantum theory. This made it possible for molecules in high-energy states to contribute to the temperature-dependent term in the Langevin-Debye formula (see our discussion in sec. 7.5.4).

In the general quantum-mechanical derivation of the Langevin-Debye formula, Van Vleck ultimately produced a quantum-theoretical analogue of eq. (7.19) (186–194). This

---

[90] The sum over m and m' in eq. (7.81) for fixed values of l and l' can be written as:

$$\sum_{m,m'} |\langle l',m'|\cos\vartheta|l,m\rangle|^2 = \sum_{m,m'} \langle l',m'|\cos\vartheta|l,m\rangle\langle l,m|\cos\vartheta|l',m'\rangle.$$

With the help of eq. (7.80) we can write the vectors $|l,m\rangle$ in terms of the vectors $|l,r\rangle$:

$$\sum_{m,m',r,r',\hat{r},\hat{r}'} \langle l',m'|l',r'\rangle\langle l',r'|\cos\vartheta|l,r\rangle\langle l,r|l,m\rangle\langle l,m|l,\hat{r}\rangle\langle l,\hat{r}|\cos\vartheta|l',\hat{r}'\rangle\langle l',\hat{r}'|l',m'\rangle,$$

where $m,r,\hat{r}$ run from $-l$ to l and $m',r',\hat{r}'$ run from $-l'$ to l'. Reordering the various factors, we find

$$\sum_{m,m',r,r',\hat{r},\hat{r}'} \langle l,r|l,m\rangle\langle l,m|l,\hat{r}\rangle\langle l',\hat{r}'|l',m'\rangle\langle l',m'|l',r'\rangle\langle l',r'|\cos\vartheta|l,r\rangle\langle l,\hat{r}|\cos\vartheta|l',\hat{r}'\rangle.$$

Since $\sum_m \langle l,r|l,m\rangle\langle l,m|l,\hat{r}\rangle = \langle l,r|l,\hat{r}\rangle = \delta_{r\hat{r}}$ and $\sum_{m'} \langle l',\hat{r}'|l',m'\rangle\langle l',m'|l',r'\rangle = \delta_{\hat{r}'r'}$, this reduces to

$$\sum_{r,r'} \langle l',r'|\cos\vartheta|l,r\rangle\langle l,r|\cos\vartheta|l',r'\rangle = \sum_{r,r'} |\langle l',r'|\cos\vartheta|l,r\rangle|^2,$$

which is what we wanted to prove.

generalized formula hinges on an assumption analogous to eq. (7.20) in the classical theory. In quantum mechanics, it takes the form (140):[91]

$$\sum_{m,m'} |\langle l,m|L_z|l',m'\rangle|^2 = \frac{1}{3}\sum_{m,m'} |\langle l,m|L|l',m'\rangle|^2. \tag{7.83}$$

As Van Vleck emphasized, and as we showed explicitly in the case of eq. (7.82) above, relations such as these are clearly, as Van Vleck put it somewhat awkwardly, "invariant of the choice of axis of quantization" (140). This relation is just one example of the more general theorem of spectroscopic stability that Van Vleck was able to prove in quantum mechanics (137–143). The upshot of this proof was that, in quantum mechanics, quantities like $\overline{\overline{p^2}}$ no longer depend on the axis of quantization as they had in the old quantum theory.

The strange story of the constant C in the Langevin-Debye formula can ultimately be seen as the story of spatial quantization's brief rise and rapid fall. The factors of $1/3$ in both the classical and quantum-mechanical formulas express that mean squares of vector components do not depend on the axes with respect to which those averages are taken. In both theories, $\overline{\overline{p_z^2}} = \frac{1}{3}\overline{\overline{p^2}}$, where the z-direction can be arbitrarily chosen. The strange values of C in the old quantum theory resulted from the elimination of this very feature, which was essential if one wanted to derive the temperature-dependent term of the Langevin-Debye formula at all. Without spatial quantization there simply was no temperature-dependent term in the old quantum theory. Unfortunately, spatial quantization came with a whole raft of problems. In light of this, we can clearly see why Van Vleck used the story of C to illustrate the defects of the old quantum theory and the success of matrix mechanics in restoring the predictions of the classical theory.

7.6 Kuhn Losses Revisited

Both Van Vleck's 1926 *Bulletin* and his 1932 book do what Kuhn said good textbooks should do: they clearly lay out the principles and the formalism of the theories they cover and show how these theories can be used to solve a number of canonical problems, thus training their readers to become researchers in the relevant fields. Yet they do so without paying the price Kuhn (1996, 137) suggested was unavoidable: though written in the midst or in the aftermath of a period of major conceptual upheaval, they do not "disguise […] the role [and] the very existence" of this upheaval nor do they "truncat[e] the scientist's sense of his discipline's history."

This is especially striking in the case of the 1932 book. Van Vleck spent roughly a third of his book (121 out of a total of 373 pages) on the classical theory (chaps. I–IV) and the old quantum theory (chap. V). One might argue that chap. V served a purely rhetorical purpose. The old quantum theory's problems with susceptibilities are a great foil for the new quantum mechanics' successes in that same area. Such use of history in a textbook can readily be reconciled with Kuhn's views. There are two further considerations regarding this chapter that would seem to be in Kuhn's favor. First, the history recounted in chap. V is somewhat misleading in that Van Vleck, inadvertently or deliberately, made it sound as if there had

[91] Instead of the angular momentum **L**, Van Vleck considered a general vector quantity **A**.

been reliable experimental evidence disproving the "wonderful nonsense" produced by the old quantum theory all along. In fact, such evidence had only become available around the time of the theory's demise. Second, we know that Van Vleck wanted to cut chap. V to make room for new material when he began revising his book for a second edition decades later. He had no such plans, however, for chaps. I–IV on the classical theory.

The pedagogical goal of those early chapters was not merely to provide propaganda for the superior, quantum-mechanical treatment of susceptibilities. Rather, their main function was to prepare the reader for the quantum-mechanical calculation of susceptibilities by showing how such calculations are done in the classical theory. In his biographical memoir about his teacher, Anderson (1987, 509) noted that this approach might not be suited for "a modern text for physicists poorly trained in classical mechanics" (see sec. 7.1.5). In the early 1930s, however, Van Vleck could certainly assume his intended readers to be well versed in classical mechanics.

Using older theories for pedagogical purposes in this way is not compatible with Kuhn's picture. A new paradigm is supposed to come with its own new suite of tools for the pursuit of normal science. It is supposed to provide its own new set of *exemplars* to "show [students] by example how their job is to be done" (Kuhn 1996, 187; discussed in sec. 7.1.4).

Van Vleck's book provides a clear example of such an exemplar. It gives a general recipe with many concrete illustrations of how one can calculate susceptibilities, say the electric susceptibility of a gas. First, one has to decide on a mechanical system to model the constituent molecules of the gas. This can be a specific system (e.g., a rigid rotator) or a generic one (a classical multiply-periodic system solvable in action-angle variables, a quantum system with an energy spectrum satisfying some not overly restrictive conditions). One then has to do a perturbative calculation to compute the time-average of the component of the electric dipole moment in the direction of the external field of one copy of this system in a given state. Finally, one has to take the average of this time-average for an individual system over a thermal ensemble of many such systems in all possible states.

This general procedure works in classical theory, in the old quantum theory, and in modern quantum mechanics. The exemplar thus cuts across two paradigm shifts! Suman Seth (2010, 265–267) makes a similar point, when he contrasts a continuity of *problems* with a discontinuity in *principles* (see our discussion in sec. 7.1.5).

That the techniques from statistical mechanics for the calculation of ensemble averages work in all three theories does not seem to call for further comment. That this is also true for the perturbative techniques used to calculate the relevant time-averages is less obvious. These techniques were originally developed in the context of celestial mechanics. They were adapted to deal with atomic mechanics, to use Born's phrase (see note 35), in the old quantum theory. A large part of Van Vleck's NRC *Bulletin* on the old quantum theory was devoted to these techniques, which were used to derive classical expressions that could then be translated into quantum expressions under the guidance of the correspondence principle, according to which the quantum expression would have to merge with the classical one in the limit of high quantum numbers. The derivation of the Kramers dispersion formula is a prime example of this strategy (see sec. 7.3.2). In the *Dreimännerarbeit*, Born, Heisenberg, and Jordan (1926) adapted these perturbative techniques to the new matrix mechanics.

What lay behind, and made possible, this continuity of technique was a remarkable continuity of formalism in the transition from classical to quantum physics. Neither the old nor the new quantum theory did away with classical mechanics. The old quantum theory just added the Sommerfeld-Wilson quantum conditions to select a subset of the classically allowed motions. For some specific simple systems, notably the one-electron atom and the harmonic oscillator, this led to satisfactory results (although even the zero-point energy of a simple harmonic oscillator had to be added by sleight of hand). In other cases, multi-electron atoms or the rigid rotator, it did not. The old quantum theory actually was at its best, if generic multiply-periodic systems could be used, such as in the derivation of the Kramers dispersion formula. In those cases one could sometimes find the quantum counterpart of a classical formula through educated guesswork guided by the correspondence principle. As we saw in sec. (7.1.2), Van Vleck (1926a, 227; 1932b, 107) also appealed to the correspondence principle to *reject* formulas for susceptibilities produced in the old quantum theory on the grounds that they did not reduce to the Langevin-Debye formula at high temperatures, where that classical formula ought to hold. In that case, however, the correspondence principle did not suggest a better candidate for a quantum formula for susceptibilities.

The "wonderful nonsense" produced on this score by Pauli (1921) and Pauling (1926b) mercilessly reveals the limitations of the old quantum theory's basic approach—imposing quantum conditions on classical mechanics. Their calculations gave nonsensical results, not because the general procedure for calculating susceptibilities described above does not work in the old quantum theory, but because of the way they quantized the angular momentum of the rigid rotator, their model for polar molecules such as HCl. The problem was twofold. First, instead of the relation $L^2 = l(l+1)\hbar^2$ ($l = 0, 1, 2, ...$), sanctioned by modern quantum mechanics, they used $L^2 = l^2\hbar^2$ (Pauli with integer values, Pauling with half-integer values for l, where $l = 0$ is forbidden in both cases). As a result, the ensemble average $\overline{\overline{L^2}}$ is not equal to three times the ensemble average $\overline{\overline{L_z^2}}$ in the old quantum theory, whereas this relation does hold both in the classical theory and in quantum mechanics. Second, they saw themselves forced to adopt what Van Vleck (1927a, 37; 1932b, 110) later derided as the "bugbear" of spatial quantization.

Matrix mechanics, the incarnation of quantum mechanics Van Vleck was most familiar and most comfortable with, retained the formalism of classical mechanics *without* inflicting this kind of disfigurement. This is remarkable because, unlike the old quantum theory, it radically changed the interpretation of the formalism. The basic idea of Heisenberg's *Umdeutung* was to conceive of the quantities related by the laws of classical mechanics as arrays of numbers. In more mature versions of the theory, these became matrices and then operators acting in Hilbert space. Unlike the old quantum theory, the new quantum mechanics came with a systematic prescription for imposing quantum conditions. It replaced the Sommerfeld-Wilson quantum conditions by the basic commutation relations of position and momentum. As Paul Dirac (1925) first pointed out, these were the quantum analogues of Poisson brackets in classical mechanics. The recovery of the Langevin-Debye formula for the electric susceptibility in gases, a Kuhn loss of the old quantum theory, beautifully illustrates the advantages of the new quantum theory over the old. Looking at the situation from this perspective, one readily understands Van Vleck's assessment at the beginning of the chapter on the old quantum theory in his 1932 book: "there is perhaps no better field than that of electric and magnetic susceptibilities to illustrate the inadequacies of the old quantum theory and how they have been removed by the new mechanics" (Van Vleck 1932b, 105).

Van Vleck saw these issues clearly only in retrospect. When he took the time to list and discuss the various flaws of the old quantum theory in his 1926 *Bulletin*, he did not include its failure to give a sensible result for electric susceptibilities. As we have seen, this was not because of ignorance (he had read the key paper by Pauli [1921] as a graduate student), but rather because of the intense focus of physicists at the time on spectroscopic phenomena. We began our paper with a quotation from an article in a chemistry journal, in which Van Vleck (1928b, 493) characterized physicists as being "entranced by spectral lines," willing to ignore the peripheral phenomena of electric and magnetic susceptibilities. In 1925 Van Vleck had been such a physicist. All of this changed as he began to focus his research on electric and magnetic susceptibilities and came to understand that some of the old quantum theory's most serious flaws, and some of the new quantum theory's most remarkable successes, were in areas that had hardly attracted attention before. When Van Vleck told the chemists that physicists tend to close their eyes to phenomena other than spectra, he was also admonishing himself.

Abbreviations and Archives

AHQP	Archive for History of Quantum Physics. American Philosophical Society, Philadelphia
AIP-NBL	American Institute of Physics, Niels Bohr Library, College Park, MD

Acknowledgments

We want to thank Massimiliano Badino, Rich Bellon, Tony Duncan, Fred Fellows, Clayton Gearhart, Don Howard, David Huber, Jeremiah James, Luc Janssen, Christian Joas, Marta Jordi, David Kaiser, Sally Gregory Kohlstedt, Christoph Lehner, Chun Lin, Joe Martin, Jaume Navarro, Jürgen Renn, Serge Rudaz, Rob "Ryno" Rynasiewicz, Roger Stuewer, and Arkady Vainshtein for helpful comments, discussion, and references. We are grateful to John Comstock for permission to use the pictures in figs. (7.2) and (7.3). Work on this paper was supported by Jürgen Renn's department of the *Max-Planck-Institut für Wissenschafts-geschichte* and by the National Science Foundation under grant STS–1027018.

References

Alexandrow, W. (1921). Eine Bemerkung zur Langevinschen Formel für die Suszeptibilität paramagnetischer Körper. *Physikalische Zeitschrift* 22: 258–259.

Anderson, Philip W. (1987). John Hasbrouck Van Vleck, March 13, 1899–October 27, 1980. *National Academy of Sciences Biographical Memoirs* 56: 501–540.

Baltas, Aristides, Kostas Gavroglu, and Vassiliki Kindi (2000). A Discussion with Thomas S. Kuhn. In: *The Road since Structure*. Ed. James Conant and John Haugeland. Chicago: The University of Chicago Press. 255–323.

Baym, Gordon (1969). *Lectures on Quantum Mechanics*. Reading, MA: Addison-Wesley.

Bohr, Niels (1913). On the Constitution of Atoms and Molecules (Part I). *Philosophical Magazine 6th Series* 26: 1–25.

— (1918). On the Quantum Theory of Line Spectra. Part II. On the Hydrogen Spectrum. *Det Kongelige Danske Videnskabernes Selskab, Matematisk-fysiske Meddelser* 4: 36–100.

Bohr, Niels, Hendrik A. Kramers, and John C. Slater (1924). The Quantum Theory of Radiation. *Philosophical Magazine 6th Series* 47: 785–802.

Born, Max (1924). Über Quantenmechanik. *Zeitschrift für Physik* 26: 379–395.

— (1925). *Vorlesungen über Atommechanik*. Berlin: Julius Springer.

— (1927). *The Mechanics of the Atom*. London: G. Bell and Sons.

Born, Max, Werner Heisenberg, and Pascual Jordan (1926). Zur Quantenmechanik II. *Zeitschrift für Physik* 35: 557–615.

Born, Max and Pascual Jordan (1925). Zur Quantenmechanik. *Zeitschrift für Physik* 34: 858–888.

— (1930). *Elementare Quantenmechanik (Zweiter Band der Vorlesungen über Atommechanik)*. Berlin: Julius Springer.

Buchwald, Jed Z. (1985). *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century*. Chicago: The University of Chicago Press.

Cassidy, David C. (2007). Oppenheimer's First Paper: Molecular Band Spectra and a Professional Style. *Historical Studies in the Physical and Biological Sciences* 37: 247–269.

Davisson, Clinton J. (1916). The Dispersion of Hydrogen and Helium on Bohr's Theory. *Physical Review* 8: 20–27.

Debye, Peter (1912). Einige Resultate einer kinetischen Theorie der Isolatoren. *Physikalische Zeitschrift* 13: 97–100.

— (1915). Die Konstitution des Wasserstoff-Moleküls. *Sitzungsberichte der mathematisch-physikalischen Klasse der Königlichen Bayerischen Akademie der Wissenschaften zu München*: 1–26.

Dennison, David M. (1926). The Rotation of Molecules. *Physical Review* 28: 318–333.

Dirac, Paul A. M. (1925). The Fundamental Equations of Quantum Mechanics. *Proceedings of the Royal Society A* 109: 642–653.

— (1930). *The Principles of Quantum Mechanics*. Oxford: Oxford University Press.

Duncan, Anthony and Michel Janssen (2007). On the Verge of *Umdeutung* in Minnesota: Van Vleck and the Correspondence Principle. 2 Pts. *Archive for History of Exact Sciences* 61: 553–624, 625–671.

— (2008). Pascual Jordan's Resolution of the Conundrum of the Wave-Particle Duality of Light. *Studies in History and Philosophy of Modern Physics* 21: 634–666.

— (2013). (Never) Mind your p's and q's: Von Neumann versus Jordan on the Foundations of Quantum Theory. *European Physical Journal H* 38: 175–259.

Einstein, Albert (1917). Zur Quantentheorie der Strahlung. *Physikalische Zeitschrift* 18: 121–128.

Enz, Charles P. (2002). *No Time to Be Brief. A Scientific Biography of Wolfgang Pauli*. Oxford: Oxford University Press.

Epstein, Paul S. (1922). Die Störungsrechnung im Dienste der Quantentheorie. III. Kritische Bemerkungen zur Dispersionstheorie. *Zeitschrift für Physik* 9: 92–110.

Fellows, Frederick H. (1985). *J. H. Van Vleck: The Early Life and Work of a Mathematical Physicist*. PhD thesis. University of Minnesota.

Feynman, Richard P., Robert B. Leighton, and Matthew Sands (1964). *The Feynman Lectures on Physics*. 3 Vols. Reading, MA: Addison-Wesley.

Gearhart, Clayton A. (2010). "Astonishing Successes" and "Bitter Disappointment": The Specific Heat of Hydrogen in Quantum Theory. *Archive for History of Exact Sciences* 64: 113–202.

Gerlach, Walther and Otto Stern (1922). Das magnetische Moment des Silberatoms. *Zeitschrift für Physik* 9: 353–355.

Heilbron, John L. and Thomas S. Kuhn (1969). The Genesis of the Bohr Atom. *Historical Studies in the Physical Sciences* 1: 211–290.

Heisenberg, Werner (1925a). Über eine Anwendung des Korrespondenzprinzips auf die Frage nach der Polarisation des Fluoreszenzlichtes. *Zeitschrift für Physik* 31: 617–626.

— (1925b). Über die quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen. *Zeitschrift für Physik* 33: 879–893.

Janssen, Michel and Matthew Mecklenburg (2007). From Classical to Relativistic Mechanics: Electromagnetic Models of the Electron. In: *Interactions: Mathematics, Physics and Philosophy, 1860–1930*. Ed. Vincent F. Hendricks, Klaus F. Jørgensen, Jesper Lützen, and Stig A. Pedersen. Berlin: Springer. 65–134.

Jordan, Pascual (1936). *Anschauliche Quantentheorie. Eine Einführung in die moderne Auffassung der Quantenerscheinungen*. Berlin: Julius Springer.

Kemble, Edwin C. (1929). The General Principles of Quantum Mechanics. Part I. *The Physical Review Supplement* 1: 157–215.

— (1937). *The Fundamental Principles of Quantum Mechanics with Elementary Applications*. New York: McGraw-Hill.

Kemble, Edwin C. and Edward L. Hill (1930). The General Principles of Quantum Mechanics. Part II. *Reviews of Modern Physics* 2: 1–59.

Kramers, Hendrik A. (1924a). The Law of Dispersion and Bohr's Theory of Spectra. *Nature* 113: 673–676.

— (1924b). The Quantum Theory of Dispersion. *Nature* 114: 310–311.

Kramers, Hendrik A. and Werner Heisenberg (1925). Über die Streuung von Strahlung durch Atome. *Zeitschrift für Physik* 31: 681–707.

Kronig, Ralph (1926a). The Dielectric Constant of Diatomic Dipole-Gases on the New Quantum Mechanics. *Proceedings of the National Academy of Sciences* 12: 488–493.

— (1926b). The Dielectric Constant of Symmetrical Polyatomic Dipole-Gases on the New Quantum Mechanics. *Proceedings of the National Academy of Sciences* 12: 608–612.

Kuhn, Thomas S. (1996). *The Structure of Scientific Revolutions*. 3rd ed. Chicago: The University of Chicago Press.

Kuhn, Thomas S., John L. Heilbron, Paul Forman, and Lini Allen (1967). *Sources for the History of Quantum Physics. An Inventory and Report*. Philadelphia: American Philosophical Society.

Kuhn, Werner (1925). Über die Gesamtstärke der von einem Zustande ausgehenden Absorptionslinien. *Zeitschrift für Physik* 33: 408–412.

Ladenburg, Rudolf (1921). Die quantentheoretische Deutung der Zahl der Dispersionselektronen. *Zeitschrift für Physik* 4: 451–468.

Ladenburg, Rudolf and Fritz Reiche (1923). Absorption, Zerstreuung und Dispersion in der Bohrschen Atomtheorie. *Die Naturwissenschaften* 11: 584–598.

Langevin, Paul (1905a). Magnétisme et théorie des électrons. *Annales de Chimie et de Physique* 5: 70–127.
— (1905b). Sur la théorie du magnétisme. *Journal de Physique* 4: 678–688.
Lin, Chun C., Roger H. Stuewer, and John H. Van Vleck (1977). "On Minnesota" gave Wisconsin Its Popular Fight Song. *Alumni News* (University of Minnesota) December 1976/January 1977: 1, 4.
— (1980). Wisconsin versus Minnesota in Song. *On Wisconsin* 2:2 (Summer 1980): 4.
Manneback, Charles (1926). Die Dielektrizitätskonstante der zweiatomigen Dipolgase nach der Wellenmechanik. *Physikalische Zeitschrift* 27: 563–569.
Mehra, Jagdish and Helmut Rechenberg (1982a). *The Historical Development of Quantum Theory.* Vol. 3. *The Formulation of Matrix Mechanics and Its Modifications, 1925–1926*. New York: Springer.
— (1982b). *The Historical Development of Quantum Mechanics.* Vol. 4. Part 1: *The Fundamental Equations of Quantum Mechanics, 1925–1926.* Part 2: *The Reception of the New Quantum Mechanics, 1925–1926*. Berlin: Springer.
Mensing, Lucy (1926). Die Rotations-Schwingungsbanden nach der Quantenmechanik. *Zeitschrift für Physik* 36: 814–823.
Mensing, Lucy and Wolfgang Pauli (1926). Über die Dielektrizitätskonstante von Dipolgasen nach der Quantenmechanik. *Physikalische Zeitschrift* 27: 509–512.
Pauli, Wolfgang (1921). Zur Theorie der Dieelektrizitätskonstante zweiatomiger Dipolgase. *Zeitschrift für Physik* 6: 319–327.
— (1930). Review of (Born and Jordan, 1930). *Die Naturwissenschaften* 18: 602.
— (1933). Review of (Van Vleck, 1932b). *Die Naturwissenschaften* 21: 239.
Pauling, Linus (1926a). The Quantum Theory of the Dielectric Constant of Hydrogen Chloride and Similar Gases. *Proceedings of the National Academy of Sciences* 12: 32–35.
— (1926b). The Quantum Theory of the Dielectric Constant of Hydrogen Chloride and Similar Gases. *Physical Review* 27: 568–577.
— (1927). The Influence of a Magnetic Field on the Dielectric Constant of a Diatomic Dipole Gas. *Physical Review* 29: 145–160.
— (1932). Review of (Van Vleck, 1932b). *The Journal of the American Chemical Society* 54: 4119–4121.
Pauling, Linus and Samuel Goudsmit (1930). *The Structure of Line Spectra*. New York: McGraw-Hill.
Rojansky, Vladimir (1938). *Introductory Quantum Mechanics*. Englewood Cliffs: Prentice-Hall.
Ruark, Arthur E. (1926). Review of (Van Vleck, 1926b). *Journal of the Optical Society of America* 13: 312.
Ruark, Arthur E. and Harold C. Urey (1930). *Atoms, Molecules and Quanta*. New York: McGraw-Hill.
Ruse, Michael (1989). *The Darwinian Paradigm. Essays on Its History, Philosophy, and Religious Implications*. London and New York: Routledge.
Schrödinger, Erwin (1926). Quantisierung als Eigenwertproblem. (Dritte Mitteilung). Störungstheorie, mit Anwendung auf den Starkeffekt der Balmerlinien. *Annalen der Physik* 80: 437–490.
Serber, Robert (1992). *The Los Alamos Primer. The First Lectures on How to Build an Atomic Bomb*. Berkeley: University of California Press.

Seth, Suman (2010). *Crafting the Quantum. Arnold Sommerfeld and the Practice of Theory, 1890–1926*. Cambridge, MA: The MIT Press.
Smekal, Adolf (1927). Review of (Van Vleck, 1926b). *Physikalische Zeitschrift* 28: 62–63.
Sommerfeld, Arnold (1915). Die allgemeine Dispersionsformel nach dem Bohrschen Modell. In: *Festschrift Julius Elster und Hans Geitel*. Ed. Karl Bergwitz. Braunschweig: Friedrich Vieweg und Sohn. 549–584.
— (1917). Die Drudesche Dispersionstheorie vom Standpunkte des Bohrschen Modelles und die Konstitution von H_2, O_2 und N_2. *Annalen der Physik* 53: 497–550.
— (1919). *Atombau und Spektrallinien*. Braunschweig: Friedrich Vieweg und Sohn.
— (1923). *Atomic Structure and Spectral Lines*. London: Methuen. (Transl. of 3rd German ed. of *Atombau und Spektrallinien*).
Stevens, K. W. H. (1995). Magnetism. In: *Twentieth Century Physics.* Vol. II. Ed. Laurie M. Brown, Abraham Pais, and Brian Pippard. Bristol and Philadelphia/New York: Institute of Physics Publishing/American Institute of Physics Press. 1111–1181.
Stoner, Edmund C. (1932). Magnetism and Quantum Mechanics (Review of Van Vleck, 1932b). *Nature* 130: 490–491.
Swann, William F. G., John T. Tate, H. Bateman, and E. H. Kennard (1922). *Electrodynamics of Moving Media*. Washington: National Research Council (Bulletin of the National Research Council 4, Part 24).
Thomas, Willy (1925). Über die Zahl der Dispersionselektronen, die einem stationären Zustande zugeordnet sind (Vorläufige Mitteillung). *Die Naturwissenschaften* 13: 627.
Van der Waerden, Bartel L., ed. (1968). *Sources of Quantum Mechanics*. New York: Dover.
Van Vleck, John H. (1922). The Normal Helium Atom and Its Relation to the Quantum Theory. *Philosophical Magazine 6th Series* 44: 842–869.
— (1924a). A Correspondence Principle for Absorption. *Journal of the Optical Society of America* 9: 27–30.
— (1924b). The Absorption of Radiation by Multiply Periodic Orbits, and Its Relation to the Correspondence Principle and the Rayleigh-Jeans Law. Part I. Some Extensions of the Correspondence Principle. *Physical Review* 24: 330–346.
— (1924c). The Absorption of Radiation by Multiply Periodic Orbits, and Its Relation to the Correspondence Principle and the Rayleigh-Jeans Law. Part II. Calculation of Absorption by Multiply Periodic Orbits. *Physical Review* 24: 347–365.
— (1926a). Magnetic Susceptibilities and Dielectric Constants in the New Quantum Mechanics. *Nature* 118: 226–227.
— (1926b). *Quantum Principles and Line Spectra*. Washington: National Research Council (Bulletin of the National Research Council 10, Part 4).
— (1927a). On Dielectric Constants and Magnetic Susceptibilities in the New Quantum Mechanics. Part I. A General Proof of the Langevin-Debye Formula. *Physical Review* 29: 727–744.
— (1927b). On Dielectric Constants and Magnetic Susceptibilities in the New Quantum Mechanics. Part II. Application to Dielectric Constants. *Physical Review* 30: 31–54.
— (1927c). The New Quantum Mechanics. *Journal of the Optical Society of America* 14: 108–112.
— (1928a). On Dielectric Constants and Magnetic Susceptibilities in the New Quantum Mechanics. Part III. Application to Dia- and Paramagnetism. *Physical Review* 29: 587–613.

— (1928b). The New Quantum Mechanics. *Chemical Reviews* 5: 467–507.

— (1928c). The New Quantum Mechanics. *Journal of the Optical Society of America* 16: 301–306.

— (1932a). Note sur les susceptibilités des ions Samarium et Europium. In: *Le magnétisme. Rapport et discussions du sixième conseil de physique tenu à Bruxelles du 20 au 25 octobre 1930*. Paris: Gauthier-Villars. 55–64.

— (1932b). *The Theory of Electric and Magnetic Susceptibilities*. Oxford: Oxford University Press.

— (1964). American Physics Comes of Age. *Physics Today* 17(6): 21–26.

— (1968). My Swiss Visits of 1906, 1926, and 1930. *Helvetica Physica Acta* 41: 1234–1237.

— (1971). Reminiscences of the First Decade of Quantum Mechanics. In: *Symposium No. 5, 1971: A Symposium Held in Honor of Van Vleck*. Ed. Per-Olov Lödwin. International Journal of Quantum Chemistry. New York: Wiley. 3–20.

— (1974). Acceptance Speech. *Koninklijke Nederlandse Akademie van Wetenschappen. Bijzondere bijeenkomst der afdeling natuurkunde ... 28 september 1974 ... voor de plechtige uitreiking van de Lorentz-medaille aan Prof. Dr. J. H. Van Vleck.*

— (1992a). John Hasbrouck Van Vleck (Autobiographical Note). In: *Nobel Lectures in Physics (1971–1980)*. Ed. Stig Lundqvist. Singapore: World Scientific Publishing. 351–352.

— (1992b). Quantum Mechanics: the Key to Understanding Magnetism. In: *Nobel Lectures in Physics (1971–1980)*. Ed. Stig Lundqvist. Singapore: World Scientific Publishing. 353–369.

Wigner, Eugene P. (1931). *Gruppentheorie und ihre Anwendung auf die Quantenmechanik der Atomspektren*. Berlin: Julius Springer.

Wigner, Eugene P. and Andrew Szanton (1992). *The Recollections of Eugene P. Wigner as Told to Andrew Szanton*. New York: Plenum.

Zahn, Charles T. (1924). The Electric Moment of Gaseous Molecules of Halogen Hydrides. *Physical Review* 24: 400–417.

Zahn, Charles T. and A. H. Spees (1938). A Critical Analysis of the Classical Experiments on the Variation of Electron Mass. *Physical Review* 53: 511–521.

Chapter 8
Max Born's *Vorlesungen über Atommechanik, Erster Band*

Domenico Giulini

8.1 Outline

A little more than half a year before matrix mechanics was born, Max Born finished his book *Vorlesungen über Atommechanik, Erster Band*, which was a state-of-the-art presentation of Bohr-Sommerfeld quantization.[1] This book is remarkable for its epistemological as well as technical aspects. In this contribution I highlight one aspect from each of these two categories, the first concerning the role of axiomatization in the heuristics of physics, and the second concerning the problem of quantization proper before Heisenberg and Schrödinger.

Max Born's monograph *Vorlesungen über Atommechanik, Erster Band,* was published in 1925 by Julius Springer Verlag (Berlin) as volume II in the Series *Struktur der Materie* (Born 1925). The second volume of the *Vorlesungen* appeared in 1930 as *Elementare Quantenmechanik*, coauthored by Pascual Jordan, and was volume IX in the same series. In the second volume the authors attempt to give a comprehensive and self-contained account of matrix mechanics (Born and Jordan 1930). The word "elementare" in the title alludes, in a sense, to the logical hierarchy of mathematical structures and is intended to mean "by algebraic methods (however sophisticated) only," as opposed to Schrödinger's wave mechanics, which uses (non-elementary) concepts from calculus. Since, by the end of 1929 (the preface is dated 6 December 1929), several comprehensive accounts of wave mechanics had already been published,[2] the authors felt that it was time to do the same for matrixmechanics.

Here I will focus entirely on the first volume, which gave a state-of-the-art account of Bohr-Sommerfeld quantization from the analytic perspective. One might therefore suspect that the book had almost no impact on the post-1924 development[3] of quantum mechanics proper, whose 1925–26 breakthrough did not originate from further analytical refinements of Bohr-Sommerfeld theory.[4] But this would be a fruitless approach to Born's book, which is truly remarkable in at least two aspects: First, for its presentation of analytical mechanics, in particular Hamilton-Jacobi theory and its applications to integrable systems, as well as perturbation theory, and second, for its epistemological orientation. Though it is tempting

[1]As usual, I use the term "Bohr-Sommerfeld quantization" throughout as shorthand for what probably should be called Bohr-Ishiwara-Wilson-Planck-Sommerfeld-Epstein-Schwarzschild … quantization.

[2]Born and Jordan mention the following four books: Arthur Haas's *Materiewellen und Quantenmechanik* (1928), Arnold Sommerfeld's *Atombau und Spektrallinien*, Vol. 2 *(Wellenmechanischer Ergänzungsband)* (1929), Louis de Broglie's *Einführung in die Wellenmechanik* (1929), and Yakov Frenkel's *Einführung in die Wellenmechanik* (1929).

[3]The preface is dated November 1924.

[4]A partial revival and refinement of Bohr-Sommerfeld quantization occurred during the late 1950s, as a tool to construct approximate solutions to Schrödinger's equation, even for non-separable systems (Keller 1958); see also (Gutzwiller 1990). Ever since it has remained an active field of research in atomic and molecular physics.

indeed to present some of the analytic delicacies that Born's book has to offer, it is equally tempting to highlight some of the epistemological aspects, since the latter do not seem to be widely appreciated. Instead, Born's book is most often cited and praised in connection with Hamilton and Hamilton-Jacobi theory, for example in the older editions of Goldstein's book on classical mechanics.[5]

8.2 Structure of the Book

The book is based on lectures Born gave in the winter semester 1923/24 at the University of Göttingen and was written with the help of Born's assistant Friedrich Hund, who wrote substantial parts and contributed important mathematical results (e.g. the uniqueness of action-angel variables). Werner Heisenberg outlined some sections, in particular the final ones dealing with the helium atom. The text is divided into 49 sections, grouped into 5 chapters, and a mathematical appendix, which together amount to almost 350 pages. It may be naturally compared and contrasted with Sommerfeld's *Atombau und Spektrallinien I*, which has about twice the pages. As already stated, Born's text is today largely cited and remembered (if at all!) for its presentation of Hamilton-Jacobi theory and perturbation theory (as originally developed for astronomical problems). Its presentation is considered comprehensive and most concise, though today one would approach some of the material using more geometric methods (compare Arnold (1978) or Abraham and Marsden (1978)).

The chapter contents are as follows:

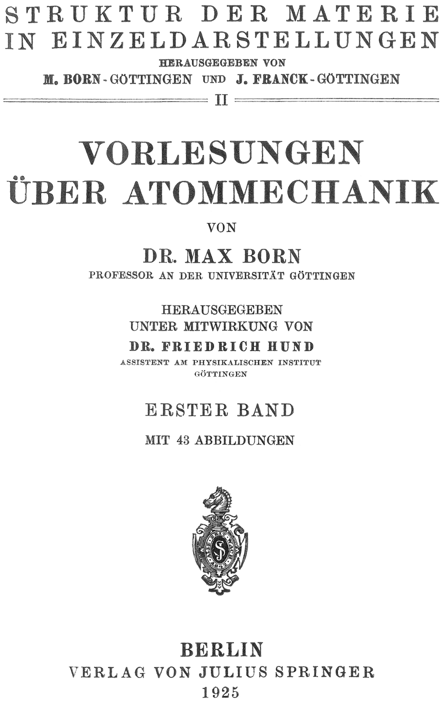
1. Introduction: Physical Foundations (3 sections, 13 Pages)
2. Chapter 1: Hamilton-Jacobi Theory (5 sections, 23 pages)
3. Chapter 2: Periodic and multiply periodic motions (12 sections, 81 pages)
4. Chapter 3: Systems with a single valence ('light') electron (19 sections, 129 pages)
5. Chapter 4: Perturbation theory (10 sections, 53 pages)

Both *Vorlesungen über Atommechanik* volumes were reviewed by Wolfgang Pauli for *Die Naturwissenschaften*. In his review of the first volume, young Pauli emphasized, in a somewhat pointed fashion, its strategy of applying mechanical principles to special problems in atomic physics. He gave the following as essential examples: Keplerian motion and the influence it receives from relativistic mass variations and external fields, general central motion (Rydberg-Ritz formula), diving orbits (*Tauchbahnen*), true principal quantum numbers of optical terms, construction of the periodic system according to Bohr, and nuclear vibrations and rotation of two-atomic molecules. He finally stresses the elaborateness of the last chapter on perturbation theory

> of which one cannot say, that the invested effort corresponds to the results achieved, which are, above all, mainly negative (invalidity of mechanics for the Helium atom). Whether this method can be the foundation of the true quantum theory of couplings, as the author believes, has to be shown by future developments. May this work itself accelerate the development of a simpler and more

[5] In the latest editions (2001 English, 2006 German) (Goldstein, Poole Jr., and Safko 2001) the authors seem to have erased all references to Born's book.

unified theory of atoms with more than one electron, the manifestly unclear character as of today is clearly pictured in this chapter.[6] (Pauli 1925, 488)


STRUKTUR DER MATERIE
IN EINZELDARSTELLUNGEN
HERAUSGEGEBEN VON
M. BORN-GÖTTINGEN UND J. FRANCK-GÖTTINGEN
II

VORLESUNGEN
ÜBER ATOMMECHANIK
VON
DR. MAX BORN
PROFESSOR AN DER UNIVERSITÄT GÖTTINGEN

HERAUSGEGEBEN
UNTER MITWIRKUNG VON
DR. FRIEDRICH HUND
ASSISTENT AM PHYSIKALISCHEN INSTITUT
GÖTTINGEN

ERSTER BAND
MIT 43 ABBILDUNGEN

BERLIN
VERLAG VON JULIUS SPRINGER
1925

Figure 8.1: Cover page.

As an amusing aside, this may be compared with Pauli's review of the second volume, which already showed considerably more of his infamous biting irony. Alluding to Born's as well as Born and Jordan's own words in the introductions to volume 1 and 2 respectively, Pauli's review starts with:

> This book is the second volume of a series, in which each time the aim and sense [*Ziel und Sinn*] of the *n*th volume is made clear by the virtual existence of the (*n*+1)st. (Pauli 1930, 602)

Having given no recommendation, the review then ends with:

> The making [*Ausstattung*] of the book with respect to print and paper is excellent [*vortrefflich*]. (Pauli 1930, 602)

[6] Translations are the author's unless otherwise noted.

Inhaltsverzeichnis.

Einleitung.

Physikalische Grundlagen.

Erstes Kapitel.

Hamilton-Jacobische Theorie.

Zweites Kapitel.

Periodische und mehrfach periodische Bewegungen.

Drittes Kapitel.

Systeme mit einem Leuchtelektron.

Figure 8.2: Table of contents.

Inhaltsverzeichnis IX

Viertes Kapitel.

Störungstheorie.

Anhang.

Figure 8.3: Table of contents.

8.3 Born's Pedagogy and the Heuristic Role of the Deductive/Axiomatic Method

8.3.1 Sommerfeld versus Born

Wilhelm von Humboldt's early nineteenth-century, programmatic vision of an intimate co-existence and cross-fertilization of teaching and research soon became a widely followed paradigm for universities in Prussia, in other parts of Germany, and around the world. And even though it is clear from experience that it cannot be a general rule that the best researchers make the best teachers, or vice versa, Humboldt's program has nevertheless proven extremely successful. In fact, outstanding examples for how to put into action Humboldt's maxim are provided by the Munich and Göttingen schools of Quantum Physics during the post-World-War-I period. Their common commitment to the "Humboldtian Ideal," with actions that speak louder than words, resulted in multiple generations of researchers and teachers of the highest originality and quality. What makes this even more convincing is the impression that this was not achieved on account of individual exceptionality; quite the contrary. Sommerfeld in Munich, for example, is well known to have had an extraordinarily fine sense for the gifts of each individual student and how to exploit these in an atmosphere of common scientific endeavor (Seth 2010). Similar things can be said of Max Born in Göttingen, though perhaps not quite as emphatically. Born's style was slightly less adapted to the non-systematic approaches of scientific newcomers, whereas Sommerfeld appreciated any and all new ideas and tricks, if only for purposes of problem solving. For Sommerfeld, teaching the art of problem solving was perhaps the single most important concern in classes and seminars (Seth 2010). Overly tight and systematic expositions were not suited to that purpose. This point was often emphasized by Sommerfeld, for example, right at the beginning of his classic five-volume "Lectures on Theoretical Physics." The first volume is called "Mechanics," not "Analytical Mechanics," as Sommerfeld stresses in a one-page preliminary note that follows the preface, because

> This name [analytical mechanics] originated in the grand work of Lagrange's of 1788, who wanted to clothe all of mechanics in a uniform language of formulae and who was proud that one would not find a single figure throughout his work. We, in contrast, will resort to intuition [*Anschauung*] whenever possible and consider not only astronomical but also physical and, to a certain extent, technical applications. (Sommerfeld 1977, Vorbemerkung)

The preface itself contains the following programmatic paragraph, which clearly characterizes Sommerfeld's approach to teaching in general:

> Accordingly, in print [as in his classes; D.G.] I will not detain myself with the mathematical foundations, but proceed as rapidly as possible to the physical problems themselves. I wish to supply the reader with a vivid picture of the highly structured material that comes within the scope of theory from a suitable chosen mathematical and physical vantage point. May there, after all, remain some gaps in the systematic justification and axiomatic consistency. In any case during my lectures I did not want to put off my students with tedious investigations of mathematical or logical nature and distract them from the physically interesting. This approach has, I believe, proven useful in class and has been

> maintained in the printed version. As compared to the lectures by Planck, which are impeccable in their systematic structure, I believe I can claim a greater variety in the material and a more flexible handling of the mathematics. (Sommerfeld 1977, v–vi)

This pragmatic paradigm has been taken over and perfected by later generations of theoretical physicists; just think of the 10-volume lecture courses by Landau and Lifshitz, which is still in print in many languages and widely used the world over.

There are many things to be said in favor of this pragmatic approach. For one thing, it takes account of the fact that developing understanding is a cyclic process. Every serious student knows that one has to go over the same material again and again in order to appreciate the details of statements, the hidden assumptions, and the intended range of validity. Often, on one's nth iteration one discovers new aspects, in view of which one's past understanding is revealed as merely apparent and ill-founded. Given that we can almost never be sure that this will not happen again in the future, one might even be tempted to measure one's own *relative* degree of understanding by the number of times this has already happened in the past. From that perspective, the pragmatic approach seems clearly much better suited, since it does not pretend to the fiction of an ultimate understanding. Hence, being able to solve concrete problems sounds like a reasonable and incorruptible criterion.

However, as Thomas Kuhn pointed out long ago, well characterized (concrete) problems, also called "puzzles" by him, must be supplied by the paradigms to which working scientists adhere. But if concrete problems become critically severe, with all hope of eventual solution under the current paradigm fading, further puzzle-solving activities will, sooner or later, decouple from further progress. The crucial question when that occurs is: Where can seeds for further progress be found and how should they be cultivated?

It is with regard to this question that I see a clear distinction between the approaches of Born and Sommerfeld. Sommerfeld once quite frankly admitted to Einstein:

> Everything works out all right [*klappt*] and yet remains fundamentally unclear. I can only cultivate [*fördern*] the techniques of the quanta, you have to provide your philosophy. (Hermann 1968, 97)

Cultivating new seeds could start with establishing simple axioms in a well-defined mathematical framework. But even that might turn out to be premature. Heisenberg was one of the figures who repeatedly expressed the optimistic view that physical problems can be "essentially" solved while still detached from such a framework. In connection with his later search for a unified field theory of elementary particles, he said in the preface to his textbook on the subject:

> At the current status of the theory it would be premature to start with a system of well defined axioms and then deduce from them the theory by means of exact mathematical methods. What one needs is a mathematical description which adequately describes the experimental situation, which does not seem to contain contradictions and which, therefore, might later be completed to an exact mathematical scheme. History of physics teaches us that, in general, a new theory can be phrased in a precise mathematical language only after all essential physical problems have been solved. (Heisenberg 1967, vi)

It seems even more obvious that, in phases of paradigmatic uncertainty, little help can be expected from attempts to establish an axiomatic framework for the doomed theory. And yet, surprisingly, this is precisely what Born did, as we shall see in the next subsection.

In a letter to Paul Ehrenfest from 1925, Einstein divided the community of physicists into the "Prinzipienfuchser" and the "Virtuosi" (Seth 2010, 186).[7] Einstein grouped Ehrenfest, Bohr, and himself in the first category and named Debye and Born as members of the latter. "Virtuosity" here refers to exceptional mathematical and calculational abilities, any encounter with which results in mental depression on the side of the "Prinzipienfuchser," as Einstein concedes to Ehrenfest, who first complained about this effect. However, Einstein adds that the opposite effect exists, too.

This dichotomy is not strictly exclusive. An obvious example of someone who could with equal right be located in both camps is Wolfgang Pauli. But also Born lives in both camps and can be best described, I think, as a "Prinzipienfuchser" amongst the "Virtuosi." The principles with which he is primarily concerned arise within the attempt to find a logical basis from which the physically relevant can be deduced without ambiguity, rather than just applying clever tricks. This difference from the Sommerfeld school was once expressed by Heisenberg in an interview with Thomas Kuhn from 15 February 1963:

> In Sommerfeld's institute one learned to solve special problems; one learned the tricks, you know. Born took it much more fundamentally, from a very general axiomatic point of view. So only in Göttingen did I really learn the techniques well. Also in this way Born's seminar was very helpful for me. I think from this Born seminar on I was able really to do perturbation calculations with all the rigor which was necessary to solve such problems. (Seth 2010, 58)

Let us now turn to how Born himself expresses the heuristic value of the axiomatic method in times of uncertainty.

8.3.2 A Remarkable Introduction

One third of the way through the book, Born recalls the basic idea of 'Quantum Mechanics' in the following way (the emphases are Born's):

> Once again, we summarize the basic idea of Quantum Mechanics, as developed so far: *For a given Model [Modell] we calculate the totality of all motions* (which are assumed to be multiply periodic) *according to the laws of Classical Mechanics* (neglecting radiation damping); *the quantum conditions select a discrete subset from this continuum of motions.* The *energies* of the selected motions shall be the *true* [*wirkliche*] ones, as measurable by electron collision, and the energy differences shall, according to Bohr's frequency condition, correspond [*zusammenhängen*] with the *true* [*wirklichen*] *light frequencies*, as observed in the spectrum. Besides frequencies, the emitted light possesses the observable properties of intensity, phase, and state of polarization, which are only approximately accounted for by the theory (§ 17). These exhaust the observable

[7] As Seth already remarked in note 29 to chapter 6 of (Seth 2010), "Prinzipienfuchser" is nearly untranslatable. Existing compound words are "Pfennigfuchser" (penny pincher) and "Federfuchser" (pedant) (not "Pfederfuchser," as stated in (Seth 2010), which does not exist).

> properties of the motion of the atomic system. However, our computation assigns *additional properties* to it, namely orbital frequencies and distances, that is, the course [*Ablauf*] of motion in time. It seems that these quantities are, as a matter of principle, not accessible to observation.[8] Therewith we arrive at the following judgement [*Urteil*], that *for the time being our procedure is just a formal computational scheme* which, for certain cases, allows us to replace the still unknown quantum laws by computations on a classical basis [*auf klassischer Grundlage*]. Of these true [*wahren*] laws we would have to require, that they only contain relations between observable quantities, that is, energy, light frequencies, intensities, and phases. As long as these laws are still unknown, we have to always face the possibility that our provisional quantum rules will fail; one of our main tasks will be to delimit [*abgrenzen*] the validity of these rules by comparison with experience. (Born 1925, 113–114)

As an (obvious) side remark, we draw attention to the similarity between Born's formulations in the second half of the above cited passage and Heisenberg's opening sentences of his *Umdeutung* paper (Heisenberg 1925).

Born's book attempts an axiomatic-deductive approach to Bohr-Sommerfeld quantization. This might seem totally misguided at first, as one could naively think that such a presentation only makes sense *after* all the essential physical notions and corresponding mathematical structures have been identified. Certainly none of the serious researchers at the time believed these to have been identified for Bohr-Sommerfeld quantization, with Born being no exception, as we have just seen from the passage cited above. So what is Born's own justification for such an attempt? This he provides in his introduction to the book, where he takes a truly remarkable heuristic attitude. I found it quite inappropriate to alter his words, so I quote directly from the introduction:

> The title 'Atommechanik' of this lecture, which I delivered in the winter-semester 1923/24 in Göttingen, is formed after the label 'Celestial Mechanics.' In the same way as the latter labels that part of theoretical astronomy which is concerned with the calculation of trajectories of heavenly bodies according to the laws of mechanics, the word 'Atommechanik' is meant to express that here we deal with the facts of atomic physics from the particular point of view of applying mechanical principles. This means that we are attempting a deductive presentation of atomic theory. The reservations, that the theory is not sufficiently mature [*reif*], I wish to disperse with the remark that we are dealing with a test case [*Versuch*], a logical experiment, the meaning of which just lies in the determination of the limits to which the principles of atomic and quantum physics succeed, and to pave the way which shall lead us beyond those limits. I called this book 'Volume I' in order to express this program already in the title; the second volume shall then contain a higher approximation to the 'final' mechanics of atoms.
>
> I am well aware that the promise of such a second volume is daring [*kühn*]; since presently we have only a few hints as to the nature of the deviations that need to

[8]Here Born adds the following footnote: "Measurements of atomic radii and the like do not lead to better approximations to reality [*Wirklichkeit*] as, say, the coincidence between orbital and light frequencies."

> be imposed onto the classical laws in order to explain the atomic properties. To these hints I count first of all Heisenberg's rendering of the laws of multiplets and anomalous Zeeman effect, the new radiation theory of Bohr, Kramers, and Slater, the ensuing *Ansätze* of Kramers for a quantum-theoretic explanation of the phenomena of dispersion, and also some general considerations concerning the adaptation of perturbation theory to the quantum principles, which I recently communicated. But all this material, however extensive it might be, does not nearly suffice to shape a deductive theory from it. Therefore, the planned '2. Volume' might remain unwritten for many years to come; its virtual existence may, for the time being, clarify the aim and sense [*Ziel und Sinn*] of this book. (Born 1925, v–vi)

Born continues and explicitly refers to (and suggests the reading of) Sommerfeld's *Atombau und Spektrallinien*, almost as a prerequisite for a successful study of his own book. But he also stresses the difference, which lies in part in the deductive approach:

> For us the mechanical-deductive approach always comes first [*steht überall obenan*]. Details of empirical facts will only be given when they are essential for the clarification, the support, or the refutation of theoretical strings of thought [*Gedankenreihen*]. (Born 1925, vi)

But, Born continues, there is a second difference from *Atombau und Spektrallinien*, namely with respect to the foundations of quantum theory, where

> differences in the emphasis of certain features [*Züge*] are present; but I leave it to the author to find these out by direct comparison. As regards the relation of my understanding to that of Bohr and his school, I am not aware of any significant opposition. I feel particularly sympathetic with the Copenhagen researchers in my conviction, that it is a rather long way to go to a 'final quantum theory.' (Born 1925, vi)

It would be an interesting project to try to work out the details of the "second difference," concerning the foundations of quantum theory, by close comparison of Born's text with *Atombau und Spektrallinien*. Later, as we know, Born in principle favored the more abstract algebraic approach (Heisenberg) over the more '*anschauliche*' wave-theoretic picture, quite in contrast to Sommerfeld, who took a more pragmatic stance. Born's feeling that conceptual merit, which is marred by the semi-*anschauliche* picture of waves traveling in (high-dimensional) configuration space, should be given greater consideration is clearly reflected in the second volume, as well as in later publications, such as in the book by Herbert Green (with a foreword by Born) (Green and Born 1965) on matrix methods in quantum mechanics. This split opinion is still very much alive today, though it is clear that, in terms of calculational economy, wave mechanics is usually preferable.

Born ends his introduction by acknowledging the help of several people, foremost his assistant Friedrich Hund for his "devoted collaboration":

> Here I specifically mention the theorem concerning the uniqueness of action-angle variables which, according to my view, lies at the foundation of *today's* quantum theory; the proof worked out by Hund forms the centre [*Mittelpunkt*] of the second chapter (§ 15). (Born 1925, vii)

Hund is also thanked for the presentation of Bohr's theory of periodic systems. Heisenberg is thanked for his advice and for outlining particular chapters, like the last one on the helium atom. Lothar Wolfgang Nordheim's help with the presentation of perturbation theory is acknowledged as is the work of H. Kornfeld, who checked selected calculations. Finally, Fritz Reiche, H. Kornfeld, and F. Zeilinger are thanked for helping with corrections.

8.4 On Technical Issues: What Is Quantization?

A central concern of Born's book is the issue of quantization rules, that is: How can one *unambiguously* generalize

$$J := \oint p\, dq = nh \tag{8.1}$$

to systems with more than one degree of freedom? The history of attempts to answer this question is interesting, but also rather intricate, and involves various suggestions by Ishiwara (1915), Wilson (1915), Planck (1916), Sommerfeld (1916), Schwarzschild (1916), Epstein (1916a; 1916b), and last but not least, the somewhat singular paper by Einstein from 1917 on "The Quantum Theorem of Sommerfeld and Epstein" (Kormos Buchwald 1987–2005, Vol. 6, Doc. 45, 556–567), to which we turn below. These papers have various logical interdependencies and also differ in subtle and partial ways. Leaving aside Einstein's paper for the moment, the rule that emerged from the discussions looked innocently similar to (8.1), namely

$$J_k := \oint p_k\, dq_k = n_k h \qquad \text{(no summation over k)} \tag{8.2}$$

where $k = 1, 2, \dots, s$ labels the degrees of freedom to be quantized, which need not necessarily exhaust all physical degrees of freedom, of which there are $f \geq s$, as we shall discuss below.[9] Here we adopt the notation from Born's book, where $(q_1, \dots, q_f; p_1, \dots, p_f)$ are the generalized coordinates (configuration variables) and momenta respectively. The apparent simplicity of (8.2) is deceptive though. One thing that needs to be clarified is the domain of integration implicit in the $\oint$-symbol. It indicates that the integration over q_k is to be performed over a full period of *that* configuration variable. In Sommerfeld's words, emphasis in the original:

> *Each coordinate shall be extended over the full range necessary to faithfully label the phase of the system.* For a cyclic azimuth in a plane this range is 0 to 2π, for the inclination in space (geographic latitude θ) twice the range between θ_{min} and θ_{max}, for a radial segment r [*Fahrstrahl*] likewise twice the covered interval from r_{min} to r_{max} for the motion in question. (Sommerfeld 1916, 7)

Another source of uncertainty concerns the choice of canonical coordinates for which (8.2) is meant to hold. Again in Sommerfeld's words of his comprehensive 1916 account:

> Unfortunately a general rule for the choice of coordinates can hardly be given; it will be necessary to collect further experience by means of specific examples.

[9] In (8.2) as well as in all formulae to follow, we never make use of the summation convention.

> In our problems it will do to use (planar and spatial) polar coordinates. We will come back to a promising rule of Schwarzschild and Epstein for the choice of coordinates in § 10. (Sommerfeld 1916, 6)

The rule that Epstein and Schwarzschild formulated independently in their papers dealing with the Stark effect (Epstein 1916a; Schwarzschild 1916)—compared by Epstein in (Epstein 1916b) shortly after Schwarzschild's death—is based on two assumptions. The first is that Hamilton's equations of motion

$$\dot{q}_k = \frac{\partial H}{\partial p_k}, \quad \dot{p}_k = -\frac{\partial H}{\partial q_k}, \tag{8.3}$$

for time-independent Hamiltonians $H(q_1, \ldots, q_f; p_1, \ldots, p_f)$ are solved by means of a general solution $S(q_1, \ldots, q_f; \alpha_1, \ldots, \alpha_f)$ for the Hamilton-Jacobi equation

$$H\left(q_1, \ldots, q_f; \frac{\partial S}{\partial q_1}, \ldots, \frac{\partial S}{\partial q_f}\right) = E\,, \tag{8.4}$$

where $p_k = \partial S/\partial q_k$ and $\alpha_1, \ldots, \alpha_f$ are constants of integration on which the energy E depends. Second, and most important, is that this solution is obtained by separation of variables:

$$S(q_1, \ldots, q_f; \alpha_1, \ldots, \alpha_f) = \sum_{i=1}^{f} S_i(q_i; \alpha_1, \ldots, \alpha_f)\,. \tag{8.5}$$

Note that this implies in particular that $p_k = p_k(q_k; \alpha_1, \ldots, \alpha_f)$, i.e. the k-th momentum only depends on the k-th configuration variable and the f constants of integration $\alpha_1, \ldots, \alpha_f$. This is indeed necessary for (8.2) to make sense, since the right hand side is a constant and can therefore not be meaningfully equated to a quantity that depends nontrivially on phase space. Rather, the meaning of (8.2) is to select a subset of solutions through equations for the α's. However, separability is a very strong requirement indeed. In particular, it requires the integrability of the dynamical system in question, a fact which only Einstein drew special attention to in his paper (Kormos Buchwald 1987–2005, Vol. 6, Doc. 45, 556–567), as we will discuss in more detail below. In fact, integrability is manifest once the $J_1, \ldots, J_f$ have been introduced as so-called 'action variables,' which are conjugate to some 'angle variables' $w_1, \ldots, w_f$; for then the action variables constitute the f observables in involution, i.e. their mutual Poisson brackets obviously all vanish.[10]

But even if we swallow integrability as a *conditio sine qua non*, does separability ensure uniqueness? What is the strongest guarantee of uniqueness one can hope for? Well, for (8.2) to make sense, any two allowed (by conditions yet to be formulated) sets of canonical coordinates $(q_i, p_i)_{i=1\ldots f}$ and $(\bar{q}_i, \bar{p}_i)_{i=1\ldots f}$ must be such that the (J_k/h)'s (calculated according to 8.2) are integers if and only if the $(\bar{J}_k/h)$'s are. This is clearly the case if the allowed transformations are such that among the action variables J_k they amount to linear transformations by invertible integer-valued matrices:[11]

[10] The implication of integrability for separability is far less clear (compare, e.g., Gutzwiller 1990). Classic results concerning sufficient conditions for separability were obtained by Stäckel (see Charlier 1902).

[11] Note that the inverse matrices must also be integer-valued; hence the matrices must have determinant equal to ± 1.

$$\bar{J}_k = \sum_{l=1}^{f} \tau_{lk} J_l \quad (\tau_{lk}) \in \mathrm{GL}(f, \mathbb{Z})\,. \tag{8.6a}$$

Here $\mathrm{GL}(f, \mathbb{Z})$ is the (modern) symbol for the group of invertible $f \times f$ matrices with integer entries. The most general transformations for the angle variables compatible with (8.6a) are

$$\bar{w}_k = \sum_{l=1}^{f} \tau_{kl}^{-1} w_l + \lambda_k(J_1, \ldots, J_f)\,, \tag{8.6b}$$

where the λ_k are general (smooth) functions.[12]

The task is now to carefully amend the Epstein-Schwarzschild condition demanding separability by further technical assumptions under which the transformations (8.6) will be the *only* residual ones. The solution of this problem is presented in § 15 of Born's book, where Born acknowledges essential help with this task from Friedrich Hund.

Born also states that the technical conditions under which this result for multiply periodic systems can be derived were already given in the unpublished thesis by Johannes M. Burgers (Burgers 1918), who is better known for his works on the adiabatic invariants. The arguments to show uniqueness in Burger's thesis are, according to Born, technically incomplete. The conditions themselves read as follows:

1. A The position of the system shall periodically depend on the angle variables $(w_1, \ldots, w_f)$ with primitive period 1.
2. B The Hamiltonian is transformed into a function W depending only on the $(J_1, \ldots, J_f)$.[13]
3. C The phase-space function:

$$S^* = S - \sum_{k=1}^{f} w_k J_k\,, \tag{8.7}$$

   considered as function of the variables (q, w), which generates the canonical transformation $(q, p) \mapsto (w, J)$ via

$$p_k = \frac{\partial S^*}{\partial q_k} \qquad J_k = -\frac{\partial S^*}{\partial w_k}\,, \tag{8.8}$$

   shall also be a periodic function of the w's with period 1.

A and B are immediately clear, but the more technical condition C is not. And, as Born remarks, A and B do not suffice to lead to the desired result. In fact, a simple canonical transformation $(w, J) \mapsto (\bar{w}, \bar{J})$ compatible with A and B is

---

[12] Our equation (8.6b) differs in a harmless fashion from the corresponding equation (7) on p. 102 of (Born 1925), which reads $w_k = \sum_{l=1}^{f} \tau_{kl} \bar{w}_l + \psi_k(J_1, \ldots, J_f)$, into which our equation turns if we redefine the functions through $\psi_k = -\sum_{l=1}^{f} \tau_{kl} \lambda_l$.

[13] We follow Born's notation, according to which the Hamiltonian, considered as function of the action variables, is denoted by W.

$$\bar{w}_k = w_k + f_k(J_1, \dots, J_f), \quad \bar{J}_k = J_k + c_k, \tag{8.9}$$

where the c_k are arbitrary constants. Their possible presence disturbs the quantization condition, since J_k and $\bar{J}_k$ cannot, in general, both simultaneously be integer multiples of h. Condition C now eliminates this freedom. After some manipulations the following result is stated:

> **Theorem (Uniqueness for non-degenerate systems)** If, for a mechanical system, variables (w, J) can be introduced satisfying conditions A-C, and if there exist no commensurabilities between the quantities
>
> $$\nu_k = \frac{\partial W}{\partial J_k}, \tag{8.10}$$
>
> then the action variables J_k are determined uniquely up to transformations of type (8.6a) [that is, linear transformations by $\mathrm{GL}(f, \mathbb{Z})$]. (Born 1925, 104)

For the proof, as well as for the ensuing interpretation of the quantization condition, the notions of *degeneracy* and *commensurability* are absolutely essential: An f-tuple $(\nu_1, \dots, \nu_f)$ of real numbers is called r-fold degenerate, where $0 \le r \le f$, if there are r but not $r+1$ independent integer relations among them, that is, if there is a set of r mutually independent f-tuples $n_1^{(\alpha)}, \dots, n_f^{(\alpha)}$, $\alpha = 1, \dots, r$ of integers, so that r relations of the form

$$\sum_{k=1}^{f} n_k^{(\alpha)} \nu_k = 0, \quad \forall \alpha = 1, \dots, r \tag{8.11}$$

hold, but there are not $r+1$ relations of this sort. The f-tuple is simply called degenerate if it is r-fold degenerate for some $r > 0$. A relation of the form (8.11) is called a commensurability. If no commensurabilities exist, the system is called non-degenerate or incommensurable.

It is clear that a relation of the form (8.11) with $n_k^{(\alpha)} \in \mathbb{Z}$ exists if and only if it exists for $n_k^{(\alpha)} \in \mathbb{Q}$ (rational numbers). Hence a more compact definition of r-fold degeneracy is the following: Consider the real numbers $\mathbb{R}$ as a vector space over the rational numbers $\mathbb{Q}$ (which is infinite dimensional). The f vectors $\nu_1, \dots, \nu_f$ are r-fold degenerate if and only if their span is s-dimensional, where $s = f - r$.

Strictly speaking, we have to distinguish between *proper* (*eigentlich*, Born) and *improper* (or contingent) (*zufällig*, Born) degeneracies. To understand the difference, recall that the frequencies are defined through (8.10), so that each of them is a function of the action variables $J_1, \dots, J_f$. A proper degeneracy holds identically for all considered values, $J_1, \dots, J_f$, (the set of which must contain at least an open interval of values around each considered value, J_k), whereas an improper degeneracy only holds for singular values of the J's. This distinction should then also be made for the notion of r-fold degeneracy: a proper r-fold degeneracy of frequencies is one that holds identically for a whole neighborhood of values $J_1, \dots, J_f$ around the considered value.

The possibility of degeneracies and their relevance for the formulation of quantization conditions was already anticipated by Schwarzschild (1916), who was very well acquainted with the more refined aspects of Hamilton-Jacobi theory, e.g. through Charlier's widely read comprehensive treatise (Charlier 1902, 1907). Schwarzschild stated in § 3 of (Schwarzschild 1916) that if action-angle variables could be found for which some of the frequencies, ν_k vanished, say $\nu_{s+1}, \ldots, \nu_{s+r}$ where $s + r = f$, then no quantum condition should be imposed on the corresponding actions $J_{s+1}, \ldots, J_{s+r}$. The rationale he gave for that description was that defining equation (8.10) for the frequencies showed that the energy W was independent of $J_1, \ldots, J_k$. In his words (but our notation):

> This amendment to the prescription [of quantization] is suggested by the remark, that for a vanishing mean motion ν_k, the equation $\nu_k = \partial W / \partial J_k$ shows that the energy becomes independent of the variables J_k, that therefore these variables have no relation to the energetic process within the system. (Schwarzschild 1916, 550)

From that it is clear that the independence of the energy W from the J_k for which $\nu_k = 0$ is only given if the system is *properly* degenerate; otherwise we just have a stationary point in W with respect to J_k at one particular J_k value. So Schwarzschild's energy argument only justifies not quantizing those action variables whose conjugate angles have frequencies that vanish identically in the J_k (for some open neighborhood).

Now, it is true that for a r-fold degenerate system (proper or improper) a canonical transformation exists such that, say, the first $s = f - r$ frequencies $\nu_1, \ldots, \nu_s$ are non-degenerate, whereas the remaining r frequencies $\nu_{s+1}, \ldots, \nu_{s+r}$ are all zero (only for the particular values of J's in the improper case). The number s of independent frequencies is called the *degree of periodicity* of the system (Born 1925, 105). Hence Schwarzschild's energy argument amounts to the statement, that for *proper degeneracies* only the s action variables $J_1, \ldots, J_s$ should be quantized, but not the remaining $J_{s+1}, \ldots, J_{s+r}$. If the degeneracies are improper, similar systems with arbitrarily close values of the J_k would have these variables quantized, so that it would seem physically unreasonable to treat such singular cases differently, as Epstein argued in reaction to Schwarzschild (Epstein 1916b).

Born now proceeds to generalize the uniqueness theorem to degenerate systems. For this, one needs to find the most general transformations that preserve conditions A-C and, in addition, preserve the separation into s independent and r mutually dependent (vanishing) frequencies. This can indeed be done, so that the above theorem has the following natural generalization:

> **Theorem (Uniqueness for degenerate systems)** If, for a mechanical system, variables (w, J) can be introduced satisfying conditions A-C, then they can always be chosen in such a way that the first s of the partial derivatives
>
> $$\nu_k = \frac{\partial W}{\partial J_k}, \tag{8.12}$$
>
> i.e. the $\nu_1, \ldots, \nu_s$ are incommensurable and the others $\nu_{s+1}, \ldots, \nu_{s+r}$, where $s + r = f$, vanish. Then the first s action variables, $J_1, \ldots, J_s$, are determined uniquely up to transformations of type (8.6a) [that is, linear transformations by $\mathrm{GL}(s, \mathbb{Z})$]. (Born 1925, 108)

In the next section (§ 16), Born completes these results by showing that adiabatic invariance holds for $J_1, \dots, J_s$ but not for J_k for $k > s$, even if the degeneracy is merely improper (Born 1925, 111). He therefore arrives at the following

> **Quantization rule:** Let the variables (w, J) for a mechanical system satisfying conditions A-C be so chosen that $\nu_1, \dots, \nu_s$ are incommensurable and $\nu_{s+1}, \dots, \nu_{s+r}$ $(s + r = f)$ vanish (possibly $r = 0$). The stationary motions of this systems are then determined by
>
> $$J_k = n_k h \qquad \text{for} \quad k = 1, \dots, s. \tag{8.13}$$
>
> (Born 1925, 112)

Born acknowledges that Schwarzschild already proposed exempting those action variables from quantization whose conjugate angles have degenerate frequencies. But, at this point, he does not distinguish sufficiently clearly between proper and improper degeneracies. This issue is taken up again later in chapter 4, on perturbation theory, where he states that the (unperturbed) system, should it have improper degeneracies, should be quantized in the corresponding action variables (cf. Born 1925, 303).

8.4.1 A Simple System with (Proper) Degeneracies

To illustrate the occurrence of degeneracies, we present, in a slightly abbreviated form, the example of the 3-dimensional harmonic oscillator, which Born discusses in § 14 for the same purpose. Its Hamiltonian reads

$$H = \frac{1}{2m}\left(p_1^2 + p_2^2 + p_3^2\right) + \frac{m}{2}\left(\omega_1^2 x_1^2 + \omega_2^2 x_2^2 + \omega_3^2 x_3^2\right). \tag{8.14}$$

The general solution to the Hamilton-Jacobi equation is $(i = 1, 2, 3)$:

$$x_i = \sqrt{\frac{J_i}{2\pi^2 \nu_i^2 m}} \sin(2\pi w_i), \tag{8.15a}$$

$$p_i = \sqrt{2\nu_i m J_i} \cos(2\pi w_i), \tag{8.15b}$$

where

$$\nu_i = \frac{\omega_i}{2\pi} \quad \text{and} \quad w_i = \nu_i t + \delta_i. \tag{8.15c}$$

The δ_i and J_i are six integration constants, in terms of which the total energy reads

$$W = \sum_{i=1}^{3} \nu_i J_i. \tag{8.16}$$

Now, a one-fold degeneracy occurs if the frequencies ν_i obey a single relation of the form

$$\sum_{i=1}^{3} \tau_i \nu_i = 0\,, \tag{8.17}$$

where $\tau_i \in \mathbb{Z}$. This happens, for example, if

$$\omega_1 = \omega_2 =: \omega \neq \omega_3\,, \tag{8.18}$$

in which case the Hamiltonian is invariant under rotations around the third axis. The energy then only depends on J_3 and the sum $J_1 + J_2$. Introducing coordinates x_i' with respect to a system of axes that are rotated by an angle α around the third axis,

$$x_1' \quad = x_1 \cos\alpha - x_2 \sin\alpha\,, \tag{8.19a}$$

$$x_2' \quad = x_1 \sin\alpha + x_2 \cos\alpha\,, \tag{8.19b}$$

$$x_3' \quad = x_3\,, \tag{8.19c}$$

under which transformation the momenta transform just like the coordinates.[14] The new action variables, J_i', are given in terms of the old (w_i, J_i) by:

$$J_1' \quad = J_1 \cos^2\alpha + J_2 \sin^2\alpha - 2\sqrt{J_1 J_2}\, \cos(w_1 - w_2) \sin\alpha \cos\alpha\,, \tag{8.20a}$$

$$J_2' \quad = J_1 \sin^2\alpha + J_2 \cos^2\alpha + 2\sqrt{J_1 J_2}\, \cos(w_1 - w_2) \sin\alpha \cos\alpha\,, \tag{8.20b}$$

$$J_3' \quad = J_3\,. \tag{8.20c}$$

As Born stresses, the J_i''s depend not only on the J_i's, but also on the w_i's, more precisely on the difference $w_1 - w_2$, which is a constant, $(\delta_1 - \delta_2)$, along the dynamical trajectory according to (8.15c) and (8.18), as it must be (since the J_i''s are constant). It is now clear that, for general α, the conditions $J_{1,2} = n_{1,2}h$ and $J_{1,2}' = n_{1,2}'h$ are mutually incompatible. However, (8.20) show that the sums are invariant

$$J_1' + J_2' = J_1 + J_2 \tag{8.21}$$

hence a condition for the sum

$$J_1' + J_2' = J_1 + J_2 = nh \tag{8.22a}$$

together with

$$J_3' = J_3 = n_3 h \tag{8.22b}$$

makes sense.

[14] Generally, the momenta, being elements of the vector space dual to the velocities, transform via the inverse-transposed of the Jacobian (differential) for the coordinate transformation. But for linear transformations the Jacobian is just the transformation matrix and it being an orthogonal matrix implies that its inverse equals its transpose.

But what about coordinate changes other than just rotations? To see what happens, Born considers instead of (8.19) the transformation to cylindrical polar coordinates (r, φ, z) with conjugate momenta (p_r, p_φ, p_z) (cf. footnote 14):

$$x_1 = r\cos\varphi \qquad p_r = p_1\cos\varphi + p_2\sin\varphi\,, \tag{8.23a}$$

$$x_2 = r\sin\varphi \qquad p_\varphi = -p_1 r\sin\varphi + p_2 r\cos\varphi\,, \tag{8.23b}$$

$$x_3 = z \qquad p_z = p_3\,. \tag{8.23c}$$

The transformation equations from the old (w_i, J_i) to the new action variables (J_r, J_φ, J_z) are:[15]

$$J_r = \tfrac{1}{2}(J_1 + J_2) - \sqrt{J_1 J_2}\,\sin\bigl(2\pi(w_1 - w_2)\bigr)\,, \tag{8.24a}$$

$$J_\varphi = 2\sqrt{J_1 J_2}\,\sin\bigl(2\pi(w_1 - w_2)\bigr)\,, \tag{8.24b}$$

$$J_z = J_3\,. \tag{8.24c}$$

The total energy expressed as a function of the new action variables reads:

$$W = \nu(2J_r + J_\varphi) + \nu_z J_z\,, \tag{8.25}$$

where here and in (8.24) $\nu := \omega/2\pi$ and $\nu_z := \omega_3/2\pi$ (cf. 8.18). Again it is only the combination $2J_r + J_\varphi$ that enters the energy expression, and from (8.24) we see immediately that

$$2J_r + J_\varphi = J_1 + J_2\,. \tag{8.26}$$

Again, conditions of the form $J_r = n_r h$, $J_\varphi = n_\varphi h$, and $J_z = n_z h$ would pick out different "quantum orbits" [*Quantenbahnen*, Born] than those corresponding to $J_i = n_i h$. The energies, however, are the same.

8.5 Einstein's View

By 1917 Einstein had already taken up the problem of quantization in his long neglected[16] paper "On the Quantum Theorem of Sommerfeld and Epstein" (Kormos Buchwald 1987–2005, Vol. 6, Doc. 45, 556–567). Einstein summarized this paper in a letter to Ehrenfest dated 3 June 1917 (Kormos Buchwald 1987–2005, Vol. 8, Part A, Doc. 350, 464–6), in which he also makes a number of interesting comments, as we shall see below. For discussions of its content from a modern viewpoint see, e.g., (Gutzwiller 1990; Stone 2005).

In this paper Einstein suggested replacing the quantum condition (8.2) with

[15] There are two errors in Born's book in the formulae corresponding to (8.24a) and (8.24b), resulting from an erroneous factor of ν^{-1} in his formula (21) in § 14 of Chapter 2. My formulae correct Born's formulae on his p. 98.

[16] Einstein's paper was cited by de Broglie in his thesis (de Broglie 1925), where he spends slightly more than a page (pages 64–65 of Section II in Chapter III) discussing the "interpretation of Einstein's quantisation condition," and also in Schrödinger's "Quantisation as Eigenvalue Problem", where in the Second Communication he states in a footnote that Einstein's quantization condition "amongst all older versions stands closest to the present one [Schrödinger's]." However, after matrix and wave mechanics settled, Einstein's paper seems to have been largely forgotten until Keller (1958) reminded the community of its existence.

$$\oint_\gamma \sum_{k=1}^{f} p_k dq_k = n_\gamma h, \qquad \forall\gamma. \tag{8.27}$$

First of all one should recognize that here the sum forms the integrand, rather than each individual term $p_k dq_k$ as in (8.2). Second, (8.27) is not just one but many conditions, as many as there are independent paths (loops) γ along which the integrand is integrated.

Let us explain the meaning of all this in a modernized terminology. For this, we first point out that the integrand has a proper geometric meaning, since

$$\theta = \sum_{k=1}^{f} p_k dq_k \tag{8.28}$$

is the coordinate expression of a global one-form on phase space (sometimes called the Liouville form),[17] quite in contrast to each individual term $p_k\, dq_k$, which have no coordinate-independent, geometric meaning. Being a one-form it makes unambiguous sense to integrate it along paths. The paths γ considered here are all closed, i.e. loops, hence the $\oint$-sign. But what are the loops γ that may enter (8.27)? For their characterisation it is crucial to assume that the system be integrable. This means that there are f (= number of degrees of freedom) functions on phase space, $F_A(q,p)$ $(A = 1, \dots, f)$, the energy being one of them, whose mutual Poisson brackets vanish:

$$\{F_A, F_B\} = 0. \tag{8.29}$$

This implies that the trajectories remain on the level sets for the f-component function $\vec{F} = (F_1, \dots, F_f)$, which can be shown to be f-dimensional tori $T_{\vec{F}}$ embedded in $2f$-dimensional phase space. From (8.29) it follows that these tori are geometrically special (Lagrangian) submanifolds: The differential of the one form (8.27), restricted to the tangent spaces of these tori, vanishes identically. By Stokes's theorem this implies that any two integrals of θ over loops γ and γ' within the same torus T coincide in value (possibly up to a sign, depending on the orientation given to the loops) if there is a 2-dimensional surface σ within T whose boundary is just the union of γ and γ'. This defines an equivalence relation on the set of loops on T whose equivalence classes are called homology classes (of dimension 1). The homology classes form a finitely generated Abelian group (since the level sets are compact) so that each member can be uniquely written as a linear combination of f basis loops (i.e. their classes) with integer coefficients. For example, if one pictures the f-torus as an f-dimensional cube with pairwise identifications of opposite faces through translations, an f-tuple of basis loops is represented by the straight lines-segments connecting the

[17] In the terminology of differential geometry, phase space is the cotangent bundle T^*Q over configuration space Q with projection map $\pi : T^*Q \to Q$. The one-form θ on T^*Q is defined by the following rule: Let z be a point in T^*Q and X_Z a vector in the tangent space of T^*Q at z, then $\theta_Z(X_Z) := z(\pi_*|_Z(X_Z))$. Here the symbol on the right denotes the differential of the projection map π, evaluated at z and then applied to X_Z. This results in a tangent vector at $\pi(z)$ on Q on which $z \in T^*_{\pi(z)}Q$ may be evaluated. In local adapted coordinates $(q_1, \dots, q_f; p_1, \dots, p_f)$ the projection map π just projects onto the qs. Then, for $X = \sum_k (Y_k \partial_{q_k} + Z_k \partial_{p_k})$ we have $\pi_*(X) = \sum_k Y_k \partial_{q_k}$ and $z(\pi_*(X)) = \sum_k p_k Y_k$, so that $\theta = \sum_k p_k\, dq_k$.

midpoints of opposite faces. Each such basis is connected to any other by a linear $\mathrm{GL}(f, \mathbb{Z})$ transformation.

Now we can understand how (8.27) should be read, namely as a condition that selects, out of a continuum, a discrete subset of tori $T_{\vec{F}}$, which may be characterized by discretized values for the f observables F_A. In light of the last remark of the previous paragraph, it does not matter which basis for the homology classes of loops one chooses to evaluate (8.27). This leads to a quantization condition independent of the need to separate variables.

What remains undecided at this stage is how to proceed in cases where degeneracies occur. In the absence of degeneracies, the torus is *uniquely* determined. It is the closure of the phase space trajectory for all times. If degeneracies exist, that closure will define a torus of dimension $s < f$, the embedding of which in a torus of dimension f is ambiguous since the latter is not uniquely determined by the motion of the system. This we have seen in Born's examples above. Even simpler examples would be the planar harmonic oscillator and planar Keplerian motion (cf. Arnold 1978, sec. 51). In such cases one has to decide whether (8.27) is meant to apply only to the s generating loops of the former, lower-dimensional torus or to all f of the latter, thus introducing an $(f - s)$-fold ambiguity in the determination of the "quantum orbits" [*Quantenbahnen*, Born].

The geometric flavor of these arguments is clearly present in Einstein's paper, though he clearly did not use the modern vocabulary. Einstein starts from the f-dimensional configuration space whose coordinates are defined by the q's and regards the p's as certain 'functions' on it, defined through an f-parameter family of solutions. Locally in q-space (i.e. in a neighborhood or each point) Hamilton's equations guarantee the existence of ordinary (i.e. single-valued) functions $p_k(q_1, \ldots, q_f)$. However, following a dynamical trajectory that is dense in a portion of q-space the values p_k need not return to their original values. Einstein distinguishes between two cases: either the number of mutually different p-values when the trajectory returns to within a small neighborhood U around a point in q-space is finite, or it is infinite. In the latter case, Einstein's quantization condition does not apply. In the former case, Einstein's considers what he, in the letter to Ehrenfest, called the Riemannisation ("*Riemannisierung*") of q-space, that is, a finite-sheeted covering. The components p_k will then be a well-defined (single-valued) co-vector field over the dynamically allowed portion of q-space (see (Stone 2005) for a lucid discussion with pictures).

In a most interesting, one and a half page supplement added as proof, Einstein points out that the first type of motion, where q-space trajectories return with infinitely many mutually different p-values, may well occur for simple systems with relatively few degrees of freedom, e.g. that of three point-like masses moving under the influence of their mutual gravitational attractions, as was first pointed out by Poincaré in the 1890s to whom Einstein refers. Einstein ends his supplement (and the paper) by stating that, for non-integrable systems, his condition also fails. In fact, as discussed above, it cannot even be written down.

Hence one arrives at the conclusion that the crucial question concerning the applicability of quantization conditions is that of *integrability*, i.e. whether sufficiently many constants of motion exist; other degrees of complexity, like the number of degrees of freedom, do not directly matter. As we know from Poincaré's work, non-integrability occurs already at the 3-body level for simple 2-body interactions. But what is the meaning of "Quantum Theory" if "quantization" is not a universally applicable procedure?[18]

[18] Even today this question has not yet received a unanimously accepted answer.

In the letter to Ehrenfest mentioned above, Einstein stresses precisely this point, i.e. that his condition is only applicable to integrable systems, and ends with a truly astonishing statement:

> As pretty as this may appear, it is just restricted to the special case where the p_ν can be represented as (multi-valued) functions of the q_ν. It is interesting that this restriction just nullifies the validity of statistical mechanics. The latter presupposes that upon recurrence of the q_ν, the p_ν of a system in isolation assume all values by and by which are compatible with the energy principle. *It seems to me, that the true [wirkliche] mechanics is such that the existence of the integrals (which exclude the validity of statistical mechanics) is already assured by the general foundations.* But how to start??[19] (Kormos Buchwald 1987–2005, Vol. 8, Part A, Doc. 350, 465, my emphasis)

Did we just witness Einstein contemplating the impossibility of any rigorous foundation of classical statistical mechanics?

8.6 Final Comments

In his book, Born also mentions Poincaré's work and cites the relevant chapters on convergence of perturbation series and the 3-body problem in Charlier's treatise (1907), but he does not seem to make the fundamental distinction between integrable and non-integrable systems in the sense Einstein made it. Born never cites Einstein's paper in his book. He mentions the well-known problem (since Bruns 1884) of small denominators (described in Charlier 1907, chap. 10, sec. 5) and also Poincaré's result on the impossibility of describing the motion for even arbitrarily small perturbation functions in terms of convergent Fourier series. From that Born concludes it is impossible to introduce constant J_k's and hence impossible to pose quantization rules in general. His conclusion from this is that, for the time being, one should adopt a pragmatic attitude:

> Even though the mentioned approximation scheme does not converge in the strict sense, it has proved useful in celestial mechanics. For it could be shown [by Poincaré] that the series showed a *type of semi-convergence*. If appropriately terminated they represent the motion of the perturbed system with great accuracy, not for arbitrarily long times, but still for practically very long times. *From this one sees on purely theoretical grounds, that the absolute stability of atoms cannot be accounted for in this way*. However, for the time being one will push aside [*sich hinwegsetzen*] this fundamental difficulty and make energy calculations test-wise, in order to see whether one obtains similar agreements as in celestial mechanics. (Born 1925, 292–293)

---

[19] So hübsch nun diese Sache ist, so ist sie eben auf den Spezialfall beschränkt, dass die p_ν als (mehrdeutige) Funktion der q_ν dargestellt werden können. Es ist interessant, dass diese Beschränkung gerade die Gültigkeit der statistischen Mechanik aufhebt. Denn diese setzt voraus, dass die p_ν eines sich selbst überlassenen Systems bei Wiederkehr der q_ν nach und nach alle mit dem Energieprinzip vereinbaren Wertsysteme annehmen. *Es scheint mir, dass die wirkliche Mechanik so ist, dass die Existenz der Integrale, (welche die Gültigkeit der statistischen Mechanik ausschliessen), schon vermöge der allgemeinen Grundlagen gesichert ist.* Aber wie ansetzen??

Ten pages before that passage, in the introduction to the chapter on perturbation theory, Born stressed the somewhat ambivalent situation perturbation theory in atomic physics faces in comparison to celestial mechanics: On one hand, 'perturbations' caused by electron-electron interactions are of the same order of magnitude than electron-nucleus interactions, quite in contrast to the solar system, where the sun is orders of magnitude heavier than the planets. On the other hand, the quantum conditions drastically constrain possible motions and could well act as regulator. As regards the analytical difficulties already mentioned above, he comments in anticipation:

> Here [convergence of Fourier series] an insurmountable analytical difficulty seems to inhibit progress, and one could arrive at the opinion that it is impossible to gain a theoretical understanding of atomic structures up to Uranium. (Born 1925, 282–283)

However,

> The aim of the investigations of this chapter shall be to demonstrate, that this difficulty is not essential. It would indeed be strange [*sonderbar*] if Nature barricaded herself behind the analytical difficulties of the n-body problem against the advancement of knowledge [*das Vordringen der Erkenntnis*]. (Born 1925, 282–283)

In the course of the development of his chapter on perturbation theory very interesting technical points come up, one of them being connected with the apparent necessity to impose quantization conditions for the unperturbed action variables conjugate to angles whose frequencies are *improperly* degenerate. But the discussion of this is quite technical and extraneous to Born's approach to the quantization procedure.

References

Abraham, Ralph and Jerrold E. Marsden (1978). *Foundations of Mechanics*. 2nd ed. Reading, MA: Benjamin/Cummings Publishing Company.

Arnold, Vladimir Igorevich (1978). *Mathematical Methods of Classical Mechanics*. New York: Springer.

Born, Max (1925). *Vorlesungen über Atommechanik. Erster Band.* Volume II of *Struktur der Materie in Einzeldarstellungen*. Berlin: Julius Springer.

Born, Max and Pascual Jordan (1930). *Elementare Quantenmechanik (zweiter Band der Vorlesungen über Atommechanik).* Volume IX of *Struktur der Materie in Einzeldarstellungen*. Berlin: Julius Springer.

Burgers, Johannes Martinus (1918). *Het atoommodel van Rutherford-Bohr*. PhD thesis. Haarlem: Loosjes: University of Leyden.

Charlier, Carl Ludwig (1902). *Die Mechanik des Himmels*. 1. Leipzig: Veit.

— (1907). *Die Mechanik des Himmels*. 2. Leipzig: Veit.

de Broglie, Louis (1925). Recherches sur la théorie des quanta. *Annales de Physique* 3(10 Série): 22–128.

— (1929). *Einführung in die Wellenmechanik*. Leipzig: Akademische Verlagsgesellschaft.

Epstein, Paul S. (1916a). Zur Theorie des Starkeffektes. *Annalen der Physik* 355(13): 489–520.

— (1916b). Zur Quantentheorie. *Annalen der Physik* 356(18): 168–188.

Frenkel, Jakov (1929). *Einführung in die Wellenmechanik*. Berlin: Julius Springer.

Goldstein, Herbert, Charles P. Poole Jr., and John L. Safko (2001). *Classical Mechanics*. 3rd ed. Reading, MA: Addison-Wesley.

Green, Herbert S. and Max Born (1965). *Matrix Mechanics*. Groningen: P. Noordhoff Ltd. Scientific Publications.

Gutzwiller, Martin C. (1990). *Chaos in Classical and Quantum Mechanics*. 1. Interdisciplinary Applied Mathematics. New York: Springer.

Haas, Arthur (1928). *Materiewellen und Quantenmechanik*. Leipzig: Akademische Verlagsgesellschaft.

Heisenberg, Werner (1925). Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen. *Zeitschrift für Physik* 33(1): 879–893.

— (1967). *Einführung in die einheitliche Feldtheorie der Elementarteilchen*. Stuttgart: S. Hirzel.

Hermann, Armin, ed. (1968). *Albert Einstein/Arnold Sommerfeld Briefwechsel. Sechzig Briefe aus dem goldenen Zeitalter der modernen Physik*. Basel: Schwabe & Co.

Ishiwara, Jun (1915). Die universelle Bedeutung des Wirkungsquantums. *Toyko Sugaku Buturigakkawi Kizi (Proceedings of the Tokyo Mathematical and Physical Society)* 8(5): 106–116.

Keller, Joseph B. (1958). Corrected Bohr-Sommerfeld Quantum Conditions for Nonseparable Systems. *Annals of Physics* 4(2): 180–188.

Kormos Buchwald, Diana et al., ed. (1987–2005). *The Collected Papers of Albert Einstein*. 1-9. Princeton: Princeton University Press.

Kronig, Ralph and Victor F. Weisskopf, eds. (1964). *Collected Scientific Papers by Wolfgang Pauli*. 1–2. New York: Interscience Publishers.

Pauli, Wolfgang (1925). Review of Max Born: Vorlesungen über Atommechanik. Erster Band. *Die Naturwissenschaften* 13: 487–488. Reprint: Kronig and Weisskopf 1964.

— (1930). Review of Max Born & Pascual Jordan: Elementare Quantenmechanik. Zweiter Band der Vorlesungen über Atommechanik. *Die Naturwissenschaften* 18: 602. Reprint: Kronig and Weisskopf 1964.

Planck, Max (1916). Die physikalische Struktur des Phasenraumes. *Annalen der Physik* 355(12): 385–418.

Schwarzschild, Karl (1916). Zur Quantenhypothese. *Sitzungsberichte der Königlich Preußischen Akademie der Wissenschaften zu Berlin*: 548–568.

Seth, Suman (2010). *Crafting the Quantum. Arnold Sommerfeld and the Practice of Theory, 1890–1926*. Transformations: Studies in the History of Science and Technology. Cambridge, MA: The MIT Press.

Sommerfeld, Arnold (1916). Zur Quantentheorie der Spektrallinien. *Annalen der Physik* 356(17): 1–94.

— (1929). *Atombau und Spektrallinien. Wellenmechanischer Ergänzungsband*. 2. Braunschweig: Friedrich Vieweg und Sohn.

— (1977). *Vorlesungen über theoretische Physik. Band 1 Mechanik*. Thun: Harri Deutsch.

Stone, Douglas A. (2005). Einstein's Unknown Insight and the Problem of Quantizing Chaos. *Physics Today* 58(8): 37–43.

Wilson, William (1915). The Quantum-Theory of Radiation and Line Spectra. *Philosophical Magazine 6th Series* 29(174): 795–802.

Chapter 9
Teaching Quantum Physics in Cambridge: George Birtwistle and His Two Textbooks

Jaume Navarro

Shortly after the end of the Great War, Charles Galton Darwin, a former student of Trinity College, Cambridge, and later fellow and lecturer at Christ's College, wrote a letter to his friend Niels Bohr complaining about the situation of the quantum theory in the old university. From his point of view:

> [P]hysics and applied mathematics here are in an awful state. I am doing my inadequate best to talk to people about quanta; everybody accepts them here now (which is better than it was in 1914 at any rate), but I don't think most of them realize their fundamental importance or have studied the arguments in connection with them [...]. There are plenty of very intelligent people, only under the blighting influence of studying such things as strains in the ether, they none of them know what it is worth doing.[1]

By 1927 things had changed. The "Mathematical Tripos" (MT) and the "Natural Science Tripos" (NST) not only included a number of courses on quantum matters, but students taking these subjects were expected to respond to questions that, only some months earlier, had troubled the best scientific minds. To give an example, in the spring of 1928, one of the questions in the final exams was the following: "Show how the Heisenberg matrix of a q-number is determined from the normalized Schrödinger characteristic functions (Eigenfunktionen) of the problem concerned. Illustrate it by the problem of the rigid rotator (molecule)."[2] This question expected an understanding not only of Werner Heisenberg's and Erwin Schrödinger's theories of quantum mechanics, but also their equivalence, all of which had been developed only two years earlier. Some students in Cambridge were thus, at this stage, quite up-to-date with contemporary quantum questions, enabling them to become actors *tout court* in the developments of the new physics.

How did this change come about? The development of quantum physics and early quantum mechanics is a story that skips Cambridge and, generally, the British world. The first main English actor, Paul Adrien Maurice Dirac, appears on stage only in the second half of the 1920s. In the background, people like James H. Jeans, Ralph H. Fowler, and Charles G. Darwin play merely secondary roles in the grand narrative of quantum physics. However, these and other characters are instrumental to the understanding of how the theory arrived and took root in Cambridge.

---

[1] Darwin to Bohr, 30 May 1919, BSC 1, 4, AHQP.

[2] Cambridge Tripos Examination Papers.

Here, I contribute to the early history of quantum physics in Cambridge by directing attention to the pedagogical side of the story. In particular, I concentrate on two books written by a quite-unknown Cambridge don, George Birtwistle (1877–1929). A senior wrangler in 1899, Birtwistle was fellow and lecturer of mathematics at Pembroke College and lectured on quantum physics and quantum mechanics between 1924 and 1929, producing two books that comprise his lectures. These two books present a number of interesting aspects. First, they help us understand the way a generation trained in the old wrangler tradition could understand and teach quantum theory. Second, they characterize the content that non-specialists in Cambridge received about the new physics. And third, they embody the tensions experienced by lecturers and students of the quantum theory at a time when it was developing and transforming rapidly.[3]

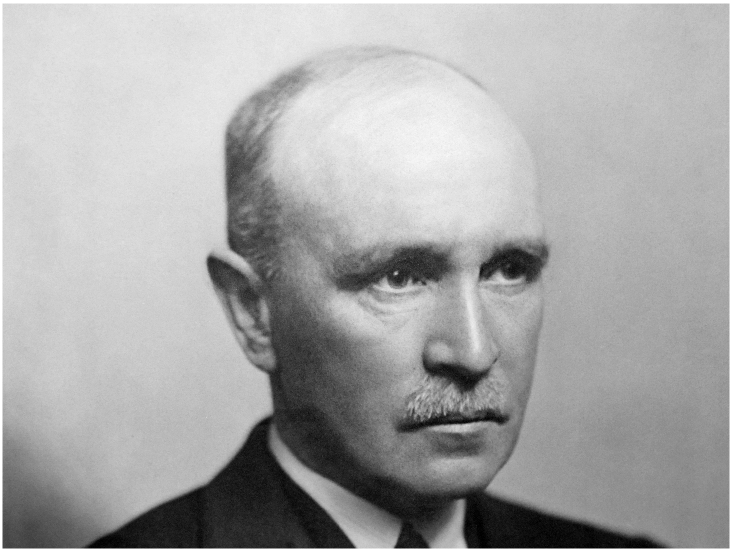


Figure 9.1: George Birtwistle. By permission of the Master and Fellows of Pembroke College, Cambridge.

In the first section (9.1), I review British scientists' early responses to quantum physics. The 1913 meeting in Birmingham of the British Association for the Advancement of Science (BAAS) was the first major public event in Britain in which positions in favor and against the theory of the quanta were discussed at length. Jeans, one of the first British *converts* to Planck's theory, wrote a report on the status quo of the quantum. This short book eventually became the source from which many British physicists got their first knowledge about the theory of quanta, during and immediately after the war. In section (9.2), I explain the evolution of teaching quantum theory in Cambridge, looking at the list of courses, examinations, and lecturers. This leads us to the two books by Birtwistle, *The Quantum Theory of the Atom* (1926) and *The New Quantum Mechanics* (1928a), in sections (9.3) and (9.4), respec-

[3] In picturing the state of physics in Cambridge before and immediately after the Great War, I closely follow the analysis of Andrew Warwick (2003).

tively. These two books may be channels for understanding the situation of the quantum in the Cambridge lecture room: undergraduate students had up-to-date resources, locally produced, through which they could keep up with the latest developments in quantum physics and quantum mechanics. As we shall see, some of these resources were not necessarily the best tools to grasp the radical novelty of the new theories.

9.1 James Jeans and His *Report on Radiation and the Quantum-Theory*

The first written reference to Planck's hypothesis in the British scientific milieu was probably Joseph Larmor's explicit rejection of it at the 1902 BAAS meeting. In the following years, the general attitude in Britain ranged from total opposition to oblivion but was, generally, one of skepticism. Ten years later, however, and after the first Solvay Conference in 1911, the increasing presence of Planck's hypothesis in the scientific literature forced a new discussion of the topic in the same forum: the BAAS meeting in Birmingham in the summer of 1913. Jeans, who had recently converted to the theory of the quantum and was one of only two British physicists present at the Solvay meeting, took on the task of explaining and defending the theory of the quanta to a reluctant audience.

Jeans had been second wrangler in 1898, being one of the first two students, together with Godfrey H. Hardy, to attempt the Cambridge Mathematical Tripos in only two years—and not in the usual three years—after which he was appointed fellow and lecturer in Trinity College (Milne 1952). During this period, he worked on radiation theory and statistical mechanics, producing his first book, *The Dynamical Theory of Gases* (Jeans 1904), and contributing to what we now know as the Rayleigh-Jeans law for the distribution of the radiation from a black-body, which was derived using the equipartition of energy. His constant failure to describe the experimental energy distribution of black-body radiation using classical arguments did not force Jeans, at first, to accept Planck's hypothesis, but to search for alternative mechanisms to explain the experimental law. Faithful to the equipartition principle, a central tenet in statistical mechanics, Jeans was first willing to challenge Planck's law on the basis that real, thermal equilibrium was impossible in a black body. But by 1910 he had changed his mind, forced by the explanatory success of Planck's law as well as by the theoretical proof that this law could be obtained *only* with the assumption of quanta (Hudson 1989). Another recent convert, Henri Poincaré, also developed a very detailed demonstration of the sufficiency and necessity of the hypothesis of quanta for obtaining Planck's law in 1912, just after the first Solvay conference. Jeans admired Poincaré's more general proof, and he used it in his subsequent defense of the quantum theory.

The *Report on Radiation and the Quantum-Theory* that Jeans prepared for the 1913 BAAS meeting, and which was published a few months later, acted as a textbook from which many British scientists learned the basic tenets of the quantum theory during the war, or immediately afterwards (McCrea 1985). That is why it serves as the starting point for this pedagogical story, even though it was not formally a textbook. The *Report* also offers a window into Jeans's own *conversion* process, emphasizing the impossibility of accounting for black-body radiation with any hypothesis other than Planck's quanta and, also, stressing the importance of Poincaré's reflections and Bohr's model of the atom. Albert Einstein's explanation of the photoelectric effect, and the theory of the specific heats of solids by Einstein, Peter Debye, and Frederick A. Lindemann are also present, but only as indirect support for the quantum hypothesis.

The *Report* is an interesting exercise of rhetoric, intended to convince British mathematical physicists, mostly influenced by the MT Cambridge tradition, of the unavoidability of the quantum hypothesis. From the beginning of the book, Jeans addresses the same criticisms of Planck's theory that he himself had offered a few years before, by acknowledging that:

> [T]he mere discovery that a phenomenon is difficult to explain in the Newtonian way is no adequate reason for abandoning a system of laws which is known to hold throughout vast regions of natural phenomena […]. From demonstrating that a matter is difficult to proving that it is impossible is a long step, but if this step can be taken with respect to the explanation of even one well-established phenomenon of Nature, then the logical necessity of rejecting the impossibility becomes unanswerable. (Jeans 1914, 2)

The tendency in Britain at the time was to follow in Larmor's footsteps, who was still trying to obtain Planck's law in terms of some continuous motion or mechanism, in spite of Jeans's and Poincaré's demonstration of the fundamental impossibility of such a project (see for example Larmor 1909; Hudson 1989, 72). For instance, Augustus E. H. Love, second wrangler in 1885 and Sedleian Professor of Natural Philosophy in Oxford since 1898, argued that "from a mathematical point of view there must be infinitely many formulae which would agree equally well with the experiments" (Anonymous 1914, 384, see also Ewald 1913). Larmor himself, and Joseph John Thomson, were the main opponents to Jeans, this time also rejecting the new theory of specific heat in solids, while Hendrik Antoon Lorentz and a young Bohr were on Jeans's side. The discussions at the Birmingham meeting of the BAAS "made it abundantly clear that the quantum theory is far from being regarded as inevitable yet by many of the English school of physicists" (Jeans 1914, 23), and that is why Jeans took in the *Report* a very pedagogical approach, including full references to the criticisms by Love, Thomson, Larmor and others, and his answers to those challenges. Incidentally, the BAAS meeting started with a presidential address given by Oliver Lodge on "Continuity," a manifesto in favor of the real existence of the ether, its essentially continuous nature, and against the theories of relativity and quanta (Lodge 1914).

To understand the *Report*, we have to bear in mind the mental framework of the public to which it was addressed, a framework which Jeans himself had, until very recently, fully shared, and which had its roots in the metaphysics embedded in the training of Cambridge mathematical physicists. The ether was a real substance—and this remains so in the *Report*—and physical explanation was synonymous with mechanical modeling. These two aspects were pivotal in the introductory chapter:

> For whatever is regarded as certain or uncertain about the ether, it must be granted as quite certain that it approaches more closely to a continuous medium than to a gas […]. And if, as seems most probable, the ether is a perfectly grainless structure, […] the total energy [in a black-body] will be infinite. […] To put the matter shortly: in all known media there is a tendency for the energy of any systems moving in the medium to be transferred to the medium and ultimately to be found, when a steady state has been reached, in the shortest vibrations of which the medium is capable. This tendency can be shown (Chapter II) to be a direct consequence of the Newtonian laws. This tendency is not observed

> in the crucial phenomenon of radiation; the inference is that the radiation phenomenon is determined by laws other than the Newtonian laws. (Jeans 1914, 6–7)

In support of the latter, chapter 2 partly repeats Jeans's own work from before 1910, in which he tried to exhaust all possible mechanisms that might account for the "full radiation" or "black-body radiation" with classical arguments. The core of the argument was, obviously, that "any radiation formula corresponding to a steady state must be derived by expressing that the amount of energy gained by the ether is equal to the amount absorbed" (Jeans 1914, 9), for which one had to think of different possible mechanisms of absorption and emission. Jeans tested three such possibilities: "resonators" of perfectly definite periods, the motion of free electrons in matter, and the photoelectric effect. In all cases, he obtained the Rayleigh-Jeans formula he had obtained from the general principle of equipartition, and therefore, he inferred that the ultraviolet catastrophe was unavoidable on classical grounds: "It is to escape from this necessary consequence of the classical mechanics that the quantum theory has been brought into being" (Jeans 1914, 23).

Chapters 3 to 6 give a very clear account of the quantum theory and its success in accounting for radiation, spectra, the photoelectric effect, and the specific heat of solids (in this order), leaving for the last chapter what he calls the "physical difficulties" or the "physical basis" of the theory (Jeans 1914, 33 and 79). And this is the chapter to which I now turn, because it is here that we find Jeans trying to understand, or better to speculate on, the physical implications of accepting the quantum theory. Because, as he well says, accepting Planck's hypothesis tells us very little about the reality of physical processes:

> The indications are that there is, underlying the most minute processes of nature, a system of mechanical laws different from the classical laws, expressible by equations in which probably the quantum-constant *h* plays a prominent part. But these general equations remain unknown, and at most all that has been discovered is the main outline of the nature of these equations when applied to isochronous vibrations. (Jeans 1914, 79)

The main problem for Jeans was not that the quantum theory was, as yet, limited in its applicability, but that "even if the complete set of equations were known, it might be no easy task to *give a physical interpretation of them, or to imagine the mechanism* from which they originate" (Jeans 1914, 79, emphasis added). I emphasize the last sentence because, for him, as for most physicists of the Cambridge school, intelligibility involved the possibility of imagining a mechanism that could account for the observed phenomena. But when faced with the quantum, any "attempt to imagine a universe in which action is atomic leads the mind into a state of hopeless confusion" (Jeans 1914, 79–80).

From dimensional considerations, Jeans underlined that Planck's constant had the physical dimensions of angular momentum, something consistent with Bohr's recent theory for the hydrogen atom. In any case:

> [T]he brilliant agreement […] with experiment may indicate that in these cases the angular momentum of the single electron certainly behaves as though it were atomic, but this does not carry us any perceptible distance towards a physical explanation of why this atomicity exists. (Jeans 1914, 80)

More interesting for Jeans, and also from dimensional considerations, *h* is related to the square of electric charge, which meant it was related to "the strength of a tube of force binding two electrons. This suggests that the atomicity of *h* may be associated with the atomicity of *e*" (Jeans 1914, 81). Jeans reminded the reader that the atomicity of the electrical charge had no basis in Maxwell's theory, and that, so far, "no reason is known why an electron with charge $\frac{1}{2}$ *e* should not exist" (Jeans 1914, 81). And, although the atomicity of the charge did not necessarily involve the quantum theory, "otherwise the quantum theory would have been fully developed long ago […] there is, perhaps, a hope that the two atomicities may be special aspects of some principle more general than either of them" (Jeans 1914, 81); and this had to be, inevitably, related to the structure of the ether.

The incorporation of Thomson's "tubes of force," a very Cambridge mathematico-mechanical device, is, I think, suggestive of the fact that Jeans was not willing to do away with the Cambridge tradition to which he belonged. Jeans regarded Einstein's hypothesis of a quantum as "corpuscles of radiation" comparable to Thomson's real existence of discrete Faraday tubes. Both constructions could account for the structure of energy exchanges, only that the latter would be in continuity with the older framework. But in both cases there was no hope of reconciling the undulatory theory of light with the quantum theory, since experimental evidence "seems almost to indicate that both theories are true simultaneously" (Jeans 1914, 89).

This last chapter finishes with a discussion on the reality of the ether, acknowledging that, in this respect, continental and British physicists play on different—opposed—sides. Jeans seems to cling to the reality of the ether, but he relegates it to a second place: the real stumbling block being the contradiction between discrete and continuous theories, both valid for different radiation phenomena. And, with this, the last pages of the book convey a certain amount of pessimism as to the status quo of physics. In a free translation from Poincaré's *Dernières Pensées* he says:

> It is impossible at present to predict the final issue. Will some entirely different solution be found? Or will the advocates of the new theory succeed in removing the obstacles which prevent us accepting it without reserve? Is discontinuity destined to reign over the physical universe, and will its triumph be final? Or will it finally be recognized that this discontinuity is only apparent, and a disguise for a series of continuous processes? […] Any attempt at present to give a judgement on these questions would be a waste of paper and ink. (Jeans 1914, 90)

While chapters 2 to 6 were an active exercise in convincing the reader of the inevitability of the quantum hypothesis and its successes, these last pages blunt that optimism by pointing to the difficulties of interpretation of the quantum theory. But this is done in a particular way: these last sentences can be interpreted as a way to encourage British physicists to embrace the theory rather than a priori rejecting it on the grounds that it is not "physical," that is, mechanical. Furthermore, the fact that these considerations appear only at the end of the book as a separate chapter may indicate that, from Jeans's point of view, one could and should accept the quantum theory without having a full answer to its ultimate physical meaning. Partly following the problem-solving tradition of the Cambridge MT pedagogy, Jeans was more concerned about proving that the quantum theory solved specific problems than attempting an overall challenge on metaphysical grounds.

9.2 Teaching Quantum Theory in the 1920s

As mentioned in the introduction, the position of quantum theory in Cambridge was far from satisfactory at the end of the Great War. In 1920, Jeans himself, when adding a last chapter on quantum theory in his third edition of *The Dynamical Theory of Gases*, regrets the absence of British scientists in the new science. He writes:

> This chapter can of necessity provide only a very brief introduction into the mysteries of Quantum Dynamics, but I hope it will be of value in stimulating the interest of English-speaking readers in a branch of science of which the development has so far been left mainly to other nations. (Jeans 1921, preface to the third edition)

One way to track the status and evolution of the quantum in the old university is to have a look, however quick, at the evolution of courses taught to undergraduates. The "advanced," optional courses were normally a reflection of the particular interests of individual researchers, and could give rise to exam questions only in what was known as Schedule B of the Tripos, Part II.[4]

It should be remembered that, following a tradition going back to the 1860s, physics in the 1920s was taught as part of the "Mathematical Tripos" (as theoretical physics or applied mathematics) and as part of the "Natural Science Tripos" (which was mainly experimental science). This meant that these two worlds were relatively independent of each other: experimental physics being taught at the Cavendish Laboratory, and mathematical physics by college lecturers. However, the special optional courses were, for the most part, open to both kinds of students.

Who could teach quantum theory in Cambridge? Certainly not people like Larmor or Thomson who were strongly opposed to it. Nor could Ernest Rutherford, whose program was basically experimental. It was young people, both trained in the Cambridge Tripos and converted to the new theory, who could teach quantum physics. And these were, at the beginning of the decade, Darwin and Fowler. In a recent paper, I discussed Darwin's early understanding of quantum physics and the evolution of his ideas throughout the decade (Navarro 2009). After his training in the MT, he moved to Manchester, where he learned experimental techniques related to spectra and radioactivity. There, he also met Bohr in the dramatic years of the development of the atom model using the quantum hypothesis. In 1919, he was appointed fellow of Christ's College and started giving the first courses on quantum theory and its relation to spectra. It is interesting to note that the first such course was primarily meant for NST students, probably supported by Rutherford.

[4] According to William McCrea, in his recollections of his undergraduate days in Cambridge:

> Apparently anyone could offer to deliver a one-term lecture course. If the appropriate Faculty Board approved, it would be announced in the Schedule B lecture list. This implied that in due time a candidate could declare a wish for there to be questions (probably two) on the course in the examination […]. If any candidate legitimately included a particular course in his list, the lecturer was responsible for producing the questions; these had then to be approved by the Part II Examiners, who had to arrange the Schedule B papers in such a way that every candidate's chosen subjects were suitably distributed through the six papers. But when it came to the examination any candidate could attempt any questions he liked; he need not confine himself to the topics in the list. (McCrea 1987, 62)

When Darwin left Cambridge in 1922, Fowler began to teach quantum physics, this time in courses open to both triposes. Unlike Darwin, Fowler was self-trained in the theory of quanta and eventually became the catalyst for work in quantum physics in Cambridge, promoting a new generation of quantum physicists by, for example, translating into English many of the key papers that were appearing in German, as well as by inviting people such as Ralph Kronig or Heisenberg to give lectures in Cambridge. He was also a sort of father figure to people like Douglas Hartree, Llewellyn H. Thomas and, of course, Dirac, all of whom made important contributions to the development of quantum physics in the late 1920s. It is also well known that Fowler became a sort of *theorist-in-residence* at the Cavendish, as well as Rutherford's son-in-law (Gavroglu and Simoes 2002).

In the academic year 1924/1925, we see a turning point in the teaching of quantum theory in Cambridge. Fowler had been, for two years, giving the only, one-term course on the "Quantum Theory of Spectra." But that was not enough now. Quantum physics was progressing, and Cambridge started to teach advanced courses. Not surprisingly, it was the younger generation that could teach the latest developments, since they had been in close contact with Copenhagen and some of the German research centers.[5] Thus, we find advanced courses taught by Dirac and by Hartree in the second half of the decade; courses that were, especially in Dirac's case, but also in Fowler's and Hartree's, reflections of science in the making.

The following is a list of all these courses taken from the information provided in the *Cambridge University Reporter* in the period 1919–1929:

1920/21	NST	Darwin: 1st Term, "Recent Developments in Spectrum Theory"
1921/22	MT	Darwin: 2nd Term, "The theory of quanta"
1922/23	MT & NST	Fowler: 2nd Term, "The quantum theory of spectra"
1923/24	MT & NST	Fowler: 2nd and 3rd Terms, "The quantum theory of spectra"
1924/25	MT & NST	Birtwistle: 2nd Term, "Introduction to the Quantum Theory" Fowler: 3rd Term, "The Quantum Theory. Recent Developments"
1925/26	MT & NST	Birtwistle: 1st Term, "Introduction to Quantum Theory" 2nd Term, "Quantum theory of Spectra" 3rd Term, "The Quantum Theory. Special Topics" Dirac: 3rd Term, "Quantum Mechanics (Recent Developments)" Hartree: 2nd Term, "Physics of the Quantum Theory"
1926/27	MT & NST	Birtwistle: 1st Term, "Quantum Theory" 3rd Term, "Quantum Mechanics," (cont.) Hartree: 2nd Term, "Physics of the Quantum Theory"

[5] Fowler, Hartree, and Dirac were visitors at Bohr's institute in Copenhagen.

1927/28	MT & NST	Birtwistle: 1st Term, "Quantum Theory of Spectra" 2nd Term, "The New Quantum Mechanics" Dirac: 1st Term, "Modern Quantum Mechanics" 2nd Term, "Modern Quantum Mechanics," (cont.) Fowler: 3rd Term, "Statistical Mechanics, Old and New" Hartree: 2nd term, "Physics of the Quantum Theory"
1928/29	MT & NST	Birtwistle: 1st Term, "Quantum Theory of Spectra" 3rd Term, "Quantum Mechanics" Dirac: 2nd Term "Modern Quantum Mechanics" Fowler: 3rd Term, "Selected Problems in Wave Mechanics" Hartree: 2nd Term, "Physics of the Quantum Theory" 3rd Term, "Physics of the Quantum Theory," (cont.)

Table 9.1: List of all courses announced in the *Cambridge University Reporter* in the period 1919–1929.

The only *outsider* named in the list of lecturers teaching quantum physics is Birtwistle, to whom the rest of this paper is devoted. And I say *outsider* not because he came from some other university, but because he was the only "real" wrangler accepting and spreading quantum physics in Cambridge, which makes him a unique example in trying to understand the ways in which the new theory was received in the old Cambridge wrangler tradition.

Birtwistle is a typical product of the MT tradition. Born in 1877, he arrived in Cambridge in 1895 and was bracketed senior wrangler in 1899. This means that he was a contemporary of Jeans, but took the usual three years to sit for the MT examination. After this, he was appointed fellow and lecturer of mathematics in his own college, Pembroke, where he remained until his sudden death in May 1929. Like many dons of the old school, "it was as a teacher rather than as an investigator that Birtwistle was known, and as a teacher that he played a conspicuous part in Cambridge mathematics" (Anonymous 1929, 881). The short description of his teaching style in the obituary note we find in *Nature* is almost all we have about him:

> As a lecturer, Birtwistle was admirably clear and easy to follow. He set, in fact, a standard of exposition which made it very difficult for anyone to attract students to any duplicate course. His books are like his lectures—admirable expositions of those sections of the subject with which he deals, written in lecture-room style. He seldom attempts to go deeply into difficult points or to present the subject as a single logical whole. His aim is the lecturer's aim—to interest the student in the subject, especially in its more outstanding or exciting parts, and lead him on to other more systematic or abstruse expositions. (Anonymous 1929, 881)

What courses did he normally teach? In the annual lists, we find him consistently teaching the general introductory courses on "Mechanics (Statics and Particle Dynamics; Rigid Dynamics)" and "Electricity," and he was among the first to take on board courses on thermodynamics when these were introduced in the list of elementary courses in 1924.

As for his more specialized courses, between 1920 and 1924, he consistently taught a one-term course on "Hydrodynamics (motion of solids and vortices in a liquid; waves)." In the academic year 1924/1925, he started teaching an "Introduction to Quantum Theory," while Fowler taught more advanced quantum matters. In the following years, he taught further quantum courses, from which he finally produced two books: *The Quantum Theory of the Atom* in 1926, and *The New Quantum Mechanics* in 1928.

9.3 *The Quantum Theory of the Atom*

The Quantum Theory of the Atom is a window into Birtwistle's first courses on quantum physics, in the early months of 1925, and in the academic year 1925/1926. It consists of a compilation of lectures from that period, and it was intended as a textbook for a similar course the following year (1926/1927). As is obvious from his correspondence with the publisher, Birtwistle rushed the printing of the book for two reasons: "as you know the subject is changing so rapidly that it would be a good thing to get it out as soon as possible; also so far there is no English book of this kind so far published and I think it will meet a real demand."[6] This book does not try to give a full, consistent, and closed picture of quantum physics, but rather to teach the mathematical apparatus needed to apply quantum physics, as known at the time. That means that the book is organized around the quantization strategy and its application to those cases for which it works. For the conceptually-minded reader, however, the book is disappointingly flat. Contrary to what happened with Jeans's *Report*, and also compared to other pedagogical works, Birtwistle's book does not provide many explanations concerning the "physical" meaning of the theory; it basically teaches the mathematical methods for applying quantum physics to different problems and shows their agreement with experimental data.

But before we go into these and other technical elements, there is an aspect of the book, present especially in the more historical first two chapters, of particular interest. Birtwistle links the history of quantum physics to developments by British, especially Cambridge, scientists. *The Quantum Theory of the Atom* describes precisely that: the quantum theory of the structure of the atom, and this is a story that, according to Birtwistle, has its beginnings in Cambridge: "the modern theory of the structure of the atom is in the first place due to J. J. Thomson" with his discovery of the electron (Birtwistle 1926, 16). In this timeline, Thomson's key contributions continued with his model of the atom, and also with his study of positive rays, since the latter was the source for Francis Aston's mass spectrograph and the discovery of isotopes. Birtwistle's story of the structure of the atom continues with Rutherford "and his school in which the instrument of the α-particle was used to disclose the nature of the atom" and to propose an atomic model "which is now generally used in theoretical work" (Birtwistle 1926, 17). This model, for instance, is used to explain the nature of Thomson's positive rays.

In this historical survey, Bohr's 1913 contribution to the atomic model comes only after a detailed explanation of the hydrogen spectrum and the need to explain Balmer's formula. But Bohr's contribution comes hand in hand with the work of another Cambridge researcher, John W. Nicholson, who was working on stellar spectra and who brought forward, in 1912, an atomic theory in which Planck's constant was interpreted as determining

[6] Birtwistle to S. C. [sic], September 1926, Cambridge University Press Archives.

the angular momentum of permissible orbits of the electrons inside an atom.[7] Birtwistle rightly distinguishes between Nicholson's and Bohr's contributions, the former giving only the condition for the angular momentum of an electronic orbit to be $nh/2\pi$ where n is an integer, while the latter gave the "new concept which was to be the key to the solution of the problem of spectra," namely that "the radiation emitted between transitions between two stationary states has a frequency ν given by the relation $E - E' = h\nu$" (Birtwistle 1926, 23). Throughout the book, however, Birtwistle keeps the expression "the Nicholson-Bohr condition," meaning the nuclear model with quantized orbits. For the reader, this British-oriented story consolidates the idea that it was the "amazing verification" of Bohr's atomic model that "at once fixed attention upon the quantum theory, which up to then had received skeptical regard from physicists in general" (Birtwistle 1926, 24).

The third chapter is a compilation of things that are related to the quantum theory but that are not dealt with in detail in the book. First is the one-page explanation of the mathematics of Bohr's correspondence principle, in the version he introduced in his 1918 paper "On the Quantum Theory of Line Spectra" (Bohr 1918). After this rather plain introduction of the correspondence principle, chapter 3 continues with a section devoted to the photoelectric effect, and another section in which he explains Einstein's 1917 deduction of Planck's radiation formula. On the former, there is an interesting clarification regarding the quantum of light:

> Einstein's theory of "light quanta" is not now generally accepted by physicists, but the argument above does not essentially depend upon their existence. All that is necessary is to assume that interchanges of energy between radiation and atoms can only occur in quanta. (Birtwistle 1926, 35–36)

If we remember that the book was written in 1926, this paragraph is somewhat surprising since, by then, the experiments of Arthur Compton *had* triggered a general acceptance of the light quantum.

Having established the existence, historical origin, and realm of application of the new theory, the rest of the book is an attempt to train students in techniques of quantization using a twofold strategy: to provide lots of examples where quantization is successfully applied, and to show that there is continuity between the methods used in "classical" and quantum theory. Because, as Birtwistle sees it, that is the only way one gets hold of the new physics: by using it, rather than by presenting it in a general form or analyzing its conceptual or philosophical implications. And this brings us to the main claim of this paper. Birtwistle, a first wrangler in the "Mathematical Tripos," tried to teach quantum physics in the same way classical physics was taught in the Cambridge MT tradition: by repetition of examples, by solving specific problems, and by a relatively uncritical embrace of particular mathematical methods.

Once Bohr's theory for the stationary states of the hydrogen atom has been introduced, the next step is to extend the quantum theory to more complex atoms. Here, he introduces Ehrenfest's adiabatic principle, as a generalization of the Nicholson-Bohr quantum condition: "The question now arises, what mechanical entity is to be equated to nh for more

[7] Nicholson was a Cambridge graduate, taught mathematical physics at the Cavendish Laboratory, and moved to King's College, London, in 1912. For a full account of Nicholson's work and influence on Bohr, see (McCormmach 1966).

complex systems than that of the hydrogen atom." The answer was given by Ehrenfest who supposes:

> [T]hat the "entity" which does not change under the influence of the slowly changing external forces must be an "adiabatic invariant" of the classical theory. This is the "adiabatic principle" of Ehrenfest, and it requires that only adiabatic invariants are to be equated to nh in order to determine the stationary states. (Birtwistle 1926, 41)

With the generalization of the quantum condition, Birtwistle embarks on a series of chapters explaining what he calls the basic "general dynamical theory," chapters in which he fully shows his conditioning as a wrangler. The variation principle, Lagrange's and Hamilton's equations, the Hamilton-Jacobi differential equation and the ways to solve it, the Keplerian orbit, angle variables, and many other mathematical tools are explained. It would seem to be a book on mathematics (or classical physics) were it not for the fact that, at the end of some sections, the "quantum condition" appears. And it appears as purely the mathematical condition that some constant in the equations is equated to nh, without further ado.

As an example, we can pick chapter 9 on the Stark effect. After a very short summary of the effect, he says that "the classical theory fails utterly to account for the Stark effect," and immediately develops the mathematics of Epstein and Schwarzschild's solutions:

> The dynamical problem to be solved is the motion of an electron due to a Coulomb center of force and a constant force parallel to a fixed direction. This is a particular case of two centers of force solved by Jacobi by the use of elliptic coordinates. (Birtwistle 1926, 97–98)

All this he explains from an exclusively mathematical point of view. At the end of the process, the quantum condition ($J = nh$) is imposed as part of a mathematical technique, through which the numerical results can be calculated and compared with experimental values. The reader is, thus, led to believe that quantum physics is in strict mathematical (and, *therefore*, physical) continuity with earlier physics, since the mathematical methods and formulas are *almost* the same.

It would be superfluous, in this paper, to give a detailed account of each chapter in Birtwistle's book. The structure is basically the same for all: classical calculations in which the quantum condition is brought in as a particular mathematical trick that needs to be implemented to get a correspondence with experimental data. In 21 chapters, one can never find words such as "provisional," "incomplete," "failed explanation," or anything that indicates that the quantum theory of the atom, as it is, might be viewed as incomplete or, worse, deficient. It is only in a rushed last chapter, written during what looks like his usual vacation in Norway,[8] that Birtwistle introduces the reader to a list of unexplained phenomena like the anomalous Zeeman effect and the Paschen-Back effect, and to new theories, like the Bohr-Kramers-Slater theory (BKS) and the new quantum kinematics of Heisenberg. But there is no sense of stress, or crisis, or revolution. There are no value judgments. One gets

[8]Letters from Birtwistle to the secretary of Cambridge University Press testify to these holidays, 26 September 1926, 22 August 1927, 9 September 1927, Cambridge University Press Archives.

the impression that everything introduced, even in these last chapters, is just steps in the development of the new physics.

Only in the last two pages, and in a statement that de facto undermines the whole project of this book, does he say:

> Heisenberg has lately put forward the beginnings of a scheme of quantum-kinematics, which when more developed should lead to the direct deduction of these quantum theory formulae, without the intermediate use of the classical formulae in each problem considered. (Birtwistle 1926, 230–231)

This undermining of his entire first book leads us very naturally to Birtwistle's second book, to which the next section is devoted. But before we move on, it is worth noting that Birtwistle's introductory course in quantum theory was substituting for Fowler's similar course from previous years. Actually, we also have a window into Fowler's lessons, through Thomas's complete classroom notes.[9] Obviously, these notes have a spontaneity that Birtwistle's book does not have, and one should compare the two documents only with caution; regardless, they show us very similar content (although with a sensibly different structure), but presented in a totally different style. Fowler was actively working on specific problems in the quantum theory and his lectures contain lots of qualitative explanations, experimental results, and a strong sense of the limitations of the current theory. It is, by far, much less mathematical than Birtwistle's presentation, and mathematical developments go hand-in-hand with constant explanations of their physical meaning, something that is nearly absent in Birtwistle's book. His style is closer to the old MT pedagogical system in which students were introduced to problem-solving techniques by repetition of cases. The aim of the lectures was seldom to challenge the status quo of the theory, but rather to give an account of how to use the accepted theory. And this is what, as I understand it, *The Quantum Theory of the Atom* is: a work to drill students in the quantization techniques, with very limited recourse to experimental results and with no critical outlook whatsoever on the limitations of the theories explained.

9.4 *The New Quantum Mechanics*

Birtwistle wrote a second book on quantum physics, related to his more advanced lectures on recent developments of quantum mechanics, the preface of which was signed in Copenhagen in October 1927.[10] From a pedagogical point of view, *The New Quantum Mechanics* is very disappointing. Even in the respectful tone of an obituary, his biographer alluded to this fact:

> Perhaps the least successful of his books was the last, on modern quantum mechanics. Here, owing to the novelty of the subject and the absence (when Birtwistle wrote) of other more systematic expositions (or indeed of any other exposition), the weakness of this deliberate method becomes more obvious. The book gives rather the impression of a collection of interesting isolated sketches. (Anonymous 1929, 881)

[9] Microfilm no. 6, AHQP.

[10] The official list of visitors does not include Birtwistle as a formal visitor to Bohr's Institute (Robertson 1979, 156–158). Furthermore, in an epistolary exchange with Bohr, they both regret that they could not meet each other in Copenhagen during Birtwistle's visit, from which I infer that his was more of a touristic visit than a research trip (microfilm no. 16a, AHQP).

The New Quantum Mechanics is precisely that: a collection of the latest developments in quantum theory. In the words of another reviewer:

> This account is very accurate and contains practically everything that has been done up to the summer of 1927. He gives us, so to speak, original abstracts of the principal papers and allows us a survey of everything that is known. This makes the work not exposition from one point of view, as is Weyl's new book; it is rather an "impartial" treatment of the methods of the different schools, with credit given to each for its results. (Struik 1930, 32)

In *Nature*, Fowler spoke of *The New Quantum Mechanics* as one of the best examples of introductory books, an otherwise dangerous genre in the current state of affairs, in which Birtwistle gave "a convenient and faithful but uncritical reproduction of much of the earlier work of the theory" (Fowler 1929, 363).

The first five chapters of this book provide further examples supporting my claims at the end of last section. Birtwistle's "impartiality" involves a neutral style in the sense that there are no critical analyses of the theories, or their theoretical or experimental limitations. These first chapters introduce the notion of spin, for which he needs to explain the problem with the anomalous Zeeman effect, the Stern-Gerlach experiment or Landé's experimental formula. All of these phenomena were well-known long before 1925, when he wrote *The Quantum Theory of the Atom*. But none of these problems were mentioned in that book, except in the last chapter. Birtwistle was not training his students in the limitations and failures of a particular theory, but in its successes.

The matter-of-fact style is clear from the first sentences of the book: "The origin of the new quantum mechanics was an epoch-making memoir by Heisenberg which contained the new concept which was to lead to the phenomenal developments of quantum mechanics of the past two years." And why was a new theory needed? "For some years before 1925, Sommerfeld, Heisenberg, Landé and Pauli had been grappling with the complex problem of the multiplets and their Zeeman separations," which were only partly solved by introducing ad hoc half integers as possible values for the quantum numbers. Yet, again:

> [A] real difficulty too had been met with in the spectrum of neutral helium, where *two* electrons revolve round the nucleus (the simplest many electron problem), all the theoretical results found being at variance with experiment; again in the problem of the "crossed" fields, where an atom is exposed to the combined action of electric and magnetic fields, fundamental difficulties arose. (Birtwistle 1928a, 1)

Obviously, in his previous book, Birtwistle never talked about these very "fundamental" problems, or about the limitations of the now "old" quantum theory, which was, at the time, the accepted way to solve those problems. It is only in 1928, after a new method has been found, that the limitations of the previous method are relevant: "Heisenberg's new theory however at once led to the formula $(n+\frac{1}{2})h\nu$ as the energy of the stationary state of Planck's oscillator, so that half odd integers came quite naturally into the new results" (Birtwistle 1928a, 2).

In the last chapter, Birtwistle tried to summarize his understanding of the latest, as yet unpublished, developments coming from Bohr's institute. Returning from his holiday in

Norway, Birtwistle visited Copenhagen, but Bohr was not there, since the visit coincided with the 1927 International Physical Congress in Como, Italy. Thus, Birtwistle got only second-hand accounts of Bohr's latest views. This was, however, one of the points that Cambridge University Press stressed in the advertising of the book. In an advertisement in *Nature* (1928), we read that the forthcoming book contains "new and hitherto unpublished speculations of Prof. Niels Bohr." Certainly, the last paragraphs of the book include two footnotes, one referring to the meeting in Como, the other to the recent Solvay Conference. And, ironically, this was the source of the only *research paper* that Birtwistle wrote in his life: a note in *Nature* in which he qualifies the tone of the last chapter. There, we read that:

> Prof. Bohr points out that the wording of the chapter may create the impression that these [probability] calculations were primarily developed in connexion with the new ideas [of complementarity], whereas they may be said to be characteristic of the whole recent developments of the quantum theory. (Birtwistle 1928b, 58)

Actually, the wording of this note was revised and changed by Oskar Klein and Bohr himself in Copenhagen.[11] This unfortunate anecdote demonstrates the limited understanding Birtwistle had of the depth of the new quantum mechanics and the conceptual, methodological, and philosophical debates around it, in spite of his relatively good mastery of the mathematics involved.

One last, revealing anecdote about the book comes from William McCrea, who was an undergraduate in Cambridge between 1923 and 1926. Talking about *The New Quantum Mechanics*, he recalled that:

> [I]t was a remarkable achievement to produce such a comprehensive account of work newly published during the two years before the appearance of the book itself. Hartree described it to me in conversation as the "bare bones" of the subject, but it need not be only medical students who find it useful to have a skeleton for their studies.[12] (McCrea 1985, 58)

9.5 Conclusion

Contrary to Fowler's or Darwin's lectures, Birtwistle's courses are seldom mentioned in the recollections of scientists who studied in Cambridge in the 1920s. That may be due to a number of different factors. It is possible that some bright students and future prominent physicists attended his lectures but forgot about them, influenced by the selective memory usual in these kinds of recollections. But it is also likely that Birtwistle's courses were seen, already at the time, only as second best, as courses to be taken only by those wanting to get a feeling for the new theory, but not to master it and to work on quantum problems. Actually, in a letter to Dirac, Fowler admits that Birtwistle's lectures are only meant for "complete beginners" who need "to get the ground work first."[13] That would explain why, among those scientists who became, in some degree, actors in the new quantum generation, we do not find

[11] Microfilm no. 9, Bohr Collection.
[12] See also (McCrea 1987).
[13] Fowler to Dirac, 12 June 1927, DRAC 3/1, Churchill College Archives.

students of Birtwistle (some of them actually remember his elementary lectures in mechanics and electricity, but not on quantum theory).

Birtwistle's case can help us to understand another fact that is normally forgotten in the histories of *revolutions*. Quantum theory was not, for everyone, that revolutionary new theory that forced them into research. Birtwistle is an example of how one could, in times of change, stick to old methodological—not conceptual—paradigms. And, again, not all the students interested in quantum physics were necessarily potential participants in the forefront of scientific research. Having both Dirac and Birtwistle teaching advanced courses on quantum mechanics suggests that, as early as the late 1920s, there was room in Cambridge for a two-tier training system in the theory of quanta: one for potential researchers, another for people wanting *only* to be up-to-date with the latest science.

Abbreviations and Archives

AHQP	Archive for History of Quantum Physics. American Philosophical Society, Philadelphia
BAAS	British Association for the Advancement of Science
Bohr Collection	Archive for History of Quantum Physics
Cambridge Tripos Examination Papers	Cambridge University Library
Cambridge University Press Archives	Manuscripts Department of the University Library at the University of Cambridge
Churchill Archives Centre	Churchill College, Cambridge
MT	Mathematical Tripos
NST	Natural Science Tripos

References

Anonymous (1914). Discussion on Radiation. In: *Report of the Eighty-Third Meeting of the British Association for the Advancement of Science. Birmingham: 1913, September 10-17*. London: John Murray. 376–386.

— (1929). Obituary. *Nature* 123: 881.

Birtwistle, George (1926). *The Quantum Theory of the Atom*. Cambridge: Cambridge University Press.

— (1928a). *The New Quantum Mechanics*. Cambridge: Cambridge University Press.

— (1928b). The Complementary Nature of the Quantum Theory. *Nature* 122: 58.

Bohr, Niels (1918). On the Quantum Theory of Line Spectra. *Det kongelige danske videnskabernes Selskab, matematisk-fysike Meddelelser* 4(1): 1–36.

Ewald, Paul P. (1913). Bericht über die Tagung der British Association in Birmingham (10. bis 17. September). *Physikalische Zeitschrift* 14: 1297–1302.

Fowler, Ralph H. (Mar. 1929). Review of *The New Quantum Mechanics*, by George Birtwistle. *Nature* 123: 363.

Gavroglu, Kostas and Anna Simoes (2002). Preparing the Ground for Quantum Chemistry in Great Britain: the Work of the Physicist R. H. Fowler and the Chemist N. V. Sidgwick. *British Journal for the History of Science* 35: 187–212.

Hudson, Rob (1989). James Jeans and Radiation Theory. *Studies in History and Philosophy of Science* 20: 55–77.

Jeans, James (1904). *The Dynamical Theory of Gases*. 1st ed. Cambridge: Cambridge University Press.

— (1914). *Report on Radiation and the Quantum-Theory*. London: The Electrician.

— (1921). *The Dynamical Theory of Gases*. 3rd ed. Cambridge: Cambridge University Press.

Larmor, Joseph (1909). The Statistical and Thermodynamical Relations of Radiant Energy. *Proceedings of the Royal Society A* 83: 82–95.

Lodge, Oliver (1914). Presidential Address. In: *Report of the British Association for the Advancement of Science, 1913*. London: John Murray. 3–42.

McCormmach, Russell (1966). The Atomic Theory of John William Nicholson. *Archive for History of Exact Sciences* 3: 160–184.

McCrea, William (1985). How Quantum Physics Came to Cambridge. *New Scientist* 96: 58–60.

— (1987). Cambridge 1923–6: Undergraduate Mathematics. In: *The Making of Physicists*. Ed. Rajkumari Williamson. Bristol: Hilger. 53–66.

Milne, Edward A. (1952). *Sir James Jeans. A Biography*. Cambridge: Cambridge University Press.

Navarro, Jaume (2009). "A Dedicated Missionary." Charles Galton Darwin and the New Quantum Mechanics in Britain. *Studies in History and Philosophy of Modern Physics* 40: 316–326.

Robertson, Peter (1979). *The Early Years. The Niels Bohr Institute, 1921–1930*. Copenhagen: Akademisk Forlag.

Struik, Dirk J. (1930). Review of *The New Quantum Mechanics*, by George Birtwistle. *The American Mathematical Monthly* 37: 32.

Warwick, Andrew (2003). *Masters of Theory: Cambridge and the Rise of Mathematical Physics*. Chicago: The University of Chicago Press.

Chapter 10
Paul Dirac and *The Principles of Quantum Mechanics*
Helge Kragh

10.1 Paul Dirac and Early Quantum Theory

Although not well known to the general public, Paul Adrien Maurice Dirac hardly needs to be introduced to physicists and historians of science. Born in Bristol in 1902 as a Swiss citizen—his father was Swiss and Paul only acquired British nationality in 1919—he became one of the most important theoretical physicists ever. His impact on modern physics may even have been greater than that of Einstein (Zichichi 2000). Young Dirac made his first breakthrough in the fall of 1925 when he developed his own version of quantum mechanics, known as q-number algebra, and over the next few years he established himself as a leading expert in the new quantum physics. In 1927–28 he made pioneering contributions to quantum statistics (Fermi-Dirac statistics), quantum electrodynamics, and relativistic quantum theory. The linear and relativistically invariant wave equation for the electron that he published in early 1928 not only explained the electron's spin and magnetic moment, but also, three years later, led to the prediction of antielectrons (positrons) and antiparticles more generally.

Dirac's genius was recognized early on. For example, he was part of the exclusive company of physicists invited to the famous Solvay conference in 1927. In 1930, at the unusually young age of 27, he was elected a fellow of the prestigious Royal Society, and the same year he published his monumental *Principles of Quantum Mechanics*, the subject of this essay. Two years later he was appointed Lucasian Professor of mathematics at Cambridge University, the chair once held by Isaac Newton and later by Stephen Hawking. Another high point of Dirac's career came in 1933, when he was awarded the Nobel Prize in physics, sharing it with Erwin Schrödinger. Although Dirac's scientific fame is closely linked to his fundamental contributions to quantum theory, and especially to those of the period 1925–34, he also dealt with other subjects, including cosmology, classical electron theory, and the general theory of relativity. Moreover, the influence of his ideas extended beyond physics, especially to mathematics (cp. the Dirac δ-function, Dirac matrices, and Dirac operators). Paul Dirac remained Lucasian Professor until his retirement in 1969, when he joined the physics department of Florida State University in Tallahassee. He died in 1984, and in 1995 a commemorative stone carrying his name and equation was unveiled at a ceremony in Westminster Abbey.[1]

[1] On Dirac's life and science, see (Dalitz and Peierls 1986; Kursunoglu and Wigner 1987; Kragh 1990). Dirac's private life is covered in detail in (Farmelo 2009). On Dirac as Lucasian Professor, see (Kragh 2003). Other secondary sources can be found in these works.

10.2 Origin and Dissemination

While still a Ph.D. student, under the supervision of Ralph Fowler, Dirac was assigned to lecture on the new and exciting developments in quantum theory. This first course ever on quantum mechanics at a British university was given in the Easter term of 1926 and attended by, among others, Nevill Mott, John A. Gaunt, Alan H. Wilson, Douglas Hartree, William McCrea, and Julius Robert Oppenheimer. Also Fowler and some of his students joined the course. McCrea recalled that the material of the lectures was close to that later presented in *Principles*, if, of course, restricted to what was known at the time (McCrea 1985; 1986).[2] The following year Dirac started giving a regular course on quantum mechanics, which he would continue to do until the 1960s.

The content of Dirac's early lectures formed the basis of the textbook that appeared in the summer of 1930, and which he subsequently used for his course. Given the scarcity of suitable textbooks in quantum mechanics at the time, and that Dirac had already prepared extensive lecture notes on the subject, it was natural for him to transform and update these into a proper textbook. Indeed, with respect to both structure and content there is a great deal of similarity between his lecture notes of 1927–28 and his textbook. Moreover, it was important for Dirac to present the principles of the new quantum mechanics in the way *he* thought they should be presented, namely as a concise and coherent symbolic calculus that allowed comparison between calculated quantities and those found experimentally. To him, the new physics was basically a formal scheme that allowed the calculation of experimental results, while it had nothing to say about ontological questions. The proper way of presenting quantum mechanics must necessarily be abstract, he wrote, for the new theory is "built up from physical concepts which cannot be explained in terms of things previously known to the student, which cannot even be explained adequately in words at all" (Dirac 1930). It was this abstract picture of quantum mechanics that *Principles* conveyed to its readers.

The idea of writing a textbook was not Dirac's, but seems to have come from James Gerald Crowther, a science journalist three years older than Dirac. This also accounts for the fact that the book was published by Oxford University Press and not, as would otherwise have been natural, by Cambridge University Press. Crowther, who had joined Oxford University Press in 1924 as representative for scientific and technical books, established a close relationship with physicists at the Cavendish Laboratory, including Ernest Rutherford and Peter Kapitza. For a time he acted as unofficial press agent for the Cavendish, and he would later write its history (Crowther 1974). On Crowther's initiative, the Oxford University Press decided to establish an International Series of Monographs on Physics with Fowler and Kapitza as general editors. The first book in the series was planned to be Dirac's work on quantum mechanics. Crowther recalled that when he first approached Dirac with the book proposal, "he was living in a simply furnished attic in St. John's College. He had a wooden desk of the kind which is used in schools. He was seated at this, apparently writing the great work straight off" (Crowther 1970, 39).[3]

---

[2] The content of Dirac's course in 1927–28 appears in his notes for "Lectures on Modern Quantum Mechanics," (AHQP).

[3] On the relationship between Crowther and Dirac, see (Farmelo 2009). The Oxford book series came to include several important monographs on physics, from the 1930s and later. Early examples are George Gamow, *Constitution of Atomic Nuclei and Radioactivity* (1931), John H. Van Vleck, *The Theory of Electric and Magnetic Susceptibilities* (1932), and Richard C. Tolman, *Relativity, Thermodynamics and Cosmology* (1934). The series continues to this very day, comprising a total of about 150 titles.

It should be kept in mind that there were very few British books on quantum theory at the time. According to a catalogue issued by the British Science Guild, in 1930 there were only fourteen British books on quantum topics, and many of them were translations from German or bound collections of lectures.[4] Although *Principles* was not the first book on quantum theory in Britain, it was one of the first. Dirac started writing the book in 1928, but due to travels and a busy scientific schedule—much of it occupied with the consequences of his new theory of the electron—progress was slow. By February 1930 the galley proofs of the book were ready, and about half a year later it appeared in the bookshops as the first volume in the Oxford International Series of Monographs on Physics. The preface was dated 29 May 1930, and the price was 17 shillings and 6 pence.

Principles of Quantum Mechanics became a great (and probably surprising) success, with the first edition selling about two thousand copies. The translations, and especially those in German and Russian, sold even better. The book quickly established itself as the standard work on quantum mechanics, not only used by students as a textbook but also by many experienced physicists. The mathematician Harish-Chandra recalled that he, while an undergraduate of Allahabad University in India, came across a copy of the 1930 edition in the university's library. "[I] was immediately fascinated by it," he said. "The exposition was so lucid and elegant that it gave me the illusion of having understood most of it and prompted in me a strong desire to devote my life to theoretical physics" (Harish-Chandra 1987, 34).[5] *Principles* came out in a substantially rewritten second edition in 1935, and still later editions appeared in 1947 and 1958, with reprints in 1967, 1971, 1974, and 1984. The third and fourth editions differed from the one of 1935 mainly in Dirac's use of the so-called "bracket" notation that he had developed in 1939 and which makes use of quantum states labeled as, for example, $< a|$ and $|b >$ (called a "bra vector" and a "ket vector") (Dirac 1939; Harish-Chandra 1987, 34).[6]

Principles was an enduring success. Paperback reprints of the fourth edition appeared as late as 1993, and the book is still in demand, eighty years after it was first published. The American physicist Philip Morrison exaggerated when he said that "everybody who had ever looked at books had a copy of Dirac," but as far as physicists were concerned, it may have been close to the truth (Weiner 1972, 131). In this essay, I shall be concerned with the first two editions only, those of 1930 and 1935.

10.3 Translations

Dirac's book on the principles of quantum mechanics was translated into German (1930), French (1931), Russian (1932), and Japanese (1936), and possibly into some other languages as well. The German translation, made by Werner Bloch, was arranged at an early time, as evidenced by a letter from Dirac to his Russian colleague Igor Tamm of January 1929, and it appeared only shortly after the English edition.[7] Dirac, who knew German well, checked the translation.

[4]The total number of physics books was 424, of which 39 were on relativity theory. In 1925 there were only 3 books on quantum topics. See (Williamson 1987, 10).

[5]Harish-Chandra became Dirac's research student in 1945. After having obtained his Ph.D. in 1947 and doing some work in theoretical physics, he moved to the United States, where he changed from physics to mathematics.

[6]On the third and fourth editions, see (Brown 2006).

[7]Dirac to Tamm, 3 January 1929, quoted in (Kragh 1990, 79). The letter is reproduced in full in (Kojevnikov 1993, 18–19). Dirac mentioned to Tamm Hermann Weyl's *Gruppentheorie und Quantenmechanik* (1928), which

With good connections to physicists in the Soviet Union, Dirac was also actively involved in bringing out a Russian translation of his book. In the summer of 1930, while attending a conference in Kharkov, he brought the corrected proof sheets with him and handed them over to the Russian theorist Dmitri Ivanenko, whom he had met two years earlier at another conference in Russia (Kojevnikov 1993, 36; Gorelik and V. Y. Frenkel 1990, 156). Edited by Ivanenko and translated by the young Leningrad physicist Matvei Bronstein, the translation appeared in 1932 as *Printsipy Kvantovoi Mekhaniki*. Although the Russian edition was very successful—it sold three thousand copies in a few months—from an ideological point of view, it was seen as somewhat problematical by Soviet commissars. This is reflected in a preface that the publishing house GTTI added to Dirac's own preface, in which it was said that "The publishing house is fully aware that this work contains many views and statements completely at variance with dialectical materialism." Yet it was argued that the material of the book, "critically mastered, can be used at the front in the struggle for dialectical materialism."[8] At the time Dirac's view of quantum mechanics was close to what would later be called the Copenhagen interpretation, and it may have been this view that the publishing house felt its duty to warn against.

Abstract and mostly concerned with foundational matters, *Principles* had little to say about the many applications of quantum mechanics. To make up for this deficiency, Dirac added, on the request of Ivanenko, an extra chapter in which he covered various approximation methods, such as those developed by Vladimir Fock and Douglas Hartree (the Hartree-Fock approximation). Another way in which the Russian edition differed from the original was that Ivanenko added several appendices and Bronstein a number of footnotes. These additions were made in agreement with Dirac. Moreover, Ivanenko provided a long editorial preface in which he praised the book and compared it to other books on quantum mechanics. His comparison is worth quoting:

> Sommerfeld's supplementary volume *Wellenmechanischer Ergänzungsband* appears as a collection of solutions of a series of particular problems; de Broglie's book *Introduction à l'étude de la mécanique ondulatoire* is only an introduction, devoted mainly to the transition from classical to quantum mechanics; Born and Jordan's *Elementare Quantenmechanik* is an exposition of a deliberately restricted part of the material that is amenable to analysis by a special method (Schrödinger's equation does not appear in the book); finally, Frenkel's *Einführung in die Wellenmechanik* is the most accessible of the books for reading but, like all the others, does not give an exposition of the *system* of quantum mechanics. It is the exposition of the system that Dirac's book gives, truly in the highest form, free from all provincialism, […] In our view, the book that is closest in nature, Weyl's *Gruppentheorie und Quantenmechanik*—highly regarded by Dirac—is significantly inferior to Dirac's book, on account of both

he praised as "very clearly written and […] far the most connected account of quantum mechanics that has yet appeared." But he also pointed out that Weyl's book was "rather mathematical and therefore not very easy."

[8] The Russian translation of the second edition of *Principles* included a similar warning from the publisher, namely that "P. Dirac […] makes some philosophical and methodological generalizations that contradict the only true scientific method of cognition–dialectical materialism." Full translations of the prefaces of the Russian editions appear in (Dalitz 1995, 471–478).

> the superfluous mathematical formalism and the actual style of the exposition.[9] (Dalitz 1995, 473)

The second edition of *Principles* also appeared in a Russian translation, published in 1937 by the same publishing house (GTTI). It was edited by Bronstein and translated by C. Angluski.

In 1930 theoretical physics in Japan was beginning to develop under the leadership of Yoshio Nishina, who had spent most of the 1920s in Europe on an extended stay. Nishina had visited Dirac in 1928 and was his host when he, together with Heisenberg, visited Japan in the summer of 1929. It was also Nishina who translated the lectures of Heisenberg and Dirac and had them published (as *Ryôshiron sho mondai*) in 1931. After the publication of *Principles* Nishina thought of producing a Japanese translation, for which he had secured the rights from Dirac and the Oxford University Press. However, for some years nothing happened, and on Dirac's advice it was agreed to make a translation of the forthcoming second edition instead of the 1930 edition. According to the recollection of Hidehiko Tamaki, "the author himself had told him [Nishina] that the time was right to bring out a Japanese version, and that the second edition was written in a style far easier to comprehend than the first one" (Tamaki 1995, 130; Brown 2006, 385). The Japanese translation was published in 1936, translated by a team of physicists consisting of Nishina, Tamaki, Minoru Kobayashi, and Sin-Itiro Tomonaga. Although no translation of the first edition appeared, much of its content was included in a 1932 book with translations of the lectures that Dirac and Heisenberg had given during their stay in 1929 (Dalitz 1995, 657–658).

10.4 Reviews of Principles

The Principles of Quantum Mechanics was widely reviewed in the physics journals, in almost all cases positively and in some enthusiastically. It was a common feature of the reviews to praise the book for its directness, generality, and completeness. Some found it to be elegant. In a review of the German translation, the young Swiss physicist (and later Nobel laureate) Felix Bloch emphasized the originality and closed nature of *Principles*, only regretting that Dirac did not refer to enough of the original literature (Bloch 1931, 456).[10] It is hardly surprising that Bronstein, the translator of the Russian edition, praised Dirac's book for being "the best exposition of quantum mechanics to have appeared so far." The reader, he said, "will never stub his toes against sham academism and silly pedantry." This quality of plain and direct presentation he contrasted with Weyl's *Gruppentheorie und Quantenmechanik*, which to his mind was unnecessarily mathematical and "marred by pedantry" (Bronstein 1931, 355–358), as quoted in Gorelik and Frenkel (1990, 45–46). On this question Bronstein and Ivanenko were of one mind.

When Pauli, known for his sharp tongue and equally sharp pen, reviewed *Principles* in *Die Naturwissenschaften*, he was unusually positive. He expressed admiration for the book as a whole, which he described as "an indispensable standard work." He included in his

[9] Yakov Frenkel's book appeared in 1932 in an English edition as *Wave Mechanics: Elementary Theory* (1932). Ivanenko also mentioned "a short introduction by Haas" that existed in a Russian translation, a reference to the Austrian physicist Arthur E. Haas's *Materiewellen und Quantenmechanik* (1928).

[10] The element of closed nature or self-consistency seems also to have impressed Paul Ehrenfest, who allegedly found it to be "ein greuliches Buch" that was difficult to understand. "A terrible book—you can't tear it apart," he supposedly said according to the recollections of Adriaan Fokker (Kragh 1990, 79).

praise the abstract and symbolic method on which Dirac based his exposition of quantum mechanics, a method Pauli found to be "greatly elegant and general." However, he also pointed out that the consistent use of the symbolic method had its disadvantages, since it might lead to "a certain danger that the theory will escape from reality" (Pauli 1931, 188). Pauli complained that Dirac's book did not reveal the crucial fact that quantum mechanical measurements require real and solid measuring devices that follow the laws of classical physics; measurements in the atomic and subatomic realm are not processes that merely involve mathematical symbols and formulae. While the classical nature of the measurement apparatus was an important element in Pauli's and Bohr's conception of quantum mechanics, it was not a point appreciated by Dirac.

Oppenheimer reviewed *Principles* for American physicists in *Physical Review*, calling it "astonishingly complete" and "unitary and coherent." He likened it to another, older classic of physics, Josiah Willard Gibbs's *Elementary Principles of Statistical Mechanics*. Like this work, Dirac's book "is clear, with a clarity dangerous for a beginner, deductive, and in its foundations abstract; its argument is predominantly analytical; the virtual contact with experiment is made quite late in the book." Oppenheimer realized that Dirac's text, in spite of all its qualities, was not ideal for a first course in quantum mechanics. "The book remains a difficult book, and one suited only to those who come to it with some familiarity with the theory. It should not be the sole text, nor the first text, in quantum theory, just as that of Gibbs's should not be the first in statistical mechanics" (Oppenheimer 1931, 97).

Dirac's book was reviewed anonymously in *Nature* alongside two other works on quantum theory, Heisenberg's *Die physikalischen Prinzipien der Quantentheorie* and Léon Brillouin's *La théorie des quanta*. The author of the reviews was almost certainly Arthur Eddington, such as revealed by the style and terminology (Eddington 1931, 699). Inspired by Dirac's relativistic wave equation of 1928, Eddington had recently begun his lonely and ambitious attempt to unify the quantum world with the universe at large, a line of work that in 1946 would lead to his posthumously published *Fundamental Theory*. The review in *Nature* focused on Dirac's more general conception of quantum mechanics as a theory that could not be understood in terms of models or classical concepts. Dirac's "logical and original mode of approach" to the problems of quantum theory greatly appealed to Eddington:

> He bids us throw aside preconceived ideas regarding the nature of phenomena and admit the existence of a substratum of which it is impossible to form a picture. We may describe this as the application of "pure thought" to physics, and it is this which makes Dirac's method more profound than that of other writers. […] He introduces a new attitude of mind towards the investigation of Nature, and the interest lies in watching the development of progress of his ideas. There can be no doubt that his work ranks as one of the high achievements of contemporary physics.

Other reviews of Dirac's book appeared in journals not read by the majority of physicists. Heisenberg reviewed it in a weekly magazine on metallurgy, the *Metallwirtschaft*, pointing out that practical applications were given much less priority than the general principles and only included to further the understanding of the latter. Like Pauli, he had his reservations with regard to the consistent use of what Dirac called the symbolic method. He had the impression, he wrote, "that perhaps Dirac presents quantum mechanics, and especially its physical content, as somewhat more 'symbolic' than is necessary" (Heisenberg 1930, 988).

The physicist and philosopher Philipp Frank, a leading figure in the school of logical positivism, was pleased with what he saw as Dirac's philosophical position, namely that physical theory can only answer questions that relate to the outcome of experiments, whether real or imagined. It has nothing to say about reality as an abstract concept, separated from experiment or observation (Frank 1933, 63). Contrary to other reviewers, the mathematician Bernard Osgood Koopman found that *Principles* was more characterized by Dirac's profound intuition than any logical clarity in presentation. With respect to clarity and mathematical rigour, he preferred the writings of John von Neumann. Also contrary to most other reviewers, Koopman commented critically on the pedagogical quality of Dirac's book: "We feel that the usefulness of the book would have been enhanced by supplying it with an appendix, and by giving more references" (Koopman 1931, 495–496).[11] And he objected to Dirac's use of the hybrid terms "eigenvalue" and "eigenfunction" which should, he thought, preferably have been replaced by the English names "characteristic number" and "characteristic function." At the time the German words "Eigenwert" and "Eigenfunktion" were sometimes transcribed as "proper value" and "proper function," but Dirac decided to stick to the hybrid forms which he had used in his earlier publications.[12]

John Lennard-Jones, who at the time was professor of theoretical chemistry at Cambridge, agreed that *Principles*, for all it qualities, was not a masterpiece of pedagogy. "It would be idle to pretend that the book is easy to read," he said in an understatement. Although he found the book to be much too difficult for the uninitiated, he concluded that "it should be read by everyone who desires to keep in touch with modern physics" (Lennard-Jones 1931, 505–506).

Finally, it is worth mentioning that Einstein was impressed by Dirac's book, which he considered a most logically clear presentation of quantum mechanics. In a volume commemorating the centenary of the birth of Maxwell, Einstein reflected on the view of physical reality as expressed by the standard probabilistic interpretation of quantum mechanics. He said:

> Dirac, to whom, in my opinion, we owe the most logically perfect presentation of this theory, rightly points out that it appears, for example, to be by no means easy to give a theoretical description of a photon that shall contain within it the reasons that determine whether or not the photon will pass a polarizer set obliquely in its path. (Einstein 1931, 73)

This was a direct reference to the introductory chapter of *Principles*, in which Dirac had discussed in detail the polarization of photons. Although Einstein did not agree with Dirac's view of quantum mechanics, he appreciated the clarity and profoundness of his exposition.

---

[11] Koopman, a student of George D. Birkhoff, did work on ergodic theory, dynamical systems, mathematical physics, and later operations research. Yet another review, by the Italian physicist Franco Rasetti, appeared in *Scientia* 51 (1932, 371). See also the reviews quoted below.

[12] The first English textbook in quantum mechanics, George Birtwistle's *The New Quantum Mechanics* (1928) used "eigenfunction" and "eigenwert."

10.5 Structure and Content

Compared with other textbooks in theoretical physics, the format of the first edition of *Principles* was unusual. With no illustrations and no index, it was not a reader-friendly work. Again in contrast to other books on quantum mechanics, it was completely ahistorical and contained almost no references to the research literature. To be precise, altogether it included twelve references in its 264 pages. Dirac admitted in his preface that his chosen way of representation had "necessitated a complete break from the historical line of development," but this he considered to be an advantage rather than a disadvantage. Although a considerable part of *Principles* was based on Dirac's own works and discoveries, there was no indication at all of which parts were his own contributions, nor were there any references to them. While some scientists use the medium of the textbook to communicate and advertise their own work, this was not the case with Dirac. Readers unacquainted with the development of quantum physics would not guess that most of the sections on transformation theory, the δ-function, radiation theory, and relativistic quantum mechanics were, in fact, about and based on the author's own works. This kind of anonymity does not imply that *Principles* was a neutral presentation of an accepted theory. Dirac clearly had an agenda in writing the book, namely to disseminate what he thought were the basic principles and proper methods of quantum mechanics. He wanted to *shape* a theory which had not yet found its final shape.

The book was basically divided in two parts of about equal size. The first part dealt with the principles and general formalism, followed by applications of the theory, including perturbation theory, collision problems, quantum statistics, and radiation theory. It ended with a chapter on the new relativistic theory of the electron. Dirac's original exposition is illustrated by Planck's constant, which in all other textbooks is introduced early on. But in *Principles* it only appeared on p. 95, in connection with the general commutation relations, as "a new universal constant having the dimensions of action." Only some lines later was it revealed that "[i]n order that the theory may agree with experiment," the new constant had to be the same as the one introduced by Planck. Incidentally, this was where Dirac introduced the symbol $\hbar$ ("Dirac's h") as shorthand for $h/2\pi$. Similarly, the Schrödinger wave equation appeared only on p. 104.

In the preface to the edition of 1930, Dirac stressed the abstract and unvisualizable nature of quantum mechanics and how different it was from classical physics. The aim of physics in the classical tradition was "to make assumptions about the mechanism and forces connecting […] observable objects, to account for their behavior in the simplest possible way." But the new developments, not only in quantum mechanics but also in relativity theory, had demonstrated that "nature works on a different plane." Nature's fundamental laws, Dirac said, "do not govern the world as it appears in our mental picture in any very direct way, but instead they control a substratum of which we cannot form a mental picture without introducing irrelevancies." Contrary to other works on quantum theory, which were based on the method of either matrix mechanics or wave mechanics, Dirac chose a more general representation. This representation, which he called the symbolic method, was harder to learn but "seems to go more deeply into the nature of things."

The first chapter, on "The Principle of Superposition," was purely qualitative, involving no mathematics. He carefully discussed the meaning of superposition by illustrating it with the case of polarization of light, emphasizing that "the superposition that occurs in quantum mechanics is of an essentially different nature from that occurring in the classical theory"

(Dirac 1930, I, 11).[13] Another very important term was the concept of "state," which he defined as referring to the condition of a system being independent of time, that is, to a region of four-dimensional space-time and not to three-dimensional space. "A system, when once prepared in a given state, remains in that state as long as it remains undisturbed," he wrote (I, 9). He further stated that if an observation is made on a system in any given state, "the result will not in general be determinate, i.e., if the experiment is repeated several times under identical conditions several different results may be obtained" (I, 10).

In the second edition of 1935, Dirac used the term "state" in a different sense, namely, to denote the condition of a physical system at a given time and not for all time. That is, he used it in a three-dimensional, non-relativistic sense, which might seem to be a retrograde step compared with the definition given in the first edition. However, Dirac motivated the change by arguing that it made the exposition clearer and also that "the fundamental ideas of the present quantum mechanics are in need of serious alterations at just this point" (II, v). He undoubtedly had in mind the problems of formulating a consistent relativistic theory of quantum electrodynamics with which he and other physicists were struggling at the time. Dirac perceived these problems to be so serious that he was willing to sacrifice the relativistic theory and perhaps even such a fundamental principle as the conservation of energy. As he wrote in a paper of early 1936:

> The present quantum mechanics [...] forms a satisfactory theory only when applied non-relativistically [...] and loses most of its generality and beauty when one attempts to make it relativistic. (Dirac 1936, 298; Kragh 1990, 168–173)

The second, somewhat enlarged edition of *Principles* retained the basic structure of the first edition, but was written in a less abstract and symbolic form. "This should make the work suitable for a wider circle of readers," Dirac said, adding that "the reader who likes abstractness for its own sake may prefer the style of the first edition" (II, v). Upon receiving a copy of the new edition, Heisenberg expressed his satisfaction with the work being "more human [*menschlicher*] than earlier,"[14] a response shared by many other physicists. For example, the American physicist Paul Epstein judged that the original version made "difficult reading, overtaxing the powers of abstraction of the less experienced student and making the book unsuitable as a classroom text." He was pleased with the changes made in the new edition, which made "the book clear and simple in all its parts, and there is no longer any reason why it should not prove of excellent service as a text in advanced courses" (Epstein 1935, 640–641).

Yet, not all reviewers were impressed by the pedagogical quality of the new, more *menschlich* edition. Charles Galton Darwin, at the time professor of physics at the University of Edinburgh, did not consider it more suitable as a textbook than the first edition. He complained that it was only helpful to those already familiar with quantum mechanics and that it lacked concrete examples to illustrate the general and formal exposition of the theory of quanta (Darwin 1935, 411–412).[15] Darwin had, since 1926, been a firm supporter of

[13] References to the first edition are denoted by "I", those to the second edition by "II".

[14] Heisenberg to Dirac, 27 March 1935, quoted in (Brown 2006, 388).

[15] Koopman agreed that the second edition was not more suitable as a textbook on quantum theory than the first one: B. O. Koopman, *Bulletin of the American Mathematical Society* 42 (1936, 472–474). Given the completely ahistorical nature of *Principles*, it is remarkable that the second edition received an extensive review in *Isis*, the journal of the History of Science Society. See H. T. Davis, *Isis* 25 (1936, 493–496).

wave mechanics, which he saw as the only picture of the quantum world that yielded a visual representation and therefore understanding. Contrary to Dirac, he thought quantum theory should and could give insight into the reality of the subatomic realm; it was more than just a mathematical formalism to handle experimental data (Navarro 2009).

At the time Dirac completed *Principles*, in early 1930, he was much occupied with the "± difficulty" that arose from his relativistic wave equation, namely, how to interpret the negative-energy solutions in physical terms. In the final chapter of *Principles*, Dirac presented his new theory of the electron much as he had presented it in 1928. In dealing with the states formally referring to negative energies, he proposed that the antielectrons—unoccupied holes in the sea of negative energy states—were protons. The unifying idea of identifying antielectrons with protons, and thus reducing all matter to one elementary particle, appealed greatly to him. But of course he realized that it was hard "to account for the very considerable observed differences between electrons and protons, in particular their different masses." In the very last sentence of the book he stated optimistically: "Possibly the solution of this difficulty will be found in a better understanding of the nature of interaction" (I, 257).

This did not happen. In 1931 Dirac famously predicted the existence of positive electrons, which were subsequently discovered in cosmic rays and known as positrons. Much of the discussion in the second edition was identical with the one in the first, except that "proton" was now replaced by "positron." Concerning the negative-energy solutions, Dirac wrote that they referred to "a new kind of particle having the mass of an electron and opposite charge. Such particles have been observed experimentally and are called *positrons*" (II, 271).

Contrary to the presentation of quantum mechanics by Heisenberg, Born, Jordan and most other authors, in Dirac's presentation the analogy with classical mechanics played an important role. Although quantum mechanics differed radically from the laws and concepts of classical physics, on the formal level there was a great deal of similarity. "Practically all the features of the classical theory to which it owes its attractiveness can be taken over unchanged into the new theory," he wrote (I, 1). Dirac had originally arrived at his formulation of quantum mechanics by noticing a close analogy between the Poisson brackets of classical dynamics and the non-commuting products found by Heisenberg, and he continued to find the analogy significant. As he showed in *Principles*, by means of the Poisson formulation of the classical equations of motion, "one can in this way obtain a quantum theory of individual dynamical systems analogous to the classical theory" (I, 93). The emphasis on the classical analogy was a special feature of Dirac's textbook and reflected his own discovery of quantum mechanics in the years 1925–26.

Among the things *not* included in either of the editions was the complementarity principle, which played such an important role in Heisenberg's contemporary *Physikalische Prinzipien der Quantentheorie*. It was the purpose of Heisenberg's book to disseminate what he called the "Kopenhagener Geist der Quantentheorie," and the complementarity principle was a crucial part of this spirit. Whereas Dirac's purpose was to establish quantum mechanics on a logically satisfying basis suitable for calculations. Questions of interpretation were of secondary importance and mostly appeared implicitly. Although Dirac was, of course, familiar with Bohr's principle of complementarity, it was foreign to his way of thinking. He could express the substance of quantum theory without the airy castle of complementarity and consequently saw no reason to include it in his book. "I don't altogether like it," he said much later about complementarity. "It is rather indefinite [and] doesn't provide you with any equations which you didn't have before."[16] He thought that it might be useful for students preparing for examinations, but not for physicists doing research.

10.6 Dirac's Style of Physics

The symbolic method which was such a characteristic feature of *Principles*, the first edition in particular, was a main reason why many readers found the book difficult to understand. The method was based on "certain symbols which we say denote physical things [… and which] we shall use in algebraic analysis in accordance with certain axioms" (I, 18). Dirac wanted to present the general theory of quantum mechanics in a way that was as free as possible from physical interpretation:

> One does not anywhere specify the exact nature of the symbols employed, nor is such specification at all necessary. They are used all the time in an abstract way, the algebraic axioms that they satisfy and the connexion between equations involving them and physical conditions being all that is required. The axioms, together with their connexions, contain a number of physical laws, which cannot conveniently be analyzed or even stated in any other way. (ibid.)

Dirac's philosophy of physics, in the form that implicitly permeated much of his book, was markedly instrumentalist and abstract. Quantum physics was presented as a formal scheme that allowed the calculation of experimental results, and there was nothing more to it. In his lecture notes from 1927–28, he emphasized that the new theory "deals essentially only with observable quantities, a very satisfactory feature." Moreover: "[t]he theory enables one to calculate only observable quantities […] and any theories which try to give a more detailed description of the phenomena are useless."[17] The same message was spelled out in *Principles*, in both the first and the second edition. For example: "[t]he description which quantum mechanics allows us to give is merely a manner of speaking which is of value in helping us to deduce and to remember the results of experiments and which never leads to wrong conclusions" (I, 5). He added, significantly: "[o]ne should not try to give too much meaning to it." In his review of the book, Lennard-Jones focused critically on Dirac's instrumentalist attitude which he paraphrased as follows:

> A mathematical machine is set up, and without asserting or believing that it is the same as Nature's machine, we put in data at one end and take out the results at the other. As long as these results tally with those of Nature, […] we regard the machine as a satisfying theory. But so soon as a result is discovered not reproduced by the machine, we proceed to modify the machine until it produces the new result as well. (Lennard-Jones 1931, 505–506)

---

[16] AHQP, interview by Thomas S. Kuhn of 14 May 1963. On Dirac and complementarity, see (Heilbron 1985; Kragh 1990, 81–84). *Principles* was not alone in ignoring complementarity: Among 43 textbooks on quantum mechanics published between 1928 and 1937, 40 included a treatment of the uncertainty principle, but only eight of them mentioned the complementarity principle.

[17] Notes for "Lectures on Modern Quantum Mechanics" (AHQP).

This was Dirac's view of quantum theory, but it was not shared by Lennard-Jones who wanted a "rather more ambitious" object for theoretical physics. He deplored that the quantum theorist, at least according to Dirac, "must for ever abandon any hope of providing a satisfying description of the whole course of phenomena." In this respect, Lennard-Jones agreed with Darwin.

Dirac, never much of a philosopher, was in general agreement with the Bohr-Heisenberg view of quantum theory, including the interpretation of the measurement process and the nature of the principle of indeterminacy. Although his book did not refer explicitly to philosophical issues, it did much to disseminate certain views of the Copenhagen school to a generation of young physicists. In accordance with Bohr, in the preface to the first edition, Dirac called attention to "the increasing recognition of the part played by the observer himself in introducing the regularities that appear in his observations." This he considered "very satisfactory from a philosophical point of view" (I, v). Also with regard to determinism and causality, he shared the view of Bohr and his circle of physicists. Quantum mechanics was fundamentally a probabilistic theory, and "the most that can be predicted is the probability of occurrence of each of the possible results" (I, 4). The uncertainty in the initial conditions of a physical system implied indeterminism and a failure of causality, which "from this point of view [is] due to a theoretically necessary clumsiness in the means of observation" (ibid.). But Dirac also pointed out that probability densities and currents, as given by the Schrödinger equation, evolve classically: "[t]he differential equations that express the causality of classical mechanics do not get lost, but are all retained in symbolic form, and indeterminacy appears only in the application of these equations to the results of observation" (II, 4).

Although one can reasonably label Dirac, at the time he wrote *Principles*, a quantum instrumentalist, there are more grounds to doubt that he shared the positivistic view of physics that characterized Heisenberg, Bohr, Jordan, and some other advocates of the Copenhagen school. As Born remarked in 1936, "Whereas he [Dirac] declares himself content with the formulae and uninterested in the question of an objective world, positivism declares the question to be meaningless" (Born 1936, 13). On the other hand, Dirac's disagreements with the Bohr-Heisenberg view were relatively minor. It is true that he came to side with Einstein, at least to some extent, and to criticize the Copenhagen-Göttingen camp, but this was only much later in life.[18]

Regarding Dirac's later advocacy of mathematical beauty as a royal road to progress in fundamental physics, it is noteworthy that, in the early 1930s, he still considered mathematics more from the perspective of an engineer than a mathematician. Although quantum mechanics, as presented in *Principles*, was said to be "essentially mathematical," this referred only to the formalism. "All the same," Dirac wrote, "the mathematics is only a tool and one should learn to hold the physical ideas in one's mind without reference to the mathematical form" (I, vi). Indeed, while many physicists and students found Dirac's book heavily mathematical, mathematicians were unimpressed by the way he used mathematics. "Dirac permits himself a number of mathematical liberties," wrote the mathematician Garrett Birkhoff in a letter.[19] One of those liberties was the δ-function, which Dirac had introduced in 1927, and

[18]Bokulich (2008) argues that Dirac sided with Einstein in the debate over the interpretation of quantum mechanics, and that he did so even in the early phase of the debate. However, her evidence is unconvincing as it is limited to some of Dirac's writings from the 1960s and 1970s.
[19]Birkhoff to Edwin Kemble, 3 March 1933, quoted in (Kragh 1990, 279).

which he dealt with in § 22 of *Principles*. For him it was merely "a convenient notation" that might be used freely for dealing with the representatives of the abstract symbols, as though it were a continuous function, without leading to incorrect results" (I, 64). Mathematicians looked upon the δ-function in a very different way and did not appreciate Dirac's more intuitive use of mathematics, see (Peters 2004).

The dissatisfaction of contemporary mathematicians with Dirac's methods was expressed by John von Neumann, who in his *Mathematische Grundlagen der Quantenmechanik* of 1932 undertook to provide quantum mechanics with a proper mathematical foundation. In the preface of the book, he said about the method of Dirac, as presented in *Principles*, that it "in no way satisfies the requirements of mathematical rigor—not even if these are reduced in a natural and proper fashion to the extent common elsewhere in theoretical physics" (von Neumann 1943, 2). In a comment in the *Mathematical Gazette*, the American physicist Henry Margenau contrasted Dirac's use of mathematics to that of von Neumann:

> While Dirac presents his reasoning with admirable simplicity and allows himself to be guided at every step by physical intuition—refusing at several places to be burdened by the impediment of mathematical rigor—von Neumann goes at his problems equipped with the nicest of modern mathematical tools and analyses it to the satisfaction of those whose demands for logical completeness are most exacting.[20]

Contrary to Weyl's textbook of 1928, which was based on the mathematical theory of groups, group theory was absent from *Principles* both in its first and later editions. Dirac was familiar with the new, mathematically abstract way of representing quantum theory, but he did not find it either more fundamental or very helpful. He preferred to treat group theory as part of quantum mechanics, which for him was the general science of non-commuting quantities (Dirac 1929).

10.7 Concluding Remarks

As I have indicated, *Principles* was a difficult work and not pedagogical in the ordinary sense. Dirac based it to a large extent on his lectures of 1927–29 and, after having completed it, used it for the lectures on quantum mechanics he gave over most of the next four decades. During the 1930s, there was another regular lecture course on quantum physics in Cambridge, given by Alan Wilson in the fall (Michaelmas) term, while Dirac gave his lectures in the spring (Lent) term. Wilson's course was more practically oriented, based on applications of the Schrödinger equation (Wilson 1984).

Although Dirac did not specifically refer to his book as a textbook, in the preface to the first edition he did mention students, and he seems to have regarded it as both a textbook and an exposition of the principles of quantum theory aimed at physicists. I doubt if he gave much thought to the intended readership. Because of Dirac's lectures, which closely followed his book, *Principles* exerted considerable influence on a generation of Cambridge physicists. "His influence was not very great as a teacher," Mott recalled, except that "he always, of course, has given this lecture on his book with admirable character."[21]

[20]Quoted in (Jammer 1966, 367). On Dirac's pragmatic use of mathematics, see (Bueno 2005).
[21]Interview with Mott in 1963 (AHQP). Quoted in (Kragh 1990, 253).

I do not know how much and at which levels *Principles* was used as a textbook outside Cambridge, but my guess is that it was not widely used for lectures or in the classroom. Even if this guess were right, however, it was much used by physicists, both young and more experienced. The number of copies sold speaks for itself. It rarely happens that textbooks are cited in research papers, but *Principles* was an exception to the rule. In the physics papers of the 1930s, there were many references to Dirac's book, which probably exerted a greater influence on research physicists than students.

In the early stages of a new science, discipline, or research field, textbooks play an important role by legitimating the field and formulating the principles on which it builds. Whether explicitly or implicitly, the first generation of textbooks articulate the constitutive features of the new research field, which is particularly important in changes of a more revolutionary nature, such as quantum mechanics. Because the field is not yet fully consolidated, early textbooks may differ considerably in their understanding of the field, both as to content and methodology. It is almost inevitable that what an author presents has the character of a partisan text, at least in the sense that the book reflects the author's view of the new field of science.[22] Dirac's *Principles of Quantum Mechanics* was far from polemical, but it was nonetheless a textbook that conveyed a view of quantum mechanics that may well be called partisan or even personal.

Abbreviations and Archives

AHQP	Archive for History of Quantum Physics. American Philosophical Society, Philadelphia

References

Birtwistle, George (1928). *The New Quantum Mechanics*. Cambridge: Cambridge University Press.

Bloch, Felix (1931). Review of *The Principles of Quantum Mechanics by P. A. M. Dirac*. *Physikalische Zeitschrift* 32: 456.

Bokulich, Alisa (2008). Paul Dirac and the Einstein-Bohr Debate. *Perspectives on Science* 16: 103–114.

Born, Max (1936). Some Philosophical Aspects of Modern Physics. *Proceedings of the Royal Society of Edinburgh* 57: 1–18.

Bronstein, Matvei (1931). The Principles of Quantum Mechanics. *Uspekhi Fizicheskikh Nauk* 11: 355–358.

Brown, Laurie M. (2006). Paul A. M. Dirac's "The Principles of Quantum Mechanics". *Physics in Perspective* 8: 381–407.

Bueno, Otávio (2005). Dirac and the Indispensability of Mathematics. *Studies in History and Philosophy of Modern Physics* 36: 465–490.

Crowther, James G. (1970). *Fifty Years with Science*. London: Barie & Jenkins.

[22] For the case of early textbooks in quantum chemistry, see (Gavroglu and Simões 2000).

— (1974). *The Cavendish Laboratory 1874-1974*. London: MacMillan and Co.

Dalitz, Richard H., ed. (1995). *The Collected Works of P. A. M. Dirac 1924-1948*. Cambridge: Cambridge University Press.

Dalitz, Richard H. and Rudolf Peierls (1986). Paul Adrien Maurice Dirac. *Biographical Memoirs of Fellows of the Royal Society* 32: 139–185.

Darwin, Charles G. (1935). Dirac's "The Principles of Quantum Mechanics". *Nature* 136: 411–412.

Davis, Howard T. (1936). The Principles of Quantum Mechanics by P. A. M. Dirac. *Isis* 25: 493–496.

Dirac, Paul A. M. (1929). Quantum Mechanics of Many-Electron Systems. *Proceedings of the Royal Society A* 123(792): 714–733.

— (1930). *The Principles of Quantum Mechanics*. 1st ed. Oxford: Oxford University Press.

— (1935). *The Principles of Quantum Mechanics*. 2nd ed. Oxford: Oxford University Press.

— (1936). Does Conservation of Energy Hold in Atomic Processes? *Nature* 137: 298–299.

— (1939). A New Notation for Quantum Mechanics. *Proceedings of the Cambridge Philosophical Society* 35: 416–418.

Eddington, Arthur S. (1931). The Principles of Quantum Mechanics. *Nature* 127: 699.

Einstein, Albert (1931). Maxwell's Influence on the Development of the Conception of Physical Reality. In: *James Clerk Maxwell. A Commemoration Volume*. Ed. Joseph J. Thomson. Cambridge: Cambridge University Press. 66–73.

Epstein, Paul S. (1935). Quantum Mechanics. *Science* 81: 640–641.

Farmelo, Graham (2009). *The Strangest Man: The Hidden Life of Paul Dirac, Quantum Genius*. London: Faber and Faber.

Frank, Philip (1933). The Principles of Quantum Mechanics. *Zeitschrift für angewandte Mathematik und Mechanik* 13: 63.

Frenkel, Yakov (1932). *Wave Mechanics: Elementary Theory*. Oxford: Clarendon Press.

Gamow, George (1931). *Constitution of Atomic Nuclei and Radioactivity*. 1st ed. Oxford: Oxford University Press.

Gavroglu, Kostas and Ana Simões (2000). One Face or Many? The Role of Textbooks in Building the New Discipline of Quantum Chemistry. In: *Communicating Chemistry: Textbooks and Their Audiences, 1789-1939*. Ed. Anders Lundgren and Bernadette Bensaude-Vincent. Canton: Science History Publications. 415–449.

Gorelik, Gennady E. and Victor Ya. Frenkel (1990). *Matvei Petrovich Bronstein and Soviet Physics in the Thirties*. Basel: Birkhäuser.

Haas, Arthur E. (1928). *Materiewellen und Quantenmechanik*. Leipzig: Akademische Verlagsgesellschaft.

Harish-Chandra, Mehrotra (1987). My Association with Professor Dirac. In: *Reminiscences about a Great Physicist: Paul Adrien Maurice Dirac*. Ed. Behram N. Kursunoglu and Eugene P. Wigner. Cambridge: Cambridge University Press. 34–36.

Heilbron, John L. (1985). The Earliest Missionaries of the Copenhagen Spirit. *Revue d'Histoire des Sciences* 38: 194–230.

Heisenberg, Werner (1930). The Principles of Quantum Mechanics. *Metallwirtschaft* 9: 988.

Jammer, Max (1966). *The Conceptual Development of Quantum Mechanics*. New York: McGraw-Hill.

Kojevnikov, Alexei B., ed. (1993). *Paul Dirac and Igor Tamm Correspondence. Part 1: 1928-1933*. Munich: Max-Planck-Institut für Physik. Preprint MPI-PH/93-80.

Koopman, Bernard O. (1931). Review of *The Principles of Quantum Mechanics by P. A. M. Dirac*. *Bulletin of the American Mathematical Society* 37: 495–496.

— (1936). Review of *The Principles of Quantum Mechanics by P. A. M. Dirac*. *Bulletin of the American Mathematical Society* 42: 472–474.

Kragh, Helge (1990). *Dirac: A Scientific Biography*. Cambridge: Cambridge University Press.

— (2003). Paul Dirac: Purest Soul in an Atomic Age. In: *From Newton to Hawking: A History of Cambridge University's Lucasian Professorship of Mathematics*. Ed. Kevin C. Knox and Richard Noakes. Cambridge: Cambridge University Press. 387–424.

Kursunoglu, Behram N. and Eugene P. Wigner, eds. (1987). *Reminiscences about a Great Physicist: Paul Adrien Maurice Dirac*. Cambridge: Cambridge University Press.

Lennard-Jones, John E. (1931). Review of *The Principles of Quantum Mechanics by P. A. M. Dirac*. *The Mathematical Gazette* 15: 505–506.

McCrea, William (1986). Cambridge Physics 1925–1929: Diamond Jubilee of Golden Years. *Interdisciplinary Science Reviews* 11: 269–284.

— (1985). How Quantum Physics Came to Cambridge. *New Scientist* 96: 58–60.

Navarro, Jaume (2009). "A Dedicated Missionary." Charles Galton Darwin and the New Quantum Mechanics in Britain. *Studies in History and Philosophy of Modern Physics* 40: 316–326.

von Neumann, John (1943). *Mathematische Grundlagen der Quantenmechanik*. New York: Dover.

Oppenheimer, Julius R. (1931). The Principles of Quantum Mechanics. *Physical Review* 37: 97.

Pauli, Wolfgang (1931). The Principles of Quantum Mechanics. *Die Naturwissenschaften* 19: 188.

Peters, Klaus-Heinrich (2004). *Schönheit, Exaktheit, Wahrheit: Der Zusammenhang von Mathematik und Physik am Beispiel der Geschichte der Distributionen*. Berlin: GNT–Verlag.

Rasetti, Franco (1932). The Principles of Quantum Mechanics. *Scientia* 51: 371.

Tamaki, Hidehiko (1995). Memories of the Theory Group, Nishina Laboratory. In: *Sin-Itiro Tomonaga: Life of a Japanese Physicist*. Ed. Hiroshi Ezawa. Tokyo: MYU K.K.. 127–132.

Tolman, Richard C. (1934). *Relativity, Thermodynamics and Cosmology*. Oxford: Clarendon Press.

Van Vleck, John H. (1932). *The Theory of Electric and Magnetic Susceptibilities*. Oxford: Clarendon Press.

Weiner, Charles (1972). *Exploring the History of Nuclear Physics*. New York: American Institute of Physics.

Weyl, Hermann (1928). *Gruppentheorie und Quantenmechanik*. Leipzig: S. Hirzel.

Williamson, Rajkumari, ed. (1987). *The Making of Physicists*. Bristol: Adam Hilger.

Wilson, Alan H. (1984). Theoretical Physics in Cambridge in the Late 1920s and Early 1930s. In: *Cambridge Physics in the Thirties*. Ed. John Hendry. Bristol: Adam Hilger. 174–175.

Zichichi, Antonino (2000). Dirac, Einstein and Physics. *Physics World* 13(2): 17–18.

Chapter 11
Quantum Mechanics in Context: Pascual Jordan's 1936 *Anschauliche Quantentheorie*

Don Howard

11.1 Introduction

Pascual Jordan's 1936, *Anschauliche Quantentheorie: Eine Einführung in die moderne Auffassung der Quantenerscheinungen* (Jordan 1936a), is an unusual and complicated textbook authored by an unusual and complicated working physicist in an unusual and complicated setting. Jordan was one of the founders of modern quantum mechanics and quantum field theory, but by 1936 he was no longer a major contributor to quantum physics, his attention and effort being diverted, in part, by a growing interest in the relationship between biology and quantum physics. Jordan had been a member of the Nazi party since 1933 and for some years before that had published conservative philosophical and cultural screed under the pseudonym, "Ernst Domeier," in the journal *Deutsches Volkstum* (Beyler 2009), but he remained an ardent supporter of modern theoretical physics, openly promoting the work of Albert Einstein, Niels Bohr, Max Born, and other Jewish physicists, in a political setting often inhospitable to *jüdische Physik* (Beyler 1994; Wise 1994; Hoffmann 2003; Hoffmann and Walker 2007). Moreover, Jordan was still, in 1936, openly allied with the left-leaning Vienna Circle, vigorously promoting his somewhat idiosyncratic version of positivist empiricism, publishing in the Vienna Circle's journal, *Erkenntnis* (Jordan 1934; 1935b), and having his work extensively debated there.[1] On the other hand, he wrote a secret report for the Nazi authorities about the political orientation of participants in the Vienna Circle's Second International Congress for the Unity of Science, which met in Copenhagen in June 1936. This was immediately after he finished, in May 1936, the manuscript of *Anschauliche Quantentheorie*, which, ironically, includes frequent, generous praise for Bohr, one of the hosts of the Copenhagen conference (Hoffmann 1988).

The book, *Anschauliche Quantentheorie*, embodies as many tensions and complexities as does its author. As the title suggests, the book aims to provide an intuitive introduction to the quantum theory, explicitly analogous to the intuitive introduction to geometry then famously on offer in David Hilbert and Stephan Cohn-Vossen's *Anschauliche Geometrie* (Hilbert and Cohn-Vossen 1933). In this aim it succeeds, after a fashion, but with "intuitive" being persuasively defined in consonance with Jordan's positivist empiricism as meaning that the theory is developed and expounded on the basis of definitive empirical evidence and not that it is presented by means of something like intuitive pictures. But the Jordan who was distinguished among his physics peers by his mathematical facility and his training in abstract algebra also provides, in the middle chapter 3, a concise and elegant

[1] See, for example, (Zilsel 1935), which elicited reactions from Hans Reichenbach, Otto Neurath, Moritz Schlick, and Philipp Frank.

exposition of quantum mechanics from a fundamental, mathematical point of view. Jordan suggests that the possibility of a refined, closed, mathematical formulation of the theory is further evidence of its essential correctness. The core physics chapters are wrapped in an introduction and a concluding chapter that portray the modern quantum theory as an inevitable consequence and vindication of Jordan's version of positivist empiricism. As well, quantum theory and positivism, taken together, are represented as an expression and vindication of the central tenets of what will come to be known as the Copenhagen interpretation, by which Jordan means, most importantly, Bohr's correspondence and complementarity principles. And considerable space is devoted to exploring the relationship between quantum mechanics and biology, with Jordan advocating a kind of descriptivist (and thus, positivist) vitalism, sanction for which is also sought in Bohr's suggestion of a complementarity between vitalism and mechanism. Today's reader might be surprised to find the book ending with a few sympathetic words for scientific research on telepathy.

It is difficult to gauge the audience for and impact of Jordan's *Anschauliche Quantentheorie*. There was no English translation, nor was there a second edition. The German edition had a limited circulation in North America after being reprinted in 1946 by J. W. Edwards, of Ann Arbor, under the Alien Property Act. For the German-speaking audience it would have competed mainly with the 1929 *Wellenmechanische Ergänzungsband* of the fifth edition of Arnold Sommerfeld's *Atombau und Spektrallinien* (Sommerfeld 1929), Jordan's own earlier 1930 book, co-authored with Max Born, *Elementare Quantenmechanik* (Born and Jordan 1930), Werner Heisenberg's 1930 introductory book, *Die physikalischen Prinzipien der Quantentheorie* (Heisenberg 1930), Werner Bloch's 1930 German translation of Paul Dirac's *The Principles of Quantum Mechanics* (Dirac 1930a; 1930b), Wolfgang Pauli's 1933 *Handbuch* article, "Die allgemeinen Prinzipien der Wellenmechanik" (Pauli 1933), and perhaps, John von Neumann's 1932 *Mathematische Grundlagen der Quantenmechanik* (von Neumann 1932). It would have enjoyed, along with Heisenberg's book, the advantage of ease of access and a clear, pedagogical style. Over all of these competitors, it would have enjoyed the advantage of offering, at the time, the most comprehensive and comprehensible, elementary introduction to the developing areas of relativistic quantum mechanics, quantum electrodynamics, second quantization, and quantum field theory. More of an audience might also have been won for the book by the more or less simultaneous appearance of Jordan's popular book, *Die Physik des 20. Jahrhunderts* (Jordan 1936b). But some of Jordan's physics colleagues would have been put off by the book's philosophical agenda, and its potential as a university textbook would have been limited by its unabashed presentation of Einstein and relativity theory at a time when Heisenberg, for example, was being branded a "white Jew" in the pages of the SS journal, *Das Schwarze Korps*, for his continuing to teach relativity in Leipzig, see (Cassidy 1992, 381).

Still, Jordan's *Anschauliche Quantenmechanik* stands out and deserves our attention as one of the most important textbooks of its era precisely because of all the tensions and complexities it embodies, it being, in this respect a reflection of its times, and also because its author was, along with Sommerfeld, Born, Heisenberg, Pauli, and Dirac, one of the most important shapers of modern quantum mechanics.

11.2 Pascual Jordan in 1936

That Jordan never won a Nobel Prize in physics is a puzzle. Some blame his inability to give elegant lectures because of a stutter; some blame his pro-Nazi politics or his support, after World War II, for a German nuclear weapons program; some blame the fact that Born misplaced Jordan's 1925 manuscript in which Fermi-Dirac statistics were first presented, thus depriving the modest Jordan of his rightful claim to priority over Pauli (Schroer 2007). But the fact remains that his contributions to the development of modern quantum theory were as fundamental and far-reaching as those of many whose achievements were recognized with a Nobel Prize. It was Jordan, more than anyone else, who developed a mathematically elegant formulation of matrix mechanics (Born and Jordan 1925; 1926). It was Jordan who went on to consolidate matrix mechanics with Dirac's alternative operator calculus (Dirac 1925) and Erwin Schrödinger's wave-mechanical formulation (Schrödinger 1926a; 1926b) in the comprehensive formalism known as statistical transformation theory (Jordan 1927a; 1927b, see also Duncan and Janssen 2009). It was Jordan who did more than anyone other than Dirac to inaugurate the program of quantum field theory, in ways such as developing the second quantization approach and being the first to discover the problem of divergences in quantum field theory (Jordan and O. Klein 1927; Jordan and Wigner 1928). And it was Jordan who, along with von Neumann and Eugene Wigner, was developing more abstract algebraic frameworks for quantum mechanics (Jordan 1933b; Jordan, von Neumann, and Wigner 1934). Not without reason has Jordan been described as "the unsung hero among the creators of quantum mechanics" (Schweber 1994, 5).

But by May of 1936, when Jordan completed work on the manuscript of *Anschauliche Quantentheorie*, his most significant contributions to the quantum theory were in the past. Jordan was then developing an interest in the relationship between physics and biology, an interest that first found expression in his 1932 paper, "Die Quantenmechanik und die Grundprobleme der Biologie und Psychologie" (Jordan 1932, see also Jordan 1934), and would continue to the end of his life. An interest in the relationship between biology and physics was shared with a number of other physicists who had worked on quantum theory, such as Schrödinger, whose *What Is Life?* appeared in 1944 (Schrödinger 1944), and Max Delbrück, who had turned to biophysics at Bohr's urging and, in 1935, had laid the foundations of modern genetics with the famous *Dreimännerarbeit*, "Über die Natur der Genmutation und der Genstruktur."[2] Jordan had not stopped doing physics, however. Later he would turn to work on gravitation, general relativity, and cosmology, see, for example, (Jordan 1955), and he continued to publish on mathematical physics and the logical and conceptual foundations of quantum mechanics.[3]

One year before completing his manuscript Jordan had been promoted to *Ordinarius* at Rostock University, at the comparatively young age of thirty-two. But Rostock, where Jordan had worked since his appointment as *Extraordinarius* in 1929, was not the kind of high-status post one might have expected for a physicist of such early achievement and promise. Jordan's stutter, which made lecturing difficult, was surely part of the reason for this. After 1933, his awkward relationship with the Nazi party—a party member but a dis-

[2]Not (Born, Heisenberg, and Jordan 1926), but (Timofeev-Resovskij, Zimmer, and Delbrück 1935). See also (Beyler 1996) for background on Jordan's interest in biology and quantum physics.
[3]For example, (Jordan 1950; 1952), among many similar works.

senter from the assault on Jewish physics—would have been an added impediment to a call to a more prestigious chair.

Jordan's early training in physics and mathematics was at the Technische Hochschule in his native Hannover. In 1923 he went to Göttingen, where he did a dissertation under Born (Jordan 1924). Between 1924 and 1926, he was an *Assistent* first to the mathematician, Richard Courant, and then to Born. He did his Habilitation in 1926, was for one year a *Privatdozent* in Göttingen, then moved to Hamburg as *Privatdozent* in 1927, before getting the call to Rostock in 1929. During his most productive years in Göttingen, between 1925 and 1927, not only did he collaborate with Born and Heisenberg on the mathematics of matrix mechanics and develop, largely on his own, statistical transformation theory, he also co-authored with James Franck a book on the excitation of quantum transitions by collisions (Franck and Jordan 1926).

It was not just physics, however, that drew Jordan's attention in Göttingen. As mentioned, he was trained in mathematics and was, briefly, an *Assistent* with Courant. The Göttingen mathematics tradition centered around David Hilbert had a major influence on the development of Jordan's thinking. The Hilbert program sought to place mathematics on a logically and conceptually secure foundation by first formulating mathematical theories axiomatically and then demonstrating the correctness, consistency, and completeness of such axiomatic theories by finitistic means. The famous early expression of the program was Hilbert's own axiomatic formulation of geometry (Hilbert 1899), but greater urgency attached to the program's extension to set theory and analysis, where, it was recognized, methodological clarity and rigor were needed to allay the fear that fatal inconsistencies might undermine such achievements as Cantor's elaboration of the transfinite hierarchy. Hilbert's key insight was that the consistency of theories describing infinities might be proven by finite means if one recast the problem syntactically, which is to say as a problem of proving that no contradiction could be derived within an axiomatic formulation of theory. Since a proof is a finite list of finite strings of symbols, reasoning about proofs should, itself, be a finite mental operation, essentially nothing more complicated than counting. Hilbert was famous, also, for promoting the extension of this program to mathematical physics. Not only did he and Courant coauthor one of the era's most influential textbooks on mathematical physics (Courant and Hilbert 1931–1937), but Hilbert also actively recruited the involvement of the Göttingen physics community in the work of his mathematics institute. Göttingen was one of the few places in the world where a mathematically sophisticated young physicist like Jordan could have been produced and could have flourished.[4]

Another major influence on Jordan was the development of logical empiricism and the Vienna Circle.[5] Claiming the heritage of Ernst Mach, the movement known as the Vienna Circle is commonly regarded as having been born in 1922, when Moritz Schlick arrived in Vienna to take up Mach's vacant chair. By the early-to-mid 1930s, the Vienna Circle had become a major intellectual force, with its own journal, *Erkenntnis*, its own book series, and sponsorship of a series of international congresses. It was allied with other influential groups, including the *Berlin Gesellschaft für wissenschaftliche Philosophie*, centered around Hans Reichenbach. The movement aimed to continue the intellectual legacy of the philosopher, historian, and physicist, Ernst Mach, while refining and elaborating Mach's positivist

[4] Corry (2004) is a helpful resource on the Hilbert program more generally but, especially, the unique way in which the Göttingen physics and mathematics communities interacted under Hilbert's patronage.

[5] Stadler (1997) provides the most comprehensive recent history of the Vienna Circle and logical empiricism.

philosophical program with the powerful new tools of symbolic logic in order to craft a new kind of empiricism that would be adequate to the task of legitimating the achievements of modern theoretical physics—especially Einstein's theory of general relativity—in the face of threats posed by traditional philosophical critiques of the revolutionary new physics, foremost among them critiques derived from various versions of neo-Kantianism and traditional metaphysics.[6]

The movement was heavily involved with theoretical physics. Schlick had taken his Ph.D. in physics with Max Planck in Berlin in 1904, and many of his earliest and most influential philosophical works concerned the philosophical implications of relativity theory.[7] Other major figures associated with the Vienna Circle were similarly engaged with physics. Philipp Frank was trained in physics under Ludwig Boltzmann in Vienna. He was Einstein's successor in physics at Prague in 1912 and later published a major study of the quantum theory's implications for causality (Frank 1932). Rudolf Carnap wrote his Jena doctoral dissertation on the problem of space in general relativity (Carnap 1921). Hans Reichenbach's first three books concerned the conceptual foundations and axiomatic formulation of relativity theory (Reichenbach 1920; 1924; 1928). But it was not only the logical empiricists who were so strongly oriented toward theoretical physics at the beginning of the twentieth century. The Marburg neo-Kantian tradition was similarly deeply engaged with physics, a high point being Ernst Cassirer's analysis of relativity theory from a neo-Kantian point of view (Cassirer 1921). But it was the Vienna Circle and logical empiricism, more than any other period philosophical movement, that claimed the title of philosophical champion of modern theoretical physics.[8]

That a young physicist like Jordan, with a broad-ranging and restive intellect, should be drawn to logical empiricism is, thus, not very surprising. Many of Jordan's contemporaries evinced similarly strong philosophical interests, though not all of them followed Jordan in his attachment to positivism. Pauli, who was Mach's godson (literally), was perhaps closest to Jordan's philosophical orientation (Enz 2002). Heisenberg claimed that his discovery of matrix mechanics was inspired by an empiricist resolve to disavow the search for picturable models of the internal structure of the atom and focus, instead, on seeking only mathematical relationships among "observables" like the frequency (color) and intensity (brightness) of spectral lines, but he combined with this a deep sympathy for an Aristotelian metaphysics of act and potency (Camilleri 2009). Whatever their differences, many of Jordan's contemporaries agreed that the road to the relativity and quantum revolutions was made easier by an empiricist resolve to let go of the traditional metaphysics of space, time, and causality if that is what the empirical facts implied.

As significant as the impacts of Hilbert and the Vienna Circle on Jordan were, more significant still was the impact of Bohr. Unlike Heisenberg, Jordan was never an intimate member of the Bohr circle in Copenhagen. He was, however, a regular visitor and a regular participant in the annual conferences on quantum physics that were a highpoint of Copenhagen physics life in the early-to-mid 1930s. Jordan also made himself one of the most ardent promoters of what he took to be Bohr's program in quantum physics after 1927. Whether

[6]For more on logical empiricism's roots in the defense of relativity against neo-Kantian critiques, see (Howard 1994).

[7]See, for example, (Schlick 1917).

[8]For more on the curiously intimate relationship between theoretical physics and philosophy of science at the beginning of the twentieth century, see (Howard 2004b).

there ever was a unitary Copenhagen interpretation of quantum mechanics and what that interpretation might have been is a contested question.[9] As we shall see, central to Bohr's program, on Jordan's reading, are the correspondence and complementarity principles. But these principles, or at least Bohr's intended meaning, require interpretation. Is the correspondence principle to be given a strong reading as asserting the existence of a general classical limit to quantum physics or a weak reading as a mere heuristic for finding appropriate quantum analogues to classical systems and behaviors?[10] Is complementarity restricted to conjugate observables within quantum physics, or is a more general epistemological principle intended?[11] Yet another question of special relevance to Jordan's understanding of Bohr's program is whether Bohr's interpretation of quantum mechanics assumes a positivist philosophy of science. That such is the case was once a rather widespread view and is still not an uncommon one today, however thin might be the warrant for such a reading in Bohr's own discussions of complementarity. As we shall see, it might well be that Jordan—a fan of both Vienna and Copenhagen—might bear more than a little responsibility for promoting a positivist version of a Copenhagen interpretation.

A final fact of note about Jordan in 1936 is that, by then, he was no longer hiding his political musings behind a pseudonym. In the series of articles that he had published in the early 1930s in the conservative, nationalist journal, *Deutsches Volkstum* under the name "Ernst Domeier" (Domeier 1930a; 1930b; 1930c; 1930d; 1931a; 1931b; 1932), Jordan complained about Marxism, secularism, and the baleful cultural effects of liberal democracy of the Weimar variety, and he had championed science and technology as engines of positive social transformation.[12] After Hitler's ascent to power in 1933, and after Jordan joined the Nazi party in May of that year, he felt comfortable voicing such views under his own name, starting with an article in the spring of 1933 in the Rostock university newspaper on "Die Wandlung der Universität" (Jordan 1933a), and continuing in 1935 in a booklet, *Physikalisches Denken in der neuen Zeit* (Jordan 1935c), published by the same press that was responsible for *Deutsches Volkstum*, as well as additional articles in that journal (Jordan 1935a; 1935d). But Jordan did not follow the lead of the anti-Semitic proponents of "Deutsche Physik," such as Johannes Stark and Philipp Lenard in attacking modern theoretical physics as "Jüdische Physik."[13] On the contrary, Jordan vigorously disputed even the more commonplace belief in different national styles in science, a mode of understanding that one could find, for example, in Felix Klein's otherwise celebrated history of mathematics (F. Klein 1926–1927). Responding to Klein's doctoral student, Ludwig Bieberbach, founder of the "Deutsche Mathematik" movement in 1936, Jordan wrote: "The differences among German and French mathematics are not any more essential than the differences between German and French machine guns."[14] For a while in the middle- to late-1930s, Jordan seems to have paid a professional price for his defense of the ideal of objective science and a theoretical physics independent of nations and races. As the influence of the "Deutsche

[9]See (Beller 1999; Howard 2004a; 2007; Faye 2008) for recent, contrasting views of the Copenhagen interpretation.
[10]Tanona (2002) and Bokulich's 2009 "Three Puzzles about Bohr's Correspondence Principle," Philsci Archive, are helpful recent discussions.
[11]Here, again, Faye (2008) is a good starting point for further investigation.
[12]Beyler (1994; 2009) and Wise (1994) are among the best sources on Jordan's pseudonymous political writings.
[13]Beyerchen (1977) remains the definitive history of the politicization of German physics during the Hitler era. See also the papers collected in (Renneberg and Walker 1994).
[14]As quoted in (Schroer 2007, 55).

Physik" movement began to wane, however, Jordan's academic standing improved, until, in 1942, he was awarded the Planck Medal by the *Deutsche Physikalische Gesellschaft* and then, in 1944, he was called to Berlin as Max von Laue's successor.

What is most important for our purposes in Jordan's political writings is his arguing that science, properly understood—which is to say, interpreted in line with antimetaphysical positivism and Bohr's principle of complementarity—undermines Marxist materialism and opens the door for both religion and a kind of descriptivist vitalism in biology. It is here, in Jordan's political writings, that the many different strands of his thinking begin to entwine into a somewhat coherent, if highly idiosyncratic world view. It is the politics that provides the glue. Here is where the physics, the biology, and the philosophy combine. While the politics is kept discreetly in the background in Jordan's *Anschauliche Quantentheorie*, understanding the book's many idiosyncrasies is impossible without an appreciation of the political context.

11.3 The Book

Jordan's *Anschauliche Quantentheorie* is a brief, 332 pages, book divided into five chapters

1. "Die Grundexperimente der Quantenphysik" ["The Basic Experiments of Quantum Physics"][15]
2. "Theoretische Analyse der quantenphysikalischen Grundexperimente" ["Theoretical Analysis of the Basic Experiments of Quantum Physics"]
3. "Quanten- und Wellenmechanik" ["Quantum and Wave Mechanics"]
4. "Mehrkörpertheorie und Elementarteilchen." ["Many-body Theory and Elementary Particles"]
5. "Atome und Organismen" ["Atoms and Organisms"]

and introduced by seven pages of introduction, *Vorbemerkungen* ("Preliminary Remarks"). It is not a typical textbook in many respects. It is not, for example, the kind of text from which the novice student will learn how to solve problems. Nor does it aim to provide a comprehensive survey of all major topics. Instead, the book emphasizes conceptual and mathematical fundamentals, though in a manner quite different from von Neumann's *Mathematische Grundlagen der Quantenmechanik* (von Neumann 1932), its very organization being driven by the author's distinctive philosophical agenda. On the other hand, it is, in many ways, an elegant book. In it, Jordan evinces a talent for clear explanation and exposition. It is a book that the sophisticated student can read with profit. For example, I do not recall having seen anywhere else a more lucid introduction to second quantization.

Jordan announces his philosophical agenda in the introduction: "The overall epistemological orientation that finds expression in modern quantum theory—and that, conversely, receives its most significant support from the quantum theory—has been designated by the author in writings on this subject as 'positivistic'" (Jordan 1936a, vii). What does "positivistic" mean?

> What I will defend is the epistemological orientation of Bohr and Heisenberg. For me, the writings of Ernst Mach have formed an indispensable preparation

[15]Unless otherwise indicated all English translations are by the author.

> for understanding these modern quantum physical conceptions, and the kinship of Mach's ideas with them seems to me more essential than the differences. (Jordan 1936a, vii–viii)

That anti-positivists such as Planck, von Laue, and Einstein also dissent from quantum orthodoxy is, for Jordan, further evidence of the deep connections between positivism and quantum theory. Jordan concludes his brief explanation of positivism with an association that will surprise many readers, but that explains the homage intended by the book's title:

> The essential and decisive principle of positivist epistemology—the restriction of admissible propositions to those that can be reduced to experimentally testable propositions—seems to me to be characterizable, furthermore, as a sensible adaptation of the same principle that forms the starting point for Hilbert's foundational investigations in mathematics, and that Hilbert calls the "finite standpoint." (Jordan 1936a, viii)

Positivism famously opposes unscientific metaphysics and so is incompatible with "dogmatic materialism." Jordan warns the reader that some authors confuse the issue by using the term "positivism" in other ways. Philipp Frank, for example, is said to represent a point of view that is a kind of compromise between positivism and materialism, and Bernhard Bavink is even worse, turning "positivism" into a virtual synonym for "materialism." But clarity on this point is essential, because, on Jordan's view, positivism and quantum theory together undermine "dogmatic materialism" and so open the way toward a new descriptivist vitalism in biology. Classical physics supported materialism, but quantum physics, especially as interpreted by means of Bohr's complementarity principle—which not only consummates the development of quantum physics but also "opens a new epoch for our entire natural scientific thought"—drives us toward an "organic view," whose concepts go beyond the physics of the inorganic and whose laws represent something "essentially new" (Jordan 1936a, ix).

If modern quantum physics is a straightforward expression of positivism, then its content must be fixed in virtually every detail by definitive experimental results. The task of chapter 1, "The Basic Experiments of Quantum Physics," is to exhibit this empirical basis. What is sought is an

> *inductive construction*[16] of the theory that makes clear the *necessary* givenness [*zwangsläufige Gegebenheit*] of its fundamental concepts and fundamental assumptions by means of direct experimental results […] in which the character of quantum physics appears […] pure and undisguised. (Jordan 1936a, 1–2)

No surprise, therefore, that the chapter begins with black-body radiation and the Planck formula. But it is a measure of Jordan's sophistication about fundamentals that the very next topic is wave-particle duality. Jordan starts with the experimental evidence for the corpuscular nature of radiation in the Wien limit, as shown in Einstein's 1905 analysis of the photoelectric effect, and follows this with the Bothe-Geiger and Compton-Simon experiments. We then turn immediately, however, to interference effects as a prelude to a discussion of de Broglie waves, and their experimental demonstration by electron diffraction and the Ramsauer effect. Later sections take up stationary states, emission, absorption,

[16] Unless otherwise indicated, all emphasis is in the original.

scattering, and the dynamics of quantization. The language of direct empirical determination is everywhere. The expression for the entropy of radiation in the Wien limit, like the Planck formula itself, is a "directly empirically secured law" (Jordan 1936a, 9). That the light quantum has a momentum of $h\nu/c$ finds a "direct experimental confirmation though investigations into the Compton effect" (Jordan 1936a, 12). Stationary states and the existence of discrete energy levels are first introduced through the Stern-Gerlach and Frank-Hertz experiments. When atomic and molecular spectral data are introduced a few pages later, it is as the "immense empirical material" that "confirms everywhere and without exception the validity of an empirical law," the Ritz combination principle, which plays a central role in Jordan's account, and which is further "proven" by the Stark and Zeeman effects (Jordan 1936a, 25–26). "The *empirical comparison of absorption and emission intensities* for the same spectral line" demonstrates the validity of Einstein's law relating the probability coefficients for absorption and spontaneous emission. Several experiments provide "direct empirical confirmation" for the adiabatic principle (Jordan 1936a, 44).

With the empirical basis thus secured, Jordan turns in chapter 2 to the "Theoretical Analysis of the Basic Experiments of Quantum Physics." The main tool is the correspondence principle, which Jordan describes as "the most important idea in all of quantum theory." Jordan notes that the correspondence principle is not like the energy, entropy, and relativity principles, which are "*laws of nature in completely worked out formulation*." It is, instead, "a *guide* to the *detection* of still *unknown* laws of quantum phenomena," which cannot be given a "mathematically precise expression" (Jordan 1936a, 51). And even now—1936—when we possess a mathematically refined quantum formalism, the correspondence principle is still crucial as a guide in figuring out the "*meaning*" of the formalism. It does this by exhibiting a "comprehensive and close analogy between classical theory and quantum theory" (Jordan 1936a, 52).

How far Jordan thinks he can push arguments based on the correspondence principle is illustrated by his introduction of electron spin. Analysis of the anomalous Zeeman-effect requires the introduction of the Landé factor, g, in the expression for the magnetic energy of the electron. A "comprehensive correspondence-like description" (*zusammenhängende korrespondenzmäßige Beschreibung*) emerges by introducing a second quantum number for the internal angular momentum that takes half-integral values in the doublet case, where $g = 2$. Jordan then writes:

> The introduction of this half-integral spin-moment of an electron [...] can be characterized as a departure from the image of the electron simply as a mass-point; there exists a certain correspondence-like analogy to a body rotating around an internal axis, in which, in addition to the angular momentum of its center-of-mass motion, there is another angular momentum of the proper rotation. But the significance of this analogy should not be over-valued: Basically, we are concerned here with relationships that cannot be understood according to classical analogies. With respect to the doubling of the statistical weights required by the introduction of the spin-moment, it would be more prudent to speak of a non-classical "two-valuedness" (Pauli). (Jordan 1936a, 100)

One might worry that the correspondence principle has been pressed beyond the breaking point if a "correspondence-like analogy" turns out not really to be an analogy at all.

That there are, of course, limits to the usefulness of classical analogies in quantum physics is an essential part of the other major idea that Bohr introduced, complementarity, the idea in which "the conceptual understanding of quantum phenomena achieves completion" (Jordan 1936a, 115). A preliminary exposition of complementarity, is the subject of the concluding section of chapter 2. The complementarity principle is needed, says Jordan, because the mathematical formalism of quantum physics requires the elaboration of an "*intuitive representation* of the essential laws." But since we cannot construct a "model" according to accustomed classical principles, the task is, instead, to produce an "adaptation and customization" of our ideas to the new laws of quantum physics. We should not expect that an "*intuitive understanding*" will assume an "unalterable maintaining of the customary and the well known" (Jordan 1936a, 114–115).

The exposition that follows is unsurprising in that Jordan presents wave and particle models as complementary descriptions of quantum phenomena, and works out more precisely the examples of position and momentum as well as energy and time as complementary magnitudes. In a standard manner, wholly in the spirit of Bohr, Jordan points to the physical incompatibility of measurement contexts as the basis of complementarity.

Bohr's own discussions of complementarity, starting with his introduction of the idea in the 1927 "Como" paper (Bohr 1928), have occasioned controversy and confusion. In 1927 he spoke of a complementarity between "space-time co-ordination" and "the claims of causality" (Bohr 1928, 54). The attentive reader of that first paper will notice that Bohr, himself, then explains that the relationship between position and momentum exhibited in the Heisenberg uncertainty principle is a "simple symbolical expression for the complementary nature of space-time description and the claims of causality" (Bohr 1928, 60). But the myth persists that it was only the challenge of the Einstein-Podolsky-Rosen paper in 1935 (Einstein, Podolsky, and Rosen 1935) that led Bohr to reformulate complementarity as a relationship between arbitrary conjugate observables. It is instructive, therefore, to find Jordan explaining that complementarity was "crystalized" already in the statistical transformation theory that he and Dirac developed in 1927, expressed in the now familiar form of a relationship between observables represented by non-commuting operators, be those position and momentum or components of spin along orthogonal axes. Indeed, when Jordan returns to this issue in chapter 3, he writes that "*noncommutativity*" acquires an "intuitive meaning" in that it "directly expresses" what is contained in the complementarity principle, namely, the existence of physical magnitudes that can be measured "individually but not simultaneously." Then, in a footnote, he makes the following startling claim: "In the historical sequence of conceptual developments, the idea of complementarity that was expounded in earlier parts of this book was, conversely, developed by Heisenberg and Bohr out of the Dirac-Jordan theory that is sketched here" (Jordan 1936a, 171). Take a moment to get over the shock of Jordan's claiming priority for the idea of complementarity, a claim that might well contain a kernel of truth, and realize that, whatever the real history, Jordan's explication of complementarity by means of the apparatus of statistical transformation theory makes clear the fact that complementarity was widely understood, well before 1935, as a generic fact about the relationship between observables represented by non-commuting operators.

Chapter 3, "Quantum and Wave Mechanics," is the technical heart of the book. As one might expect from a mathematician such as Jordan, it is written at a comparatively high level of abstraction, though a few standard examples and applications—the harmonic oscillator, angular momentum, the hydrogen atom—are worked out in detail. Nowhere else in the textbook literature available in the mid-1930s would one have found such a succinct, lucid, indeed eloquent presentation of the fundamental mathematics of quantum physics. Matrix and wave mechanics are developed in detail. Their equivalence is demonstrated in a reasonably intuitive way, without the elaborate algebraic apparatus one might have expected from Jordan.[17] The two are then subsumed under the broader framework of statistical transformation theory, and the power of that formalism is exhibited through its application to the problem of electron spin. The level of abstraction was higher still in von Neumann's *Mathematische Grundlagen der Quantenmechanik* (von Neumann 1932), but in no way could it be taken to provide an "intuitive" introduction to the theory. Dirac's *Principles of Quantum Mechanics* (Dirac 1930a; 1930b) was comparable to Jordan's chapter 3 in its level of abstraction, but everyone complained about its opacity, as Dirac lacked Jordan's pedagogical instincts. Born and Jordan's own 1930 *Elementare Quantenmechanik* is equally elegant, from a mathematical point of view, but it is far more detailed and thorough in presenting the theory's mathematical essentials, taking four hundred and forty-eight pages to cover the terrain that Jordan, by himself, covers in a mere forty-nine pages in chapter 3. Jordan's 1936 textbook stands alone in offering just as much and just as little as the bright student interested in both conceptual and mathematical fundamentals might want.

Chapter 3 presents non-relativistic quantum mechanics in a closed, mathematical form. Chapter 4 turns to the messy business of relativistic quantum mechanics, quantum field theory, and nuclear physics. Jordan had as much or more claim to authority on these topics than any of his contemporaries; still, it was a challenge to write such a chapter in 1936. Dirac had put quantum electrodynamics into reasonably good shape. Jordan himself had further developed the technique of second quantization for matter waves. In 1930, Pauli had introduced the concept of the neutrino to account for the continuous energy spectrum of electrons in beta decay. In 1932 Chadwick had discovered the neutron, and the discovery of the positron in 1933 had finally solved the problem of the negative energy solutions. In 1934 Enrico Fermi had introduced the theory of weak interactions to explain the process of beta decay. And in February of 1936, Bohr had introduced the liquid-drop model of the nucleus. But problems were everywhere, foremost among them the endemic divergences of quantum field theory, a problem for which no one had a solution. In 1935 Hideki Yukawa had postulated the existence of massive bosons to mediate nucleon-nucleon interactions, but a satisfactory theory of the strong nuclear force was decades away.

Jordan does a good job of bringing order to this confused material. His discussions are clear and to the point. Especially nice are the presentations of quantum statistics and second quantization. But Jordan himself emphasizes the incomplete state of things in 1936. He introduces the chapter by declaring, modestly, that its aim is "to make clear how far we have come and what we are still lacking" (Jordan 1936a, 179). The chapter can be read with profit today as a kind of historical snapshot of physics in the making. This is true even with respect to the one bit of self-indulgence that mars an otherwise balanced and dispassionate presentation, namely, the nine pages that Jordan devotes to his own neutrino theory of light, an idea first introduced by de Broglie, according to which the photon might be regarded as a composite particle, made up of a neutrino-antineutrino pair. In fairness, Jordan had made an important contribution by tackling the problem of deriving the correct Bose-Einstein statistics on the basis of this model. Still, while the neutrino theory of light

[17] For example, there is no mention of the Stone-von Neumann theorem.

might rightly have, by now, largely disappeared from view, its presentation in chapter 4 simply adds to the chapter's importance as an historical record of the early years of quantum field theory and particle physics.

A little self-indulgence at the end of chapter 4 is an apt prelude to chapter 5, "Atoms and Organisms," where the unique intellectual personality of Jordan comes into full view. The chapter contains three sections: § 1. "Die positivistische Methode" ["The Positivistic Method"]; § 2. "Kausalität, Statistik und Finalität" ["Causality, Statistics, and Finality"]; and § 3. "Die Aufbau der realen Welt" ["The Construction of the Real World"].

Section one begins with a reiteration of Jordan's positivist view of the quantum theory, asserting again, as at the beginning of the book, that the principles of quantum mechanics, at least "within the limits of the *non-relativistic* theory," are "unavoidable consequences of the empirically given." Non-relativistic quantum mechanics is said to constitute "a consistent, closed, conceptual structure in which no fundamental problem remains unsolved and in whose framework every possible question appears as a clearly defined mathematical problem" (Jordan 1936a, 271). But the new theory stands in such "stark contrast" to classical physics that its proper, "intuitive" understanding requires our overcoming of classical "prejudices" through "a thorough, methodological-epistemological analysis" (Jordan 1936a, 272).

Central to that epistemological analysis are two distinctions famously asserted by Vienna Circle logical empiricists: (a) the distinction between genuine problems and "pseudo-problems" (*Scheinprobleme*) (Carnap 1928); and (b) the distinction between meaningful and meaningless propositions (Carnap 1932). Pseudo-problems are those inaccessible to scientific investigation because of the nature of the scientific method. Mathematics affords many examples of pseudo-problems, such as puzzles about the nature of imaginary numbers and infinitesimals. In physics, says Jordan, we have Bohr to thank, more than anyone else, for "brushing aside countless pseudo-problems" (Jordan 1936a, 273). Jordan's main example concerns quantum jumps. That we can accurately predict statistical averages for emission intensities might lead one to ask what "remarkable and secret mechanism leads to these peculiar results." Bohr has emphasized, however, that this is "*not* to be regarded as something requiring explanation, but as something that is *a primitive given* [*ursprünglich Gegebenes*]." He continues:

> This view does away with the whole tangled mess [*Wust*] of the countless, well-known pseudo-problems that must arise out of any attempt to find an explanation for the laws arrived at via the correspondence principle by means of detailed models for the "course" or "mechanism" of quantum jumps. (Jordan 1936a, 276)

Recognizing pseudo-problems as what they are requires, in turn, one's understanding the distinction between meaningful and meaningless propositions. Meaningful propositions are those that are either true or false, and in such a way that deciding between these alternatives is a "*solvable problem*." Moreover, the only meaningful propositions are those "that refer directly to sense experiences" or can be shown to be "equivalent" to such basic empirical propositions through "definitions and terminological stipulations" (Jordan 1936a, 276–277). Einstein's analysis of distant simultaneity is the famous example from physics, for the assertion that two distant events are simultaneous, without specification of a frame of reference, lacks empirical content.

Jordan's main reason for promoting this positivist perspective on the epistemology of quantum mechanics is revealed in the next section, "Causality, Statistics, and Finality." Quantum mechanics is a non-deterministic theory. In this respect it differs fundamentally from the deterministic classical physics of Newton (and Einstein's relativity theory) that inspired the mechanistic materialism of Laplace. Moreover, as a mathematically closed theory, quantum mechanics can accommodate no "completion" by means of a deterministic model. Jordan cites approvingly von Neumann's proof of the impossibility of a hidden variables interpretation of theory (von Neumann 1932), adding laconically, in a footnote, that "the objections raised against Neumann's proof are unfounded" (Jordan 1936a, 283). But with classical determinism thus overthrown by quantum mechanics, the door is opened to a revival of vitalism and teleology in biology.

For a positivist like Jordan, teleology cannot be a metaphysical thesis. It is, instead, a claim about the appropriate descriptive vocabulary for biology. For the description of biological structures and processes the language of purposiveness is "*indispensable*," which means, says Jordan, "that the teleological point of view is an indispensable element of biological concept formation" (Jordan 1936a, 287). That the teleological mode of description can be made to work scientifically is further demonstrated by the fact that it can be given clear mathematical formulations. Thus, the idea of purposiveness can be expressed mathematically in the form of variational problems. The related and equally indispensable concept of "wholeness" or the "*indivisibility of individual organisms*" can be expressed in the form of integral equations (Jordan 1936a, 290–291).

Jordan goes on to discuss an array of more specific questions concerning the relationship between quantum physics and biology. Mendel's research into combining ratios proves that discreteness plays a fundamental role in biological processes. That random quantum jumps play a role in biology is suggested by the evidence then accumulating for mutations induced by radiation so weak that no more than a single photon could be involved. That randomness at the quantum level can have effects at the biological level is suggested by the possibility of individual, quantum-scale events "directing" mesoscopic and macroscopic biological processes. And if quantum randomness can be "amplified" to the macroscale in this manner, then quantum randomness might be an explanation for our subjective sense of free will. What does this all imply for the fraught question of the reducibility of biology to physics? Jordan has an interesting answer. He suggests that, instead of regarding biology as a complicated, macroscopic limit of microphysics, it might be more appropriate to regard microphysics as the "simplified limit case of the organic, characterized as a minimum of the generation of integral whole," by which he means just that the properties of macroscopic biological structures and processes commonly represent statistical averages that result from the integration of individual atomic processes. Thus biological laws may be seen, by comparison with the laws of the inorganic, as "the more comprehensive and general" (Jordan 1936a, 302).

Jordan's *Anschauliche Quantentheorie* concludes with section three of chapter 5, a highly philosophical discussion of "The Construction of the Real World." The section begins with a long quotation from Planck's widely read 1931 essay, "Positivismus und reale Aussenwelt" (Planck 1931), in which Planck defends an unabashedly metaphysical version of realism and denounces positivism for its denial of the existence of an objective, external reality. Jordan brushes off Planck's argument on the grounds that his notion of the real fails to pass the positivist's test of meaningfulness. But Jordan notes that there is an interesting and empirically meaningful assertion hidden within Planck's realism, namely, the assertion of the "univocal determination" of the physicist's model of the world. This formulation of the realism problem, which, Jordan remarks, had already been proposed by Carnap,[18] makes it into a real problem, not a pseudo-problem, for whether or not there is such univocal determination is a question that physics can answer.

Thus formulated, however, the realism question receives an affirmative answer within the framework of classical physics, but after the development of the quantum theory, it receives a definitive negative answer because of quantum indeterminism. And this is connected, in turn, with Bohr's principle of complementarity and the role of the observer in quantum measurement, for the impossibility of organizing the results of individual acts of observation in "a single, *coherent*, objective model" means that the connections among the various observational results can be "only *statistical*" (Jordan 1936a, 309). But while the classical conception of reality is, thus, repudiated by quantum theory, the new physics is the equal of the old in "clarity and mathematical precision," and far from this representing the end of physics, it means that research enters "newly opened realms of knowledge" (Jordan 1936a, 309).

Letting go of the classical notion of physical reality also has implications for the way we think of the relationship between the physical and the organic. Bohr had famously extended the principle of complementarity to describe the relationship between physics and biology. Beyond a certain limit, only the dead organism can be dissected and its parts studied in isolation and detail. The living organism can be studied only as an organic whole. Bohr suggested a similarly complementary relationship between physiology and psychology, and yet again, within psychology, a kind of complementarity between consciousness and its explanation, for the moment one steps back to think *about* one's conscience experience or thought, that conscious experience or thought becomes something other than what it was when one was not reflecting upon it (Bohr 1933). Jordan now adds examples from the subconscious or the semi-conscious, as with the phenomenon of falling asleep. If, in introspection, one tries to observe oneself at the moment one falls asleep, then one does not fall asleep (Jordan 1936a, 313). Jordan sees in such examples additional evidence for the surmise that the development of quantum mechanics has taught us something new and important about the relationship between subject and object. What is suggested is "*the blurring of the boundary between subject and object in the process of observation*" (Jordan 1936a, 315). If we define the subjective "inner world" as the "private," and the objective "outer world" as the "social," then one comes to suspect that the two are distinguished not in kind but only in degree (Jordan 1936a, 318). And it follows that there probably are, then, intermediate states—Jordan dubs them *Zwischenstufe*—about which one cannot say whether they are part of reality or not. As a possible example of such a "*Zwischenstufe*," Jordan suggests the telepathic communication of thoughts. It is unfortunate, on Jordan's view, that serious scientific investigation of these phenomena has been impeded by the unjustified inclination to think them impossible on a priori grounds.

[18] See (Howard 1996) for a discussion of Carnap on univocal determination.

11.4 Conclusion

With good reason one might say that these last paragraphs of Jordan's *Anschauliche Quantentheorie* represent the reductio ad absurdum of his larger philosophical project. But simply to dismiss the book because it ends in such silliness would be to miss the book's larger significance. For there are two ways in which the book affords, in fact, an interesting perspective on its author and the many contexts in which the book lives.

There is, first, the fact that Jordan and his *Anschauliche Quantentheorie* probably did more than any other person and text to establish the association between Bohr's interpretation of quantum mechanics and positivism. No other thinker was as central as Jordan both to the core community of quantum physicists associated with Copenhagen and to the core community of philosophers of science associated with Vienna. Nowhere else in the literature of the 1930s will one find as extensive and technically adept a presentation of the case for an essential link between quantum mechanics and positivist epistemology. Never mind the fact that Bohr, himself, never endorsed such a linkage. That this was not Bohr's own understanding of the philosophical significance of quantum theory is of minimal relevance to our understanding how it was that the widespread, popular association of quantum mechanics and positivism was established.

There is, second, the fact that the only way to make Jordan's odd mixing of quantum mechanics, positivism, and vitalism at all coherent is to embed the whole in the political context of Germany in the mid-1930s. For it is Jordan's politically driven opposition to materialism that ties all of the pieces together. And therein lies a great irony. For it is Jordan, a member of the Nazi party, who in this way secured the popular association of quantum mechanics with a positivism that otherwise bore almost exclusively a left-liberal, even socialist political stamp.

Abbreviations and Archives

Philsci Archive	University Library System of the University of Pittsburgh, PA, USA, www.philsci-archive.pitt.edu

References

Beller, Mara (1999). *Quantum Dialogue: The Making of a Revolution*. Chicago: The University of Chicago Press.

Beyerchen, Alan (1977). *Scientists under Hitler: Politics and the Physics Community in the Third Reich*. New Haven: Yale University Press.

Beyler, Richard (1994). *From Positivism to Organism: Pascual Jordan's Interpretations of Modern Physics in Cultural Context*. PhD thesis. Harvard University.

— (1996). Targeting the Organism: The Scientific and Cultural Context of Pascual Jordan's Quantum Biology, 1932–1947. *Isis* 87: 248–273.

Beyler, Richard (Nov. 2009). *Science and Late Weimar Conservatism: Jordan alias Domeier and the Journal Deutsches Volkstum (German National Character)*. Lecture. HPS Graduate Program: University of Notre Dame.
Bohr, Niels (1928). The Quantum Postulate and the Recent Development of Atomic Theory. *Nature* 121: 580–590.
— (1933). Light and Life. *Nature* 133: 421–423, 457–459.
Born, Max, Werner Heisenberg, and Pascual Jordan (1926). Zur Quantenmechanik II. *Zeitschrift für Physik* 35: 557–615.
Born, Max and Pascual Jordan (1925). Zur Quantenmechanik. *Zeitschrift für Physik* 34: 858–888.
— (1930). *Elementare Quantenmechanik*. Berlin: Julius Springer.
Camilleri, Kristian (2009). *Heisenberg and the Interpretation of Quantum Mechanics: The Physicist as Philosopher*. Cambridge: Cambridge University Press.
Carnap, Rudolf (1921). *Der Raum. Ein Beitrag zur Wissenschaftslehre*. Göttingen: Dieterichsche Universitäts Buchdruckerei W. Fr. Kästner.
— (1928). *Scheinprobleme der Philosophie*. Berlin: Weltkreis Verlag.
— (1932). Überwindung der Metaphysik durch logische Analyse der Sprache. *Erkenntnis* 2: 219–241.
Cassidy, David (1992). *Uncertainty: The Life and Science of Werner Heisenberg*. New York: W. H. Freeman.
Cassirer, Ernst (1921). *Zur Einsteinschen Relativitätstheorie. Erkenntnistheoretische Betrachtungen*. Berlin: Bruno Cassirer.
Corry, Leo (2004). *David Hilbert and the Axiomatization of Physics (1898–1918): From Grundlagen der Geometrie to Grundlagen der Physik*. Dordrecht: Kluwer.
Courant, Richard and David Hilbert (1931–1937). *Methoden der mathematischen Physik*. 1–2. Berlin: Julius Springer.
Dirac, Paul A. M. (1925). The Fundamental Equations of Quantum Mechanics. *Proceedings of the Royal Society A*: 642–653.
— (1930a). *The Principles of Quantum Mechanics*. Oxford: Clarendon Press.
— (1930b). *Die Prinzipien der Quantenmechanik*. Leipzig: S. Hirzel. Translated by Werner Bloch.
Domeier, Ernst (1930a). Religion, Moralismus und soziale Gesinnung. *Deutsches Volkstum* 12: 905–912.
— (1930b). Überschulung und Bürokratie. *Deutsches Volkstum* 12: 175–179.
— (1930c). Überschulung und Klassenkampf. *Deutsches Volkstum* 12: 271–276.
— (1930d). Wider die Geistreichigkeit der Kunstschriftstellerei. *Deutsches Volkstum* 12: 757.
— (1931a). Gulliver besucht die Marxisten. *Deutsches Volkstum* 13: 107.
— (1931b). Technik, Arbeitslosigkeit und Krieg. *Deutsches Volkstum* 13: 838–846.
— (1932). Surrogatismus. *Deutsches Volkstum* 13: 431.
Duncan, Anthony and Michel Janssen (2009). From Canonical Transformations to Transformation Theory, 1926–1927: The Road to Jordan's *Neue Begründung*. *Studies in History and Philosophy of Modern Physics* 40: 352–362.
Einstein, Albert, Boris Podolsky, and Nathan Rosen (1935). Can Quantum-mechanical Description of Physical Reality Be Considered Complete? *Physical Review* 47: 777–780.

Enz, Charles P. (2002). *No Time to Be Brief. A Scientific Biography of Wolfgang Pauli*. Oxford: Oxford University Press.
Faye, Jan (2008). Copenhagen Interpretation of Quantum Mechanics. In: *The Stanford Encyclopedia of Philosophy*. Ed. Edward N. Zalta. Stanford: Stanford University. http://plato.stanford.edu/archives/fall2008/entries/qm-copenhagen/ (accessed 06/28/2012).
Franck, James and Pascual Jordan (1926). *Anregung von Quantensprüngen durch Stöße*. Berlin: Julius Springer.
Frank, Philipp (1932). *Das Kausalgesetz und seine Grenzen*. Wien: Julius Springer.
Heisenberg, Werner (1930). *Die physikalischen Prinzipien der Quantentheorie*. Leipzig: S. Hirzel.
Hilbert, David (1899). *Grundlagen der Geometrie*. Leipzig: B. G. Teubner.
Hilbert, David and Stephan Cohn-Vossen (1933). *Anschauliche Geometrie*. Berlin: Julius Springer.
Hoffmann, Dieter (1988). Zur Teilnahme deutscher Physiker an den Kopenhagener Physikerkonferenzen nach 1933. *Schriftenreihe für Geschichte der Naturwissenschaften, Technik, und Medizin* 25: 49–55.
— (2003). *Pascual Jordan im Dritten Reich: Schlaglichter*. Berlin: Max-Planck-Institut für Wissenschaftsgeschichte. Preprint 248.
Hoffmann, Dieter and Mark Walker (2007). Der gute Nazi: Pascual Jordan und das Dritte Reich. In: *Pascual Jordan (1902–1980). Mainzer Symposium zum 100. Geburtstag*. Ed. Dieter Hoffmann, Jürgen Ehlers, and Jürgen Renn. Berlin: Max-Planck-Institut für Wissenschaftsgeschichte. 83–114. Preprint 329.
Howard, Don (1994). Einstein, Kant, and the Origins of Logical Empiricism. In: *Language, Logic, and the Structure of Scientific Theories*. Ed. Wesley Salmon and Gereon Wolters. Pittsburgh: University of Pittsburgh Press. 45–105.
— (1996). Relativity, Eindeutigkeit, and Monomorphism: Rudolf Carnap and the Development of the Categoricity Concept in Formal Semantics. In: *Origins of Logical Empiricism*. Ed. Ronald N. Giere and Alan Richardson. Minneapolis: University of Minnesota Press. 115–164.
— (2004a). Who Invented the Copenhagen Interpretation? A Study in Mythology. *Philosophy of Science* 71: 669–682.
— (2004b). Fisica e filosofia della scienza all'alba del XX secolo. In: *Storia della scienza*. Ed. Umberto Bottazzini, John L. Heilbron, Gilberto Corbellini, and Daniel J. Kevles. 8. La seconda revoluzione scientifica. Rome: Istituto della Enciclopedia Italiana. 3–16.
— (2007). Revisiting the Einstein-Bohr Dialogue. *Iyyun: The Jerusalem Philosophical Quarterly* 56: 57–90.
Jordan, Pascual (1924). Zur Theorie der Quantenstrahlung. *Zeitschrift für Physik* 30: 297–319.
— (1927a). Über eine neue Begründung der Quantenmechanik. *Zeitschrift für Physik* 40: 809–838.
— (1927b). Über eine neue Begründung der Quantenmechanik II. *Zeitschrift für Physik* 44: 1–25.
— (1932). Die Quantenmechanik und die Grundprobleme der Biologie und Psychologie. *Die Naturwissenschaften* 20: 815–821.
— (1933a). Die Wandlung der Universität. *Rostocker Universitätszeitung* 9: 3–5.

Jordan, Pascual (1933b). Über Verallgemeinerungsmöglichkeiten des Formalismus der Quantenmechanik. *Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse. Fachgruppe II. Physik, Astronomie, Geophysik, Technik. Nachrichten*: 209–217.

— (1934). Quantenmechanische Bemerkungen zur Biologie und Psychologie. *Erkenntnis* 4: 215–252.

— (1935a). Die biologischen Perspektiven der neuen Physik. *Deutsches Volkstum* 17: 682–689.

— (1935b). Ergänzende Bemerkungen über Biologie und Quantenmechanik. *Erkenntnis* 5: 348–352.

— (1935c). *Physikalisches Denken in der neuen Zeit*. Hamburg: Hanseatische Verlagsanstalt.

— (1935d). Wandlungen des physikalischen Denkens. *Deutsches Volkstum* 17: 362–368.

— (1935e). Zur Neutrinotheorie des Lichtes. *Zeitschrift für Physik* 93: 464–472.

— (1936a). *Anschauliche Quantentheorie: Eine Einführung in die moderne Auffassung der Quantenerscheinungen*. Berlin: Julius Springer.

— (1936b). *Die Physik des 20. Jahrhunderts. Einführung in den Gedankeninhalt der modernen Physik*. Braunschweig: Friedrich Vieweg und Sohn.

— (1950). Zur Axiomatik der Quanten-Algebra. *Akademie der Wissenschaften und der Literatur (Mainz). Mathematisch-naturwissenschaftliche Klasse. Abhandlungen*: 123–131.

— (1952). Zur axiomatischen Begründung der Quantenmechanik. *Zeitschrift für Physik* 133: 21–29.

— (1955). *Schwerkraft und Weltall. Grundlagen der theoretischen Kosmologie*. Braunschweig: Friedrich Vieweg und Sohn.

Jordan, Pascual and Oskar Klein (1927). Zum Mehrkörperproblem der Quantentheorie. *Zeitschrift für Physik* 45: 751–765.

Jordan, Pascual, John von Neumann, and Eugene Wigner (1934). On an Algebraic Generalization of the Quantum Mechanical Formalism. *Annals of Mathematics* 35: 29–64.

Jordan, Pascual and Eugene Wigner (1928). Über das Paulische Äquivalenzverbot. *Zeitschrift für Physik* 47: 631–651.

Klein, Felix (1926–1927). *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*. 1–2. Berlin: Julius Springer.

von Neumann, John (1932). *Mathematische Grundlagen der Quantenmechanik*. Berlin: Julius Springer.

Pauli, Wolfgang (1933). Die allgemeinen Prinzipien der Wellenmechanik. In: *Handbuch der Physik*. Ed. Hans Geiger and Karl Scheel. 2nd ed. 24. 1. Berlin: Julius Springer. 83–269.

Planck, Max (1931). *Positivismus und reale Aussenwelt*. Leipzig: Akademische Verlagsgesellschaft.

Reichenbach, Hans (1920). *Relativitätstheorie und Erkenntnis Apriori*. Berlin: Julius Springer.

— (1924). *Axiomatik der relativistischen Raum-Zeit-Lehre*. Braunschweig: Friedrich Vieweg und Sohn.

— (1928). *Philosophie der Raum-Zeit-Lehre*. Berlin and Leipzig: Walter de Gruyter.

Renneberg, Monika and Mark Walker, eds. (1994). *Science, Technology, and National Socialism*. Cambridge: Cambridge University Press.

Schlick, Moritz (1917). *Raum und Zeit in der gegenwärtigen Physik. Zur Einführung in das Verständnis der allgemeinen Relativitätstheorie*. Berlin: Julius Springer.

Schrödinger, Erwin (1926a). Quantisierung als Eigenwertproblem (Erste Mitteilung). *Annalen der Physik* 79: 361–376.

— (1926b). Quantisierung als Eigenwertproblem (Zweite Mitteilung). *Annalen der Physik* 79: 489–527.

— (1944). *What Is Life? The Physical Aspect of the Living Cell*. Cambridge: Cambridge University Press.

Schroer, Bert (2007). Pascual Jordan, Biographical Notes, His Contributions to Quantum Mechanics and His Role as a Protagonist of Quantum Field Theory. In: *Pascual Jordan (1902–1980). Mainzer Symposium zum 100. Geburtstag*. Ed. Dieter Hoffmann, Jürgen Ehlers, and Jürgen Renn. Berlin: Max-Planck-Institut für Wissenschaftsgeschichte. 47–68. Preprint 329.

Schweber, Silvan S. (1994). *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga*. Princeton: Princeton University Press.

Sommerfeld, Arnold (1929). *Atombau und Spektrallinien. Wellenmechanischer Ergänzungsband*. Braunschweig: Friedrich Vieweg und Sohn.

Stadler, Friedrich (1997). *Studien zum Wiener Kreis: Ursprung, Entwicklung und Wirkung des logischen Empirismus im Kontext*. Frankfurt am Main: Suhrkamp.

Tanona, Scott (2002). *From Correspondence to Complementarity: The Emergence of Bohr's Copenhagen Interpretation of Quantum Mechanics*. PhD thesis. Indiana University.

Timofeev-Resovskij, Nikolay Vladimirovich, Karl G. Zimmer, and Max Delbrück (1935). Über die Natur der Genmutation und der Genstruktur. *Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse. Fachgruppe VI. Biologie. Nachrichten* 1: 190–245.

Wise, Norton (1994). Pascual Jordan: Quantum Mechanics, Psychology, National Socialism. In: *Science, Technology, and National Socialism*. Ed. Monika Renneberg and Mark Walker. Cambridge: Cambridge University Press. 224–254.

Zilsel, Edgar (1935). P. Jordan's Versuch, den Vitalismus quantenmechanisch zu retten. *Erkenntnis* 5: 56–64.

Chapter 12
Epilogue: Textbooks and the Emergence of a Conceptual Trajectory
David Kaiser

Of what use are scientific textbooks? To scientists and their students, textbooks can inspire admiration and nostalgia, but also a sense of limits, of being far from the intellectual frontier. After all, research in the physical sciences long ago ceased to be a bookish affair. For at least a century and a half, the most important developments have been communicated in journal articles and cognate forms such as conference talks and preprints (Frasca-Spada and Jardine 2000; Gross, Harmon, and Reidy 2002). The British scholar and statesman C. P. Snow—who spent much of his career trapped in a superposition, both physicist and novelist—observed in his famous lecture on *The Two Cultures* that “perhaps not many [scientists] would go as far as one hero who, when asked what books he read, replied firmly and confidently: ‘Books? I prefer to use my books as tools.’” Snow continued with a flourish: “It was very hard not to let the mind wander—what sort of tool would a book make? Perhaps a hammer? A primitive digging instrument?” (Snow 1959, 14)

At the same time that Snow offered his observation, Thomas Kuhn elevated scientific textbooks to a central position in his analysis of scientific change. He declared, for example, that “The single most striking feature of this education [in the natural sciences] is that, to an extent totally unknown in other creative fields, it is conducted entirely through textbooks.” (Kuhn 1977, 228). Yet Kuhn was clearly ambivalent. Scientific textbooks “may be the right place for philosophers to discover the logical structure of finished scientific theories,” he explained in a 1961 article, but “they are more likely to mislead than to help the unwary individual who asks about productive methods”—not least, Kuhn insisted, because “science textbooks do not describe the sorts of problems that the professional may be asked to solve.” (Kuhn 1977, 180, 229). In the end, Kuhn concluded, textbooks “are the unique repository of the finished achievements of modern physical scientists” (Kuhn 1977, 186): mausoleums for yesterday’s achievements, where creative ideas went to die.

Snow’s and Kuhn’s dour views did little to inspire close historical scrutiny of the ways in which scientific textbooks have been composed, produced, or utilized. In recent years, however, historians of science have rediscovered the textbook, and with good reason. The general incorporation of perspectives from cultural history has encouraged attention beyond the elites of scientific practice and the seemingly placeless march of ideas. The rise of interest in pedagogy and training on the one hand, and in histories of the book and material reading practices on the other, have rightly refocused interest on textbooks’ production, circulation, and appropriation. Kuhn’s at-once exaggerated and dismissive assessment of the roles of scientific textbooks will no longer suffice.[1]

[1] For recent historiographical reviews, see (Warwick and Kaiser 2005, 393–409; Bensaude-Vincent 2006, 667–670; Olesko 2006, 863–880; Mody and Kaiser 2007, 377–402; Vicedo 2012, 83–87).

If Snow and Kuhn downplayed the usefulness of textbooks to scientists, nowadays few can question these books' value for historians. One usage is to add textbooks to the source-base of materials to be sifted for clues about the chronology of conceptual developments, alongside research articles, unpublished correspondence, notebooks, and oral histories. Chapters in this collection by Clayton Gearhart, Domenico Giulini, Michel Janssen and Charles Midwinter, Helge Kragh, and Don Howard exemplify the richness that scientific textbooks can offer for this kind of study: How did practitioners at the time think about particular ideas, such as Einstein's light-quantum hypothesis or the challenge of quantizing systems with multiple degrees of freedom? How did textbook authors read or cite one development alongside another? As Massimiliano Badino and Jaume Navarro make clear in their introduction, early textbooks on quantum theory are especially valuable for such historical investigations, since the books date from a time of tremendous conceptual uncertainty. Quantum theory as we know it had yet to congeal at the time that many of these books were published. Given the relative length of textbooks as compared to research articles, and the textbook authors' clear intention to impose order on recent, scattershot developments, these textbooks offer detailed documentation of how moments of rapid conceptual change appeared to physicists at the time.

We might liken this historical use of textbooks to physicists' uses of test-bodies when marking out an invisible field. Like pith balls charged with slight static electricity or tiny iron filings sprinkled near a bar magnet, early textbooks on quantum theory might help to delineate a clearer path, enabling historians to chart a conceptual trajectory during times of unusual variation. How did the physics community move from early hints about black-body radiation, specific heats, and the photoelectric effect to a full-blown armory of state vectors, Hilbert spaces, and Hermitian operators? Surely textbooks composed at intermediate steps along the journey are invaluable resources for reconstructing that path.

The pith-ball approach assumes that textbooks reflect underlying conceptual developments, but do not affect them: there existed a genuine conceptual trajectory, and textbooks help to reveal it. Yet many chapters in this collection suggest reasons to reconsider such an assumption. Consider the range of books produced in short order by physicists working at the same university, for example: quite a gulf separates George Birtwistle's *The Quantum Theory of the Atom* (1926) and *The New Quantum Mechanics* (1928) from Paul Dirac's *Principles of Quantum Mechanics* (1930), even though (as we learn from Jaume Navarro's and Helge Kragh's chapters here) all three books emerged from courses taught at Cambridge University, often during the same semester.[2] More generally, if we take seriously the notion that research in quantum theory often unfolded hand-in-hand with teaching for many physicists at the time—as documented so clearly in the chapters by Massimiliano Badino, Michel Janssen and Charles Midwinter, Michel Eckert, Domenico Giulini, Jaume Navarro, and Helge Kragh—then why should we assume that a research-oriented conceptual trajectory existed prior to or independent from all these pedagogical exertions? See also (Warwick 2003; Kaiser 2005; Seth 2010).

In place of the pith-ball analogy—which, after all, hearkens back to the era of classical physics—we might turn to John Wheeler's evocative metaphor for quantum theory, his "Great Smoky Dragon." Wheeler introduced his metaphor to try to capture what it means for quantum particles not to possess sharp trajectories through space and time, such as when

[2]These distinctions are similar to the contrasts drawn by Andrew Warwick in the teaching of special relativity at Cambridge a decade earlier: see (Warwick 2003, chap. 8).

moving through a double-slit apparatus. The tail of the dragon could often be pinned down with accuracy, Wheeler argued; that corresponded to the source that emitted the quantum particles. Likewise the dragon's fiery mouth could usually be found: that was the place, past the screen with two slits, where a detector registered the particle's position. But in between those two spots, nothing definite could be said about the particle's location as it traversed the apparatus—the body of the dragon dissolved into a puffy cloud of smoke.[3]

Much like Wheeler's smoky dragon, what had once been taken to be a relatively clear conceptual trajectory for early quantum theory no longer appears so sharp. Recent scholarship has highlighted the striking heterogeneity—even cacophony—of competing assumptions, approaches, and interpretations during the early years of quantum theory, even among physicists who worked closely together and whose views had earlier been considered synonymous (Beller 1999; Howard 2004, 669–682; Camilleri 2009; Carson 2010). The wide array of textbooks sampled in this volume only reinforces the point. Indeed, we might well wonder whether any coherent conceptual trajectory connected, say, Planck's publications in 1900 with Heisenberg's, Born's, Jordan's, Schrödinger's, or Dirac's papers in the mid-1920s. Did quantum theory itself follow a path as indeterminate as Wheeler's Great Smoky Dragon?

With hindsight, of course, physicists, historians, and philosophers have drawn and redrawn various candidate trajectories for the conceptual history of quantum theory. Indeed for a long time the history of modern physics seemed almost indistinguishable from the history of quantum theory, given the great mass of work published on the topic. Moreover, though significant challenges of interpretation remain open even to this day, the range of approaches and techniques to quantum theory has surely narrowed compared to the turmoil and tumult of the period from 1900 through 1930. Some shadow of a conceptual trajectory appears to have emerged from all the dust and smoke.

We might therefore pose some new questions, inverted from the type that animate pith-ball historiography. Among the wide range of possible (and competing) efforts at the time, through what means did a narrowing of approaches and interpretations occur? What work was required for something approximating a conceptual trajectory to emerge? These last questions suggest yet a third analogy, alongside the pith-ball and smoky dragon: decoherence and the emergence of classical behavior from quantum systems. An influential line of thought among contemporary physicists suggests that classical behavior—such as the possession of a sharp trajectory through space and time—might emerge from quantum objects' interactions with the environment. Repeated scatterings between a quantum particle and the flotsam and jetsam of its surroundings can cause the strange superpositions endemic to quantum theory effectively to get washed out. Even inside the tyrannical box, Schrödinger's cat might well have been all-alive or all-dead the whole time, never caught in a ghostly superposition of both states at once (Zurek 1991, 36–44).

In addition to providing hints of competing approaches or supplying fodder for adjudicating priority claims, scientific textbooks like the ones examined throughout this volume can be used to chart just those interactions with the "environment"—pedagogical, institutional, intellectual—by means of which something approximating a conceptual trajectory emerged. Rather than assume that the textbooks and ancillary pedagogical efforts from the time reflect an underlying trajectory, we might train our attention on the means by which

[3] See, e.g., the interview with John Wheeler in (Davies and Brown 1986, 58–69, on 66–67).

books like these helped to reduce the ever-multiplying possibilities, producing what would later appear to be a recognizable conceptual path. The detailed and revealing chapters in this volume provide an excellent resource with which to pursue just such an investigation. As quantum physicists learned not so long ago, sometimes a little decoherence can be a very useful thing.

References

Beller, Mara (1999). *Quantum Dialogue: The Making of a Revolution*. Chicago: The University of Chicago Press.

Bensaude-Vincent, Bernadette (2006). Textbooks on the Map of Science Studies. *Science and Education* 15: 667–670.

Camilleri, Kristian (2009). *Heisenberg and the Interpretion of Quantum Mechanics: The Physicist as Philosopher*. New York: Cambridge University Press.

Carson, Cathryn (2010). *Heisenberg in the Atomic Age: Science and the Public Sphere*. New York: Cambridge University Press.

Davies, P. Charles W. and R. Julian Brown, eds. (1986). *The Ghost in the Atom*. Cambridge: Cambridge University Press.

Frasca-Spada, Marina and Nick Jardine, eds. (2000). *Books and the Sciences in History*. New York: Cambridge University Press.

Gross, Alan G., Joseph E. Harmon, and Michael Reidy (2002). *Communicating Science: The Scientific Article from the 17th Century to the Present*. New York: Oxford University Press.

Howard, Don (2004). Who Invented the Copenhagen Interpretation? A Study in Mythology. *Philosophy of Science* 71: 669–682.

Kaiser, David (2005). *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics*. Chicago: The University of Chicago Press.

Kuhn, Thomas (1977). The Essential Tension: Tradition and Innovation in Scientific Research. In: *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: The University of Chicago Press. 225–239.

Mody, Cyrus and David Kaiser (2007). Scientific Training and the Creation of Scientific Knowledge: Historical, Sociological, and Anthropological Perspectives. In: *Handbook of Science and Technology Studies*. Cambridge, MA: The MIT Press. 377–402.

Olesko, Kathryn M. (2006). Science Pedagogy as a Category of Historical Analysis: Past, Present, and Future. *Science and Education* 15: 863–880.

Seth, Suman (2010). *Crafting the Quantum: Arnold Sommerfeld and the Practice of Theory, 1890–1926*. Cambridge, MA: The MIT Press.

Snow, Charles P. (1959). *The Two Cultures and the Scientific Revolution*. New York: Cambridge University Press.

Vicedo, Marga (2012). Introduction: The Secret Lives of Textbooks. *Isis* 103: 83–87.

Warwick, Andrew (2003). *Masters of Theory: Cambridge and the Rise of Mathematical Physics*. Chicago: The University of Chicago Press.

Warwick, Andrew and David Kaiser (2005). Kuhn, Foucault, and the Power of Pedagogy. In: *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*. Ed. David Kaiser. Cambridge, MA: The MIT Press. 393–409.

Zurek, Wojciech (Oct. 1991). Decoherence and the Transition from Quantum to Classical. *Physics Today* 44: 36–44.

Index

A

B

C

D

Y

Z