On the verge of *Umdeutung* in Minnesota: Van Vleck and the correspondence principle. Part One. ⋆

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Abstract

In October 1924, *The Physical Review*, a relatively minor journal at the time, published a remarkable two-part paper by John H. Van Vleck, working in virtual isolation at the University of Minnesota. Using Bohr's correspondence principle and Einstein's quantum theory of radiation along with advanced techniques from classical mechanics, Van Vleck showed that quantum formulae for emission, absorption, and dispersion of radiation merge with their classical counterparts in the limit of high quantum numbers. For modern readers Van Vleck's paper is much easier to follow than the famous paper by Kramers and Heisenberg on dispersion theory, which covers similar terrain and is widely credited to have led directly to Heisenberg's *Umdeutung* paper. This makes Van Vleck's paper extremely valuable for the reconstruction of the genesis of matrix mechanics. It also makes it tempting to ask why Van Vleck did not take the next step and develop matrix mechanics himself.

Key words: Dispersion theory, John H. Van Vleck, Correspondence Principle, Bohr-Kramers-Slater (BKS) theory, Virtual oscillators, Matrix mechanics

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1 Introduction

Most historians of modern physics agree that the famous *Umdeutung* [reinterpretation] paper with which Werner Heisenberg (1901–1976) laid the basis for matrix mechanics (Heisenberg, 1925c) grew out of a paper he and Hendrik A. (Hans) Kramers (1894–1952) co-authored on dispersion theory (Kramers and Heisenberg, 1925). Although hardly impartial as one of Kramers' students and his biographer, Max Dresden (1987) calls the Kramers-Heisenberg paper "the direct, immediate, and exclusive precursor to the Heisenberg paper on matrix mechanics" (p. 275). Martin J. Klein (1970) is more restrained but agrees that "this work was the immediate predecessor of Heisenberg's new quantum mechanics" (p. 31). To understand the origin of matrix mechanics, one thus has to come to grips with the contents of the Kramers-Heisenberg paper. According to Jagdish Mehra and Helmut Rechenberg, this paper was written "in such a way that every physicist, theoretician or experimentalist, interested in the subject could understand" (Mehra and Rechenberg, 1982–2001, Vol. 2, pp. 181).[1] An uniniated modern reader turning to the Kramers-Heisenberg paper after these encouraging words is likely to be disappointed. The authors assume their readers to be thoroughly familiar with techniques, borrowed from celestial mechanics, for dealing with multiply-periodic systems, including canonical transformations, action-angle variables, and related perturbation methods. As far as their contemporaries in theoretical physics were concerned, this was undoubtedly a reasonable assumption. So, Mehra and Rechenberg are probably right to the extent that the intended audience would have had no special difficulties with the paper. The same cannot be said for most modern readers, who no longer have the relevant techniques at their fingertips. Fortunately, there is another paper from the same period covering some of the same terrain that is much easier to follow for such readers.

Immediately preceding the translation of (Kramers and Heisenberg, 1925) in the well-known anthology on the development of matrix mechanics edited by Bartel Leendert van der Waerden (1903–1996) (1968) is a paper by the American theoretical physicist John Hasbrouck Van Vleck (1899–1980) (1924b). Like the Kramers-Heisenberg paper, it combines some sophisticated classical mechanics with the correspondence principle of Niels Bohr (1885–1962) and elements of the quantum radiation theory of Albert Einstein (1879–1955). In the last section of this paper, Van Vleck showed that the Kramers dispersion formula, which Kramers (1924a,b) had only presented in two short notes in *Nature* at that point, merges with the classical formula in the limit of high

[1] This multi-volume history of quantum physics brings together a wealth of information and we shall frequently refer to it. However, it needs to be used with some caution (see, e.g., notes 5, 79, and 172 below as well as the review of the first few volumes by John L. Heilbron (1985)).

quantum numbers. Van Vleck's paper is a paragon of clarity. In an interview by Thomas S. Kuhn for the *Archive for History of Quantum Physics* (AHQP) in 1963,[2] Van Vleck acknowledged the influence of his father, the mathematician Edward Burr Van Vleck (1863–1943), in developing his exceptionally lucid writing style:

> My father got after me for my very poor style of scientific exposition. I feel I owe a great deal to him for his splitting up my sentences into shorter sentences, avoiding dangling participles—i.e., tightening up my prose style—the same kind of drill I try to give my own graduate students now.[3]

Van der Waerden only included the quantum part, (Van Vleck, 1924b), of a two-part paper in his anthology. In the second part, Van Vleck (1924c) clearly laid out the results from classical mechanics needed to understand the first part as well as those parts of (Kramers and Heisenberg, 1925) that are most important for understanding Heisenberg's *Umdeutung* paper. This is true even though Van Vleck only covered *coherent* scattering, in which the frequency of the incident and the scattered radiation is the same, whereas a large part of the Kramers-Heisenberg paper is devoted to *incoherent* scattering, first predicted in (Smekal, 1923) and verified experimentally a few years later (Raman, 1928; Landsberg and Mandelstam, 1928). In his interview with Kuhn, Heisenberg emphasized the importance of this part of his paper with Kramers for the *Umdeutung* paper.[4] Of course, this is also the part to which Heisenberg materially contributed.[5] Still, the non-commutative multiplication rule introduced in the *Umdeutung* paper may well have been inspired, as Heisenberg suggests, by manipulations in this part of the Kramers-Heisenberg paper. To understand where the arrays of numbers subject to this rule come from, however, it suffices to understand how coherent scattering is treated in Kramers'

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[2] Between February 1962 and May 1964, about 95 people were interviewed for the AHQP project (Kuhn *et al.*, 1967, p. 3). With one exception (see sec. 2.4) the exact dates of these interviews are unimportant for our purposes and will not be given when we quote from the transcripts.

[3] P. 21 of the transcript of the first of two sessions of the interview, quoted in (Fellows, 1985, p. 57). Van Vleck is talking specifically about the summer of 1925, when he was working on his book-length (Van Vleck, 1926), but his father had probably given him a few pointers before. (Van Vleck, 1924b) definitely belies the author's harsh judgment of his earlier writing style.

[4] P. 18 of the transcript of session 4 of a total of 12 sessions of the AHQP interview with Heisenberg.

[5] According to Dresden (1987, pp. 273–274), Kramers added Heisenberg's name to (Kramers and Heisenberg, 1925) mainly as a courtesy. For Heisenberg's side of the story, see pp. 15–18 of the transcript of session 4 of the AHQP interview with Heisenberg, several passages of which can be found in (Mehra and Rechenberg, 1982–2001, Vol. 2, pp. 178–179), although the authors cite their own conversations with Heisenberg as their source (cf. the foreword to Vol. 2).

dispersion theory: indeed, the only explicit use of dispersion theory in the *Umdeutung* paper are results for coherent scattering.

1.1 On the verge of Umdeutung

As in the case of (Kramers and Heisenberg, 1925), one is struck in hindsight by how close (Van Vleck, 1924b,c) comes to anticipating matrix mechanics. During the AHQP interview, Kuhn reminded Van Vleck of a remark he had made two years earlier to the effect that, if he had been "a little more perceptive," he "might have taken off from that paper to do what Heisenberg did." "That's true," Van Vleck conceded, but added with characteristic modesty: "Perhaps I should say *considerably* more perceptive." [6] In the biographical information he supplied for the AHQP, Van Vleck noted:

> In the two or three years after my doctorate . . . my most significant paper was one on the correspondence principle for absorption . . . It was somewhat related to considerations based on the correspondence principle that led Heisenberg to the discovery of quantum mechanics, but I did not have sufficient insight for this. [7]

This modest assessment is reflected in the discussion of the relation between Van Vleck's work and matrix mechanics by Fred Fellows (1985, pp. 74–81), who wrote a superb dissertation covering the first half of Van Vleck's life and career. In a biographical memoir about his teacher and fellow Nobel laureate, Phil Anderson (1987) [8] is less reserved: "This paper comes tantalizingly close to the kind of considerations that led to Heisenberg's matrix mechanics" (p. 506).

Van Vleck did not pursue his own research any further in 1924 and instead spent months writing—and, as he jokingly put it, being a "galley slave" (Fel-

[6] See p. 24 of the transcript of the first session of the interview. Kuhn's recollection is that Van Vleck's earlier remark was made during a meeting in Philadelphia in March 1961 to plan for the AHQP project (Kuhn *et al.*, 1967, p. viii). Van Vleck was Kuhn's Ph.D. advisor and the two men co-authored (Kuhn and Van Vleck, 1950) (Anderson, 1987, p. 518). It was Van Vleck who approached Kuhn in February 1961 to offer him the directorship of the AHQP project (Kuhn *et al.*, 1967, p. viii) (see also Baltas *et al.*, 2000, pp. 302–303).

[7] Biographical information prepared for the American Institute of Physics project on the history of recent physics in the United States (included in the folder on Van Vleck in the AHQP), p. 1.

[8] Van Vleck, Anderson, and Sir Nevill Mott shared the 1977 Nobel Prize "for their fundamental theoretical investigations of the electronic structure of magnetic and disordered systems." Van Vleck won for work begun in the early 1930s that earned him the title of "father of modern magnetism."

lows, 1985, p. 100) of—a *Bulletin* for the *National Research Council* (NRC) on the old quantum theory (Van Vleck, 1926). With his masterful survey he would surely have rendered a great service to the American physics community had it not been for the quantum revolution of 1925–1926. Like the better-known *Handbuch* article by Wolfgang Pauli (1900–1958) (1926), the *Bulletin* was, as Van Vleck (1971) recognized, "in a sense ... obsolete by the time it was off the press" (p. 6).[9] One is left wondering what would have happened, had the young assistant professor at the University of Minnesota continued to ponder the interaction between radiation and matter and the correspondence principle instead of fulfilling his duties as a newly minted member of the American physics community.

That Kramers and Van Vleck—and, one may add, Max Born (1882–1970) and Pascual Jordan (1902–1980)—came so close to beating Heisenberg to the punch makes the birth of matrix mechanics reminiscent of the birth of special relativity. The comparison seems apt, even though none of these authors anticipated as much of the new theory as H. A. Lorentz (1853–1928) and Henri Poincaré (1854–1912) in the case of relativity.[10] Heisenberg (1971, p. 63) himself actually compared his *Umdeutung* paper to Einstein's relativity paper (Einstein, 1905), arguing that what they had in common was their insistence on allowing only observable quantities into physical theory. The analogy is considerably richer than that.

The breakthroughs of Einstein and Heisenberg consisted, to a large extent, in reinterpreting elements already present in the work of their predecessors, extending the domain of application of these elements, and discarding unnecessary scaffolding. Einstein recognized the importance of Lorentz invariance beyond electromagnetism, reinterpreted it as reflecting a new space-time structure, and discarded the ether (Janssen, 2002). In the case of (Heisenberg, 1925c), the element of *Umdeutung* or reinterpretation is emphasized in the title of the paper. Heisenberg reinterpreted elements of the Fourier expansion of the position of an electron entering into the demonstration that the Kramers dispersion formula merges with the classical result in the correspondence limit, discarded the orbits supposedly given by that position, and recognized that the non-commuting arrays of numbers associated with transitions between different states and representing position in his new scheme were meaningful far beyond the dispersion theory from which they originated.

[9] For the reception of Van Vleck's *Bulletin*, see (Fellows, 1985, pp. 88–89). Van Vleck's *Bulletin* and Pauli's *Handbuch* article were not the only treatises on the old quantum theory that were out of date before the ink was dry. (Born, 1925) and (Birtwistle, 1926), two books on atomic mechanics, suffered the same fate.
[10] In his autobiography, Born (1978, pp. 216–217) exaggerated how close he came to matrix mechanics before Heisenberg.

A further point of analogy is that neither Einstein nor Heisenberg presented the new theory in a particularly elegant mathematical form. In the case of relativity, this had to await the four-dimensional geometry of Hermann Minkowski (1864–1909) and the theory's further elaboration in terms of it by Arnold Sommerfeld (1868–1951), Max Laue (1879–1960), and others (Janssen and Mecklenburg, 2006). Even so, a modern reader will have no trouble recognizing special relativity in Einstein's 1905 paper. The same reader, however, will probably only start recognizing matrix mechanics in two follow-up papers to the *Umdeutung* paper, (Born and Jordan, 1925b) and (Born, Heisenberg, and Jordan, 1925), the famous *Dreimännerarbeit.*[11] Born first recognized that Heisenberg's new non-commuting quantities are matrices. Born and Jordan first introduced the familiar commutation relations for position and momentum. In the *Umdeutung* paper Heisenberg had used the Thomas-Kuhn sum rule, a by-product of the Kramers dispersion formula, as his fundamental quantization condition. As we shall see, Van Vleck had actually been the first to find the sum rule, although he only recognized the importance of the result later.

In the collective memory of the physics community, major discoveries understandably tend to get linked to singular events even though they are almost invariably stretched over time. The "discovery" of the electron by J. J. Thomson (1856–1940) in 1897 or the "discovery" of the quantum of action by Max Planck (1858–1947) in 1900 are well-known examples of this phenomenon. Special relativity is another good example of a "discovery" that came to be associated with a single flash of insight, Einstein's recognition of the relativity of simultaneity, and a single emblematic text, "On the electrodynamics of moving bodies" (Einstein, 1905). Much the same can be said about Heisenberg's famous trip to Helgoland in June 1925 to seek relief from his seasonal allergies and the *Umdeutung* paper resulting from his epiphany on this barren island. The way in which such stories become part of physics lore can be seen as a manifestation of what Robert K. Merton (1968) has dubbed the "Matthew effect," the disproportional accrual of credit to individuals perceived (sometimes retroactively) as leaders in the field.[12] We do, of course, recognize the singular importance of the contributions of Einstein to special relativity and of Heisenberg to matrix mechanics. But there is no need to exaggerate the extent of their achievements. They may have been the first to enter the promised

[11] During a lunch break in his AHQP interview, Alfred Landé (1888–1976) told Heilbron and Kuhn: "Heisenberg stammered something. Born made sense of it" (p. 10a of the transcript of sessions 1–4 of the interview; cf. note 174). Kuhn and Heilbron report that they wrote this down right after the conversation took place and call it a "Quasi-Direct Quote."

[12] The effect is named for the following passage from the Gospel According to St. Matthew: "For unto everyone that hath shall be given, and he shall have in abundance: but from him that hath not shall be taken away even that which he hath."

land, to use another admittedly strained biblical metaphor, but they would never have laid eyes on it without some Moses-figure(s) leading the way.

In his biography of Kramers, Dresden makes a convincing case that his subject deserves more credit for matrix mechanics than he received: "Kramers certainly hoped and probably expected to be the single author of the Kramers-Heisenberg paper. It is probably futile to speculate how the credit for the discovery of matrix mechanics would have been distributed in that case. There would be an indispensable preliminary paper by Kramers alone, followed by a seminal paper by Heisenberg; this might well have altered the balance of recognition" (Dresden, 1987, p. 252). Citing this passage, Dirk ter Haar (1998, p. 23), like Dresden one of Kramers' students, raises the question whether Kramers would have shared Heisenberg's 1932 Nobel Prize in that case. In a review of Dresden's book, however, Nico van Kampen, another one of Kramers' students, takes issue with the pattern of "near misses" that Dresden (1987, pp. 446–461) sees in Kramers' career, the discovery of matrix mechanics being one of them (Dresden, 1987, pp. 285–288). Van Kampen asks: "Is it necessary to explain that, once you have, with a lot of sweat and tears, constructed a dispersion formula on the basis of the correspondence principle, it is not possible to forget that background and that it takes a fresh mind to take the next step?" (Van Kampen, 1988). Similar claims can be made and similar questions can be raised in the case of Van Vleck, even though his work, unlike that of Kramers, did not directly influence Heisenberg.

Van Vleck's contribution has receded even further into the background in the history of quantum mechanics than Kramers'. (Van Vleck, 1924b,c) is not discussed in any of the currently standard secondary sources on quantum dispersion theory and matrix mechanics, such as (Jammer, 1966), (Dresden, 1987), or (Darrigol, 1992). Nor is it mentioned in Vol. 2 of (Mehra and Rechenberg, 1982–2001) on the discovery of matrix mechanics, although it is discussed briefly in Vol. 1 (pp. 646–647) on the old quantum theory.[13] That he worked in faraway Minnesota rather than in Copenhagen or Göttingen, we surmise, is a major factor in this neglect of Van Vleck. Whatever the reason, the neglect is regrettable. For a modern reader, it is much easier to see in (Van Vleck, 1924b,c) than in (Kramers and Heisenberg, 1925) or in (Born, 1924) that matrix mechanics did not come as a bolt out of the blue, but was the natural outgrowth of earlier applications of the correspondence principle to the interaction of radiation and matter.

[13] It is also mentioned in (Van der Waerden and Rechenberg, 1985, pp. 330–331) and in (Hund, 1984, pp. 131–132). As noted in (Mehra and Rechenberg, 1982–2001, Vol. 6, p. 348, note 407), Van Vleck's work is discussed prominently in a paper by Hiroyuki Konno (1993) on Kramers' dispersion theory.

Aitchison *et al.* (2004) have recently given a detailed reconstruction of the notoriously opaque mathematics of (Heisenberg, 1925c). By way of motivating their enterprise, they quote the confession of Steven Weinberg (1992) that he has "never understood Heisenberg's *motivations* for the mathematical steps in his paper" (p. 67; our emphasis). These authors clearly explain the mathematical steps. The motivations for these steps, however, cannot be understood, we submit, without recourse to the dispersion theory leading up to his paper. And if we want to retrace Heisenberg's steps on his sojourn to Helgoland, Van Vleck may well be our best guide.

1.2 Structure of our paper

Like Van Vleck's 1924 paper, our paper comes in two parts, the second providing the technical results needed to understand the first in full detail. To provide some context for Van Vleck's work, undertaken far from the European centers in quantum theory, we begin Part One by addressing the question of America's "coming of age" in theoretical physics in the 1920s (sec. 2). In sec. 3, we relate the story of how matrix mechanics grew out of dispersion theory in the old quantum theory, drawing on the extensive secondary literature on this episode as well as on the materials brought together in the AHQP. This story is usually told from a Eurocentric perspective. Following our discussion in sec. 2, we shall look at it from a more American vantage point. Discussion of the famous BKS theory (Bohr, Kramers, and Slater, 1924a), which is prominently mentioned in many papers on dispersion theory in 1924–1925, is postponed until sec. 4. We shall pay special attention to the role of Van Vleck's fellow graduate student at Harvard, John C. Slater (1900–1976).[14] The reason for keeping the discussion of BKS separate from the discussion of dispersion theory is that we want to argue that the rise and fall of BKS was largely a sideshow distracting from the main plot line, which runs directly from dispersion theory to matrix mechanics. In hindsight, BKS mainly deserves credit for the broad dissemination of its concept of 'virtual oscillators.' Contrary to widespread opinion, both among contemporaries and among later historians, these virtual oscillators did not originate in the BKS theory. They were introduced the year before, under a different name and in the context of dispersion theory, by the Breslau (now Wrocław, Poland) physicists Rudolf Ladenburg (1882–1952) and Fritz Reiche (1883–1969), who called them 'substitute oscillators' [*Ersatzoszillatoren*[15]] (Ladenburg and Reiche, 1923, p. 588, p. 590). This paper is important in its own right and underscores the key achievement of Van Vleck's two-part paper. Both Van Vleck (1924b,c) and Ladenburg and Reiche (1923) discuss the relation between quantum and classical expressions

[14] On Slater, see, e.g., (Schweber, 1990).
[15] We follow the translation used in (Konno, 1993, e.g., p. 139).

for emission, absorption, and dispersion in view of Bohr's correspondence principle. Van Vleck's discussion is impeccable in all three cases; Ladenburg and Reiche made serious errors in the case of both dispersion and absorption. The expertise Van Vleck had gained in classical mechanics through his work on the problem of helium in the old quantum theory (Van Vleck, 1922a,b) put him in an ideal position to correct these errors. We suggest that this is in part what he wanted to do with (Van Vleck, 1924b,c).[16]

In sec. 5, the first section of Part Two, we give an elementary and self-contained presentation, drawing on (Van Vleck, 1924b,c), of the technical results on which our narrative in secs. 3 and 4 rests. In particular, we use canonical perturbation theory in action-angle variables to derive a classical formula for the dispersion of radiation by a charged harmonic oscillator and apply the correspondence principle to that formula to obtain the Kramers dispersion formula for this special case.[17] This fills an important pedagogical gap in the historical literature. Given the central importance of the Kramers dispersion formula for the development of quantum mechanics, it is to be lamented that there is no explicit easy-to-follow derivation of this result in the extensive literature on the subject. In the later parts of sec. 5 and in sec. 6, we take a closer look at Van Vleck's main concerns in his 1924 paper, which was absorption rather than dispersion and the extension of results for the special case of a charged harmonic oscillator (which suffices to understand how matrix mechanics grew out of dispersion theory) to arbitrary non-degenerate multiply-periodic systems. In sec. 7, we present a simple modern derivation of the Kramers dispersion formula and related results, which we hope will throw further light on derivations and results in secs. 5 and 6 as well as on the narrative in secs. 3 and 4. Finally, in sec. 8, we bring together the main conclusions of our investigation.

2 Americans and quantum theory in the early 1920s

"[A]lthough we did not start the orgy of quantum mechanics, our young theorists joined it promptly" (Van Vleck, 1964, p. 24).[18] This is how our main protagonist, known to his colleagues simply as 'Van', described the American participation in the quantum revolution of the mid-1920s for an audience in

[16] (Ladenburg, 1921) and (Ladenburg and Reiche, 1923) are cited in (Van Vleck, 1924b, p. 339).

[17] Van Vleck did it the other way around: he derived the classical formula and showed that it merges with Kramers' quantum formula in the correspondence limit. In sec. 5.2, we shall quote from an exchange between Born and Van Vleck that makes it clear that Van Vleck felt that it did not really matter whether one used the correspondence principle to *construct* quantum formulae or to *check* them.

[18] Quoted and discussed in (Coben, 1971, p. 456)

Cleveland in 1963. Van Vleck spoke as the first recipient of an award named for America's first Nobel Prize winner in physics, Albert A. Michelson (1825–1931). Van Vleck was selling himself and his countrymen short by characterizing the American contribution to the quantum revolution as simply a matter of joining an orgy started by the Europeans and in full swing by the time the Americans arrived on the scene.

Eight years later, Van Vleck, in fact, took exception to what sounds like a similar characterization given by another leading American physicist of his generation, Isidor I. Rabi (1898–1988). Van Vleck quoted a comment that Rabi made in a TV documentary about Enrico Fermi (1901–1954):

> We had produced a large number of people who had been brought up to a certain level, then needed some help, some leadership to get over the hump. Once they were over the hump they were tremendous. People of my generation brought them over the hump, largely from attitudes, tastes, and developments which we had learned in Europe (Van Vleck, 1971, p. 7).

As Kuhn and others have emphasized, Rabi's point was that American physicists returning from Europe rather than European émigrés were mainly responsible for the coming of age of American physics.[19] This issue has been hotly debated in the history of physics literature.[20] Our study of some early American contributions to quantum theory supports the observation by Sam Schweber (1986) that in the 1930s theoretical physics was "already a thriving enterprise in the United States. The refugee scientists resonated with and reinforced American strength and methods: they did not create them" (p. 58).

Commenting on Rabi's remark, Van Vleck (1971) reiterated the point of his Michelson address that "quantum mechanics was a basically European discovery" (p. 6). In (Van Vleck, 1929), he had likewise characterized it as "the result of the reaction of mind on mind among European talent in theoretical physics" (p. 467). In 1971, however, he added that "there has been too much of an impression that American physicists, even in the application of quantum mechanics, were effective only because they had the aid of European physicists, either by going to Europe, or because of their migration to America" (Van Vleck, 1971, p. 6). Van Vleck, who was proud to be a tenth-generation American,[21] received his entire education in the United States. He hardly

[19] See p. 20 of the transcript of the last of five sessions of Kuhn's AHQP interview with George E. Uhlenbeck (1900–1988).

[20] For a concise summary and detailed references to the older literature, see (Moyer, 1985, pp. 171–173). Whereas our focus will be on American contributions to atomic physics, Assmus (1992, 1999) has argued that American theoretical physics came of age in molecular physics (cf. note 45 below).

[21] He could trace his ancestry back to the fifteenth century, to a certain Johan van Vleeck of Maastricht. One of the latter's descendants, Tielman van Vleeck (or von

had any contact with European physicists before 1925, although he did meet a few on a trip to Europe with his parents in the summer of 1923. In Copenhagen, he called on Bohr, who suggested that he get in touch with Kramers,[22] Bohr's right-hand man throughout the period of interest to us. Kramers was not in Denmark at the time but in his native Holland. Decades later, when he received the prestigious Lorentz medal from the *Koninklijke Akademie van Wetenschappen* in Amsterdam, Van Vleck recalled how he had searched for Kramers high and low. After he had finally tracked him down—it can no longer be established whether this was in Bergen aan Zee or in Schoorl—the two men went for a long walk in the dunes along the North-Sea coast: "This was the beginning of a friendship that lasted until his passing in 1952" (Van Vleck, 1974, p. 9). Unfortunately, Van Vleck does not tell us what he and Kramers talked about.

2.1 Education

Van Vleck learned the old quantum theory of Bohr and Sommerfeld at Harvard as one of the first students to take the new course on quantum theory offered by Edwin C. Kemble (1889–1984), the first American physicist to write a predominantly theoretical quantum dissertation. Kemble, Van Vleck (1992) wrote in a biographical note accompanying the published version of his Nobel lecture, "was the one person in America at that time qualified to direct purely theoretical research in quantum atomic physics" (p. 351). Kemble's course roughly followed (Sommerfeld, 1919), the bible of the old quantum theory. Van Vleck supplemented his studies by reading (Bohr, 1918) and (Kramers, 1919) (Fellows, 1985, p. 17).

Van Vleck was part of a remarkable cohort of young American quantum theorists, which also included Slater, Gregory Breit (1899–1981), Harold C. Urey (1893–1981), Robert S. Mulliken (1896–1987), and David M. Dennison (1900–1976). Just as Van Vleck was the first to write a purely theoretical dissertation at Harvard in 1922, Dennison was the first to do so at the University of Michigan in 1924.[23] Dennison could take advantage of the presence of Oskar Klein (1894–1977), an early associate of Bohr,[24] who was a visiting faculty member in the physics department in Michigan from 1923 to 1925 (Sopka, 1988, p. 321). This is where Klein came up with what is now known as the Klein-Gordon equation; it is also where he made his contribution to what is now

Fleck), left Bremen for New Amsterdam in 1658 (Fellows, 1985, pp. 5–6)

[22] See p. 14 of the transcript of session 1 of the AHQP interview with Van Vleck.

[23] See p. 10 of the transcript of the first of three sessions of Kuhn's AHQP interview with Dennison.

[24] See (O. Klein, 1967) for his reminiscences about his early days in Copenhagen.

known as the Kaluza-Klein theory.[25]

Reminiscences about the early days of quantum physics in the United States can be found in (Van Vleck, 1964, 1971), (Slater, 1968, 1973, 1975), and (Rabi, 2006). It is also an important topic of conversation in the AHQP interviews with Van Vleck, Slater, Dennison, and Kemble. These interviews need to be handled with care. In the case of Slater and Van Vleck, one can say, roughly speaking, that the former had a tendency to exaggerate the importance of American contributions, especially his own, while the latter tended to downplay their importance. In sharp contrast, for instance, to the modest remarks by Van Vleck quoted in sec. 1.1, Slater boasted that he "was really working toward quantum mechanics before quantum mechanics came out. I'm sure if it was delayed a year or so more, I would have got it before the others did."[26]

The older generation—men such as Michelson and Robert A. Millikan (1868–1953)—recognized that the United States badly needed to catch up with Europe in quantum physics. The Americans were already doing first-rate experimental work. Theory, however, was seriously lagging behind. As the German-American-Dutch physicist Ralph Kronig (1904–1995) described the situation in an important essay in the Pauli memorial volume:

> While in experimental physics a number of investigators like Michelson, Millikan, Langmuir, Compton and R. W. Wood, ranking among the foremost in the world, continued a tradition of pioneer research that went back to Franklin, Henry, and Rowland, theoretical physics, after the meteoric appearance of Gibbs, could not boast of a similar record ... There was, it is true, a somewhat disperse group of younger men in America, endeavouring to come up to scratch in [atomic physics], of which I should mention Kemble, Van Vleck, Breit, Slater, and Mulliken, but their mutual contacts were limited (Kronig, 1960, p. 17).

Kronig, born in Dresden, came to the United States in 1919 and got his Ph.D. at Columbia University in 1924. After an extended trip to Europe on a Columbia traveling fellowship, he taught in Columbia for two years before returning to Europe for good in 1927 (see the folder on Kronig in the AHQP). Kronig's impression is confirmed by Van Vleck's teacher, Ted Kemble:

> [T]he only theoretical physicists in the country at that time were really men on whom the load of teaching all the mathematical physics courses lay, and they all spent their time teaching. It wasn't, as I remember, a constructive occupation.[27]

[25] See p. 13 of the transcript of session 5 of the AHQP interview with Uhlenbeck.
[26] P. 40 of the transcript of the first session of the AHQP interview with Slater.
[27] P. 4 of the transcript of the last two of three sessions of the AHQP interview with Kemble. See also p. 10 of the transcript of the first of session.

The one theorist who, in Kemble's estimation, was active in research in classical theory, Arthur Gordon Webster (1863–1923), was never able to make the transition to quantum theory. Webster, Kemble said,

> just couldn't keep up with what was going on when the quantum theory began. I always understood that the reason he killed himself was simply because he discovered that suddenly physics had gone off in a new direction and he was unable to follow, and couldn't bear to take a seat in the back and be silent.[28]

When quantum theory arrived on the scene, some experimentalists tried their hands at teaching it themselves (Coben, 1971, p. 444). In this climate, young American physicists with a knack for theory became a hot commodity. They received fellowships to learn the theory at the feet of the masters in Europe and offers of faculty positions straight out of graduate school.[29]

2.2 Postdocs and faculty positions

The careers of the young theorists listed above amply illustrate the new opportunities in the mid-1920s. Slater went to Europe on a Sheldon fellowship from Harvard and spent the first half of 1924 with Bohr and Kramers in Copenhagen. During this period, Urey and Frank C. Hoyt (1898–1977) were in Copenhagen as well, Urey on a small fellowship from the American-Scandinavian Foundation, Hoyt on a more generous NRC fellowship paid for by the Rockefeller foundation.[30] Among the visitors the Americans got to meet in Bohr's institute were Heisenberg and Pauli. Hoyt, a promising student who never reached the level of distinction of the cohort immediately following him,[31] was in Copenhagen for almost two years, from October 1922 to September 1924, Urey for less than one, from September 1923 to June 1924,

[28] P. 12 of the transcript of the first session of the AHQP interview with Kemble.
[29] For further discussion of quantum physics in America before the mid-30s, see (Coben, 1971), (Seidel, 1978), (Kevles, 1978, pp. 168–169), (Weart, 1979), (Schweber, 1986), (Holton, 1988), and, especially, (Sopka, 1988).
[30] See (Robertson, 1979, p. 157), (Sopka, 1988, pp. 71, 97), and Slater to Van Vleck, July 27, 1924 (AHQP)
[31] He wrote several papers on applications of Bohr's correspondence principle (Hoyt, 1923, 1924, 1925a,b). The first two are cited in (Van Vleck, 1924b, p. 334) and all but the second are cited in (Van Vleck, 1926, pp. 124, 146). The second paper is cited in (Ladenburg and Reiche, 1924, p. 672). Hoyt also translated Bohr's Nobel lecture into English (Bohr, 1923a). Hoyt ended up making a career in weapons research rather than in academic physics. After the war, he worked at Argonne National Laboratory, Los Alamos, and Lockheed. He was interviewed for the AHQP by Heilbron but did not remember much of the early days of quantum theory.

and Slater only for a few months, from December 1923 to April 1924. Slater did not have a good experience in Copenhagen. This transpires, for instance, in the letter he wrote to Van Vleck on his way back to the United States. Off the coast of Nantucket, a few hours before his ship—The Cunard R.M.S. "Lancastria"—docked in New York, he wrote:

> Don't remember just how much I told you about my stay in Copenhagen. The paper with Bohr and Kramers [proposing the BKS theory] was got out of the way the first six weeks or so—written entirely by Bohr and Kramers. That was very nearly the only paper that came from the institute at all the time I was there; there seemed to be very little doing. Bohr does very little and is chronically overworked by it . . . Bohr had to go on several vacations in the spring, and came back worse from each one. [32]

In October 1924, Dennison arrived in Copenhagen, on an *International Education Board* (IEB) fellowship, another fellowship paid for by the Rockefeller Foundation. [33] The state of quantum theory in America was already beginning to change at that point. Like Hoyt, Dennison had been awarded a NRC fellowship, but had been told that he could only spend the money at an American institution. [34] In 1923, the NRC had likewise rejected the proposal of Mulliken to go work with Ernest Rutherford (1871–1937) in Cambridge. Mulliken became a NRC research fellow at Harvard instead (Assmus, 1992, p. 23).

Van Vleck and Slater, who both started graduate school at Harvard in 1920 (Van Vleck in February, Slater in September) and lived in the same dormitory, [35] had at one point discussed going to Copenhagen together upon completion of their Ph.D. degrees in 1923. In the end, Van Vleck went to Minneapolis instead. In the biographical note accompanying his Nobel lecture from which we already quoted above, he reflected:

> I was fortunate in being offered an assistant professorship at the University of Minnesota . . . with purely graduate courses to teach. This was an unusual move by that institution, as at that time, posts with this type of teaching were usually reserved for older men, and recent Ph.D.'s were traditionally handicapped by heavy loads of undergraduate teaching which left little time to think about research (Van Vleck, 1992, p. 351).

[32] Slater to Van Vleck, July 27, 1924 (AHQP). The second sentence of this passage is quoted by Dresden (1987, p. 165) in the course of his detailed discussion of Slater's reaction to his experiences in Copenhagen.

[33] Bohr arranged for one of these fellowships to pay for Heisenberg's visit to Copenhagen in the fall of 1924 (Cassidy, 1991, pp. 180, 183). See also the acknowledgment in (Heisenberg, 1925b, p. 860).

[34] See p. 12 of the transcript of session 1 of the AHQP interview with Dennison.

[35] See Van Vleck, *1920–1930. The first ten years of John Slater's scientific career.* Unpublished manuscript, American Institute of Physics (AIP), p. 2.

When the university hired Van Vleck it also hired Breit so that its new recruits would not feel isolated.[36] Breit is one of the more eccentric figures of 20th-century American physics. He was born in Russia and came to the United States in 1915. In a biographical memoir of the *National Academy of Sciences*, we read that

> John Wheeler relates a story told to him by Lubov [Gregory's sister] that she and Gregory were vacationing on the sea when the call to leave Russia came, and they 'came as they were.' For Gregory this meant dressed in a sailor suit with short pants; he was still wearing it when he enrolled in Johns Hopkins (at age sixteen!). Wheeler attributes some of Gregory's subsequent reticence to the ragging he took at the hand of his classmates for his dress (Hull, 1998, pp. 29–30).

True to form, Breit declined to be interviewed for the AHQP. In a memorandum dated April 8, 1964 (included in the folder on Breit in the AHQP), Kuhn describes how they met for lunch, but did not get beyond "casual reminiscences." Kuhn ends on a positively irritated note: "we broke off amicably but with zero achievement to report for the project."

Breit and Van Vleck replaced W. F. G. Swann (1884–1962) who had left Minneapolis for Chicago, taking his star graduate student Ernest O. Lawrence (1901–1958) with him. As Van Vleck (1971) notes wryly: "A common unwitting remark of the lady next to me at a dinner party was "Wasn't it too bad Minnesota lost Swann—it took two men to replace him!"" (p. 6).

Just as Minnesota hired both Breit and Van Vleck in 1923, the University of Michigan hired not one but two students of Paul Ehrenfest (1880–1933) in 1927, Uhlenbeck and Samuel A. Goudsmit (1902–1978) (Coben, 1971, p. 460).[37] In addition Michigan hired Dennison, its own alumnus, upon his return from Copenhagen. Ann Arbor thus became an important center for quantum theory, especially in molecular physics (Assmus, 1992, pp. 4, 26, 30). While Uhlenbeck and Goudsmit essentially remained in Ann Arbor for the rest of their careers, neither Breit nor Van Vleck stayed long in Minneapolis. Breit left for the Carnegie Institution of Washington after only one year, Van Vleck for the University of Wisconsin, his *alma mater*, after five.[38] Van Vleck agonized over the decision to leave Minnesota, where he had been promoted to associate

[36] See p. 14 and p. 18 of the transcript of session 1 of the AHQP interview with Van Vleck.

[37] See also (Sopka, 1988, p. 149) and the AHQP interview with Dennison. The recruiters were Walter F. Colby (1880–1970) and Harrison M. Randall (1870–1969).

[38] The mathematics building in Madison is named after Van Vleck's father, who was a professor of mathematics at the University of Wisconsin from 1906 until his retirement in 1929.

professor in June 1926 and, only a year later, to full professor (Fellows, 1985, Ch. VII). Moreover, on June 10, 1927, he had married Abigail Pearson (1900–1989), whom he had met while she was an undergraduate at the University of Minnesota and who had strong ties to Minneapolis.[39]

To replace Van Vleck, Minnesota made the irresistible offer of a full professorship to the young Edward U. Condon (1902–1974). Minnesota had offered Condon an assistant professorship the year before. At that point, Condon had received six such offers and had decided on Princeton (Condon, 1973, p. 321). His laconic response to this embarrassment of riches: "The market conditions for young theoretical physicists continues [sic] to surprise me" (Coben, 1971, p. 463). Before his first Minnesota winter as a full professor, Condon already regretted leaving New Jersey. He returned to Princeton the following year. Condon, Rabi, and J. Robert Oppenheimer (1904–1967)[40] were the leaders of the cohort of American quantum theorists graduating right after the quantum revolution of 1925. The cohort most relevant to our story graduated right before that watershed.

2.3 The Physical Review

It was during Van Vleck's tenure in Minnesota that his senior colleague John T. (Jack) Tate (1889–1950) took over as editor-in-chief of *The Physical Review* (Sopka, 1988, pp. 142–145, 203, note 11). Tate edited the journal from 1926 to 1950.[41] Van Vleck (1971) described the change of editorship as "another revolution" in the "middle of the quantum revolution" (pp. 7–8). Van Vleck was highly appreciative of Tate's role: "He published my papers very promptly, and also often let me see manuscripts of submitted papers, usually to referee" (ibid.). Thanks in no small measure to Van Vleck and other young whippersnappers in quantum theory, Tate turned what had been a lack-luster publication into the prestigious journal it still is today. Van Vleck recalled the transformation:

> *The Physical Review* was only so-so, especially in theory, and in 1922 I was greatly pleased that my doctor's thesis [Van Vleck, 1922] was accepted for

[39] After her husband's death, Abigail made a generous donation to the University of Minnesota to support the Abigail and John van Vleck Lecture Series. Phil Anderson gave the inaugural lecture in 1983 and the series has brought several Nobel Prize winners to Minneapolis since. The main auditorium in the building currently housing the University of Minnesota physics department is also named after the couple.
[40] Oppenheimer enrolled as an undergraduate at Harvard in 1922, two years after Van Vleck and Slater started graduate school there.
[41] It is largely in recognition of this achievement that the current Minnesota physics building is named after him.

> publication by the *Philosophical Magazine* in England ... By 1930 or so, the relative standings of *The Physical Review* and *Philosophical Magazine* were interchanged ... Prompt publication, beginning in 1929, of "Letters to the Editor" in *The Physical Review* ... obviated the necessity of sending notes to *Nature*, a practice previously followed by our more eager colleagues [see, e.g., (Breit, 1924b), (Slater, 1924, 1925c)] (Van Vleck, 1964, pp. 22, 24).

Van Vleck's impression is corroborated by two foreign-born theorists who made their careers in the United States, Rabi and Uhlenbeck (Coben, 1971, p. 456). Rabi was born in Galicia but moved to New York City as an infant. Rabi liked to tell the story of how, when he returned to Europe to study quantum theory in Germany in 1927, he discovered that *The Physical Review* "was so lowly regarded that the University of Göttingen waited until the end of the year and ordered all twelve monthly issues at once to save postage" (ibid.). On other occasions, Rabi told this story about Hamburg University (Rigden, 1987, p. 4). He told Jeremy Bernstein (2004) that "in Hamburg so little was thought of the journal ... that the librarian uncrated the issues only once a year" (p. 28). The following exchange between Kuhn and Heisenberg, talking about the early twenties, is also revealing:

> Heisenberg: "What was the American paper at that time?"
> Kuhn: "The *Physical Review*?"
> Heisenberg: "No, that didn't exist at that time. I don't think so. Well, in these early times it probably didn't play a very important role." [42]

In a talk about Condon, Rabi elaborated on the mediocrity of *The Physical Review*:

> it was not a very exciting journal even though I published my dissertation in it. And we felt this very keenly. Here was the United States, a vast and rich country but on a rather less than modest level in its contribution to physics, at least per capita. And we resolved that we would change the situation. And I think we did. By 1937 the *Physical Review* was a leading journal in the world (Rabi, 1975, p. 7)

Uhlenbeck remembered how as a student in Leyden he viewed *The Physical Review* as "one of the funny journals just like the Japanese." [43] His initial reaction to the job offer from Michigan suggests that, at least at the time, his disdain for American physics journals extended to the country as a whole: "If it had been Egypt or somewhere like that, I would have gone right away, or China, or even India, I always wanted to go to exotic places [Uhlenbeck was born in Batavia in the Dutch East Indies, now Jakarta, Indonesia]; but America seemed terribly dull and uninteresting"(Coben, 1971, p. 460). In the

[42] P. 5 of the transcript of session 3 of the AHQP interview with Heisenberg.
[43] See p. 20 of the transcript of session 5 of the AHQP interview with Uhlenbeck.

AHQP interview with Uhlenbeck, one finds no such disparaging remarks. In fact, Uhlenbeck talks about how he had reluctantly agreed to return to the Netherlands in 1935 to replace Kramers, who had left Utrecht for Leyden to become Ehrenfest's successor after the latter's suicide.[44] Uhlenbeck was back in Ann Arbor in 1939.

2.4 The lack of recognition of early American contributions to quantum theory

Given the disadvantage they started out with, American theorists in the early 1920s would have done well had they just absorbed the work of their European counterparts and transmitted it to the next generation. They did considerably better than that. Even before the breakthrough of Heisenberg they started making important contributions themselves. According to Alexi Assmus (1992), however, "[a]tomic physics was shark infested waters and was to be avoided; U.S. physicists would flourish and mature in the calmer and safer tidepools of molecular physics" (p. 8; see also Assmus, 1999, p. 187). She sees the early contributions of Van Vleck and Slater to atomic physics, which will be the focus of our study, as exceptions to this rule:

> Van Vleck and Slater viewed themselves as the younger generation, as central figures in the "coming of age" of U.S. physics. They had been given the knowledge that Kemble and his generation could provide and felt themselves capable of pushing into areas where the physics community in the United States had not dared to venture. Still, after experiences had muted their youthful exuberance, they turned to the by-then traditional problems of American quantum physics[,] problems that addressed the building up of matter rather than its deconstruction (Assmus, 1992, p. 22).

We hope to show that American work in atomic physics was significantly more important—if not in quantity, then at least in quality—than these remarks suggest.[45] Slater was one of the architects of the short-lived but highly influential Bohr-Kramers-Slater (BKS) theory (Bohr, Kramers, and Slater, 1924a) (see sec. 4). Van Vleck's two-part article in *The Physical Review* (Van Vleck, 1924b,c), which is the focus of our study, is less well-known.

[44] See p. 9 of the transcript of session 5 of the AHQP interview with Uhlenbeck..
[45] Assmus is probably right, however, that the Americans contributed more to molecular than to atomic physics. This would fit with the thesis of Schweber (1990) that "Americans contributed most significantly to the development of quantum mechanics in quantum chemistry" (pp. 398–406)

Originally, Van Vleck's paper was to have three parts. A rough draft of the third part has been preserved.[46] Van Vleck did not finish the third part at the time. As he explained in a letter to Born on November 30, 1924 (AHQP): "Part III which is not yet ready relates to classical black body radiation rather than quantum theory." It was only toward the end of his life that he returned to the masterpiece of his youth. Three years before he died he published a paper, co-authored with D. L. Huber, that can be seen as a substitute for part III. As the authors explain:

> Part III was to be concerned with the equilibrium between absorption and emission under the Rayleigh-Jeans law. It was never written up for publication because in 1925 the author was busy writing his book [Van Vleck, 1926a] and of course the advent of quantum mechanics presented innumerable research problems more timely than a purely classical investigation. The idea occurred to him to use the 50th anniversary of Parts I and II as the date for publishing a paper which would start with Part III and might even bear its title. Although he did not succeed in meeting the deadline, it still provided a partial motivation for collaborating on the present article (Van Vleck and Huber, 1977, p. 939).

It was at the suggestion of Jordan, that van der Waerden included the first (quantum) part of Van Vleck's 1924 paper in his anthology on matrix mechanics (Van der Waerden, 1968, see the preface).[47] Interviewing Van Vleck for the AHQP in October 1963, Kuhn claimed that Jordan had told him that Born and Jordan "were working quite hard in an attempt to reformulate it [Van Vleck, 1924b,c] and had been multiplying Fourier coefficients together,[48] just at the time they got the Heisenberg paper that was going to be matrix mechanics."[49] In fact, a paper by Born and Jordan (1925a) building on (Van Vleck, 1924b,c) was submitted to *Zeitschrift für Physik* on June 11, 1925, several weeks *before* Heisenberg's breakthrough (Cassidy, 1991, p. 198). We therefore suspect that Kuhn misremembered or misconstrued what Jordan had told him during an interview for the AHQP in June 1963, a few months before the interview with Van Vleck. Van Vleck's paper is brought up during the second session of the interview (see p. 14 of the transcript). In this exchange Kuhn insisted that (Born and Jordan, 1925a) had come out *before* (Van Vleck, 1924b,c). Jordan corrected Kuhn at the beginning of the third session, which prompted some further discussion of Van Vleck's paper. However, it was Kuhn, not Jordan, who suggested at that point that Born and

[46] *American Institute of Physics*, Van Vleck papers, Box 17. We are grateful to Fred Fellows for alerting us to this manuscript.
[47] See also (Sopka, 1988, pp. 110–111).
[48] The multiplication of quantum-theoretical quantities corresponding to classical Fourier components is one of the key elements of Heisenberg's *Umdeutung* paper.
[49] See p. 24 of the transcript of session 1 of the AHQP interview with Van Vleck.

Jordan continued to pursue the ideas in Van Vleck's paper even after publishing (Born and Jordan, 1925a). Jordan did not confirm this. Still, although Kuhn probably embellished the story, there is no question that Van Vleck's paper had a big impact on the work of Born and Jordan. Jordan emphasized this in the interview with Kuhn, in a letter to van der Waerden of December 1, 1961 (quoted in Van der Waerden, 1968, p. 17), and in (Jordan, 1973). We quote from this last source:

> Van Vleck gave a derivation of Einstein's laws of the relation between the probabilities of spontaneous emission and positive and negative absorption. This result of Einstein's had been looked upon for a long time in a sceptical manner by Niels Bohr; now it was highly interesting to see, just how from Bohr's preferred way of thinking, a derivation of Einstein's law could be given. Born and I performed a simplified mathematical derivation of the results of Van Vleck. Our article on this topic [Born and Jordan, 1925a] *did not contain anything new apart from our simpler form of the calculation*, but by studying this topic we both came to a more intimate understanding of Bohr's leading ideas (Jordan, 1973, p. 294, our emphasis).[50]

Incidentally, Van Vleck (1971, p. 7) pointed to this important pre-1925 contribution of his own as well as to Slater's role in BKS and Kemble's work on helium to demonstrate the inaccuracy of Rabi's characterization of American work in quantum theory quoted earlier. Even at the time, Van Vleck had felt that the Europeans were not giving the Americans their due. He complained about this in a letter to Born:

> I am writing this letter regarding some of the references to my work in your articles. I fully realize that an occasional error in a reference is unavoidable, for I have made such mistakes myself. I would gladly overlook any one error, but inasmuch as there are two or three instances, it is perhaps worth while to call them to your attention. On p. 332 of your treatise on "Atommechanik" [Born, 1925], the reference to my work on the crossed-orbit model of the normal helium atom is given as [Van Vleck, 1923]. This reference is only to the abstract of some work on *excited* helium and the references to my articles on *normal helium* are [Van Vleck, 1922a] ... and especially [Van Vleck, 1922b], where the details of the computations are given. This incorrect reference to a paper on another subject published a year later makes it appear as though my computation was published simultaneously or later than that of Kramer[s] [(Kramers, 1923), cited in the same footnote as (Van Vleck, 1923) in (Born, 1925, p. 332)]. The same error is also found in your

[50] See secs. 5.2, 5.3 and 6.1 below for discussion of Van Vleck's correspondence principles for emission and absorption. As in the case of (Kramers and Heisenberg, 1925), we suspect that (Born and Jordan, 1925a) is actually more difficult to follow for most modern readers than (Van Vleck, 1924b,c).

article [Born, 1924b] on perturbation theory . . . Also in your book on Atommechanik [(Born, 1925, p. 332), the sentence with the footnote referring to (Kramers, 1923) and (Van Vleck, 1923)] you say "das raumliche [sic] Modell ist ebenfalls von Bohr vorgeschlagen" [the spatial model has also been proposed by Bohr], without any mention of the name Kemble, who proposed the crossed-orbit model in [Kemble, 1921] before [Bohr, 1922].[51]

Van Vleck then comes to the most egregious case, Born's failure to properly acknowledge his two-part paper on the correspondence principle in (Born and Jordan, 1925a). Especially in view of Jordan's comments on the importance of this paper quoted above, the authors were very stingy in giving him credit. Van Vleck's letter continues:

> I was much interested in your recent article on the Quantization of Aperiodic Systems, in which you show that the method of Fourier integrals gives many results obtained by "Niessen and Van Vleck" [Born and Jordan, 1925a, p. 486], placing my name after Niessen's [Kare Frederick Niessen (1895–1967)], even though his paper [Niessen, 1924] did not appear until Dec. 1924 while the details of my computations were given in the Physical Review for Oct. 1924 [Van Vleck, 1924b, 1924c] and a preliminary notice published in the Journal of the Optical Society for July 1924 [Van Vleck, 1924a], before Niessen's article was even submitted for publication. I think you wrote me inquiring about my work shortly after the appearance of this preliminary note, and so you must be aware that it was the first to appear . . . inasmuch as Niessen's discussion is somewhat less general than my own, it seems to me that it scarcely merits being listed first (Ibid.).

Writing from Cambridge, Massachusetts, where he was visiting MIT, Born apologized.[52] Born had indeed written to Van Vleck concerning (Van Vleck, 1924a), albeit a little later than the latter remembered:

> While we already came close to one another in the calculation of the helium atom, I see from your paper "A Correspondence Principle for Absorption" [Van Vleck, 1924a] that we now approach each other very closely with our trains of thought . . . I am sending you my paper "On Quantum Mechanics" [Born, 1924], which pursues a goal similar to yours.[53]

This goes to show—Rabi's anecdotal evidence to the contrary notwithstanding—that at least some European physicists did keep up with theoretical work published in American journals, the *Journal of the Optical Society of America* in this case, even if they were not particularly generous acknowledging its

[51] Van Vleck to Born, October 19, 1925, draft (AHQP).
[52] Born to Van Vleck, November 25, 1925 (AHQP). Born had been less generous in the case of a similar complaint from America a few years earlier (see sec. 3.2 below).
[53] Born to Van Vleck, October 24, 1924 (AHQP).

importance in print.

3 Dispersion theory as the bridge between the old quantum theory and matrix mechanics

From the point of view of modern quantum mechanics, the old quantum theory of Bohr and Sommerfeld—especially in the hands of the latter and members of his Munich school—was largely an elaborate attempt at damage control. In classical physics the state of a physical system is represented by a point in the phase space spanned by a system's generalized coordinates and momenta (q_i, p_i). All its properties are represented by functions $f(q_i, p_i)$ defined on this phase space. In quantum mechanics the state of a system is represented by a ray in the Hilbert space associated with the system; its properties are represented by operators acting in this Hilbert space, i.e., by rules for *transitions* from one ray to another. In the old quantum theory, one bent over backward to retain classical phase space. Quantum conditions formulated in various ways in (Sommerfeld, 1915a), (Wilson, 1915), (Ishiwara, 1915), (Schwarzschild, 1916), and (Epstein, 1916) only restricted the allowed orbits of points in phase space. These conditions restricted the value of so-called action integrals for every degree of freedom of some multiply-periodic system to integer multiples of Planck's constant h,

$$\oint p_i dq_i = n_i h, \tag{1}$$

where the integral is extended over one period of the generalized coordinate q_i (there is no summation over i). This condition must be imposed in coordinates in which the so-called Hamilton-Jacobi equation for the system is separable.

Imposing such quantum conditions on classical phase space would not do in the end. As the picture of the interaction of matter and radiation in the old quantum theory already suggests, more drastic steps were required. In Bohr's theory the frequency $\nu_{i\to f}$ of the radiation emitted when an electron makes the transition from an initial state i to a final state f is given by the energy difference $E_i - E_f$ between the two states divided by h. Except in the limiting case of high quantum numbers, this radiation frequency differs sharply from the frequencies with which the electron traverses its quantized orbits in classical phase space before and after emission. This was widely recognized as the most radical aspect of the Bohr model. Erwin Schrödinger (1887–1961), for instance, opined in 1926 that this discrepancy between radiation frequency and orbital frequency

> . . . seems to me, (and has indeed seemed to me since 1914), to be something

> so *monstrous*, that I should like to characterize the excitation of light in this way as really almost *inconceivable.*[54]

Imre Lakatos (1970) produces a lengthy quotation from an obituary of Planck by Born (1948), in which the same point is made more forcefully. It even repeats some of the language of Schrödinger's letter:

> That within the atom certain quantized orbits ... should play a special role, could well be granted; somewhat less easy to accept is the further assumption that the electrons moving on these curvilinear orbits ... radiate no energy. But that the sharply defined frequency of an emitted light quantum should be different from the frequency of the emitting electron would be regarded by a theoretician who had grown up in the classical school as *monstrous* and *almost inconceivable* (Lakatos, 1970, pp. 150–151, our emphasis).

Unfortunately, this passage is nowhere to be found in (Born, 1948)!

One area of the old quantum theory in which the "monstrous" element became glaringly and unavoidably apparent was in the treatment of optical dispersion, the differential refraction of light of different colors. It was in this area that physicists most keenly felt the tension between orbital frequencies associated with individual states (the quantized electron orbits of the Bohr-Sommerfeld model) and radiation frequencies associated with *transitions* between such states. One of the key points of Heisenberg's *Umdeutung* paper was to formulate a new theory not in terms of properties of individual quantum states but in terms of quantities associated with transitions between states *without even attempting to specify the states themselves.*[55] What, above all, prepared the ground for this move, as we shall show in this section, was the development of a quantum theory of dispersion by Ladenburg, Reiche, Bohr, Kramers, and others. As Friedrich Hund (1896–1997) put it in his concise but rather cryptic history of quantum theory:

> In 1924 the question of the *dispersion of light* came to the foreground. It brought new points of view, and *it paved the way for quantum mechanics* (Hund, 1984, p. 128).

By comparison, many of the other preoccupations of the old quantum theory, such as a detailed understanding of spectral lines, the Zeeman and Stark effects, and the extension of the Bohr-Sommerfeld model to multi-electron atoms (in particular, helium) mostly added to the overall confusion and did

[54] Schrödinger to Lorentz, June 6, 1926 (M. Klein, 1967, p. 61).

[55] As Klaas Landsman (2007) emphasizes, "Heisenberg ... identified the mathematical nature of the observables, whereas Schrödinger ... found the description of the states" (p. 428).

little to stimulate the shift to the new mode of thinking exemplified by the *Umdeutung* paper.[56]

The same is true—*pace* Roger Stuewer (1975)—for the broad acceptance of Einstein's 1905 light-quantum hypothesis following the discovery in late 1922 by Arthur H. Compton (1892–1962) of the effect soon to be named after him. What *was* crucial for the development of matrix mechanics were the A and B coefficients for emission and absorption *even though* they had been introduced in the context of a theory involving light quanta (Einstein, 1916a,b, 1917). Physicists working on dispersion theory were happy to use these coefficients but were just as happy to continue thinking of light as consisting of waves rather than particles. John Hendry (1981) makes the provocative claim that "since Sommerfeld was the only known convert to the light-quantum concept as a result of the Compton effect whose opinions were of any real historical importance, this places Stuewer's thesis on the importance of the effect in some doubt" (p. 197). It is our impression that the Compton effect *did* convince many physicists of the reality of light quanta, just as Stuewer says it did, but we agree with Hendry (1981, p. 6) that this made surprisingly little difference for the quantum revolution of 1925–1926.

3.1 The classical dispersion theory of Helmholtz, Lorentz and Drude

Optical dispersion can boast of a venerable history in the annals of science reaching back at least to Descartes' rainbow and Newton's prism. The old quantum theory was certainly not the first theory for which dispersion presented serious difficulties. Both proponents of Newtonian particle theories of light in the 18th century and proponents of wave theories of light in the 19th century struggled with dispersion (Cantor, 1983).

In a review article on wave optics for the *British Association for Advancement of Science* published in 1886, Richard Tetley Glazebrook (1854–1935), a student of James Clerk Maxwell (1831–1879), divided the century into three periods. During the first period, which lasted well into the 1860s, optical phenomena were explained purely in terms of properties of the luminiferous ether, the medium thought to carry light waves. Refraction and dispersion, for instance, were explained by assuming that some property of the ether inside transparent media is different from what it is outside. The dispersion theories of this period typically also depend on the distance between the molecules of the transparent medium with which the ether was supposed to co-exist, but there was no consideration of any dynamical interaction between the ether and the transparent medium. This changed in the second period, which began

[56] For detailed analyses of some of these bewildering developments, see, e.g., (Serwer, 1977; Forman, 1968, 1970).

in the late 1860s. Theorists now began to account for refraction and dispersion in terms of waves in the ether setting harmonically-bound particles inside transparent media oscillating. During the third period, which was just starting when Glazebrook wrote his review article and which would not bear fruit until the 1890s, the models proposed in the second period were reworked to reflect that it had meanwhile become clear that light is an electromagnetic wave and that the particles in matter with which they interact are charged particles, to be identified with electrons by the end of the 1890s.

Some of the more well-known physicists and mathematicians contributing to the theory of dispersion during the first period distinguished by Glazebrook were Augustin Jean Fresnel (1788–1827), James MacCullagh (1809–1847), and Augustin Louis Cauchy (1789–1857). What distinguished their theories from one another was to a large extent simply which property of the ether was made responsible for the different behavior of light in different media. MacCullagh and Cauchy, who did their most important work on dispersion in the 1830s, assumed that the rigidity or the elasticity of the ether was the key variable (Glazebrook, 1886, p. 158, pp. 164–165). Many theorists, however, followed Fresnel's original idea that it was its density. Fresnel assumed that the index of refraction is proportional to the square of the ether density inside the transparent medium (ibid., p. 157). This view became popular even though it implied that a transparent medium contains different amounts of ether for different colors. The index of refraction, after all, must depend on frequency to account for dispersion. This also affected the optics of moving bodies. To account for the absence of any signs of motion of the earth with respect to the ether, Fresnel, in 1818, introduced the "drag" coefficient. A transparent medium with index of refraction n would carry along the ether inside of it with a fraction $f = 1 - 1/n^2$ of its velocity with respect to the ether. Although it was widely recognized that the drag coefficient was needed to account for the null results of numerous ether drift experiments, many physicists throughout the 19th century expressed strong reservations about the underlying physical mechanism proposed by Fresnel, since it implied that, because of dispersion, matter had to drag along different amounts of ether for different colors (Janssen and Stachel, 2004; Stachel, 2005).

Despite such conceptual difficulties and despite limited agreement with the experimental data, progress was made in the first half of the 19th century in understanding such phenomena as dispersion with, to use Glazebrook's terminology, "theories based solely on the elastic solid theory [of the ether]" (Glazebrook, 1886, p. 210), in which all optical phenomena in transparent matter are attributed to some modification of the properties of the ether inside. Concluding his discussion of such theories in his review article, Glazebrook wrote:

> while the elastic solid theory, taken strictly, fails to represent all the facts

> of experiment, we have learnt an immense amount by its development, and have been taught where to look for modifications and improvements. We may, I think, infer that the optical differences of bodies depend mainly on differences in the density or effective density of the ether in those bodies, and not on differences of rigidity (ibid., p. 211).

Glazebrook then turned to the second period and the second class of theories that he distinguished in his review article: "[t]heories based on the mutual reaction between ether and matter" (Glazebrook, 1886, Part III, pp. 212–251). In such theories, the ether is typically assumed to have the same properties everywhere and refraction and dispersion are explained in terms of momentum transfer between the ether and the molecules of ponderable matter. Before discussing various theories of this kind, Glazebrook explained why it was to be expected that a satisfactory theory for the behavior of light in transparent media calls for such a theory:

> The properties we have been considering depend on the presence of matter, and we have to deal with two systems of mutually interpenetrating particles. It is clearly a very rough approximation to suppose that the effect of the matter is merely to alter the rigidity or the density of the ether. The motion of the ether will be disturbed by the presence of the matter; motion may even be set up in the matter particles. The forces to which this gives rise may, so far as they affect the ether, enter its equations in such a way as to be equivalent to a change in its density or rigidity, but they may, and probably will, in some cases do more than this. The matter motion will depend in great measure on the ratio which the period of the incident light bears to the free period of the matter particles. If this be nearly unity, most of the energy in the incident vibration will be absorbed in setting the matter into motion, and the solution will be modified accordingly (ibid., p. 212).

More than anything else, it was the phenomenon of anomalous dispersion that necessitated these more sophisticated theories. Anomalous dispersion was first noticed in 1840 by the early photographer William Henry Fox Talbot (1800–1877) but only recognized for what it really was in 1870 by the Danish physicist Christian Christiansen (1843–1917) (Buchwald, 1985, p. 233). Whereas in normal dispersion the angle of refraction increases with the frequency of the refracted light, in anomalous dispersion there are frequency intervals in which the angle of refraction decreases (if we consider some fixed angle of incidence) with increasing frequency. As Glazebrook emphasized, this phenomenon is inexplicable in the older class of theories.[57] Anomalous dispersion calls for a

[57] "The suggestions of Cauchy and [Charles Auguste] Briot [(1817–1882)] ... lead to expressions for the relation between the refractive index and wave length which agree well with experiment so long as we steer clear of substances which present the phenomena of anomalous dispersion, but of this they give no account" (Glazebrook, 1886, p. 212; see also p. 217).

theory “based on the mutual reaction between ether and matter.”

The first such theory appears to have been formulated in 1867 by Joseph Valentin Boussinesq (1842–1929) (Glazebrook, 1886, p. 213). Independently of Boussinesq, it seems, Wolfgang Sellmeier, a student of Franz Neumann (1798–1895), developed a similar theory and used it in 1872 to account for dispersion, including anomalous dispersion. Roughly, according to Sellmeier's theory, what happens when a light wave of a certain frequency hits a transparent medium is that it produces (additional) oscillations of harmonically-bound particles in the medium. The result of this interaction, as Glazebrook points out in the passage quoted above, will depend on how close the frequency of the incoming light is to the resonance frequencies of these particles. The dispersion formula given by Sellmeier has a pole at the resonance frequencies. The same is true for the dispersion formula derived from electromagnetic theories later in the century (cf. eqs. (6)–(7) below). If these poles are in the ultraviolet, optical dispersion is normal (i.e., the angle of refraction increases with frequency throughout the optical spectrum); if, however, there are poles at optical frequencies, the angle of refraction decreases with frequency in the range immediately above them (Glazebrook, 1886, p. 219).

In 1875, Hermann von Helmholtz (1821–1894) proposed a highly influential dispersion theory in the spirit of Boussinesq and Sellmeier but substantially improving on their work (Buchwald, 1985, Ch. 27). Helmholtz's theory was based on “twin equations” for the coupled oscillations in the ether and in the transparent medium (ibid, p. 235; cf. Glazebrook 1886, p. 222). Helmholtz's theory, like Sellmeier's, is a purely mechanical one. Newtonian mechanics governs both ether and matter. In 1893, seven years after Glazebrook's review article, Helmholtz adapted his theory to reflect that light is an electromagnetic wave and that such waves act on and are emitted by charged particles in matter (Buchwald, 1985, sec. 27.2; Darrigol, 2000, sec. 8.3). The year before and apparently unbeknownst to Helmholtz, Lorentz had already published such an electromagnetic dispersion theory, building on work he had done in the 1870s, which in turn built on Helmholtz's synthesis of British and continental ideas about electromagnetism.[58] Lorentz's theory of 1892 is similar but superior to Helmholtz's theory of 1893. For one thing, Lorentz immediately derived the Fresnel drag coefficient from his theory, whereas it was left to Richard August Reiff (1855–1927) to do so for Helmholtz's theory later in 1893 (Darrigol, 2000, p. 322). Moreover, while it is perfectly clear that Lorentz derived the drag coefficient without introducing any actual ether drag, this is not so clear in the case of Reiff (Buchwald, 1985, p. 241). Neither Helmholtz's theory nor Lorentz's left room for ether drag. In both theories, the ether is im-

[58] This earlier work by Lorentz is not mentioned in the brief discussion of electromagnetic theories in (Glazebrook, 1886, Part IV, pp. 251–261). For a detailed discussion of Lorentz's dispersion theory, see (Buchwald, 1985, Appendix 7).

mobile, its properties are the same everywhere, and the index of refraction is related to the polarization of harmonically-bound electric charges. These theories thus avoid the absurdity in Fresnel's original picture that matter drags along different amounts of ether for different frequencies.

Lorentz's theory constituted a much more radical move into microphysics than Helmholtz's and partly as a result of that, it seems, held less appeal for German physicists in the 1890s, although Helmholtz's greater authority in the German physics community may also have been a factor (Buchwald, 1985, pp. 238–241). The approach to optics based on an electron theory à la Helmholtz and Lorentz only became popular with the appearance of *Lehrbuch der Optik* (Drude, 1900), in which Paul Drude (1863–1906) presented and extended the theory.[59] The English translation of Drude's book in 1905 made the approach popular in Britain and the United States as well.

This classical electron theory of dispersion was remarkably successful in accounting for the experimental data. Hence, two centuries after Newton, there finally was a reasonably satisfactory theory for dispersion, including anomalous dispersion. Only two decades later, however, the model of matter underlying this theory was called into question again with the rise of the old quantum theory (Jammer, 1966, p. 189). The electrons oscillating inside atoms in the Helmholtz-Lorentz-Drude model were replaced by electrons orbiting the nucleus in the Rutherford-Bohr model. As we shall see, the classical electron theory of dispersion nonetheless played an important role in the development of a quantum theory of dispersion in the early 1920s.

The basic model of dispersion in this classical theory is very simple.[60] Suppose an electromagnetic wave of frequency ν (we are not concerned with how and where this wave originated) strikes a charged one-dimensional simple harmonic

[59] Olivier Darrigol (1992, p. 331) suggests that Drude converted to Lorentz's theory after the 1898 *Naturforscherversammlung* in Düsseldorf, where Lorentz was the guest of honor for a session on the problem of optics and electrodynamics in moving bodies. Jed Buchwald (1985, p. 250), however, points out that (Drude, 1900) only refers to Lorentz in the discussion of optics in moving bodies and suggests that Drude, like most German physicists, followed Helmholtz rather than Lorentz. Dispersion is covered in Pt. II, Sec. II, Ch. V of Drude's book.

[60] The theory is covered elegantly in ch. 31 of Vol. 1 of the Feynman lectures (see also ch. 32 of Vol. 2). Feynman makes it clear that this classical theory remains relevant in modern physics: "we will assume that the atoms are little oscillators, that is that the electrons are fastened elastically to the atoms ... You may think that this is a funny model of an atom if you have heard about electrons whirling around in orbits. But that is just an oversimplified picture. The correct picture of an atom, which is given by the theory of wave mechanics, says that, *so far as problems involving light are concerned*, the electrons behave as though they were held by springs" (Feynman *et al.*, 1964, Vol. 1, sec. 31-4).

oscillator with characteristic frequency ν_0. We focus on the case where the frequency ν of the electromagnetic wave is far from the resonance frequency ν_0 of the oscillator. We can picture the oscillator as a point particle with mass m and charge $-e$ (where e is the *absolute value* of the electron charge) on a spring with equilibrium position $x = 0$ and spring constant k, resulting in a restoring force $F = -kx$. The characteristic angular frequency $\omega_0 = 2\pi\nu_0$ is then given by $\sqrt{k/m}$. The electric field E of the incident electromagnetic wave[61] will induce an additional component of the motion at the imposed frequency ν. This component will be superimposed on any preexisting oscillations at the characteristic frequency ν_0 of the unperturbed system. It is this additional component of the particle motion, coherent with the incident wave (i.e., oscillating with frequency ν), that is responsible for the secondary radiation that gives rise to dispersion. The time-dependence of this component is given by:

$$\Delta x_{\text{coh}}(t) = A\cos\omega t, \tag{2}$$

where $\omega = 2\pi\nu$. To determine the amplitude A, we substitute eq. (2) into the equation of motion for the system. As long as we are far from resonance, radiation damping can be ignored and the equation of motion is simply:[62]

$$m\ddot{x} = -m\omega_0^2 x - eE\cos\omega t, \tag{3}$$

where dots indicate time derivatives and where we have made the innocuous simplifying assumption that the electric field of the incident wave is in the x-direction. Substituting $\Delta x_{\text{coh}}(t)$ in eq. (2) for $x(t)$ in eq. (3), we find:

$$-m\omega^2 A\cos\omega t = (-m\omega_0^2 A - eE)\cos\omega t. \tag{4}$$

It follows that

$$A = \frac{eE}{m(\omega^2 - \omega_0^2)}. \tag{5}$$

The central quantity in the classical dispersion theory is the dipole moment $p(t) \equiv -e\Delta x_{\text{coh}}(t)$ of the oscillator induced by the electric field of the incident

[61] We need not worry about the effects of the magnetic field B. The velocity of electrons in typical atoms is of order αc, where c is the velocity of light and $\alpha \simeq \frac{1}{137}$ is the fine-structure constant. The effects due to the magnetic field are thus a factor $\frac{1}{137}$ smaller than those due to the electric field and can be ignored in all situations considered in this paper.

[62] In sec. 5.3, we show how to take into account the effects of radiation damping.

electromagnetic wave. From eqs. (2) and (5) it follows that:

$$p(t) = -e\Delta x_{\mathrm{coh}}(t) = \frac{e^2 E}{4\pi^2 m(\nu_0^2 - \nu^2)} \cos 2\pi\nu t. \tag{6}$$

For groups of n_i oscillators of characteristic frequencies ν_i per unit volume, this formula for the dipole moment naturally generalizes to the following result for the polarization (i.e., the dipole moment per unit volume):

$$P(t) = \frac{e^2 E}{4\pi^2 m} \sum_i \frac{n_i}{\nu_i^2 - \nu^2} \cos 2\pi\nu t. \tag{7}$$

The number of oscillators of characteristic frequency ν_i will be some fraction f_i of the numbers of atoms in the volume under consideration. This fraction was often called the 'oscillator strength' in the literature of the time. The polarization P determines the index of refraction n (see, e.g., Feynman *et al.*, 1964, Vol. 1, 31-5). The agreement of eq. (7) with the data from experiments on dispersion was not perfect, but dispersion was nonetheless seen as an important success for the classical theory.

3.2 The Sommerfeld-Debye theory and its critics

An early and influential attempt to bring dispersion theory under the umbrella of the old quantum theory was made by Sommerfeld (1915b, 1917) and by his former student Peter Debye (1884–1966) (Debye, 1915).[63] Clinton J. Davisson

[63] For other historical discussions of the development of quantum dispersion theory, see, e.g., (Darrigol, 1992, pp. 224–230), (Dresden, 1987, pp. 146–159, pp. 215–222), (Jammer, 1966, p. 165 and sec. 4.3, especially pp. 188–195), (Mehra and Rechenberg, 1982–2001, Vol. 1, sec. VI.1; Vol. 2, sec. III.5, pp. 170–190; Vol. 6, sec. III.1 (b), pp. 348–353), and (Whittaker, 1953, Vol. 1, p. 401; Vol. 2, pp. 200–206). Van Vleck (1926, sec. 49, pp. 156–159) briefly discusses the early attempts to formulate a quantum theory of dispersion in his review article on the old quantum theory. We focus on the theory of Sommerfeld and Debye of the late 1910s and on the theories developed by Ladenburg and Reiche and by Kramers in the early 1920s. Van Vleck also mentions theories of the latter period by Charles Galton Darwin (1887–1962), Adolf Gustav Smekal (1895–1959), and Karl F. Herzfeld (1892–1978). All three of these theories make use of light quanta. In addition, strict energy conservation is given up in the theory of Darwin (1922, 1923), while in the theories of Smekal (1923) and Herzfeld (1924) orbits other than those picked out by the Bohr-Sommerfeld condition are allowed, a feature known as "diffuse quantization." For other (near) contemporary reviews of dispersion theory, see (Pauli, 1926, pp. 86–96), (Andrade, 1927, pp. 669–682), and (Breit, 1932). Stolzenburg (1984, pp. 17–18) briefly discusses Bohr's critical reaction to Darwin's dispersion theory.

(1881–1958), then working at the Carnegie Institute of Technology in Pittsburgh, also contributed (Davisson, 1916).[64] The Sommerfeld-Debye theory, as it came to be known, was based on the dubious assumption that the secondary radiation coming from small perturbations of a Bohr orbit induced by incident radiation could be calculated on the basis of ordinary classical electrodynamics, even though, by the basic tenets of the Bohr model, the classical theory did *not* apply to the original unperturbed orbit. In other words, it was assumed that, while the large accelerations of electrons moving on Bohr orbits would produce no radiation whatsoever, the comparatively small accelerations involved in the slight deviations from these orbits caused by weak incident radiation would produce radiation.[65] Otherwise, the theory stayed close to the classical theory, substituting small deviations in the motion of electrons from their Bohr orbits for small deviations from the vibrations of simple harmonic oscillators at their characteristic frequencies.

Both the Swedish physicist Carl Wilhelm Oseen (1879–1944) and Bohr severely criticized the way in which Sommerfeld and Debye modeled their quantum dispersion theory on the classical theory. Oseen (1915) wrote: "Bohr's atom model can in no way be reconciled with the fundamental assumptions of Lorentz's electron theory. We have to make our choice between these two theories" (p. 405).[66] Bohr agreed. The central problem was that in Bohr's theory the link between radiation frequencies and orbital frequencies had been severed. As Bohr explained to Oseen in a letter of December 20, 1915, if the characteristic frequencies involved in dispersion

> . . . are determined by the laws for quantum emission, the dispersion cannot, whatever its explanation, be calculated from the motion of the electrons and the usual electrodynamics, which does not have the slightest connection with the frequencies considered (Bohr, 1972–1996, Vol. 2, p. 337).

Bohr elaborated on his criticism of the Sommerfeld-Debye theory in a lengthy paper intended for publication in *Philosophical Magazine* in 1916 but withdrawn after it was already typeset.[67] Bohr argued (we leave out the specifics of the experiments on dispersion in various gases that Bohr mentions in this passage):

[64] In 1927 at Bell Labs, Davisson and his assistant Lester H. Germer (1896–1971) would do their celebrated work on electron diffraction (Davisson and Germer, 1927), another great American contribution to (experimental) quantum physics for which the authors received the 1937 Nobel Prize (Kevles, 1978, pp. 188–189).

[65] Sommerfeld (1915b, p. 502) realized that this assumption was problematic and tried (unconvincingly) to justify it.

[66] Quoted and discussed in (Bohr, 1972–1996, Vol. 2, p. 337)

[67] It can be found in (Bohr, 1972–1996, Vol. 2, pp. 433–461). For further discussion of Bohr's early views on dispersion, see (Heilbron and Kuhn, 1969, pp. 281–283).

> [E]xperiments ... show that the dispersion ... can be represented with a high degree of approximation by a simple Sellmeier formula in which the characteristic frequencies coincide with the frequencies of the lines in the ... spectra ... [T]hese frequencies correspond with transitions between the normal states of the atom ... On this view we must consequently assume that the dispersion ... depends on the same mechanism as the transition between different stationary states, and that it cannot be calculated by application of ordinary electrodynamics from the configuration and motions of the electrons in these states (Bohr, 1972–1996, Vol. 2, pp. 448–449).

In the next paragraph, Bohr added a prescient comment. Inverting the line of reasoning in the passage above that dispersion should depend on the same mechanism as transitions between states, he suggested that transitions between states, about which the Bohr theory famously says nothing, should depend on the same mechanism as dispersion: "[i]f the above view is correct ... we must, on the other hand, assume that this mechanism [of transitions between states] shows a close analogy to an ordinary electrodynamic vibrator" (ibid.).

As we shall see, in the quantum dispersion theory of the 1920s, the oscillators of the classical theory were grafted onto the Bohr model. For the time being, however, it was unclear how to arrive at a satisfactory quantum theory of dispersion. The quasi-classical Sommerfeld-Debye theory led to a formula for the induced polarization of the form of eq. (7) but with resonance poles at the orbital frequencies. As Oseen and Bohr pointed out, this was in blatant contradiction with the experimental data, which clearly indicated that the poles should be at the radiation frequencies, which in Bohr's theory differed sharply from the orbital frequencies.

This criticism is repeated in more sophisticated form in a paper by Paul Sophus Epstein (1883–1966) with the subtitle "Critical comments on dispersion." This paper is the concluding installment of a trilogy on the application of classical perturbation theory to problems in the old quantum theory (Epstein, 1922a,b,c). Epstein, a Russian Jew who studied with Sommerfeld in Munich, was the first European quantum theorist to be lured to America. In 1921 Millikan brought him to the California Institute of Technology in Pasadena, despite prevailing anti-Semitic attitudes (Kevles, 1978, pp. 211–212).[68] In his 1926 review article Van Vleck emphasizes the importance of the work of his colleague at Caltech and notes that it "is rather too often overlooked" (Van Vleck, 1926, p. 164, note 268), to which one might add: "by European physicists." As we saw in sec. 2.4, Van Vleck felt the same way about his own contributions. Like Van Vleck, Epstein apparently complained about this lack

[68] For further discussion of Epstein's position at Caltech, see (Seidel, 1978, pp. 507–520).

of recognition to Born. This can be inferred from a letter from Born to Sommerfeld of January 5, 1923, shortly before a visit of the latter to the United States:

> When you talk to Epstein in Pasadena and he complains about me, tell him that he should show you the very unfriendly letter he wrote to me because he felt that his right as first-born had been compromised by the paper on perturbation theory by Pauli and me [Born and Pauli, 1922, which appeared shortly after Epstein's trilogy]. Also tell him that I do not answer such letters but that I do not hold a grudge against him because of his impoliteness (to put it mildly) ... In terms of perturbative quantization we are ahead of him anyway (Sommerfeld, 2004, p. 137).[69]

To deal with the kind of multiply-periodic systems that represent hydrogenic atoms (i.e., atoms with only one valence electron) in the old quantum theory, Epstein customized techniques developed in celestial mechanics for computing the perturbations of the orbits of the inner planets due to the gravitational pull of the outer ones.[70] The perihelion advance of Mercury due to such perturbations, for instance, is more than ten times the well-known $43''$ per century due to the gravitational field of the sun as given by general relativity. Such calculations in classical mechanics are also the starting point of the later more successful approach to dispersion theory by Kramers and Van Vleck. Epstein clearly recognized that these calculations by themselves do not lead to a satisfactory theory of dispersion. In the introduction of his paper, Epstein (1922c, p. 92) explains that he discusses dispersion mainly because it nicely illustrates some of the techniques developed in the first two parts of his trilogy. He warns the reader that he is essentially following the Sommerfeld-Debye theory, and emphasizes that "this point of view leads to internal contradictions so strong that I consider the Debye-Davysson [sic] dispersion theory [as Epstein in Pasadena referred to it] to be untenable" (ibid.). The central problem is once again the discrepancy between radiation frequencies and orbital frequencies. As Epstein wrote in the conclusion of his paper:

> the positions of maximal dispersion and absorption [in the formula he derived] 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, 1922c, pp. 107–108).

[69] This letter is quoted and discussed in (Eckert, 1993, p. 96)
[70] One of the sources cited by Epstein (1922a, p. 216) is (Charlier, 1902–1907). This source is also cited in (Bohr, 1918, p. 114), (Kramers, 1919, p. 8), and (Born and Pauli, 1922, p. 154). In their interviews for the AHQP, both Van Vleck (p. 14 of the transcript of session 1) and Heisenberg (p. 24 of the transcript of session 5) mention that they studied Charlier as well.

Epstein recognized that a fundamentally new approach was required: "We believe that ... dispersion theory must be put on a whole new basis, in which one takes the Bohr frequency condition into account from the very beginning" (ibid., p. 110).[71]

3.3 Dispersion in Breslau: Ladenburg and Reiche

Unbeknownst to Epstein, quantum dispersion theory had already begun to emerge from the impasse he called attention to in 1922. The year before, Ladenburg had introduced one of two key ingredients needed for a satisfactory treatment of dispersion in the old quantum theory: the emission and absorption coefficients of Einstein's quantum theory of radiation. The other critical ingredient, as we shall see below, was Bohr's correspondence principle.

Ladenburg spent most of his career doing experiments on dispersion in gases. He started in 1908, about two years after he joined the physics department, then headed by Otto Lummer (1860–1925), at the University of Breslau, his hometown (Ladenburg, 1908).[72] He stayed in Breslau until 1924, when he accepted a position at the *Kaiser Wilhelm Institut* in Berlin. There he continued his work with the help of students such as Hans Kopfermann (1895–1963), Agathe Carst, S. Levy, and G. Wolfsohn. Ladenburg and his group reported the results of their experiments on dispersion in a series of papers published between 1926 and 1934.[73] Ladenburg's direct involvement ceased with his emigration to the United States in 1931.

Ladenburg and Stanislaw Loria (1883–1958) had established early on that the frequency of the H_α line in the Balmer series in the hydrogen spectrum corresponds to a pole in the classical dispersion formula (Ladenburg and Loria, 1908, p. 866). Given that the Sommerfeld-Debye theory flies in the face of this experimental fact, Ladenburg was never attracted to that theory. He simply kept using a dispersion formula with poles at the observed radiation frequen-

[71] Epstein had already voiced this criticism before he left for the United States. From Zurich, he had written to Einstein on October 15, 1919: "Meanwhile, I have carried out the calculations for dispersion theory from the point of view of quantum theory that I mentioned at one point in conversation: the result is definitely that the Debye-Sommerfeld theory only has the status of an approximation and that the true theory must take into account the [Bohr] frequency condition. It is not surprising, therefore, that Sommerfeld's results are occasionally off" (Einstein, 1987–2006, Vol. 9, Doc. 136).
[72] See the entry on Ladenburg by A. G. Shenstone (1973) in the *Dictionary of Scientific Biography.*
[73] See (Mehra and Rechenberg, 1982–2001, Vol. 6, Ch. 3(b), pp. 348–353) and (Shenstone, 1973, p. 555) for detailed references and brief discussions.

cies. He focused on the numerator rather than the denominator of the dispersion formula. This is made particularly clear in the AHQP interviews with two of his collaborators in the early 1920s—Rudolph Minkowski (1895–1976), a nephew of Hermann Minkowski, who took his doctorate under Ladenburg in 1921 and co-authored (Ladenburg and Minkowski, 1921); and Fritz Reiche, who was appointed in Breslau in 1921.[74] After his doctorate (with Planck) in Berlin in 1907, Reiche had already spent three years in Breslau. He and Ladenburg had become close friends. Reiche had gone back to Berlin in 1911. When he returned to Breslau ten years later, he stayed until he was dismissed in 1933.[75] Reiche's help is prominently acknowledged in (Ladenburg, 1921, p. 140, note). Ladenburg was first and foremost an experimentalist and he welcomed input from his theoretician friend and colleague.[76] The two of them co-authored a pair of follow-up papers (Ladenburg and Reiche, 1923, 1924). Discussing the first of these, Reiche told Kuhn and Uhlenbeck in 1962:

> we did not derive a consistent dispersion theory, in which instead of the revolution numbers the emitted lines came out. We thought it completely self-evident, that one had to change the denominator of the dispersion formula in such a way that the frequencies were the emitted line frequencies, and not something which has to do with (the orbit) [sic].[77]

Reiche made it clear that he and Ladenburg were concerned only with explaining "the N which is on top of the dispersion formula:"

> It never came out correctly equal to the number of atoms, or to the number of atoms multiplied by the number of electrons in an atom. It gave, under certain conditions, even numbers which are less than the whole number of atoms. They were written very often with a German N ... This was the main aim of the whole thing [Ladenburg and Reiche, 1923]. There, based on a previous paper by Ladenburg [1921], we found a relation between the German N and the real number of atoms. The f were not 1 or 2 or 3 or something like this, but could be point 5 or the like. And the explanation of this was the aim of this dispersion paper. But it did not come out that we had a correct and consistent theory in which the denominator gave now the emitted frequencies. This, I think, was only done by Kramers [1924a,

[74] The following information is based on an autobiographical statement by Reiche published as an appendix to (Bederson, 2005).
[75] It was not until 1941 that he finally managed to emigrate to the United States.
[76] Asked by Kuhn whether Ladenburg was "strictly an experimentalist," Reiche said: "He was, as far as I understand, a very good experimental man, but he was one of the men who could make, let me say, easy theoretical work" (p. 10 of the transcript of the last of three sessions of the interview).
[77] P. 11 of the transcript of the second of three sessions of the AHQP interview with Reiche.

b], first of all.[78]

Ladenburg's dispersion experiments had indicated all along that the oscillator strength f_i, the number of dispersion electrons with characteristic frequency ν_i per atom, was not on the order of unity, as one would expect on the basis of the classical theory, but much smaller. For the frequency ν_i corresponding to the H_α line in the Balmer series in the hydrogen spectrum, for instance, Ladenburg and Loria (1908, p. 865) found that there was only 1 dispersion electron per 50,000 molecules, and they cited findings of 1 dispersion electron per 200 molecules in sodium vapor. Such low values were quite inexplicable on classical grounds. In the Bohr model the H_α (absorption) line corresponds to a transition from the $n = 2$ to the $n = 3$ state of the hydrogen atom. That Ladenburg found such a low value for what he interpreted classically as the number of dispersion electrons at the frequency of the H_α line is explained in Bohr's theory simply by noting that only a tiny fraction of the atoms will be in the $n = 2$ state (Ladenburg, 1921, p. 156). Ladenburg's key contribution was that he recognized that the oscillator strengths corresponding to various transitions could all be interpreted in terms of transition probabilities, given by Einstein's A and B coefficients. Hence the title of his paper: "The quantum-theoretical interpretation of the number of dispersion electrons" (Ladenburg, 1921).

Ladenburg obtained a relation between the oscillator strengths and the A and B coefficients by equating results derived for what would seem to be two mutually exclusive models of matter, a classical and a quantum model. He calculated the energy absorption rate both for a collection of classical oscillators à la Helmholtz, Lorentz and Drude, resonating at the absorption frequencies, and for a collection of atoms à la Bohr and Einstein with transitions between discrete energy levels corresponding to these same frequencies. Ladenburg set the two absorption rates equal to one another. His paper only gives the resulting expression for the numerator of the dispersion formula. Combining Ladenburg's theoretical relation between classical oscillator strengths and quantum transition probabilities with his experimental evidence that the resonance poles should be at the radiation frequencies, we arrive at the following formula—in our notation, based on (Van Vleck, 1924b)—for the induced polarization of a group of N_r atomic systems in their ground state r

$$P_r(t) = \frac{N_r c^3 E}{32\pi^4} \sum_s \frac{A_{s\to r}}{\nu_{s\to r}^2(\nu_{s\to r}^2 - \nu^2)} \cos 2\pi\nu t, \tag{8}$$

where $\nu_{s\to r}$ is the frequency for a transition from the excited states s to r and $A_{s\to r}$ is Einstein's emission coefficient for this transition.

[78] Ibid. Dispersion is discussed at greater length during the third session of the interview (see pp. 10–14 of the transcript).

Ladenburg's paper initially did not attract much attention. It is not mentioned in Epstein's trilogy the following year, but then Epstein was working in faraway California. More surprisingly, quantum physicists in Göttingen, Munich, and Copenhagen, it seems, also failed to take notice, even though Ladenburg was well-known to his Göttingen colleagues Born and James Franck (1882–1964). Ladenburg had actually prevented that Born, a fellow Breslau native, was sent to the trenches in World War I. Ladenburg had recruited Born for an army unit under his command in Berlin devoted to artillery research (Thorndike Greenspan, 2005, pp. 71–72). Bohr and Ladenburg also knew each other personally: Ladenburg had attended Bohr's colloquium in Berlin in April 1920 and the two men had exchanged a few letters since (Bohr, 1972–1996, Vol. 4, pp. 709–717).

Heisenberg later attributed the neglect of Ladenburg in Göttingen and Munich to the problem of connecting Ladenburg's work, closely tied to Einstein's radiation theory, to the dominant Bohr-Sommerfeld theory.[79] According to Heisenberg, it was only when Kramers (1924a,b) rederived Ladenburg's formula as a special case of his own more general dispersion formula that its significance was widely appreciated.[80] Ladenburg's own derivation had been unconvincing, at least to most physicists.[81] In addition to just assuming the poles in the dispersion formula to be at the radiation frequencies rather than at the orbital frequencies, Ladenburg offered no justification for equating classical and quantum energy absorption rates. Van der Waerden (1968, p. 10) suggests that Ladenburg appealed to Bohr's correspondence principle in his derivation of the relation between oscillator strengths and A and B coefficients, but the correspondence principle is not mentioned anywhere in Ladenburg's paper. The full dispersion formula (8)—admittedly only implicit in Ladenburg's paper but associated with it, not just by later historians but also by

[79] See p. 8 of the transcript of session 4 of the AHQP interview with Heisenberg, parts of which can be found in (Mehra and Rechenberg, 1982–2001, Vol. 2, pp. 175–176), although the authors cite their own conversations with Heisenberg as their source (cf. note 5).

[80] Jordan had the same impression (see pp. 24–25 of the transcript of the first session of Kuhn's interview with Jordan for the AHQP in June 1963). It also fits with Born's recollections. In his autobiography, Born (1978) notes: "An important step was made by my old friend from Breslau ... Ladenburg ... A detailed account was given by Ladenburg and Reiche, my other old friend from Breslau ... On the basis of these investigations, Kramers ... succeeded in developing a complete 'dispersion formula'" (pp. 215–216).

[81] As Kuhn put it in his AHQP interview with Slater: "Of course, there was a good deal that appeared to most physicists as pretty totally *ad hoc* about the Reiche-Ladenburg work, and the whole question as to why it was the transition frequencies that occurred in the denominator rather than the orbital frequencies." Slater disagreed: "This seemed to me perfectly obvious ... " (p. 41 of the transcript of the first session of the interview).

his contemporaries—can certainly not be derived with the help of the correspondence principle, since it only holds for atoms in their ground state and *not* for atoms in highly excited states where classical and quantum theory may be expected to merge in the sense of the correspondence principle. Still, if Heisenberg's later recollections are to be trusted, it might have helped the reception of Ladenburg's paper had he made some reference to the correspondence principle.

Unlike his colleagues in Göttingen and Munich, Bohr did take notice of Ladenburg's paper early on. He was just slow, as usual, to express himself about it in print. As noted in (Hendry, 1981, p. 192), Bohr referred to (Ladenburg, 1921) in the very last sentence of a manuscript he did not date but probably started and abandoned in 1921 (Bohr, 1972–1996, Vol. 3, pp. 397–414). In a paper submitted in November 1922, Bohr (1923b, p. 162) finally discussed Ladenburg's work in print. After repeating some of the observations about dispersion in the passages of his unpublished 1916 paper quoted in sec. 3.2, Bohr, in his tortuous verbose style, made some highly interesting remarks that anticipate aspects of the BKS theory of 1924 (see sec. 4):

> the phenomena of dispersion must thus be so conceived that the reaction of the atom on being subjected to radiation is closely connected with the unknown mechanism which is answerable [the German has *verantwortlich*: responsible] for the emission of the radiation on the transition between stationary states. In order to take account of the observations, it must be assumed that this mechanism . . . becomes active when the atom is illuminated in such a way that the total reaction of a number of atoms is the same as that of a number of harmonic oscillators in the classical theory,[82] the frequencies of which are equal to those of the radiation emitted by the atom in the possible processes of transition, and the relative number of which is determined by the probability of occurrence of such processes of transition under the influence of illumination. A train of thought of this kind was first followed out closely in a work by Ladenburg [1921] in which he has tried, in a very interesting and promising manner, to set up a direct connection between the quantities which are important for a quantitative description of the phenomena of dispersion according to the classical theory and the coefficients of probability appearing in the deduction of the law of temperature radiation by Einstein (Bohr, 1972–1996, Vol. 3, p. 496).

A letter from Bohr to Ladenburg of May 17, 1923 offers further insights into Bohr's developing views on the mechanism of radiation:

> to interpret the actual observations, it . . . seems necessary to me that the

[82] Note the similarity between Bohr's description here to Feynman's observation (quoted in note 60 above) that atoms behave like oscillators "so far as problems involving light are concerned."

> quantum jumps are not the direct cause of the absorption of radiation, but that they represent an effect which accompanies the continuously dispersing (and absorbing) effect of the atom on the radiation, even though we cannot account in detail for the quantitative relation [between these two effects] with the usual concepts of physics (Bohr, 1972–1996, Vol. 5, p. 400).

At the beginning of this letter, Bohr mentioned the vagueness of some of his earlier pronouncements on the topic. After the passage just quoted he acknowledged “that these comments are not far behind the earlier ones in terms of vagueness. I do of course reckon with the possibility that I am on the wrong track but, if my view contains even a kernel of truth, then it lies in the nature of the matter that the demand for clarity in the current state of the theory is not easily met” (ibid.). Bohr need not have been so apologetic. His comments proved to be an inspiration to Ladenburg and Reiche. On June 14, 1923, Ladenburg wrote to Bohr:

> Over the last few months Reiche and I have often discussed [the absorption and scattering of radiation] following up on your comments in [Bohr 1923b] about reflection and dispersion phenomena and on my own considerations [Ladenburg 1921] which you were kind enough to mention there (Bohr, 1972–1996, Vol. 5, pp. 400–401).

In this same letter, Ladenburg announced his forthcoming paper with Reiche in a special issue of *Die Naturwissenschaften* to mark the tenth anniversary of Bohr’s atomic theory. In the conclusion of this paper, they wrote:[83]

> Surveying the whole area of scattering and dispersion discussed here, we have to admit that we do not know the real [*eigentlich*] mechanism through which an incident wave acts on the atoms and that we cannot describe the reaction of the atom in detail. This is no different by the way in the case of the real [*eigentlich*] quantum process, be it that an external wave ν_0 lifts electrons into higher quantum states, or be it that a wave ν_0 is sent out upon the return to lower states. We nevertheless believe on the grounds of the observed phenomena that the end result of a process in which a wave of frequency ν acts upon the atom should not be seen as fundamentally different from the effect that such a wave exerts on classical oscillators (Ladenburg and Reiche, 1923, p. 597).

Ladenburg and Reiche (1923, p. 588, p. 590) introduced the term “substitute oscillators” [*Ersatzoszillatoren*] for such classical oscillators representing the atom as far as its interaction with radiation is concerned. They credited Bohr with the basic idea.[84] As we shall see in sec. 4, these substitute oscillators

[83] Quoted and discussed in (Hendry, 1981, p. 192).
[84] See also (Ladenburg and Reiche, 1924, p. 672). Van Vleck (1926, p. 159, note 260) reports that Lorentz made a similar suggestion at the third Solvay congress in

became the virtual oscillators of BKS. Ladenburg and Reiche (1924, p. 672) themselves noted the following year that substitute oscillators were now called virtual oscillators (Konno, 1993, p. 141). The Berlin physicist Richard Becker (1887–1955) likewise noted in a paper written in the context of BKS the following year: "these virtual oscillators are substantially identical with the 'substitute oscillators' already introduced by Ladenburg and Reiche" (Becker, 1924, p. 174, note 2).[85] That same year, Herzfeld (1924, p. 350) still used the term 'substitute oscillators,' citing (Ladenburg and Reiche, 1923). The term can also be found, without attribution, in the famous paper by Born and Jordan (1925b, p. 884) on matrix mechanics.[86]

Unlike Ladenburg in 1921, Ladenburg and Reiche prominently mentioned both Bohr's atomic theory and the correspondence principle in their 1923 paper. The authors' understanding and use of the correspondence principle, however, are still tied strongly to Einstein's quantum theory of radiation. Their "correspondence" arguments apply not to individual quantum systems, for which Bohr's correspondence principle was formulated, but to collections of such systems in thermal equilibrium with the ambient radiation.[87] The authors also do not limit their "correspondence" arguments to the regime of high quantum numbers (Ladenburg and Reiche, 1923, especially secs. 4–5, pp. 586–589). These problems invalidate many of the results purportedly derived from the correspondence principle in their paper. Drawing on earlier work by Planck, they derived a result for emission consistent with the correspondence principle (i.e., merging with the classical result in the limit of high quantum numbers), but their attempts to derive similar results for absorption and dispersion were unconvincing. In fact, it may well be that these dubious attempts inspired Van Vleck to formulate correspondence principles for emission and absorption himself (see sec. 5.3 for further discussion).

1921 (Verschaffelt *et al.*, 1923, p. 24), but does not mention Ladenburg and Reiche in this context, attributing the idea to (Slater, 1924) instead.

[85] Quoted in (Konno, 1993, p. 141).

[86] We are grateful to Jürgen Ehlers for drawing our attention to this passage, which is not in the part of (Born and Jordan, 1925b) included in (Van der Waerden, 1968).

[87] That Ladenburg and Reiche did not carefully distinguish between individual systems and collections of such systems becomes more understandable if we bear in mind that they were trying to combine Einstein's quantum theory of radiation and Bohr's correspondence principle. These two elements belong to two different strands in the development of quantum physics, characterized as follows in a concise and perceptive overview of the early history of quantum physics: "The first approach, dominated by the Berlin physicists Einstein, Planck, and Nernst, and by . . . Ehrenfest . . . involved the thermodynamics properties of matter and the nature of radiation . . . The other trend, centered socially in Copenhagen, Munich and Göttingen, consisted of the application of the quantum to individual atoms and molecules" (Darrigol, 2002, p. 336).

Given Bohr's strong interest in the subject, it is not surprising that his first lieutenant Kramers took the next big step in quantum dispersion theory.[88] Formula (8) based on Ladenburg's insights only holds for systems in the ground state. The correspondence principle only applies to highly excited states. Kramers (1924a,b) found that the correspondence principle requires a formula with *two* terms.[89] In our notation—which once again follows (Van Vleck, 1924b, p. 344, eq. 17)—the induced polarization P_r of N_r atoms in a state labeled by the quantum number r is given by

$$P_r(t) = \frac{N_r c^3 E}{32\pi^4} \left(\sum_{s>r} \frac{A_{s\to r}}{\nu^2_{s\to r}(\nu^2_{s\to r} - \nu^2)} - \sum_{t<r} \frac{A_{r\to t}}{\nu^2_{r\to t}(\nu^2_{r\to t} - \nu^2)} \right) \cos 2\pi\nu t, \quad (9)$$

where s and t are the quantum numbers labeling states above and below r, respectively (see secs. 5.1–5.2 and 6.2 for detailed derivations). For high values of r this formula merges with the classical result. In the spirit of the correspondence principle, Kramers took the leap of faith that it holds all the way down to low quantum numbers. If r is the ground state, the second term vanishes and the Kramers formula (9) reduces to the Ladenburg formula (8). Like Ladenburg and Reiche (1923), Kramers interpreted his formula in terms of oscillators, distinguishing between "absorption oscillators" for the first term and "emission oscillators" for the second term (Kramers, 1924a, pp. 179–180). Kramers introduced the characteristic times $\tau_{i\to f}$ inversely proportional to $(e^2/m)\nu^2_{i\to f}$. So instead of factors $\nu^2_{i\to f}$ in the denominators in the two terms in eq. (9), the formula given by Kramers (1924a, p. 179, eq. 5) has factors $(e^2/m)\tau_{i\to f}$ in the numerators.[90] Because of the minus sign in front of the second term, the emission oscillators appear to have negative mass, which is why Kramers also called them "negative oscillators" (ibid.). Van Vleck (1924a, p. 30, note 2) gave a more satisfactory interpretation of this minus sign, interpreting Kramers' formula for dispersion the same way as a formula for absorption he himself had proposed on the basis of Einstein's quantum radiation theory, as giving the net dispersion in a given quantum state as the difference between contributions from transitions to higher and

[88] Hendry (1984, p. 46) goes as far as calling Kramers' theory "the Bohr-Kramers dispersion theory."
[89] In addition to the literature cited in note 63, see (Ter Haar, 1998, pp. 23–30) and, especially, (Konno, 1993) for discussion of Kramers' work on dispersion theory.
[90] The polarization given by Kramers' formula is three times the polarization given by Van Vleck (i.e., by our eq (9)). This is because Kramers assumed that the vibrations in the atom are lined up with the electric field, whereas Van Vleck assumed the relative orientation of vibrations and fields to be random (Van Vleck, 1924b, p. 344, note 25).

transitions to lower states.

Kramers initially only published two notes in *Nature* on his new dispersion formula (Kramers, 1924a,b). Since these were submitted *after* (Bohr, Kramers, and Slater, 1924a), he used the new BKS terminology of 'virtual oscillators' in both of them. As we shall see in sec. 4, this caused considerable confusion, both at the time and in the historical literature, about the relation between BKS and dispersion theory. Kramers' notes, moreover, are short on detail. The first, submitted on March 25, contains only the briefest of hints as to how the new dispersion formula had been found. The second, submitted on July 22 in response to a letter by Minnesota's Gregory Breit (1924b), contains at least an outline of the derivation. Kramers did not get around to publishing the derivation in full until his paper with Heisenberg, completed over the Christmas break of 1924, received by *Zeitschrift für Physik* on January 5, 1925, and published two months later (Mehra and Rechenberg, 1982–2001, Vol. 2, p. 181). According to Slater, however, the basic results had been in place by the time he, Slater, arrived in Copenhagen in December 1923. After dissing Bohr in the letter to Van Vleck quoted in sec. 2.2, Slater goes on to say that

> Kramers hasn't got much done, either. You perhaps noticed his letter to Nature on dispersion [Kramers, 1924a]; the formulas & that he had before I came, although he didn't see the exact application; and except for that he hasn't done anything, so far as I know. They seem to have too much administrative work to do. Even at that, I don't see what they do all the time. Bohr hasn't been teaching at all, Kramers has been giving one or two courses.[91]

Part of what kept Kramers from his work in early 1924, as can be gathered from correspondence with Ladenburg and Reiche, was that his wife had fallen ill. In 1923, the Breslau physicists had already exchanged a few letters about dispersion with their colleague in Copenhagen.[92] On February 28, 1924, Ladenburg gently reminded Kramers that he had promised in January to give his "opinion on dispersion and its quantum interpretation"[93] within a few days. A little over a month later, on April 2, Ladenburg wrote another letter to Kramers, in which he thanked him for sending what must have been either a manuscript or proofs of (Kramers, 1924a) (which only appeared in the May 10 issue of *Nature*) and, apparently having been informed by Kramers that

[91] Slater to Van Vleck, July 27, 1924 (AHQP).
[92] See Reiche to Kramers, May 9, 1923 and December 28, 1923, and Ladenburg to Kramers, December 28, 1923 (AHQP). Kramers' responses, it seems, are no longer extant.
[93] Ladenburg to Kramers, February 28, 1924 (AHQP). This fits with Slater's recollection that Kramers already had his new dispersion formula around Christmas 1923.

the delay had been due to his wife's illness, apologized for his impatience.[94]

Understandably, given the importance of their own work for Kramers' breakthrough, Ladenburg and Reiche were enthusiastic about the new dispersion formula. Immediately after the one sentence devoted to the illness of Kramers' wife, without so much as starting a new paragraph, Ladenburg wrote in his letter of April 2:

> Now your opinion about the dispersion question is of course of the highest interest and I don't want to pass up the opportunity to tell you how much it pleases me that you have managed to give a correspondence derivation of the relation between dispersion and transition probabilities. In this way a solid basis has now been created. Your formula . . . is undoubtedly preferable to ours because of its greater generality. I also agree with you that one cannot extract contributions of the "negative" oscillators from existing experiments.[95]

Ladenburg thus immediately zeroed in on the key experimental question raised by the new formula. In the late 1920s Ladenburg and his collaborators embarked on a ambitious program to verify the second term in the Kramers dispersion formula experimentally. Reiche, writing to Kramers a week later, focused on the theoretical justification of the new formula:

> I wanted to tell you again how delighted I am with your beautiful correspondence derivation. Following Epstein's paper [Epstein, 1922c] and using the Born-Pauli [1922] method, I easily derived the classical expression for P [the polarization] which you indicate in your letter[96] and have also had no trouble reconstructing the correspondence argument for the transition to the quantum formula.[97]

Fearing that few Germans would have access to *Nature*, Ladenburg and Reiche prepared a detailed report on (Kramers, 1924a) for *Die Naturwissenschaften*. In late May, Ladenburg asked Kramers whether he would have any objections if they included a derivation of the new dispersion formula, adding that they

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[94] Ladenburg to Kramers, April 2, 1924 (AHQP). Reiche likewise apologized seven days later (Reiche to Kramers, April 9, 1924 [AHQP]).
[95] Ladenburg to Kramers, April 2, 1924 (AHQP). Ladenburg was not familiar with the BKS paper at this point, neither with the English version which appeared in April 1924, nor with the German translation which only appeared on May 22.
[96] This expression—equivalent eq. (14) below—is not given in (Kramers, 1924a) but does occur in (Kramers, 1924b, p. 199, eq. 2*) (reproduced as eq. (50) in sec. 5.1).
[97] Reiche to Kramers, April 9, 1924 (AHQP).

were not sure how close it was to Kramers' derivation.[98] Kramers welcomed the idea, telling Ladenburg that their derivation would probably not be all that different from his own. He had every intention of writing a longer paper on dispersion and absorption himself, he added, which would obviously include the derivation of his dispersion formula, but recognized that "it will probably be a while before I have time to write such an article; because of lack of time I have not thought through many details and I consequently would not mind it at all if your note appears first."[99] In the end, the editors of *Die Naturwissenschaften* insisted that Ladenburg and Reiche shorten their article.[100] It eventually appeared without the derivation of Kramers' dispersion formula (Ladenburg and Reiche, 1924).

The first ones to publish a full derivation of this important result were Born and Van Vleck. (Born, 1924) was received by *Zeitschrift für Physik* on June 13, 1924, and was published in August; the two-part paper (Van Vleck, 1924b,c) was signed June 19, 1924 and appeared in *The Physical Review* in October. They thus arrived at their results independently.[101] Van Vleck read Kramers' first *Nature* note shortly after he finished his first paper on a correspondence principle for absorption (Van Vleck, 1924a) and when he was about to submit (Van Vleck, 1924b,c). In a footnote added to (Van Vleck, 1924a), he wrote:

> Since the writing of the present article, Dr. H. A. Kramers has published . . . a very interesting formula for dispersion, in which the polarization is imagined as coming not from actual orbits, but from "virtual oscillators" such as have been suggested by Slater and advocated by Bohr. Kramers states that his formula merges asymptotically [i.e., in the limit of high quantum numbers] into the classical dispersion. To verify this in the general case, the writer has computed the classical polarization formula for an arbitrary non-degenerate multiply periodic orbit . . . By pairing together positive and negative terms in the Kramers formula, a differential dispersion may be defined resembling the differential absorption of the present article. It is found that this differential quantum theory dispersion approaches asymptotically the classical dispersion . . . the behavior being very similar to that in the correspondence principle for absorption. This must be regarded as an important argument for the Kramers formula (Van Vleck, 1924a, p. 30).

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[98] Ladenburg to Kramers, May 31, 1924 (AHQP). Ladenburg and Reiche had meanwhile read (Bohr, Kramers, and Slater, 1924b) and, unsurprisingly given the importance of their concept of 'substitute oscillators' for BKS, were instant converts to the theory. For further discussion, see sec. 4.2.

[99] Kramers to Ladenburg, June 5, 1924 (AHQP).

[100] Ladenburg to Kramers, June 8, 1924 (AHQP).

[101] See secs. 2.4 and 5.2 for quotations from correspondence between Born and Van Vleck in October-November 1924 pertaining to these papers.

It was not clear to Van Vleck on the basis of Kramers' note exactly what Kramers had and had not yet done. Van Vleck thought that his calculations extended Kramers' results. As he wrote to Kramers in September 1924:

> I am enclosing under separate cover a reprint [Van Vleck, 1924a] which I think may be of interest to you, especially the footnote at the very end, where I mention some computations I have made relative to your dispersion formula. A longer paper [Van Vleck 1924b, c] is now in proof, and should appear shortly in the Physical Review. This more extensive article was ready to send to the printer about the time we received the copy of Nature containing your dispersion formula. In your note [Kramers, 1924a] I did not understand you to state how generally you had verified the asymptotic connection with the classical dispersion from the actual orbit, and it immediately occurred to me that this question could easily be investigated by the perturbation theory method I had previously developed in connection with what I call the "correspondence principle for absorption". I therefore inserted two sections (# 6 and # 15 . . .) showing that your formula merged into the classical one.
>
> Inasmuch as the classical dispersion formula had apparently not been developed for the general non-degenerate multiply periodic orbit, and as you did not give this in your note to Nature, I conjectured that you had verified the asymptotic connection only in special cases, such as a linear oscillator, so that my computations on dispersion would not be a duplication of what you had done. However, while visiting at Cambridge, Mass. last week I learned from Dr. Slater that your calculation of the asymptotic connection was almost identical with my own in scope and generality. I have therefore altered the proof of my Physical Review article to include a note [Van Vleck 1924b, 345] stating that you have also established the correspondence theorem in the general case. I hope this is satisfactory to you. The concept and introduction of the virtual-oscillator formula is entirely yours, and I refer always to the "Kramers dispersion formula", but I had developed the perturbation theory method for absorption etc. prior to learning of any of your work.
>
> I am sorry that we are again apparently duplicating each other in some of our work. Slater tells me that by extending your computations he has independently derived an absorption formula similar to mine, and also noted the asymptotic connection of the two theories in this case. [102]

As in the case of Ladenburg and Reiche half a year earlier, Kramers did not seem to mind at all that Van Vleck was poaching on his preserves. He generously wrote back to Van Vleck: "Your note on absorption made me much pleasure and I think it very just of Providence that you got it published before

[102] Van Vleck to Kramers, September 22, 1924 (AHQP).

hearing of our work."[103]

The construction of the dispersion formula (9) requires as a prelude to the application of the correspondence principle, a derivation of the classical formula for the dipole moment of an arbitrary (non-degenerate) multiply-periodic system. This is where Ladenburg and Reiche (1923) came up short, even though, as we saw above, Reiche was able to reconstruct the derivation once Kramers had outlined it for him. Kramers and Van Vleck, like Epstein before them, used canonical perturbation techniques from celestial mechanics to derive this classical formula. In Part Two of our paper, closely following the classical part of Van Vleck's two-part paper (Van Vleck, 1924c), we shall present a detailed derivation of this crucial classical formula, for the special case of the harmonic oscillator in sec. 5.1 and for a general non-degenerate multiply-periodic system in sec. 6.2. Guided by the correspondence principle and introducing the A and B coefficients we then construct a quantum formula that merges with the classical formula for high quantum numbers (see secs. 5.1 and 6.2).[104] Here we summarize the main steps of this derivation.

In general coordinates (q_i, p_i), Hamilton's equations are:

$$\dot{q}_i = \frac{\partial H}{\partial p_i}, \quad \dot{p}_i = -\frac{\partial H}{\partial q_i}, \tag{10}$$

where dots indicate time derivatives. Given the Hamiltonian H of some multiply-periodic system, one can often find special coordinates (w_i, J_i), so-called *action-angle variables*, in which Hamilton's equations take on a particularly simple form:

$$\dot{w}_i = \frac{\partial H}{\partial J_i} = \nu_i, \quad \dot{J}_i = -\frac{\partial H}{\partial w_i} = 0. \tag{11}$$

The angle variables, $w_i = \nu_i t$, give the characteristic frequencies of the system; the (conserved) action variables are subject to the Bohr-Sommerfeld quantum condition, $J_i = n_i h$. This, of course, is why these variables are of particular interest in this context.

Suppose we have a Hamiltonian H that is the sum of H_0, describing some multiply-periodic system representing an electron orbiting the nucleus of an atom in the Bohr-Sommerfeld theory (or, an inner planet like Mercury orbiting the sun), and $H_{\text{int}} = eEx\cos 2\pi\nu t$, a small perturbation describing the

[103] Kramers to Van Vleck, November 11, 1924 (AHQP).

[104] Recall that Van Vleck actually did it the other way around: he started with the quantum formula and checked that this formula merges with the classical formula in the correspondence limit (see note 17).

interaction of this system with a weak periodic electric field in the x-direction (or, the periodic weak gravitational interaction with a distant outer planet). To find the induced polarization responsible for dispersion in this system we need to calculate the coherent part Δx_{coh} of the displacement caused by the perturbation (cf. eqs. (2)–(6) in sec. 3.1). We assume that the unperturbed system can be solved in action-angle variables, which means that $x(t)$ in the absence of H_{int} can be written as a Fourier series:

$$x(t) = \sum_{i,\tau_i} A_{\tau_i}(J_l) e^{2\pi i \tau_i w_i} \tag{12}$$

(where i runs from 1 to 3 and τ_i runs over all positive and negative integers). The complex amplitudes have to satisfy the conjugacy relations $A_{\tau_i} = A^*_{-\tau_i}$ to ensure that $x(t)$ is real. Assuming the interaction is switched on at $t = 0$, we can use Hamilton's equations in action-angle variables—still those for H_0 rather than those for the full Hamiltonian H [105]—to calculate Δw_i and ΔJ_i due to the perturbation. We insert the results into

$$\Delta x = \sum_k \left(\frac{\partial x}{\partial J_k} \Delta J_k + \frac{\partial x}{\partial w_k} \Delta w_k \right), \tag{13}$$

and collect the coherent terms (i.e., all terms with a factor $e^{2\pi i \nu t}$). The result is:

$$\Delta x_{\text{coh}} = 2eE \sum_{i,k,\tau_i} \tau_k \frac{\partial}{\partial J_k} \left(\frac{\tau_i \nu_i}{\nu^2 - (\tau_i \nu_i)^2} |A_{\tau_i}(J_l)|^2 \right) \cos 2\pi \nu t. \tag{14}$$

For the special case of a charged harmonic oscillator, this expression reduces to the simple expression (6) found earlier (as we shall show in detail at the end of sec. 5.1).

We now translate this classical formula into a quantum formula. The idea is to construct a quantum formula that merges with the classical formula in the limit of high quantum numbers. This is done in three steps. For high values of the quantum number i, the derivatives $\partial/\partial J_i$ can be replaced by difference quotients, [106] the square of the amplitudes $A_{\tau_i}(J_l)$ by transition probabilities $A_{i \to j}$ (where $|i - j|$ is small compared to i), and orbital frequencies ν_i by transition frequencies $\nu_{i \to j}$. We then take the leap of faith that the resulting formula holds for all quantum numbers. Multiplying by the charge $-e$ and

[105] Here Van Vleck's calculation differs from those of Born (1924) or Kramers and Heisenberg (1925).

[106] This replacement is known as "Born's correspondence rule." In fact, both Kramers and Van Vleck found it independently of Born. We return to this point in sec. 5.2.

the number of atoms N to get from the coherent part of the displacement of one atom to the polarization of a group of atoms, we arrive at the Kramers dispersion formula (9).

3.5 Heisenberg's Umdeutung *and dispersion theory*

The Kramers dispersion formula was a crucial step in the transition from the old quantum theory to matrix mechanics, and thereby in the transition from functions on classical phase spaces to operators on Hilbert spaces. As Kramers pointed out in his second *Nature* note, the formula

> only contains such quantities as allow of a direct physical interpretation on the basis of the fundamental postulates of the quantum theory ... and exhibits no further reminiscence of the mathematical theory of multiple [sic] periodic systems (Kramers, 1924b, p. 311)

This point is amplified in the Kramers-Heisenberg paper:

> we shall obtain, quite naturally, formulae which contain only the frequencies and amplitudes *which are characteristic for the transitions*, while all those symbols which refer to the mathematical theory of periodic systems will have disappeared (Kramers and Heisenberg, 1925, p. 234, our emphasis).

Orbits do not correspond to observable quantities, but transitions do, namely to the frequency $\nu_{i\to f}$ of the emitted radiation, and, through the Einstein coefficients $A_{i\to f}$, to its intensity. In the introduction of his *Umdeutung* paper, Heisenberg (1925c) explained that he wanted "to establish a theoretical quantum mechanics, analogous to classical mechanics, but in which only relationships between observable quantities occur" (p. 262). In the next sentence he identified the Kramers dispersion theory as one of "the most important first steps toward such a quantum-theoretical mechanics" (ibid.).

Rather than using classical mechanics to analyze features of electron orbits and translating the end result into a quantum formula, as Kramers and others had done (cf. eqs. (10)–(14) above), Heisenberg translated the Fourier series for the position of an electron that forms the starting point of such classical calculations into a quantum expression. He replaced the amplitudes and frequencies by two-index quantities, referring to the initial and final state of a quantum transition, respectively, and thus replaced classical position by an array of numbers associated with transitions between states. Reinterpreting rather than replacing the old theory, he assumed that these new quantities would satisfy all the familiar relations of Newtonian mechanics. Note that Heisenberg thus formulated a new theory directly in terms of transition quantities without bothering to find a representation for the states connected by the transitions.

The Bohr-Sommerfeld quantization condition (1) has the form of a restriction on orbits in phase space. With the elimination of orbits, it could no longer be used, at least not in its original form. As Heisenberg recalls in his AHQP interview:

> I had, of course, to think about the quantum condition. And that was an important point. But there I knew so much from Copenhagen how important this Thomas-Kuhn sum rule was. That took some time. That I think I had done in Göttingen, [I] had seen how I could translate the Thomas-Kuhn sum rule into what I call a quantum mechanical statement, into a statement in which only differences occurred. I did not see that it was a commutation rule [but with this translation] I can bring this sum rule into my whole scheme and then this sum rule actually fixes everything. I could see that this fixes the quantization.[107]

The Thomas-Kuhn sum rule, a corollary of the Kramers dispersion formula (see sec. 7.1 for a derivation in modern quantum mechanics), had been found independently by Werner Kuhn (1899–1963) (1925) in Copenhagen[108] and by Willy Thomas (1925) in Breslau.[109] Kuhn (i.e., Thomas Kuhn) pressed Heisenberg a little on how he had settled on this rule as his fundamental quantization condition: "Using the Kuhn-Thomas [sic] rule is a stroke of genius but one supposes that there were a lot of other intermediate attempts." Apparently there were none. Heisenberg insisted:

> No, I would say it was rather trivial for the following reasons: First of all, there was the integral pdq . . . I felt that perhaps only the difference of integral pdq between one quantum state and the next quantum state is an important thing. So I actually felt, "Well, perhaps I should write down integral pdq in one state minus integral pdq in the neighboring state." Then I saw that if I write down this and try to translate it according to *the scheme of the dispersion theory*, then I get the Thomas-Kuhn sum rule. And that is the point. Then I thought, "Well, that is apparently the way how it is done."[110]

[107] P. 10 of the transcript of session 7 of the AHQP interview with Heisenberg

[108] The publication of Kuhn's paper had been delayed in typical Copenhagen fashion: "A paper on the summation rule had been submitted to Prof. Bohr and Prof. Kramers about half a year before the final one, but it was rejected at that time because it contained besides the main good argument some unsuitable passages" (Werner Kuhn to Thomas Kuhn, May 3, 1962 [included in the folder on Kuhn in the AHQP])

[109] Thomas was a student of Reiche in Breslau who died young of tuberculosis. See p. 14 of the transcript of the third session of the AHQP interview with Reiche.

[110] P. 10 of the transcript of session 7 of the AHQP interview with Heisenberg (our

In other words, following the general recipe introduced in the *Umdeutung* paper for the translation of classical formulae into quantum-mechanical ones—"the scheme of the dispersion theory"—Heisenberg (1925c, p. 268) was able to convert *a derivative of* the Bohr-Sommerfeld condition into an equation that contains only amplitudes and frequencies. Since Heisenberg's theory only deals with transitions between states, the absolute value of the action J does not matter; only the difference in J-value between two states does.

The sum rule is sometimes called the Thomas-Kuhn-Reiche sum rule because Reiche and Thomas (1925) were the first to publish a detailed derivation of it in a paper *submitted* to the *Zeitschrift für Physik* in early August 1925 about a month before (Heisenberg, 1925c) *appeared* in the same journal. In formulating the goal of their paper, Reiche and Thomas not only used the term 'Umdeutung' in very much the same way as Heisenberg in his *Umdeutung* paper, they also explicitly tied this usage to Kramers' dispersion theory:

> We use . . . the correspondence principle in the same way in which it was applied by Kramers in the derivation of the dispersion formula by reinterpreting (*umdeuten*) the mechanical orbital frequencies as radiation frequencies, the Fourier coefficients as the "characteristic amplitudes" that determine the quantum radiation, and, finally, in analogy to the Bohr frequency condition,[111] differential quotients as difference quotients. In the realm of high quantum numbers the classical and quantum-theoretical representations become identical. We try to arrive at a general relation, by maintaining the reinterpretation (*Umdeutung*) of classical quantities into quantum-theoretical ones for all quantum numbers (Reiche and Thomas, 1925, pp. 511–512).

In view of the tendency of European theorists to neglect American contributions (see sec. 2.4), it is also interesting to note that Reiche and Thomas (1925, p. 513) cite (Van Vleck, 1924b).

Although he failed to recognize the importance of the result at the time, Van Vleck had, in fact, been the first to find the sum rule (Sopka, 1988, p. 135, note 184). As he wrote in his NRC *Bulletin*:

> Eq. (62a) [a version of the sum rule] appears to have been first incidentally suggested by the writer [Van Vleck 1924c, pp. 359–360, footnote 43] and then was later and independently much more strongly advanced by Thomas . . . Kuhn . . . and Reiche and Thomas (Van Vleck, 1926, p. 152).

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emphasis).

[111]In the limit of high quantum numbers, the Bohr frequency condition, $\nu_{i\to j} = (E_i - E_j)/h$, merges with the relation $\nu_i = \partial H/\partial J_i$ (cf. eq. (11)). Van Vleck (1924b, p. 333) calls this the correspondence theorem for frequencies.

Van Vleck is referring to a footnote in the section on dispersion in the classical part of his paper. In this footnote he mentioned two objections that explain why he did not put greater emphasis on the sum rule himself. Van Vleck's idea—which he calls "tempting (but probably futile)" (Van Vleck, 1924c, p. 359, footnote 43)—was that the sum rule would allow him to compute the Einstein A coefficients. He was under the impression, however, that "such a method is hard to reconcile with the [experimental] work of F. C. Hoyt [1923, 1924]" on X-ray absorption and that it "would lead to transitions from positive to negative quantum numbers, which can scarcely correspond to any physical reality" (ibid.).

As Heisenberg (1925c, pp. 269–270) shows briefly in his paper, the sum rule follows from the Kramers dispersion formula (9) if one takes the limit in which the frequency ν of the incident radiation is much greater than any of the absorption frequencies $\nu_{i\to j}$ (see sec. 7.1). That the quantization condition obtained by massaging the Bohr-Sommerfeld condition also follows from the Kramers dispersion theory, widely recognized as one of the most secure parts of the old quantum theory, must have bolstered Heisenberg's confidence in the translation procedure of his *Umdeutung* paper. It was left to Born and Jordan (1925b) to extract the now standard commutation relations for position and momentum from the Thomas-Kuhn sum rule (in sec. 7.1 we shall show in detail how this is done). That Heisenberg stopped short of making this move, as we shall argue in sec. 7.1, is largely because he was thinking in terms of the positions and velocities of the Lagrangian formalism rather than in terms of the positions and momenta of the Hamiltonian formalism.

Although Heisenberg thus relied heavily on dispersion theory in his *Umdeutung* paper, he gave his positivist methodology pride of place. This philosophical outlook probably came from a variety of sources. Pauli, Heisenberg's fellow student and frequent discussion partner (both in person and in writing), was a devoted follower of his godfather Ernst Mach (1838–1916).[112] As Pauli had written to Bohr, for instance, on December 12, 1924:

> We must not ... put the atoms in the shackles of our prejudices (of which in my opinion the assumption of the existence of electron orbits in the sense of the ordinary kinematics is an example); on the contrary, we must adapt our concepts to experience (Bohr, 1972–1996, Vol. 5, pp. 35–36).

We already indicated in sec. 1.1 that Heisenberg himself later claimed that his positivist attitude came in part from his reading of Einstein's 1905 special relativity paper.[113] His biographer David Cassidy (1991, p. 198) makes the suggestive observation that Born and Jordan (1925a, p. 493), in a paper completed by June 11, 1925, not only emphasized the observability principle but

[112] On Pauli's positivism, see, e.g., (Hendry, 1984, pp. 19–23) and (Gustavson, 2004).
[113] See, e.g., (Holton, 2005, pp. 26–31) for discussion.

also appealed to Einstein's analysis of distant simultaneity in support of it.

As Helge Kragh (1999) notes: "there was no royal road from the observability principle to quantum mechanics" (p. 162). This truism is nicely illustrated by a conversation between Einstein and Heisenberg reported years later by the latter. The following exchange supposedly took place in Berlin in the spring of 1926:

> "But you don't seriously believe," Einstein protested, "that none but observable magnitudes must go into a physical theory?" "Isn't that precisely what you have done with relativity?" I asked in some surprise ... "Possibly I did use this kind of reasoning," Einstein admitted, "but it is nonsense all the same" (Heisenberg, 1971, p. 63). [114]

With his S-matrix program in the 1940s, [115] Heisenberg once again tried to force a theoretical breakthrough by restricting himself to observable quantities, this time with the qualification that he had taken to heart Einstein's lesson that, in the end, it is the theory that determines what the observables are. Heisenberg (1971, p. 63) has Einstein make this point a few sentences after the passage quoted above and acknowledges it as a source of inspiration for his 1927 uncertainty principle. Nearly two decades after the *Umdeutung* paper, Heisenberg (1943) wrote: "in this situation it seems useful to raise the question which concepts of the present theory can be retained in the future theory, and this question is roughly equivalent to a different question, namely which quantities of the current theory are "observable" ... Of course, it will always only be decided by the completed theory which quantities are truly "observable"" (p. 514).

As Einstein complained in 1917 in a letter to his friend Michele Besso (1873–1955), referring to the excessive Machian positivism of their mutual acquaintance Friedrich Adler (1879–1960): "He is riding the Machian nag [*den Machschen Klepper*] to exhaustion." In a follow-up letter he elaborated: "It cannot give birth to anything living, it can only stamp out harmful vermin." [116] This is true in the case of matrix mechanics as well. Heisenberg's positivism would have been perfectly sterile if it had not been for Kramers' dispersion theory. In that context, positivism was not a blanket injunction against unobservable quantities in general but was directed at a specific set of increasingly prob-

[114] Quoted and discussed, for instance, in (MacKinnon, 1977, p. 185) and in (Holton, 2005, pp. 30–31). For other versions of the same anecdote, see (Heisenberg, 1983, pp. 113–114) and pp. 18–19 of the transcript of session 5 of the AHQP interview with Heisenberg.

[115] See (Pais, 1986, 497–503), (Dresden, 1987, 453–458), and, especially, (Cushing, 1990) for discussion. See also pp. 20–21 of the transcript cited in note 114.

[116] Einstein to Besso, April 29 and May 13, 1917, respectively (Einstein, 1987–2006, Vol. 8, Docs. 331 and 339). For further discussion, see, e.g., (Holton, 1968).

lematic unobservables, the electron orbits of the Bohr-Sommerfeld theory.

4 The Bohr-Kramers-Slater (BKS) theory as a detour on the road from dispersion theory to matrix mechanics

4.1 Virtual oscillators and virtual radiation

Kramers presented his work on dispersion theory in the context of the BKS theory, not just in the two preliminary notes to *Nature* discussed in sec. 3.4, but also in the authoritative exposition of his dispersion theory in the paper with Heisenberg. In the abstract of this paper, the authors announce that

> [t]he arguments are based throughout on the interpretation of the connection of the wave radiation of the atom with the stationary states advocated in a recent paper by Bohr, Kramers and Slater [1924a,b], and the conclusions, should they be confirmed, would form an interesting support for this interpretation (Kramers and Heisenberg, 1925, p. 223).[117]

It should thus come as no surprise that the Kramers dispersion theory has been portrayed as an application of the BKS theory in most older and even in some more recent historical literature.[118] Jammer (1966), for instance, writes that BKS "was the point of departure of Kramers's detailed theory of dispersion" (p. 184). Mara Beller (1999) still characterized (Kramers and Heisenberg, 1925) as a paper that "spelled out, in a rigorous mathematical way, the ideas only roughly outlined in the presentation of Bohr, Kramers, and Slater" (p. 23). More than a decade earlier, Dresden (1987, pp. 144–146, pp. 220–221) had in fact already set the record straight.[119] Darrigol (1992, p. 225) duly emphasizes that *the Kramers dispersion theory was developed before and independently of BKS*. Even before Dresden, Hendry (1981) had already made it clear that *BKS got its virtual oscillators from dispersion theory—the substitute oscillators of (Ladenburg and Reiche, 1923)—and not the other way around*. We briefly review the evidence in support of the italicized claims above.

[117]We use the translation of Stolzenburg (1984, p. 87) at this point, which is more accurate than the standard translation in (Van der Waerden, 1968, p. 223).

[118]There is an extensive literature on BKS; see, e.g., (Klein, 1970, pp. 23–39), (Stuewer, 1975, pp. 291–305), (Hendry, 1981), the dissertation of Neil Wasserman (1981), (Mehra and Rechenberg, 1982–2001, Vol. 1, sec. V.2), the essay by Klaus Stolzenburg (1984) in (Bohr, 1972–1996, Vol. 5, pp. 3–96), and (Dresden, 1987, pp. 159–215).

[119]See (Dresden, 1987, p. 221) for a helpful chronology of events in 1923–1925 pertaining to BKS and dispersion theory.

We know from the passage quoted in sec. 3.4 from a letter from Slater to Van Vleck that by the time the former arrived in Copenhagen around Christmas 1923 Kramers already had his dispersion formula. Kramers must have used the substitute oscillators of Ladenburg and Reiche (1923) at that point even though by the time he finally got around to publishing his formula he called them virtual oscillators (see sec. 3.4). Slater's arrival in Copenhagen marks the lower limit for the birth of the BKS theory. The theory, after all, grew around an idea that Slater hit upon shortly before he left for Europe late that year.[120] Slater suggested that the wave and particle properties of light might be reconciled by having an electromagnetic field guide corpuscular light quanta.[121] Bohr and Kramers cannibalized Slater's idea and stripped it of all reference to light quanta. Against his better judgment—as he insisted decades later in a letter of November 4, 1964 to van der Waerden (1968, p. 13)—Slater went along and his idea entered the literature via the BKS paper. In a short letter sent to *Nature* a week after this joint paper had been submitted, Slater explained how Bohr and Kramers had convinced him of their point of view. Accordingly, he presented his idea couched in BKS terms:

> Any atom may, in fact, be supposed to communicate with other atoms all the time it is in a stationary state, by means of a virtual field of radiation originating from oscillators having the frequencies of possible quantum transitions and the function of which is to provide for the statistical conservation of energy and momentum by determining the probabilities for quantum transitions (Slater, 1924, p. 307).

The final clause about the statistical conservation of energy and momentum was foisted upon Slater by Bohr and Kramers.[122] Bohr had been contemplating such a move for several years, as can be inferred, for instance, from correspondence with Ehrenfest in 1921 in connection with the third Solvay congress held that year (Klein, 1970, p. 19) and with Darwin in 1922 (Stolzenburg, 1984, pp. 13–19). Slater's concept of virtual radiation emitted while an atom is in a stationary state fit nicely with Bohr's tentative ideas concerning the mechanism of emission and absorption of radiation. In secs. 3.2–3.3, we quoted various comments by Bohr on dispersion from the period 1916–1923 showing how he came to embrace the notion that an atom interacts with radiation like a set of oscillators.

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[120]See Slater to his mother, November 8, 1923 (quoted in Dresden, 1987, p. 161); Slater to Kramers, December 8, 1923 (AHQP). For discussions of Slater's idea, see (Klein, 1970, p. 23), (Stuewer, 1975, pp. 291–294), (Hendry, 1981, pp. 213–214), (Stolzenburg, 1984, pp. 6–11), and (Darrigol, 1992, pp. 218–219).
[121]Slater was probably unaware that Einstein and Louis de Broglie (1892–1987) had already made similar suggestions (Hendry, 1981, p. 199; Darrigol, 1992, p. 218).
[122]See also (Bohr, Kramers, and Slater, 1924a, p. 160).

The concept of virtual oscillators is often attributed to Slater, not just by later historians (see, e.g., Stuewer, 1975, p. 291, p. 303) but also by his contemporaries. In the abstract of (Van Vleck, 1924b), for instance, we read that the Kramers dispersion formula "assumes the dispersion to be due not to the actual orbits but to Slater's 'virtual' or 'ghost' oscillators having the spectroscopic rather than orbital frequencies" (p. 330).[123] In the BKS paper itself, however, the concept is unambiguously attributed to Ladenburg:[124]

> The correspondence principle has led to comparing the reaction of an atom on a field of radiation with the reaction on such a field which, according to the classical theory of electrodynamics, should be expected from a set of 'virtual' harmonic oscillators with frequencies equal to those determined by [$h\nu = E_1 - E_2$] for the various possible transitions between stationary states.[125] Such a picture has been used by Ladenburg[126] in an attempt to connect the experimental results on dispersion quantitatively with considerations on the probability of transitions between stationary states (Bohr, Kramers, and Slater, 1924a, pp. 163–164).

As we saw in sec. 3.3, Ladenburg and Reiche in turn attributed the idea to Bohr. In 1924, for instance, they wrote:

> Formally, we can describe the relation [between oscillator strengths and transition probabilities] following an assumption introduced by Bohr [1923b, pp. 161–162], by imagining that the atom responds to external radiation like a system of electrical oscillators, whose characteristic frequencies ν agree with the emitted or absorbed frequencies in *possible* quantum transitions (Ladenburg and Reiche, 1924, p. 672).

In the next sentence they use their own term "substitute oscillators" (in quotation marks) and add: "(now called "virtual oscillators")" (ibid.). Likewise, in the introduction of the opening installment of a series of papers on the experimental verification of the Kramers dispersion formula, Ladenburg talks about "the "substitute oscillators"," which were introduced, "at Bohr's suggestion, as the carriers of the scattered radiation needed for dispersion" (Ladenburg,

[123]See also (Van Vleck, 1924a, p. 30), quoted in sec. 3.4, and (Van Vleck, 1926, p. 163).

[124]The mistakes with the prepositions in the passage below ('reaction on' instead of 'reaction to' and 'considerations on' instead of 'considerations of') would tend to support Slater's claim that the paper was "written entirely by Bohr and Kramers" (Slater to Van Vleck, July 27, 1924, quoted in sec. 2.2).

[125]At this point, the authors refer to Ch. III, sec. 3 of the (English translation of) (Bohr, 1923b), the section in which (Ladenburg, 1921) is discussed and which triggered the correspondence between Bohr and Ladenburg discussed in sec. 3.3.

[126]At this point, the authors append a footnote referring to (Ladenburg, 1921) and (Ladenburg and Reiche, 1923).

1928, p. 16).[127]

Bohr had communicated the idea in a letter to Ladenburg (see sec. 3.3). This may explain why, when interviewed for the AHQP, Reiche did not remember who originally came up with it:

> I do not know whether we or Kramers first used this terminology of virtual oscillators . . . It might be it is Kramers. If it was Kramers then we certainly at once incorporated it into our thinking.[128]

In his AHQP interview with Slater, Kuhn also asked about virtual oscillators:

> to what extent did that come from [the BKS] paper, to what extent does it really go back to the Ladenburg, and Ladenburg-Reiche [papers]? It could have grown out [of the] Ladenburg and Ladenburg-Reiche papers, yet my impression from the literature is that there was little done with that until after the Bohr-Kramers-Slater paper.[129]

Slater concurred, though his comments would have been more valuable had he not been asked such a leading question:

> I think that's true. Of course, I was very familiar with the Ladenburg-Reiche things,[130] so was Bohr. I think that we helped popularize it in a sense. Of course, this also came at the same time, approximately, that Kramers was working on his dispersion formula. That again is operating with things very much like the virtual oscillator, so they all seem to hang together, and I think it was a combination of the oscillators from our paper, from the Ladenburg-Reiche, and the Heisenberg-Kramers dispersion that really set them in operation.[131]

Despite the loaded question that elicited this response and even though Slater is wrong to suggest that BKS and Kramers' dispersion theory were developed independently of the earlier work of Ladenburg and Reiche, the overall characterization of the situation seems to be accurate. BKS officially sanctioned the dual representation of the atom as simultaneously a quantum system à la Einstein and Bohr and a set of oscillators à la Helmholtz, Lorentz and Drude.

[127]The passage from which these clauses are taken is quoted in full at the beginning of sec. 7.
[128]See p. 11 of the transcript of session 3 of the interview with Reiche. It could be, however, that Reiche was only referring to the new *term* for the Bohr-Ladenburg-Reiche concept of substitute oscillators.
[129]P. 34 of the transcript of the first session of the AHQP interview with Slater.
[130](Ladenburg, 1921) and (Ladenburg and Reiche, 1923) are cited in (Slater, 1925a, p. 397).
[131]Ibid., pp. 34–35.

This dual picture had been implicit in (Ladenburg, 1921). It was made explicit, under Bohr's influence, in (Ladenburg and Reiche, 1923). That it was endorsed by the highest authorities in Copenhagen undoubtedly helped its dissemination. Even so it was typically presented with some trepidation. In his second *Nature* note, Kramers tried to pass it off as merely a matter of words: [132]

> In this connexion it may be emphasized that the notation 'virtual oscillator' used in my former letter [Kramers, 1924a] does not mean the introduction of any additional hypothetical mechanism, but is meant only as a terminology suitable to characterise certain main features of the connexion between the description of optical phenomena and the theoretical interpretation of spectra (Kramers, 1924b, p. 311).

Van Vleck was more upfront:

> The introduction of these virtual resonators is, to be sure, in some ways very artificial, but is nevertheless apparently the most satisfactory way of combining the elements of truth in both the classical and quantum theories. In particular this avoids the otherwise almost insuperable difficulty that it is the spectroscopic rather than the orbital frequencies . . . which figure in dispersion (Van Vleck, 1924b, p. 344).

Despite such disclaimers, Kramers and Van Vleck—as well as Slater, Born, Breit and others working in the general area of dispersion theory in 1924–1925—used a model of the atom in which the electron orbits of the Bohr-Sommerfeld theory were supplemented by an "orchestra of virtual oscillators" [133] with characteristic frequencies corresponding to each and every transition that an electron in a given orbit can undergo. Thanks to virtual oscillators—to paraphrase Heisenberg's succinct statement to van der Waerden (1968, p. 29) in 1963—at least *something* in the atom was vibrating with the right frequency again.

The dual representation of physical systems (of electrons rather than atoms in this case) was also key to the BKS explanation of the Compton effect. BKS was Bohr's last stand against light quanta after the Compton effect had finally convinced most other physicists that they were unavoidable (Klein, 1970, p. 3). [134] BKS explains the Compton effect without light quanta. It attributes the frequency shift between incoming and scattered X-rays to a Doppler shift

[132] In the work that led to (Kramers and Heisenberg, 1925), however, Kramers, according to Hendry (1981), "ignored their virtual nature altogether and treated the oscillator model as naively as he had the orbital model" (p. 202).

[133] The term "virtual orchestra" comes from (Landé, 1926, p. 456) (Jammer, 1966, p. 187).

[134] Cf. our comments in the introduction to sec. 3.

in the X-ray wave fronts instead. Compton (1923) thought this option was ruled out because, as he showed in his paper, the recoil velocity needed to get the right Doppler shift is different from the recoil velocity needed to ensure conservation of energy and momentum in the process, and one and the same electron cannot recoil with two different velocities. In the BKS theory, however, there is room for two recoil velocities, one for the electron itself, one for the orchestra of virtual oscillators associated with it.[135] The Compton effect can be interpreted as a Doppler shift if the appropriate recoil velocity is assigned to the virtual oscillators. Energy and momentum can be conserved if a different recoil velocity is assigned to the electrons themselves. Bohr and his co-authors wasted few words on the justification of this startling maneuver:

> That in this case the virtual oscillator moves with a velocity different from that of the illuminated electrons themselves is certainly a feature strikingly unfamiliar to the classical conceptions. In view of the fundamental departures from the classical space-time description, involved in the very idea of virtual oscillators, it seems at the present state of science hardly justifiable to reject a formal interpretation as that under consideration as inadequate (Bohr, Kramers, and Slater, 1924a, p. 173).

This is almost as bad as pieces of glass dragging along different amounts of ether for different colors of light in early-19th-century ether theory (see sec. 3.1)!

The problem carries over to the dispersion theory based on the dual representation of atoms in terms of classical orbits and virtual oscillators, as is acknowledged, if only in passing, by Kramers and Heisenberg (1925): "We shall not discuss in any detail the curious fact that the centre of these spherical waves moves relative to the excited atom" (p. 229). This exacerbated the problem of the Bohr-Sommerfeld orbits in the theory. Not only were they responsible for the discrepancy between orbital frequencies and radiation frequencies, they also make it harder to picture an atom in space and time. After all, the system of electron orbits does not even move in concert with its orchestra of virtual oscillators.

Edward MacKinnon (1977, 1982) has suggested that the resulting problem of combining different pictures of the atom into one coherent picture forced Heisenberg to make a choice between them (see also Beller, 1999, p. 23). Since the virtual oscillators carry all the physical information while the electron orbits are completely unobservable, the choice is obvious. MacKinnon (1977, p.

[135]What Compton (1923) actually said in his paper is very suggestive of this option: "It is clear ... that so far as the effect on the wave-length is concerned, we may replace the recoiling electron by a scattering electron" with an "effective velocity" different from that of the recoiling electron (p. 487; quoted and discussed in Stuewer, 1975, p. 230).

138) has gone as far as describing Heisenberg's *Umdeutung* paper as proposing a theory of virtual oscillators. Of course, there is no explicit reference to virtual oscillators anywhere in the *Umdeutung* paper. MacKinnon (1977, pp. 155–156, 162, 177) speculates that this is because Heisenberg suppressed all talk about virtual oscillators as a response to Pauli's objections to the "virtualization" of physics. [136] We shall return to the relation between BKS and Heisenberg's work in sec. 4.3.

Pauli had originally promised not to subvert Bohr's efforts to get the physics community to accept the term 'virtual' as used in the context of BKS. Working on the German translation of the paper (Bohr, Kramers, and Slater, 1924b), Bohr was anxious to ensure that Pauli approved of "the words "communicate" and "virtual", for after lengthy consideration, we have agreed here on these basic pillars of the exposition." [137] In typical Bohr fashion, he first announced that the manuscript would be submitted that same day and that he would enclose a copy, then added a postscript saying that there had been further delays and that it would be sent later. [138] Amused, Pauli wrote back a few days later:

> I laughed a little (you will certainly forgive me for that) about your warm recommendation of the words "communicate" and "virtual" and about your postscript that the manuscript is still not yet completed. On the basis of my knowledge of these two words (*which I definitely promise you not to undermine*), I have tried to guess what your paper may deal with. But I have not succeeded. [139]

The term 'virtual' also puzzled the group of physicists in Ann Arbor studying the BKS paper with Bohr's former associate Klein, who wrote to Bohr on June 30, 1924: "Colby [cf. note 37], who is also most interested in it, asked me about the meaning of the term 'virtual radiation'" (Stolzenburg, 1984, p. 29).

Exactly what does the 'virtual' in virtual oscillator and virtual radiation mean? Virtual oscillators can be thought of in analogy to virtual images in geometric optics. Just as the light reflected from a mirror appears to come from an imaginary point behind the mirror, the light scattered by an atom appears to come from an imaginary oscillator. This analogy, however, is nowhere to be found in the BKS paper. Whatever its exact meaning, the designation 'virtual' does serve as a warning that these oscillators are not just classical oscillators.

[136] In a letter of January 8, 1925, Heisenberg told Bohr that Pauli did not believe "in virtual oscillators and is outraged at the 'virtualization' of physics" (MacKinnon, 1977, p. 156).

[137] Bohr to Pauli, February 16, 1924 (Bohr, 1972–1996, Vol. 5, p. 409).

[138] Contrary to what is suggested by these delays, the German translation simply follows the English original.

[139] Pauli to Bohr, February 21, 1924 (Bohr, 1972–1996, Vol. 5, p. 412; our emphasis).

The authors warn, for instance, that "the absorption and emission of radiation are coupled to different processes of transition, and thereby to different virtual oscillators" (Bohr, Kramers, and Slater, 1924a, p. 171).

Unlike the light coming from virtual images in geometric optics, the radiation coming from virtual oscillators is *also* called virtual in the BKS paper. Again, it is not clear why. As the analogy with geometric optics shows, that a source is virtual does not mean that the radiation must be virtual as well. In Slater's original conception, the radiation might be called virtual in the sense that the light quanta are the primary reality and that the radiation is there only to guide them. In the BKS theory, however, there are no light quanta, only the radiation.

The way Heisenberg later remembered it, the virtual radiation of BKS had a status similar to that of the Schrödinger wave function in Born's statistical interpretation a few years later. As Heisenberg told Kuhn in his AHQP interview:

> What Bohr, Kramers, and Slater did was to establish the probability as a kind of reality . . . one felt that by making the probability become some kind of reality, you get hold of something which is there. It was at that time of course, very difficult to say what it was that you had gotten hold of. I would say only through the paper of Born [1926] did it become quite clear that one should say, "All right, the Schrödinger wave means that probability that an electron should be there." But the main point was that the probability itself was something real. It was not only in the mind of the people, but it was something in nature . . . Up to that time people had two possibilities. One possibility was that the reality is a wave. There is an electric field, and a magnetic field acting upon an atom, shaking the electron, and then the atom does something, it makes a transition . . . There is an entirely different picture of reality in which there is a light quantum . . . hitting the atom, and then something happens. But now the idea is that there is a wave. But this wave is not the reality. This wave is a probability—this wave is a tendency. It means that when this wave is present then the atom gets a tendency to emit light quanta. So this idea of the wave field being a tendency was something just in the middle between reality and non-reality . . . That was the striking thing about [BKS], you know, this new invention of a possibility which was a reality in some way but not a real reality—a half reality.[140]

Unsurprisingly, Born took exception to Heisenberg's suggestion that the Born interpretation had been anticipated in this way by BKS. As Heisenberg said in a subsequent session of the interview: "I felt once, when I discussed this matter with Born, that he was a bit angry that I had quoted too much the

[140]P. 2 of the transcript of session 4 of the AHQP interview with Heisenberg

Bohr-Kramers-Slater paper in connection with the probability interpretation of waves."[141] We sympathize with Born. Heisenberg's comments, we feel, have all the flavor of an after-the-fact rationalization.

In subsequent expositions of the BKS theory by both Kramers and Slater, the radiation from virtual oscillators is presented as every bit as real as the external radiation. It is hard to see how this could be otherwise since the two types of radiation are supposed to interfere with one another. Bohr, Kramers, and Slater (1924a) write: "we shall assume that [illuminated atoms] will act as secondary sources of *virtual* wave radiation which interferes with the incident radiation" (p. 167, our emphasis). A few pages later, they talk about the same "secondary wavelets set up by each of the illuminated atoms" (ibid., p. 172) without labeling them virtual. On the following page they suddenly refer to the external radiation as "incident *virtual* radiation" (ibid., p. 173, our emphasis). And the final paragraph of the paper discusses the "(virtual) radiation field" (ibid., p. 175) produced by ordinary antennas. The concluding sentence, which has Bohr written all over it, shows how the authors struggled with their own terminology:

> It will in this connexion be observed that the emphasizing of the 'virtual' character of the radiation field, which at the present state of science seems so essential for an adequate description of atomic phenomena, automatically loses its importance in a limiting case like that just considered [i.e., a classical antenna], where the field, as regards its observable interaction with matter, is endowed with all the attributes of an electromagnetic field in classical electrodynamics (Bohr, Kramers, and Slater, 1924a, p. 175).

Subsequent expositions of BKS by Slater and Kramers removed much of the tentativeness of this passage.

In a lengthy paper signed December 1, 1924, and published in the April 1925 issue of *The Physical Review*, Slater tried to work out a "consistent detailed theory of optical phenomena" based on BKS (Slater, 1925a, p. 395). Slater presented this work at a meeting of the *American Physical Society* in Washington, D.C., in December 1924 (Slater, 1925b). At this same meeting—which also marked the end of the controversy between Compton and Harvard's William Duane (1872–1935) over the Compton effect (Stuewer, 1975, p. 273)—Van Vleck (1925) talked about (Van Vleck, 1924b,c) and Breit (1925) talked about (Breit, 1924a).[142] Slater sent a copy of his paper to Bohr in December 1924

[141] P. 21 of the transcript of session 6 of the AHQP interview with Heisenberg. A very similar discussion of BKS can be found in an essay, "The history of quantum theory" (Heisenberg, 1958, pp. 40–41).

[142] The AHQP contains some correspondence between Slater and Van Vleck regarding this meeting and regarding (Slater, 1925a): Slater to Van Vleck, December 8, 1924; Van Vleck to Slater, December 15, 1924.

and defended his elaboration of BKS in a letter to Bohr of January 6, 1925 (Bohr, 1972–1996, Vol. 5, pp. 65–66).

In the introduction of his paper, Slater presents the dilemma that led him to embrace Bohr's statistical conservation laws.[143] The problem, he argues, is that

> in the quantum theory the energy of atoms must change by jumps; and in the electromagnetic theory the energy of a radiation field must change continuously . . . Two paths of escape from this difficulty have been followed with more or less success. The first is to redefine energy [i.e., to adopt Einstein's light-quantum hypothesis]; the second to discard conservation. Optical theory on [the first interpretation] would be a set of laws telling in what paths the quanta travel . . . [One way to do this is] to set up a sort of ghost field, similar to the classical field, whose function was in some way to guide the quanta. For example, the quanta might travel in the direction of Poynting's vector in such a field. The author was at one time of the opinion that this method was the most hopeful one for solving the problem . . . The other direction of escape from the conflict between quantum theory and wave theory has been to retain intact the quantum theory and as much of the wave theory as relates to the field, but to discard conservation of energy in the interaction between them (Slater, 1925a, pp. 396–397).

Slater sketches some difficulties facing this second approach, but makes it clear that this is the approach he now favors:

> An attempt was made by the writer, in a note to Nature [Slater, 1924], enlarged upon in collaboration with Bohr and Kramers, to contribute slightly to the solution of these difficulties. In the present paper, the suggestions made in those papers are developed into a more specific theory (ibid., p. 398).

Slater then describes more carefully how to picture the interaction between matter and radiation in BKS and makes it clear that the proposed mechanism is incompatible with strict energy conservation. According to Slater, the "one . . . essentially new" suggestion of BKS (note that he does not claim credit for the concept of virtual oscillators) was:

> that the wavelets sent out by an atom in connection with a given transition were sent out, not as a consequence of the occurrence of the transition, but as a consequence of the existence of the atom in the stationary state from which it could make that transition.[144] On this assumption, the stationary

[143] See also the brief discussion of BKS in (Van Vleck, 1926, pp. 285–286).
[144] Note the similarity with the comments of Bohr to Ladenburg quoted in sec. 3.3: "the quantum jumps are not the direct cause of the absorption of radiation, but

> state is the time during which the atom is radiating or absorbing; the transition from one state to another is not accompanied by radiation, but so far as the field is concerned, merely marks the end of the radiation or absorption characteristic of one state, and the beginning of that characteristic of another. The radiation emitted or absorbed during the stationary state is further not merely of the particular frequency connected with the transition which the atom is going to make; it includes all the frequencies connected with all the transitions which the atom could make ... Although the atom is radiating or absorbing during the stationary states, its own energy does not vary, but changes only discontinuously at transitions ... It is quite obvious that the mechanism becomes possible only by discarding conservation (ibid., pp. 397–398).

On the next page, Slater inserts a disclaimer similar to the one by Van Vleck quoted above:

> It must be admitted that a theory of the kind suggested has unattractive features; there is an apparent duplication between the atoms on the one hand, and the mechanism of oscillators producing the field on the other. But this duplication seems to be indicated by the experimental facts, and it is difficult at the present stage to see how it is to be avoided (ibid., p. 399).

Slater's portrayal of BKS agrees with the exposition given by Kramers and Helge Holst (1871–1944) in the German edition (Kramers and Holst, 1925) of a popular book on Bohr's atomic theory originally published in Danish (Kramers and Holst, 1922).[145] In a section, entitled "Bohr's new conception of the fundamental postulates," that was added to the German edition, Kramers explained that BKS breaks with one of the basic tenets of Bohr's original theory, namely that atoms only emit light when one of its electrons makes a transition from, to use his example, the second to the first stationary state. "According to the new conception," Kramers wrote, "radiation with frequency ν_{2-1} is still tied to the *possibility* of a transition to the first state, but it is assumed that the emission takes place during the entire time the atom is in the second state" (Kramers and Holst, 1925, p. 135). Another difference is that "if the atom is in the third state, it will simultaneously emit the frequencies ν_{3-2} and ν_{3-1} until it either jumps to the second or to the first state" (ibid.). Kramers emphasizes that this makes the new conception preferable to Bohr's

... represent an effect which accompanies the continuously disperging (and absorbing) effect of the atom on the radiation" (Bohr, 1972–1996, Vol. 5, p. 400).

[145]The translation was done by Fritz Arndt (1885–1969), a chemist and a colleague of Ladenburg and Reiche in Breslau (see the correspondence between Kramers and Ladenburg of 1923–1925 in the AHQP). The preface of this translation is dated March 1925. Dresden (1987) writes that the treatment of BKS in this book "is without much doubt the most understandable exposition of the BKS ideas" (p. 195).

original one from the point of view of the correspondence principle:

> This situation shows that the new conception is closer to the classical electron theory than the old one; the simultaneous emission of two frequencies mentioned above has its counterpart in that an electron moving on an ellipse emits both its fundamental tone and its first overtone . . . while earlier one had to assume that these two frequencies were produced by *different* transitions in *different* atoms. It is a welcome consequence, especially from the point of view of the correspondence principle, that the radiation emitted by a *single* atom contains all the frequencies that correspond to possible transitions; for in the border region of large quantum numbers the radiation demanded by the quantum theory will now merge very smoothly with the radiation demanded by the classical theory (Kramers and Holst, 1925, pp. 135–136).

The final paragraph of the BKS paper itself, from which we quoted above, can be seen as a garbled version of Kramers' argument here. Note that the term 'virtual radiation' is absent from these expositions by Slater and Kramers. In his detailed critique of the physics of BKS, Dresden (1987) struggles mightily to make sense of the "somewhat vague, tenuous relation between the virtual field and the real electromagnetic field" (p. 179). The presentations of BKS by Slater and Kramers suggest that there is no fundamental difference between the two. BKS does not introduce two different kinds of radiation, real and virtual, but a new picture of the *interaction* between radiation and matter, which is different both from the classical picture and from Einstein's light-quantum picture. As Heisenberg put it in his AHQP interview (see the passage quoted above), radiation is a "half reality" in this new picture in that it only determines the *probabilities* of quantum transitions in matter.

4.2 The demise of BKS

The BKS theory was decisively refuted in experiments by Walther Bothe (1891–1957) and Hans Geiger (1882–1945) in Berlin and by Compton and Alfred Walter Simon in Chicago. These experiments showed that energy-momentum is strictly conserved in Compton scattering (i.e., event by event) and not just statistically (Stuewer, 1975, pp. 299–302; Stolzenburg, 1984, pp. 75–80). The detection of a scattered electron almost always coincided with the detection of a light quantum, which went against the BKS picture that light is emitted and absorbed continuously, whereas the electron changes its energy and momentum only at discrete intervals. Of course, radiation is detected via its effect on electrons in some detector and, in the BKS picture, radiation only determines the probability of an electron absorbing energy. The crucial difference between BKS and the light-quantum prediction is that according to the latter there is a perfect correlation between detection of a scattered electron and detection of a light quantum, whereas the former predicts no such correlation. The experiments that eventually disproved BKS were begun shortly after the BKS paper was published (see Bothe and Geiger, 1924), but the final verdict did not come in until the following year. Bothe and Geiger (1925a,b) published their results in April 1925. The paper by Compton and Simon (1925) is signed June 23, 1925, and appeared in September 1925.[146] On April 17, 1925, Geiger sent Bohr a letter forewarning him of the results of his experiments with Bothe. When Geiger's letter arrived in Copenhagen four days later, Bohr was in the process of writing to Ralph H. Fowler (1889–1944) in Cambridge. In the postscript to this letter, Bohr conceded that "there is nothing else to do than to give our revolutionary efforts as honourable a funeral as possible" (Stuewer, 1975, p. 301). His co-authors Kramers and Slater took the fall of BKS harder. So did other supporters of the theory, such as Ladenburg, Reiche, and Born. By contrast, Einstein and Pauli, the theory's most vocal critics, rejoiced. As we shall see, Born, Pauli, and Van Vleck all explicitly recognized that the demise of BKS did not affect Kramers' dispersion theory and its virtual oscillators.

Ladenburg and Reiche had first read (the German version of) the BKS paper (Bohr, Kramers, and Slater, 1924b) in May 1924. "We are pleased," Ladenburg wrote to Kramers, "that our considerations harmonize so well with your ideas."[147] In the same letter, Ladenburg invited Kramers to come to Breslau to give a talk and to discuss in person what the two of them and Reiche had been discussing in correspondence (see sec. 3.4). Kramers accepted the invitation and suggested he talk about the new radiation theory, "which, I hope, will soon meet with approval from most physicists (although I heard that Einstein has expressed a relatively unfavorable opinion)."[148] Less than a week later, Kramers received the following intelligence from Ladenburg, directly addressing his parenthetical remark:

> As far as Einstein's opinion about your new conception of radiation is concerned, I can give you a very precise report, since I attended his talk on May 28 in the Berlin colloquium. His opinion was decidedly not unfavorable. He declared the new conception to be internally fully consistent and not in direct contradiction with any facts. The mechanism of the undulatory theory

[146] Stuewer (1975, p. 301) draws attention to a footnote in this paper that makes it clear that the experiment had been discussed even before Slater's arrival in Copenhagen: "The possibility of such a test was suggested by W. F. G. Swann in conversation with Bohr and one of us [Compton] in November 1923" (Compton and Simon, 1925, p. 290, note 6). Swann, the reader may recall, had just started in Chicago that fall, leaving the vacancy in Minnesota that was filled by Breit and Van Vleck (see sec. 2.2).

[147] Ladenburg to Kramers, May 31, 1924 (AHQP).

[148] Kramers to Ladenburg, June 5, 1924 (AHQP).

> would have to be preserved in his opinion. He put great emphasis, however, on the conceptual logical difficulties of the new theory, of the "preestablished harmony," which the fundamental introduction of probability instead of causality brings with it. Specific objections that he raised seemed to rest only on a not yet complete knowledge of all your considerations. He pointed to the asymmetry, for instance, that the production of virtual radiation was tied to a specific atomic state. In discussion, I pointed out in response to that that the virtual oscillators have the frequencies of *possible* transitions—at which point he immediately withdrew the objection.[149]

Privately, Einstein was less guarded. A month earlier—in a letter to Born and his wife Hedi (1892–1972) of April 29, 1924—he had already delivered his oft-quoted put-down that, should BKS turn out to be correct, he "would rather have been a shoemaker or even an employee in a gambling casino than a physicist" (Klein, 1970, p. 32).[150] Talking to Kramers in late June, Einstein expressed himself more diplomatically again. Kramers stopped in Berlin on his return trip from Breslau, where he had given a well-received talk on BKS on June 24, 1924. As he reported to Ladenburg once he was back in Copenhagen: "It was very interesting to hear Einstein's considerations; as he himself says, they are all arguments based on intuition."[151]

Ladenburg also attended the colloquium in Berlin in May 1925 in which Bothe and Geiger presented their results. Ladenburg had just received a copy of the German edition of Kramers' popular book with Holst from which we quoted above. He clearly had a hard time accepting the refutation of BKS at this point. Referring to the discussion of BKS in ch. 6 of (Kramers and Holst, 1925), he wrote:

> In this connection, I must report to you that yesterday Geiger and Bothe presented their important and beautiful experiments on counting electrons and [light] quanta in the Compton effect. Apparently, as you know, they have shown that the emission of electrons and quanta is simultaneous within one-thousandth of a second or less. Can I ask you to what extent you and Bohr consider this as standing in contradiction to your theory? Does your theory really require the complete independence of these two processes, so that only chance could cause the simultaneous occurrence of the two processes within one-thousandth of a second? You can imagine how these questions also affect us and if you have time to write to me to give your opinion I would be very grateful.[152]

[149] Ladenburg to Kramers, June 8, 1924 (AHQP).
[150] For further discussion of Einstein's objections to BKS, see (Klein, 1970, pp. 32–35), (Wasserman, 1981, pp. 255–263), and (Stolzenburg, 1984, pp. 24–28, pp. 31–34).
[151] Kramers to Ladenburg, July 3, 1924 (AHQP).
[152] Ladenburg to Kramers, May 15, 1925 (AHQP).

Unfortunately, we do not know whether and, if so, how Kramers replied.

When Slater found out about the experimental refutation of BKS, he dashed off another letter to *Nature* (dated July 25, 1925) announcing that he had once more changed his mind: "The simplest solution to the radiation problem then seems to be to return to the view of a virtual field to guide corpuscular quanta" (Slater, 1925c). Kramers and Bohr concurred: "we think that Slater's original hypothesis contains a good deal of truth." [153] Slater thus reverted to the position that, as he reminds the reader, he had been talked out of by Bohr and Kramers. Slater also noted that Swann had argued for this view during the December 1924 meeting of the *American Association for the Advancement of Science*, unaware that he, Slater, had been thinking along the same lines. [154] The following year, Bohr mentioned in passing in a letter to Slater that he had "a bad conscience in persuading you to our view." Slater told him not to worry about it. [155]

The way in which the BKS paper had come to be written, however, had left Slater with a bitter taste in his mouth (Schweber, 1990, pp. 350–356). We already quoted from his letter to Van Vleck of July 27, 1924, in which his disenchantment with Copenhagen shines through very brightly (see secs. 2.2 and 3.4). Interestingly, on that very same day, Slater wrote to Bohr, thanking him for his "great kindness and attention to me while I was in Copenhagen. Even if we did have some disagreements, I felt very well repaid for my time there, and I look back to it very pleasantly" (Bohr, 1972–1996, Vol. 5, p. 494). This sounds disingenuous in view of his comments to Van Vleck, but Slater had also been very positive about Bohr writing to his Harvard teacher Percy Bridgman (1882–1961) on February 1, 1924 (Schweber, 1990, p. 354). In his AHQP interview, however, Slater was very negative about Bohr and his institute. In fact, when he found out that Copenhagen would be one of the depositories for the AHQP materials, Slater asked Kuhn to keep the interview out of the copy going to Denmark. [156]

Initially, Slater was angry with both Bohr and Kramers, but his attitude toward the latter later softened (Dresden, 1987, pp. 168–171). His wife, fellow-physicist Rose Mooney (1902–1981), may have had something to do with that (Dresden, 1987, pp. 527–528). [157] Before Ms. Mooney became Mrs. Slater in

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[153] Kramers to Urey, July 16, 1925, quoted by Stolzenburg (1984, p. 86).
[154] Cf. (Swann, 1925). See (Stuewer, 1975, pp. 321–322) for discussion of Swann's proposal.
[155] Bohr to Slater, January 28, 1926; Slater to Bohr, May 27, 1926 (Bohr, 1972–1996, Vol. 5, pp. 68–69).
[156] Slater to Kuhn, November 22, 1963, included in the folder on Slater in the AHQP.
[157] A caveat is in order here. As pointed out in a review of (Dresden, 1987), "[t]he wealth of intimate detail about Kramers that Dresden provides relies so heavily on personal interviews (Dresden himself notes the "'soft' character" of this information)

1948, she had been close to Kramers, whom she had met at a summer school in Michigan in 1938. The two of them almost certainly had an affair. Kramers was profoundly unhappy in his marriage to Anna 'Storm' Petersen, a Danish singer he had met in artistic circles in Copenhagen and married in 1920 after she got pregnant.[158] In one of the most memorable passages of his book, Dresden (1987, pp. 289–295) reveals that Kramers had told Storm many years after the fact that he himself had on at least one occasion been railroaded by Bohr. Kramers apparently thought of the Compton effect around 1920, well before Compton and Debye did. Bohr, however, detested the notion of light quanta so much that he worked on Kramers until he recanted. According to what Storm told Dresden, Kramers had to be hospitalized after one of these sessions with Bohr! Bohr's victory was complete. Even more strongly than Slater in the case of BKS a few years later, Kramers joined Bohr's crusade against light quanta with "all the passion of a repentant convert" (Dresden, 1987, p. 171).[159] Slater may well have found out about this episode from his wife, Kramers' former mistress. Whether or not he did, in his autobiography, as Dresden (1987, p. 528) points out, Slater (1975) refers to his BKS co-author as "my old friend Kramers" (p. 233).

Born had also been a supporter of BKS. With only Kramers' *Nature* notes to go on, he assumed that Kramers' dispersion theory was a product of BKS. He had no way of knowing that Kramers had obtained these results before BKS. By the time (Born, 1924) was published, however, Born realized that one did not have to subscribe to all articles of the BKS philosophy to extend the results of Kramers' dispersion theory. At the beginning of the paper, Born still writes as if the two stand or fall together:

> Recently . . . considerable progress has been made by Bohr, Kramers and Slater on just this matter of the connection between radiation and atomic structure . . . How fruitful these ideas are, is also shown by Kramers' success in setting up a dispersion formula . . . In this situation, one might consider

that it is difficult for others to assess the evidence until the interviews (which I hope were taped), as well as Kramers's personal papers, are made available to others" (Stachel, 1988, p. 745).

[158] Kramers was on the rebound at the time from the on-again-off-again relationship with his Dutch girlfriend, Waldi van Eck. Dresden's description of Kramers' relationship with van Eck (not to be confused with Van Vleck) conjures up the image of a virtual oscillator: "no commitments were made, no decisions were taken, the relationship was never defined, it was certainly never consummated, nor ever terminated" (Dresden, 1987, p. 525).

[159] Bohr apparently commiserated with Pauli a few years later about Kramers' lingering bitterness over this episode. Pauli later told his colleague Res Jost (1918–1990) at the ETH in Zurich that he had consoled Bohr by arguing that discovering the Compton effect was hardly an impressive feat since Compton and Debye had come up with it independently of one another (Dresden, 1987, 294).

> whether it would not be possible to extend Kramers' ideas, which he applied so successfully to the interaction between radiation field and radiating electron, to the case of the interaction between several electrons of an atom . . . The present paper is an attempt to carry out this idea (Born, 1924, pp. 181–182).

A footnote appended to this passage reads: "By a happy coincidence I was able to discuss the contents of this paper with Mr. Niels Bohr, which contributed greatly to a clarification of the concepts." Bohr had visited Born and Heisenberg in Göttingen in early June 1924 (Cassidy, 1991, pp. 177–179). Heisenberg had already told Born all about BKS and Born had expressed his admiration for the theory in a letter to Bohr of April 16, 1924.[160] Bohr's visit must have further solidified his enthusiasm. A week later, however, Einstein passed through town and trashed BKS.[161] As a result of Einstein's onslaught, Born hedged his bets and did not throw in his fate with the more controversial aspects of BKS (see Mehra and Rechenberg, 1982–2001, Vol. 2, p. 144; Cassidy, 1991, p. 179). At the beginning of sec. 3 of his paper, he writes:

> it will be profitable to make use of the intuitive ideas, introduced by Bohr, Kramers and Slater . . . but our line of reasoning will be independent of the critically important and still disputed conceptual framework of that theory, such as the statistical interpretation of energy and momentum transfer (Born, 1924, p. 189).[162]

Born, however, continued to be a true believer in BKS and took its collapse harder than Bohr himself. On April 24, 1925, he wrote to Bohr:

> Today Franck showed me your letter [of April 21, 1925, the day that Bohr had received word from Geiger about the results of the Bothe-Geiger experiment] . . . which interested me exceedingly and indeed almost shocked me, because in it you abandon the radiation theory that obeyed no conservation laws (Bohr, 1972–1996, Vol. 5, p. 84).

In contrast to Born, Pauli called the demise of BKS "a magnificent stroke of

[160]See (Bohr, 1972–1996, Vol. 5, p. 299), discussed in (Mehra and Rechenberg, 1982–2001, Vol. 2, p. 143).

[161]See Heisenberg to Pauli, June 8, 1924 (Pauli, 1979, Doc. 62). This is the same day that Ladenburg wrote to Kramers that Einstein's opinion of BKS was "decidedly not unfavorable" (see above).

[162]This illustrates the importance of what Beller (1999) has called the "dialogical approach" to the history of quantum mechanics (an approach adopted *avant la lettre* by Hendry [1984]): to resolve the tension between the two quoted passages in Born's paper, it is important to be attuned to the voices of both Bohr and Einstein in his text.

luck."[163] Pauli's opposition to BKS was probably fueled by Einstein, who gave him an earful about the theory during the annual meeting of the *Gesellschaft Deutscher Naturforscher und Ärzte* in Innsbruck in September 1924.[164] Pauli clearly recognized that Kramers' dispersion theory was independent of BKS and that the fall of the latter did not affect the former. A footnote in (Pauli, 1925) emphasizes

> that the formulas of [Kramers and Heisenberg, 1925] used here are independent of the special theoretical interpretation concerning the detailed description of the radiation phenomena in the quantum theory taken as a basis by them [i.e., BKS], since these formulas only apply to averages over a large number of elementary phenomena (Pauli, 1925, p. 5).

As he explained to Kramers, Pauli wanted to distance himself from the suggestion in the abstract of (Kramers and Heisenberg, 1925) that "the conclusions, should they be confirmed, would form an interesting support for this [i.e., the BKS] interpretation" (cf. sec. 4.1). Alerting Kramers to the footnote quoted above, Pauli wrote:

> if I had not added the footnote in question, it would also have been true that the conclusions of *my* paper, if they should be confirmed, 'would form an interesting support for this interpretation.' This impression I had, of course, to counteract![165]

This letter was written after Pauli had read the manuscript of Heisenberg's *Umdeutung* paper, which was much more to his liking. In the same letter, in cruel Pauli fashion, he proceeded to berate Kramers for pushing BKS. That this did not affect Pauli's appreciation for Kramers' work on dispersion is clear from what he wrote to another correspondent a few months after this scathing letter: "[m]any greetings also to Kramers, whom I am very fond of after all, especially when I think of his beautiful dispersion formula."[166]

In his NRC *Bulletin*, written after the Bothe-Geiger and Compton-Simon experiments, Van Vleck, like Pauli, stressed the independence of the Kramers dispersion theory and BKS. The rejection of BKS and the acceptance of the light-quantum hypothesis, he wrote

> [do] not mean that Slater's concept of virtual oscillators is not a useful one. We may assume that the fields which guide the light-quants come from a hy-

[163] Pauli to Kramers, July 27, 1925 (Pauli, 1979, pp. 232–234; Bohr, 1972–1996, Vol. 5, p. 87).
[164] See Pauli to Bohr, October 2, 1924 (Pauli, 1979, Doc. 66), quoted and discussed in (Wasserman, 1981, pp. 260–263).
[165] Pauli to Kramers, July 27, 1925 (cf. note 163).
[166] Pauli to Kronig, October 9, 1925, quoted in (Stolzenburg, 1984, p. 91).

> pothetical set of oscillators rather than from the actual electron orbits of the conventional electrodynamics.[167] In this way the appearance of the spectroscopic rather than the orbital frequency in dispersion can be explained, and the essential features of the virtual oscillator theory of dispersion ... can still be retained. There is an exact conservation of energy between the atoms and the actual corpuscular light-quants, but only a statistical conservation of energy between the atoms and the hypothetical virtual fields (Van Vleck, 1926, pp. 286–287).

Virtual oscillators survived the demise of BKS and happily lived on in the dispersion theory from which they originated.

These observations by Pauli and Van Vleck make it clear that BKS only played a very limited role in the developments that led to matrix mechanics. It is important to keep that in mind. As long as we think of the Kramers dispersion theory as part and parcel of BKS, it looks as if matrix mechanics replaced a decisively refuted theory. Once we recognize that the Kramers dispersion theory was developed before and independently of BKS, we see that matrix mechanics grew naturally out of an eminently successful earlier theory. The BKS theory and its refutation by the Bothe-Geiger and Compton-Simon experiments then become a sideshow distracting from the main plot line, which runs directly from dispersion theory to matrix mechanics. A corollary to this last observation is that the acceptance of the light-quantum hypothesis was irrelevant to the development of matrix mechanics. Compton scattering provided convincing evidence for the light-quantum hypothesis and against BKS, but it had no bearing on dispersion theory. The work of Ladenburg, Kramers, Born, and Van Vleck crucially depended on Einstein's A and B coefficients, but not on the theory of light quanta in which these coefficients were originally introduced.

4.3 Heisenberg, BKS, and virtual oscillators

When Heisenberg first read the BKS paper, he was not impressed: "Bohr's paper on radiation is certainly very interesting; but I do not really see any fundamental progress."[168] He subsequently warmed to the theory, writing to Copenhagen on April 6, 1924 that he hoped Bohr had meanwhile convinced Pauli.[169] To Sommerfeld he wrote on November 18, 1924: "Maybe Bohr's

[167] At this point, the following footnote is appended: "This viewpoint has been advocated by Slater during the printing of the present Bulletin. See [Slater, 1925a]."
[168] Heisenberg to Pauli, March 4, 1924 (Pauli, 1979, Doc. 57); quoted by Dresden (1987, p. 202) and Wasserman (1981, p. 250).
[169] See (Bohr, 1972–1996, Vol. 5, pp. 354–355), cited by Cassidy (1991, p. 176) to support his claim that "by the end of his March 1924 visit to Copenhagen, Werner

radiation theory is a most felicitous [*sehr glücklicke*] description of this dualism [i.e., the wave-particle duality of radiation] after all" (Sommerfeld, 2004, p. 174, quoted in Wasserman, 1981, p. 251). Five years later, Heisenberg was praising BKS effusively:

> This investigation represented the real high point in the crisis of quantum theory, and, although it could not overcome the difficulties, it contributed, more than any other work of that time, to the clarification of the situation in quantum theory (Heisenberg, 1929, p. 492; translated and quoted in Stuewer, 1975, p. 291).

And thirty years later, Heisenberg (1955, p. 12) remembered BKS as "the first serious attempt to resolve the paradoxes of radiation into rational physics" (quoted in Klein, 1970, p. 37).

Why was Heisenberg so taken with BKS? We already came across part of the answer. As he told Kuhn in his AHQP interview, Heisenberg saw in BKS a precursor to the Born interpretation of the Schrödinger wave function (see sec. 4.1). This, we feel, mainly helps explain Heisenberg's profuse praise after the fact. In the same interview, however, Heisenberg identified another aspect of BKS that can account for his enthusiasm for BKS before *Umdeutung*—or rather, Kuhn identified it for him. What triggered Heisenberg's ruminations on probability in BKS and in the Born interpretation was the observation by Kuhn that despite the experimental refutation of BKS, "a large part of the basic ideas and the whole use of the Correspondence Principle formulated in terms of virtual oscillators goes on quite unshaken." [170] Heisenberg's response does not address this issue at all, whereupon Kuhn tries again: "In order to do that paper [BKS] one talks not only about . . . probability . . . but also transforms one's idea of the atom into a collection of virtual oscillators that operate between states" (ibid., p. 3). This time Heisenberg takes the bait:

> Yes, that was it. This idea, of course, also was there already that an atom was really a collection of virtual oscillators. Now this . . . was in some way contrary to the idea of an electron moving around a nucleus. The obvious connection, the only possible connection, was that the Fourier components of this motion in some way corresponded, as Bohr said, to the oscillators. But certainly this paper [BKS] then prepared the way for this later idea that the assembly of oscillators is nothing but a matrix. For instance, we can simply say that matrix elements are the collection of oscillators. In this way, you can say that matrix mechanics was already contained in this paper [BKS] (ibid., p. 3).

was a convert."

[170] P. 2 of the transcript of session 4 of the AHQP interview with Heisenberg.

This supports the thesis in (MacKinnon, 1977) mentioned in sec. 4.2 that matrix mechanics can be seen as a theory of virtual oscillators. What we want to emphasize is that what initially seems to have attracted Heisenberg to BKS was the notion of virtual oscillators. Given the origin of this concept, Heisenberg's intellectual debt on this point was not to BKS but—once again (see sec. 3.5)—to dispersion theory. During a subsequent session of the AHQP interview, Heisenberg, in fact, talks about the link between Fourier components and oscillators in the context Kramers' dispersion theory. "When you say the dispersion formula started from a physical idea," Kuhn asked, "do you have a particular thing in mind?" Heisenberg replied:

> Well, I would say that his [i.e., Kramers'] idea was that there was the Einstein paper [with the A and B coefficients] and there was the Ladenburg [1921] paper connected with Einstein's. On the other hand there was Bohr's Correspondence Principle and the idea finally that this has to do somehow with Fourier components as oscillators. Kramers had the force to combine these two possibilities in one simple formula—the dispersion formula. And this I think was a very important idea that one should combine the Einstein paper, which was very far from the Bohr model[,] with the Bohr model . . . Behind this idea was already the idea of connecting the oscillators with the Fourier components, which, as I have said many times, was in the air somehow in these years. [171]

Heisenberg explicitly availed himself of virtual oscillators in (Heisenberg, 1925a), a paper on the polarization of fluorescent light submitted from Copenhagen in November 1924 (i.e., before the Kramers-Heisenberg paper). Talking about this paper in his interview with Kuhn, Heisenberg said:

> I would say that all this is part of the game to make the total table of linear oscillators be the real picture of the atom. One felt that in the Correspondence Principle, one should compare one of these linear oscillators with one Fourier component of a motion . . . So the whole thing was a program which one had consciously or unconsciously in one's mind. That is, how can we actually replace everywhere the orbits of the electron by the Fourier components and thereby get into better touch with what happens? Well, that was the main idea of quantum mechanics later on. One could see, more and more clearly, that the reality were the Fourier components and not the orbits. [172]

[171] P. 13 of the transcript of session 6 of the AHQP interview with Heisenberg.

[172] P. 15 of the transcript of session 4 of the AHQP interview with Heisenberg. Parts of this passage are quoted in (MacKinnon, 1977, p. 155) and in (Mehra and Rechenberg, 1982–2001, Vol. 2, p. 165) (although the latter cite their own conversations with Heisenberg as their source; cf. notes 5 and 79).

MacKinnon (1977, pp. 148–155) stresses the importance of (Heisenberg, 1925a) for the development of matrix mechanics.[173] Heisenberg agreed. Commenting on a draft of MacKinnon's article, he wrote to the author in July 1974: "I was especially glad to see that you noticed how important the paper on the polarization of fluorescent light has been for my further work on quantum mechanics. Actually, in Copenhagen I felt that this paper contained the first step in which I could go beyond the views of Bohr and Kramers" (MacKinnon, 1977, p. 149, note 29). As he proudly recounts in his AHQP interview (see pp. 13–14 of the transcript of session 4), Heisenberg managed to convince Bohr and Kramers of his approach to this problem, an approach they initially questioned.

MacKinnon (1977, pp. 157–162) also sees (Heisenberg, 1925b) on the anomalous Zeeman effect as an important step on the way to matrix mechanics:

> In the conclusion Heisenberg outlined a new program for quantum theory. One should use the virtual oscillator model to work out all the Fourier components for the electrons in an atom and for the coupling between electrons. In the rest of this article I will attempt to trace through in detail the way Heisenberg implemented this program and developed quantum mechanics (MacKinnon, 1977, pp. 161–162).

Here we part company with MacKinnon. Virtual oscillators are not mentioned at all in (Heisenberg, 1925b) (though Fourier components are). Heisenberg (1925b, p. 857) does not even refer to virtual oscillators when discussing results pertaining to incoherent radiation from (Kramers and Heisenberg, 1925). This paper, far from being another step toward *Umdeutung*, seems to be mired in the intractable problems of the old quantum theory: the Zeeman effect, multi-electron atoms, and mysterious factors of 2 later to be accounted for in terms of electron spin.

MacKinnon, in our opinion, thus overstates his case. Yet, even if we discard what he has to say about (Heisenberg, 1925b) on the Zeeman effect, ample evidence remains for his claim that "[t]he virtual oscillator model played an essential role in the process of reasoning that led Heisenberg to the development of quantum mechanics" (MacKinnon, 1977, p. 184). In fact, this thesis is not nearly as controversial as MacKinnon makes it sound. In the entry on Kramers for the *Dictionary of Scientific Biography*, the sober-minded Dutch physicist Hendrik B. G. Casimir (1909–2000) states matter-of-factly: "The notion of virtual oscillators was the starting point of Heisenberg's quantum mechanics—the virtual oscillators became the matrix elements of the coordinates" (Casimir, 1973, p. 492). MacKinnon (1977) claims that "after [the *Umdeutung* paper] was written the virtual oscillator model sunk from sight

[173] For other historical discussion of (Heisenberg, 1925a), see (Cassidy, 1991, pp. 187–188) and (Mehra and Rechenberg, 1982–2001, Vol. 2, pp. 159–169).

and never resurfaced" (p. 184). We already noted, however, that the term "substitute oscillators" can still be found in the famous post-*Umdeutung* paper of Born and Jordan (1925b) (see sec. 3.3). What we did not mention so far is that Landé (1926, p. 456) actually introduced the phrase "virtual orchestras" to describe not BKS but matrix mechanics![174] The imagery, if not exactly the language, of an "orchestra of virtual oscillators" was also used in early popular expositions of matrix mechanics. In a popular book of the 1930s that went through many editions and was endorsed by Max Planck in a short preface, Ernst Zimmer wrote:[175]

> The state of an atom should no longer be described by the unobservable position and momentum of its electrons, but by the measurable frequencies and intensities of its spectral lines ... Regardless of the nature of the real musicians who play the optical music of the atoms for us, Heisenberg imagines assistant or auxiliary musicians [*Hilfsmusiker*]: every one plays just one note at a certain volume. Every one of these musicians is represented by a mathematical expression, q_{mn}, which contains the volume and the frequency of the spectral line as in expressions in acoustics familiar to physicists. These auxiliary musicians are lined up in an orchestra [*Kapelle*] according to the initial and final states n and m of the transition under consideration. The mathematician calls such an arrangement a "matrix" (Zimmer, 1934, pp. 161–162).

Zimmer's *Kapelle der Hilfsmusiker* was clearly inspired by Landé's *Ersatzorchester der virtuellen Oszillatoren*. Virtual oscillators thus not only survived the demise of BKS but also the transition to matrix mechanics. In fact, as we shall see in sec. 7.1, the features captured by the notion of virtual oscillators can still readily be identified in the formalism of modern quantum mechanics. From the point of view of the quantum theory that emerged in the immediate aftermath of Heisenberg's *Umdeutung* paper, in which the atomic system is quantized but not (as yet) the electromagnetic field, virtual oscillators are nothing but the Fourier components of the Schrödinger wave function of the electron. The perturbing electromagnetic field induces additional Fourier components in this wave function, which in turn results in secondary electromagnetic radiation. In terms of the language borrowed from BKS, this radiation is emitted by virtual oscillators.

[174] Landé had worked with Heisenberg in 1924 (Cassidy, 1991, p. 177), resulting in a joint paper (Landé and Heisenberg, 1924). In his AHQP interview, Landé nonetheless said that (Heisenberg, 1925c) had been incomprehensible to him and that it had taken (Born, Heisenberg, and Jordan, 1925) for him to understand matrix mechanics (p. 3 of the transcript of session 5 of the interview; cf. note 11). These comments seem to be colored, however, by lingering resentment. Landé felt strongly that Born should have won the Nobel Prize for his contribution to matrix mechanics and that German anti-Semitism was the only reason he had not.

[175] We are grateful to Jürgen Ehlers for drawing our attention to Zimmer's book.

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