Philosophical Rhetoric in Early Quantum Mechanics 1925–27: High Principles, Cultural Values and Professional Anxieties

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> 'I look on most general reasoning in science as [an] opportunistic (success- or unsuccessful) relationship between conceptions more or less defined by other conception[s] and helping us to overlook [danicism for "survey"] things.'
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> Niels Bohr (1919)[1]

This paper considers the role played by philosophical conceptions in the process of the development of quantum mechanics, 1925–1927, and analyses stances taken by key participants on four main issues of the controversy (*Anschaulichkeit*, quantum discontinuity, the wave-particle dilemma and causality). Social and cultural values and anxieties at the time of general crisis, as identified by Paul Forman, strongly affected the language of the debate. At the same time, individual philosophical positions presented as strongly-held principles were in fact flexible and sometimes reversible to almost their opposites. One can understand the dynamics of rhetorical shifts and changing strategies, if one considers interpretational debates as a way

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The following abbreviations are used: AHQP, Archive for History of Quantum Physics, NBA, Copenhagen; *AP*, *Annalen der Physik*; *HSPS*, *Historical Studies in the Physical Sciences*; NBA, Niels Bohr Archive, Niels Bohr Institute, Copenhagen; *NW*, *Die Naturwissenschaften*; PWB, Wolfgang Pauli, *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg a.o.*, Band I: 1919–1929, ed. A. Hermann, K.V. Meyenn, and V.F. Weisskopf (New York: Springer, 1979); RKZ, Ralph Kronig Nachlass, Eidgenössische Technische Hochschule, Zurich; *SHPS*, *Studies in the History and Philosophy of Science*; WWM, Wilhelm Wien Nachlass, Deutsches Museum, Munich; *ZP*, *Zeitschrift für Physik*.

[1] Niels Bohr to C.G. Darwin, draft of a presumably unsent letter, around July 1919, NBA.

of justifying property claims over the emerging new revolutionary theory, at a time when its major concepts were *in statu nascendi*, while personal relations and institutional hierarchies between its major contributors were still developing and negotiable.

Ehrenfest, Darwin, and Pauli on the Forman Thesis

In a June 1919 letter to his friend and colleague Niels Bohr, Paul Ehrenfest, professor of theoretical physics at the University of Leiden, commented insightfully on changes in the European intellectual climate after World War I:

> [I]t is remarkable that precisely here, in the circles of men having much to do with technology, production, industry, patents etc., opinions develop so uniformly about perspectives of culture. Overall there is building up an uncannily intensive reaction *against rationalism* ... If I am not entirely mistaken, in the next 5–10 years we will see the following happening at the institutes of higher learning (including technical!). Professors raised as relatively *rational* and disciplined individuals will despairingly and uncomprehendingly face the complaints and demands of a relatively "*mystical*" student body. At the same time, scientifically less clear but personally warmer teachers will gain the main influence over students.[2]

Ehrenfest's observation remarkably and almost literally supports the core claim of Paul Forman's article of 1971 that, in the immediate wake of World War I, a strong wave of reaction against rationalism and favouring more mystical lines of thought swept through not just the intellectual public in general, but also such professionals as engineers and exact scientists previously expected to strongly resist such trends.[3] It is worth noting that the letter was from a physicist in the Netherlands to his colleague in Denmark, signifying the mood did not remain confined to Germany and Austria, the countries who had lost the war, but also affected at least the neighbouring neutral countries. Although not abandoning his personal rationalistic convictions, Ehrenfest appeared to defer to the opinions of the younger, as academics often do with the newest intellectual fashions.

Ehrenfest's letter also contains a strikingly self-conscious recognition and expectation that professors would adapt knowingly, rather than unreflectively, to

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[2] Paul Ehrenfest to Niels Bohr, 4 June 1919, NBA (emphasis in the original).

[3] Paul Forman, 'Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment', *HSPS* 3 (1971): 1–115.

the direction of the prevailing intellectual wind. He did not state it explicitly in this letter, but his other correspondence of the time reveals quite clearly that, in his own field of theoretical physics, he admired and regarded Bohr as precisely the kind of professor who would resonate with and inspire the younger generation of students.[4] The quoted description, indeed, sits well with characteristic hagiographic references in existing recollections regarding the way Bohr's charisma influenced younger students. Bohr appears there as a kind of philosophical guru, whose thoughts were too profound to be understood or even expressed clearly, but this only helped them to be tremendously inspiring. Whether or not Ehrenfest's letter thus contained implicit advice to Bohr, and whether or not Bohr accepted the hint or arrived at similar ideas on his own, around the same time he was already inclined 'to take the most radical *or rather mystical* views imaginable' with regard to the daunting problem of the quantum interaction of matter and radiation. That much Bohr admitted himself while composing a reply to his British friend and colleague Charles Galton Darwin.[5]

Darwin's letter and his enclosed manuscript suggested several possible ideas of this kind, including acausality. Darwin 'felt that the fundamental basis of physics is in a desperate state. The great positive successes of the quantum theory have accentuated … also the essential contradiction on which it rests.' As a remedy, he proposed 'to knock away the props of classical physics one by one and find, after a particular one has been removed, that our difficulties have become reconciled. It may be that it will prove necessary to make fundamental changes in our ideas of time and space or to abandon the conservation of matter and electricity or even as a last forlorn hope to endow electrons with free will.' Personally, Darwin preferred to think that 'contradictions in physics all rest on the exact conservation of energy' and his favourite solution thus involved 'denying that conservation is anything more than statistical.'[6] Four years later, this acausal hypothesis would become Bohr's preferred choice, too, but in 1919 he was not yet ready to decide which, if any, of the mentioned 'mystical' hypotheses could be endorsed.

Ehrenfest's 1919 formulation of the Forman thesis *avant la lettre* makes an important addition to it by specifying the effective milieu whose demands made

[4] For example, Ehrenfest to A.F. Joffe, 6 September 1920, in *Erenfest–Ioffe: Nauchnaia Perepiska, 1907–1933* (Leningrad: Nauka, 1973), 146–150.

[5] Bohr to Darwin, July 1919, NBA (ref. 1): 'or rather mystical' is inserted into the sentence above the line. *Cf. also* Bohr to Ehrenfest, 22 October 1919, NBA.

[6] Darwin to Bohr, 20 July 1919, with an enclosed manuscript 'A Critique of the Foundations of Physics', NBA.

professors adapt — students in auditoriums. Indeed, students may well have been the immediate audience many professors cared about the most. In this respect, however, Bohr's situation was rather peculiar and differed from Ehrenfest's. From early on in his professorship at the University of Copenhagen, Bohr delegated the task of giving general lectures to his assistants and did not teach undergraduate students. With only a couple of exceptions, he did not train doctoral students either. Almost all of the young physicists who worked with him received their degrees elsewhere and usually came to his institute as postdoctoral fellows. Arguably the most outspoken among them, Wolfgang Pauli, has also left a well-documented manuscript record that reflects the development of his views on quantum acausality.

Pauli's probably earliest extant comment on the unpredictability of quantum behaviour can be found in his letter written in Copenhagen in June 1923 to Arnold Sommerfeld in Munich. The remark, however brief, is quite provocative, since it compared Pauli's personal feeling of insecurity and professional anxiety with the uncertainty of a microscopic object, thus suggesting a possible additional motivation for the development of acausal thought. The letter primarily dealt with Sommerfeld's proposal that Pauli obtain his Habilitation and become a *Privatdozent* at the University of Munich. Pauli declined the tempting, if belated, offer from his former teacher and doctoral adviser, hoping instead that Bohr, upon returning from an American trip, would invite him again for another stay in Copenhagen. He did understand, however, that tiny Denmark could offer him only temporary support but no long-term professional perspective:

> I will certainly not be able to remain here [in Copenhagen] forever and must sooner or later habilitate at one of the German universities ... Bohr's [possible] offer, however, makes me leave the question of my Habilitation open, for now … The only thing certain is that I will still spend the coming semester in Hamburg … *What happens later I know as little as an electron knows in advance where it will jump in* 10^{-8} *seconds* (I have only described the forces deflecting me from Munich, but … of course, very strongly attractive forces come from Munich as well).[7]

At the moment of writing, Pauli's professional future looked very uncertain indeed. A year earlier he had accepted a temporary appointment in Copenhagen to help Bohr write papers in German, which nearly coincided with an abrupt collapse of the German mark and the start of hyperinflation. Pauli was unable even to

[7] Wolfgang Pauli to Arnold Sommerfeld, 6 June 1923, in PWB, p. 94 (emphasis added).

purchase a railway ticket to travel to Copenhagen from Hamburg and had to ask Bohr to send him an advance in Danish crowns.[8] Erwin Schrödinger, who had just recently obtained a professorship in financially secure Switzerland, congratulated Pauli on his escape and suggested that he not return to Germany in the foreseeable future.[9] They understood each other as fellow Austrians: both knew how hyperinflation had decimated science in their home country immediately after World War I and both had then left Austria to seek better career opportunities in the still relatively stable economy of Germany. Now that the financial collapse had caught up with Germany, too, both anticipated the worst on the basis of their Austrian experiences and sought professional appointments elsewhere.

Returning to Hamburg after his Copenhagen year, Pauli was relieved to discover that his worst fears had not materialised. Indeed, inflation did not damage science in Germany as severely as it had in Austria. Research and publishing continued at a fast pace, even if a prohibitive exchange rate had destroyed many possibilities for foreign travel and subscriptions to foreign publications. Scientific infrastructure and laboratories built during the imperial period were still far better than anywhere else in Europe. Salaried professors maintained tolerable incomes. The negative effects could be described as structural rather than outright destructive. Arguably in the most difficult situation were younger academics, like Pauli, in a career stage between doctorate and first professorial appointment. Formerly they had typically taught at universities as independent *Privatdozenten*, but inflation made this professional class almost extinct, or at least at a practical level indistinguishable from lower assistants, as it was no longer possible to sustain one's livelihood on 'soft money' such as students' fees.[10]

Pauli's metaphorical comparison of the uncertainty of his career trajectory with that of a quantum particle is very appropriate. Formerly, the professional paths of younger academics in Germany resembled trajectories of classical particles determined by the power fields of their professors. Acting in this fashion, Max Born asked for and received Sommerfeld's permission earlier in 1923 to hire the latter's student Heisenberg for a year. In a similar deal, Pauli also worked for Born in Göttingen during the previous year, and as of January 1923 both professors

[8] Pauli to Bohr, 5 September 1922, and Bohr to Pauli, 8 September 1922, both in PWB, pp. 63–4.

[9] Erwin Schrödinger to Pauli, 8 November 1922, in PWB, p. 69.

[10] Pauli to Bohr, 16 July 1923, in PWB, p. 102. For more on the effects of inflation on academic life and the fate of *Privandozenten*, see Paul Forman, *The Environment and Practice of Atomic Physics in Weimar Germany: A Study in the History of Science* (PhD dissertation, University of California, Berkeley, 1967), 206–37. For observers' remarks on the disappearance of *Privatdozenten*, see Alexi Assmus, 'The Creation of Postdoctoral Fellowships and the Siting of American Scientific Research', *Minerva* 31 (1993): 151–83, on 178.

assumed Pauli would eventually return to Munich as Sommerfeld's *Privatdozent*.[11] Had Sommerfeld actually made such an offer to him earlier, his invitation would have been hard to decline, but in the meantime inflation had set in and undermined the resources of German professors, sending Sommerfeld, and later also Born, to visiting professorships in the United States as a way to improve personal finances. Younger academics had a much harder time surviving the intermediate state of negative finances, which was what *Privatdozent* became. Some effectively worked for their professors as lower-level assistants. Others, like Pauli, were able to become quasi-independent, abandon the notion of determined classical paths and experiment with less predictable professional jumps via metastable states such as temporary positions or postdoctoral fellowships abroad.

Metaphors are dangerous, however, as they encourage reasoning in both directions. Pauli's anxiety over his personal career at the height of the German crisis could quite plausibly make him more inclined to think of electrons in similar terms and by 1925 arrive at an important conclusion that the notion of their classical trajectories inside atoms should be completely abandoned, which in turn inspired Heisenberg onto the path leading to the new quantum mechanics.[12] No matter how tempting, however, one cannot assume a direct transition from Pauli's indeterministic remark of 1923 to his advocacy of probabilistic quantum mechanics three years later. The problem is that in the meantime he also made contradictory pronouncements in no uncertain terms: 'I definitely believe that *the probability concept should not be allowed in the fundamental laws of a satisfying physical theory.* I am prepared to pay any price for the fulfilment of this desire, but unfortunately I still do not know the price for which it is to be had'. The above declaration of faith sprang not from the pen of Einstein in his 'God does not throw dice' mood, but from Pauli writing to Bohr in November 1925.[13] It contradicts much of what Pauli is otherwise known for, but at the time was made as seriously and sincerely as, later on, he would express his probabilistic convictions.

We can understand this quote in the context of its precise timing and reference. Pauli was describing to Bohr the promise of Heisenberg's new matrix mechanics but obviously taking critical aim at, without the need to mention explicitly, Bohr's earlier failed attempt to introduce the acausal principle into the foundations of quantum theory — the Bohr-Kramers-Slater theory of 1923–24. The subversive idea that energy in atomic interactions may be conserved only

[11] Max Born to Sommerfeld, 5 January 1923, quoted in Jagdish Mehra and Helmut Rechenberg, *The Historical Development of Quantum Theory*, vol. 2 (New York: Springer, 1982), 73.

[12] For a detailed account of the development of Pauli's ideas, see John Hendry, *The Creation of Quantum Mechanics and the Bohr-Pauli Dialogue* (Dordrecht: Reidel, 1984).

[13] Wolfgang Pauli to Niels Bohr, 17 November 1925, in PWB, p. 260 (emphasis added).

statistically, rather than strictly, had been discussed earlier by Darwin and a few other authors in letters and unpublished manuscripts. By 1923, Bohr's opposition to the rising popularity of light quanta developed to the point of desperation when he became ready to accept and endorse the idea in a formal publication, and endure the associated risks. Once Pauli left Copenhagen for Hamburg and was no longer directly employed by Bohr, he could not hold back anymore with his opposition to the 'reactionary Copenhagen putsch'. To his relief, by 1925 experimentalists decisively refuted the Bohr-Kramers-Slater proposal.[14] Feeling vindicated in his devotion to the strict validity of the law of energy conservation, Pauli was also ready, as the above quote shows, to reject the fundamentality of the probabilistic approach altogether.

His views would change once again in less than a year. In the summer of 1926, Pauli introduced the probabilistic understanding of Schrödinger's wave function and remained ever after a proponent of the statistical interpretation of quantum mechanics. Pauli's flip-flops on the issue of causality, however, convey an important lesson, namely, that philosophical pronouncements of quantum physicists, no matter how strongly expressed, should not be taken as general and long-term commitments, but as context-dependent and flexible. As a matter of fact, such drastic shifts on fundamental issues and principles were not characteristic of Pauli alone, and not only with regard to the question of causality. Rather, they can be regarded as a distinctive feature of the early quantum philosophy in general.

The Problem with Quantum Philosophy

Not long before his death in 1962, Bohr confessed to Thomas Kuhn that he had hardly any hope of achieving an understanding between quantum physicists and philosophers. He expressed the complaint in, for Bohr, unusually strong and categorical terms: 'I think it would be reasonable to say that no man who is called a philosopher really understands what one means by the complementarity description'.[15] As if they were aware of this charge, philosophers retaliated some 30 years later in a volume devoted to the assessment of Bohr's contribution to philosophy. In equally strong words, Don Howard expressed doubts 'whether or not Bohr's philosophy of physics can be given a coherent interpretation'. As Howard summarised the problem, 'There was a time, not so very long ago, when Niels Bohr's influence and stature as a philosopher of physics rivalled his standing as a physicist. But now

[14] Pauli to Hendrik Anthony Kramers, 27 July 1925, in PWB, p. 234.
[15] Bohr, interview by Kuhn, 17 November 1962, AHQP.

there are signs of a growing despair — much in evidence during the 1985 Bohr centennial — about our ever being able to make good sense out of his philosophical views'.[16]

The *noblesse oblige* of the professional philosopher, however, did not permit Howard to give up:

> I think that the despair is premature .... What is needed at the present juncture is really quite simple. We need to return to Bohr's own words, filtered through no preconceived philosophical dogmas. We need to apply the critical tool of the historian in order to establish what those words were and how they changed over time. We need to assume, at least provisionally, that Bohr's words make sense. And we need to apply the synthetic tools of the philosopher in order to reconstruct from Bohr's words a coherent philosophy of physics.[17]

In the main part of this paper I will take up the historical part of Howard's advice and follow the twists and turns of quantum philosophy during the years 1925 to 1927, from Bothe and Geiger's refutation of the Bohr-Kramers-Slater theory and Heisenberg's first paper on matrix mechanics to the Solvay congress of 1927 and the first open disputes between Einstein and Bohr. Simultaneous with the invention of quantum mechanics itself — to which nearly 200 authors contributed over this period — a half-dozen physicists were developing competing philosophical interpretations of the not yet completed theory.[18] Ordered by age, this group included Einstein, Bohr, Born, Schrödinger, Pauli, Heisenberg and Jordan. Altogether, they expressed quite a variety of conflicting philosophical views, which can be grouped around four main issues: *Anschaulichkeit-Unanschaulichkeit* (roughly translated as visualisability-unvisualisability), continuity-discontinuity, the wave-particle dilemma and causality-acausality.

For an historian analysing these views, the main difficulty lies not in the lack or paucity of sources, but on the contrary, in their intimidating overabundance and contradictory nature. That most of the above-mentioned participants and also others

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[16] Don Howard, 'What Makes a Classical Concept Classical? Toward a Reconstruction of Niels Bohr's Philosophy of Physics', in *Niels Bohr and Contemporary Philosophy*, ed. J. Faye and H.J. Folse (Dordrecht: Kluwer, 1994), 201–29, on 201.

[17] Ibid. *See also* Henry J. Folse, 'Niels Bohr and the Construction of a New Philosophy: Essay Review', *Studies in the History and Philosophy of Modern Physics* 26 (1995): 107–16, on 108; Don Howard, review of *Niels Bohr: A Centenary Volume*, ed. A.P. French and P.J. Kennedy, *Annals of Science* 44 (1987): 196–98.

[18] For a scientometric overview of the emerging field of quantum mechanics, see A. Kozhevnikov and O. Novik, 'Analysis of Informational Ties in Early Quantum Mechanics (1925–1927)', *Acta historiae rerum naturalium necnon technicarum* 20 (1989): 115–59.

on their behalf continued the dispute in some form for many years after 1927 further complicates the situation. They kept commenting, explaining and restating their positions, usually without acknowledging that their views continued to shift as the times and situation changed. Not only did the authoritative spokesmen of quantum mechanics disagree with each other, sometimes openly and sometimes subtly, but, as the Pauli example above has demonstrated, even the extant record of an individual prolific writer contains mutually contradictory philosophical declarations which could only be understood within the statements' short-term context.

In order to reduce unavoidable confusion and make sense of changing allegiances, the following analysis imposes two strong chronological restrictions on the use of sources. First, it generally avoids using post-1927 texts in which physicists explained and reinterpreted their earlier views, such as the famous recollections by Bohr, Heisenberg and Born. These later accounts were developed in the context of continued post-1927 disagreements over the foundations of quantum mechanics and they tend to add more contradictions than clarifications if used as sources of historical information about the earlier periods. Second, even within the period of 1925–27, I will impose a finer time scale. The theory developed so quickly that its basic principles underwent fundamental changes approximately every six months. Statements concerning its interpretation also changed at a corresponding rate and it makes little sense to use, say, Heisenberg's pronouncements of spring 1927 for the purpose of understanding what he thought and meant in the fall of 1925, or, conversely, in relying on his initial programmatic statements of 1925 as valid for the resulting mature quantum mechanics. One can, however, describe the state of quantum philosophy at a given stage of characteristic six-month lengths by using only those historical sources which come from that same time period and find, on the one hand, a sufficient number of such sources, and on the other, a significant reduction in contradictions among them.

The following reconstruction depends very heavily on the wealth of the historiography of quantum physics and the insightful observations and interpretative ideas from the existing literature on the topic. Let me mention at the outset only those studies to which I owe the most. Paul Forman in several papers, including the classic 'Weimar Culture, Causality, and Quantum Theory', demonstrated how the ideologically laden concepts of *Anschaulichkeit*, acausality and *Individualität* entered physicists' discourse even prior to 1925 and were subsequently ascribed to quantum mechanics.[19] In another paper, John L. Heilbron described the post-1927

[19] Forman, 'Weimar Culture' (ref. 3); *see also* Paul Forman, 'Kausalität, Anschaulichkeit, and Individualität, or, How Cultural Values Prescribed the Character and the Lessons Ascribed to Quantum Mechanics', in *Society and Knowledge: Contemporary Perspectives in the Sociology of Knowledge and Science*, ed. Nico Stehr and Volker Meja (New Brunswick, NJ: Transaction Books, 1984), 333–48.

spread of the Copenhagen philosophy with its characteristic 'combination of imperialism and resignation'.[20] I shall concentrate on the intermediate period in the hope of establishing a bridge between these two works. John Hendry in his book on the Bohr-Pauli dialogue presented a history of quantum mechanics in the making, along with its philosophy, from a more or less Copenhagen perspective. In contrast, Mara Beller, in a series of papers and a resulting book, developed a critique of the historical myth and of the Copenhagen orthodoxy.[21] For my reconstruction, I use many of their important findings, but also disagree on some points. The reasons for my disagreements are generally twofold — restrictions on the use of sources explained above and my neutral stance on philosophical issues. While admiring physicists' earlier and later interpretations of quantum mechanics as exciting intellectual achievements, I do not feel committed, at least for the purpose of this study, to any of their interpretations in particular.

One of the main conclusions of the analysis below may ultimately disappoint philosophers, namely that having fulfilled the first, historical part of Don Howard's advice and made some sense of the physicists' philosophical statements, it would become clear that the last, philosophical part of his proposal — i.e., to synthesise from them a 'coherent philosophy of physics' — is unrealisable. Physicists' shifting views on philosophical issues can be explained historically, in their own local times and contexts, but taken together as a set they constitute a self-contradictory body of propositions that allows for a variety of irreconcilable interpretations. Overall, the philosophical discourse of quantum physicists appears opportunistic in the sense of Niels Bohr's quote in the epigraph to this paper. Physicists made philosophical statements as if announcing strongly-held principles, but they also kept changing them rather easily, sometimes to almost the opposite in the course of a single year. Furthermore, they also used those statements as rhetorical resources in their intradisciplinary rivalry, in some cases overstating the existing differences, or downplaying and hiding them away, due to tactical reasons and personal relationships.

Mara Beller came to a similar conclusion in her argument 'against the possibility of a consistent version of the Copenhagen interpretation', namely that

[20] J.L. Heilbron, 'The Earliest Missionaries of the Copenhagen Spirit', *Revue d'histoire des sciences* 38 (1985): 195–230.

[21] Hendry, *Creation* (ref. 12); Mara Beller, *Quantum Dialogue: The Making of a Revolution* (Chicago: University of Chicago Press, 1999), and her earlier papers: 'Matrix Theory before Schrödinger: Problems, Philosophy, Consequences', *Isis* 74 (1983): 469–91; 'Born's Probabilistic Interpretation: A Case Study of 'Concepts in Flux', *SHPS* 21 (1990): 563–88; 'The Birth of Bohr's Complementarity: The Context and the Dialogues', *SHPS* 23 (1992): 147–80; 'Schrödinger's Dialogue with Göttingen-Copenhagen Physicists', in *Erwin Schrödinger: Philosophy and the Birth of Quantum Mechanics*, ed. Michel Bitbol and Olivier Darrigol (Gif-sur-Yvette, France: Editions Frontières, 1992), 277–306.

'philosophical pronouncements by quantum physicists are most adequately understood as local, shifting, and opportunistic'. In her view, 'numerous inconsistencies in the Copenhagen interpretation of quantum physics' cannot be explained 'on the basis of the conceptual evolution alone' but 'are of a psychosocial origin'.[22] Rather than placing the chief blame on a particular interpretation, I see in such inconsistencies a general pattern of behaviour of the entire group, regardless of the sides they took in the controversy. The point is therefore not that a particular version of quantum philosophy is unsatisfactory, but that the entire interpretational debate was something else dressed up in philosophical garb. The professional philosophers' feeling of despair comes not from the deficiency of their 'synthetic tools', but from the *a priori* assumption that some consistent and coherent doctrine was hiding behind the pronouncements by physicists. For a philosopher, dropping this assumption would amount to admitting that the discourse was not philosophical in the strict sense. In the conclusion, therefore, I will have to switch the mode of analysis from history of ideas to cultural history in order to understand what kind of activity it was, if not philosophical.

Matrix Mechanics (Fall 1925)

Familiar concepts and images of classical physics were not faring well in the atomic domain. In quieter and more positive times scientists could have remained more tolerant of the developing contradictions, but those who shared the existential experiences of life in Europe during the second decade of the twentieth century were accustomed to seeing crises and revolutions in every venue of life, including science. Often they were more willing than reluctant to read existing problems as signs of foundational crises.[23] The quantum theory of the atom developed since 1913 by Bohr and Sommerfeld with co-workers indicated a radical solution for one such crisis at the price of revising some basic and proven postulates of classical mechanics and electrodynamics. After spectacular successes in understanding and calculating atomic spectra of hydrogen, the theory also encountered problems, in particular, in attempts to generalise it to the case of multi-electron atoms. Again, in some other epoch, ours for example, physicists would have been more inclined to see the glass as half-full rather than half-empty, or at least allow the adolescent theory a little more time to prove itself. In the radical 1920s, however, revolutionary proposals themselves, and not just traditional

[22] Mara Beller, 'The Rhetoric of Antirealism and the Copenhagen Spirit', *Philosophy of Science* 63 (1996): 183–204, on 183, 185; and Mara Beller, 'The Conceptual and Anecdotal History of Quantum Mechanics', *Foundations of Physics* 26 (1996): 545–57, on 545, 550.

[23] On 'crisis' in science, see Forman, 'Weimar Culture' (ref. 3), 26–29.

beliefs, were subject to heightened degrees of criticism. By 1924, circles of physicists around Bohr in Copenhagen and Max Born in Göttingen came to the conclusion that the quantum theory of the atom, too, no matter how young and radical, had entered a state of serious crisis.

To find another radical solution, they were prepared for further sacrifices in the most basic principles of physics. 'Most basic' to them meant philosophical, and being 'philosophically minded' constituted a praise within this circle. It was not quite obvious, however, what exactly had to be sacrificed. The list of possible and tried victims included, but was not limited to: 1) ideas of space and time, 2) energy conservation, 3) causal description, 4) the concept of electromagnetic field and 5) continuity of kinematics.[24] After a number of unsuccessful attempts, they found much promise in a July 1925 paper by Heisenberg[25] and collaborated on the theory, which became known as matrix mechanics. It existed in its original form until the beginning of 1926 with its own characteristic set of philosophical preferences.

Unanschaulichkeit. The first and most distinctive on the list, as demonstrated by Beller's analysis,[26] suggested abandoning the usual ideas about space and time. Our common visual intuitions, one could argue, relied on human experiences in the macroscopic world with objects roughly the size of our own, but did not have to remain valid within the microscopic domain of the atom. Trying to make sense of atomic phenomena with the help of such inadequate intuitive visual (*anschauliche*) representations could be the chief source of contradictions encountered within the quantum theory of the atom. Different formulations of this idea were provided by Bohr (complete space-time representation of atomic processes is impossible), Born (geometry fails within the atom), Heisenberg (positions and trajectories of the electron in the atom do not exist) and Pauli (abandonment of the mechanical, spatial-temporal representation of the stationary state of the hydrogen atom). To build a new theory from the ground up, it had 'first to throw away visual representations of the atom', the *Anschaulichkeit*.[27] Not necessarily rejoicing about this feature, Heisenberg, Pauli, Born, Jordan and Dirac accepted *Unanschaulichkeit* as the basic and necessary premise of the new theory.

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[24] See Hendry, *Creation* (ref. 12), 20, 29, 31, 33, 36, 37, 55 and 64, for relevant quotations from various authors; *see also* PWB, and Darwin to Bohr, 20 July 1919, NBA (ref. 6).

[25] Werner Heisenberg, 'Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen', *ZP* 33 (1925): 879–93.

[26] Beller, 'Matrix Theory' (ref. 21).

[27] Pauli to Bohr, 12 December 1924 and 17 November 1925, both in PWB, pp. 188, 260; Werner Heisenberg, "Über quantentheoretische Kinematik und Mechanik", *Mathematische Annalen* 95 (1926): 683–705, on 684.

Discontinuity. In matrix mechanics, the atomic world was *unanschaulich* in large part because of its fundamental discontinuity. 'In processes at microscopic dimensions of space and time, a discontinuous element plays the dominant role', which could not be adequately expressed and represented with the usual, continuous space-time conceptions.[28] Matrix mechanics inherited not only the discontinuous energy states of Bohr's early atomic theory, but also Born's 1924 program of a 'truly discontinuous theory' which proposed to consistently replace all continuous physical concepts with discrete sets. In matrix mechanics, the transition from classical to quantum theory was achieved accordingly by substituting continuous variables with discrete matrices.

The following two philosophical issues did not play such a major role at the matrix mechanics stage as they had and would in some earlier and later versions of quantum theory. Their very absence is significant, nevertheless.

No waves, no particles. Since matrix mechanics and wave mechanics competed with each other, some commentators tended to assume matrix mechanics favoured corpuscular ontology over waves. Beller rightly criticised this view, but her assertion that matrix mechanics 'was thoroughly permeated by wave-theoretical concepts'[29] is equally untenable (she supported this claim mostly with quotes from the earlier period of the Bohr-Kramers-Slater theory). Both waves and particles were visual representations and thus unsuited for an *unanschaulich* theory. Only outside of the atom did radiation consist of waves while electrons were corpuscles, but in the inside, the electron and its radiation together were represented by a discontinuous and unvisualisable set of matrix elements. Neither pictures of waves nor of particles were useful for its description. The only exception to this attitude came in Born's American lectures of winter 1925, where he tried to combine, somewhat artificially, the *Unanschaulichkeit* of matrix mechanics with Einstein's wave theory of matter, and suggested (even before wave mechanics) the existence of some undulatory process within the atom.[30]

No time, no acausality, no statistics. The idea of acausality together with the statistical conservation of energy had been tried earlier in the Bohr-Kramers-Slater theory of 1923–24. Bohr turned to that risky hypothesis in a last desperate attempt to save the wave theory of electromagnetic radiation from the abhorrent (to him) notion of light quanta. At that juncture, Schrödinger welcomed the acausal idea, while Pauli and Einstein criticised it (the latter not yet doing so as a matter of philosophical principle, but because he had already tried it earlier without much

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[28] Werner Heisenberg, 'Quantenmechanik', *NW* 14 (1926): 989–94, on 989.
[29] Beller, 'Matrix Theory' (ref. 21).
[30] Max Born, *Problems of Atomic Dynamics* (Cambridge, MA: MIT Press, 1926), 69–70.

success). Born and James Franck did not feel happy about it, either, but did not want to contradict Bohr and were trying to say something polite, if vague. Heisenberg, Bohr's formal employee during that year, appeared to accept the idea on the surface, but likely not in his heart: in his papers, he used the language and approach of the Bohr-Kramers-Slater theory, but carefully avoided its most dangerous assumption.

Refuted by Bothe and Geiger's experiment in 1925, the idea seemed to be totally discredited and did not appear in matrix mechanics at all. Pauli's comment in November 1925 to this effect, which strongly rejected the very use of probability in fundamental physical theory, has been quoted above. Heisenberg distanced his new approach from the discredited attempt by purging the very word 'probability' from his matrix mechanics papers. Instead of 'probability of [atomic] transitions', he consistently used 'intensities of emitted radiation'. The two phrases can be used interchangeably in our times, but in the context of 1925 physicists were quite sensitive to this choice of words.

Handling disagreements. The authors of matrix mechanics did not agree on some other interpretational issues. The most serious of these concerned the definition of the basic quantities of the new theory. Born defined them mathematically simply as matrix elements, thus deviating from Heisenberg's original (and not entirely satisfactory) physical definition of them as amplitudes of emitted radiation. What Heisenberg took for the most important physical postulate of matrix mechanics does not even appear in the core of the theory in Born and Jordan's presentation. They only introduce it as an auxiliary assumption ('Heisenberg's *Annahme*') for the purpose of calculating intensities of spectral lines at the very end of their paper.[31]

This discrepancy helps to explain why Heisenberg disliked Born's matrices and was unhappy about the very name 'matrix mechanics'. He contrasted his 'physical' approach to the 'mathematical' one of the Göttingen physicists and had Pauli and probably Bohr on his side. Heisenberg struggled (largely unsuccessfully) to insist on his interpretation of the theory while collaborating with Born and Jordan on the famous *Dreimännerarbeit* of November 1925. From Göttingen, he wrote to Pauli:

> I tried as hard as I could to make the theory more physical, but am only half-satisfied with the result. I am still quite unhappy about the whole thing and was so glad to hear that, with regard to mathematics and physics, you are completely on my side. Here I am in a milieu that thinks and feels exactly the opposite and I

[31] A. Kozhevnikov, 'Electrodynamics in Matrix Mechanics: Discord in Interpretation of the Theory' (in Russian) (Moscow: Institute for History of Science and Technology, 1987).

> worry whether I am just too stupid to understand mathematics … I always feel irritated when the theory is called matrix mechanics and for a time seriously wanted to cross the word "matrix" completely out of the paper and replace it with, for example, "quantum-theoretical variable". (After all, "matrix" is one of the dumbest mathematical words.)[32]

Despite these private complaints, conflicts did not go public. The authors of matrix mechanics chose to collaborate on the new theory. They advanced their diverging interpretations in separate publications, but did not explicitly set them against each other and avoided discussing their disagreements in public.

Wave Mechanics (Spring 1926)

Schrödinger's first paper on wave mechanics of January 1926 cautiously emphasised formalism rather than interpretation. As another precaution, he made a friendly gesture toward matrix mechanics in mentioning that both theories had one basic feature in common: the abandonment of the notion of electron trajectories.[33] The statement was hardly sincere, because the reasons for this abandonment were very different in the two theories. In wave mechanics, the electron did not have a definite position not because of *Unanschaulichkeit*, but because it was represented by a continuous wave and spread out in three-dimensional space. Once Schrödinger had become more confident of the success and power of his theory, he did not need the protective rhetoric any longer and fully engaged in the interpretation business. In March, he established a mathematical connection between the basic formulae of the two theories and proclaimed them 'mathematically equivalent'.[34] This was an understatement — wave mechanics was certainly much more powerful in calculations than matrix mechanics — but the implication was that the criterion for choosing between the two should be interpretation rather than formalism. At that stage, Schrödinger was confident his interpretation had to be preferred.

Complete restoration of Anschaulichkeit. Wave mechanics' main philosophical advantage appeared in the rehabilitation of *Anschaulichkeit*. Not only did the usual three-dimensional geometry remain completely valid on the microscopic

[32] Heisenberg to Pauli, 16 November 1925, in PWB, p. 255. Dirac subsequently designed a special term 'q-numbers' for quantum variables.

[33] Erwin Schrödinger, 'Quantisierung als Eigenwertproblem (Erste Mitteilung)', *AP* 79 (1926): 361–76.

[34] Erwin Schrödinger, 'Über das Verhältnis der Heisenberg-Born-Jordanschen Quantenmechanik zu der meinen', *AP* 79 (1926): 734–56.

scale but even the motion of the electron within the atom could be represented pictorially (the difference from classical theory being the visual image was of a vibrating string instead of a moving corpuscle). The space-time visualisation of microscopic processes once again became possible.

Continuity. In wave mechanics, discrete energy levels are obtained as solutions of a continuous wave equation. One could still, in principle, choose which particular aspect to emphasise as fundamental — continuity or discontinuity — and the question turned into a heated debate in 1926. Bohr wanted to welcome wave mechanics but insisted it should be understood precisely as a description of discontinuous atomic states. Schrödinger, on the other hand, emphasised the continuity aspect alone, taking 'a departure from fundamental discontinuity' as his main philosophical slogan and programmatic goal.[35] For him, not only were discrete energy states artefacts of continuous undulatory processes, but quantum transitions themselves had to be explained as continuous changes from one vibrational mode to another, point particles had to be understood as wave packets and the very relationship between classical and quantum descriptions was to be conceived as 'the *continuous* transition from micro- to macro-mechanics'.[36]

Wave ontology. Although de Broglie's dualistic papers on waves and particles provided initial inspiration to Schrödinger, duality did not figure prominently in wave mechanics during its heyday in the spring of 1926. Schrödinger openly and obviously preferred waves to corpuscles as ontological reality. Radiation appeared in his theory in the form of classical electromagnetic waves. Electrons were seen as corpuscles only on the scale of lower resolution, whereas at the truly microscopic quantum scale they were wave packets of a finite size. (The difference is similar to that between the geometrical and the more fundamental wave optics.) Schrödinger hoped at the time to develop a theory in which all particle-related concepts would be replaced consistently by undulatory ones (for instance, energy would have to be replaced by frequency and the concept of quantum transitions by resonance). Such an ultimate field-like view had no need for wave-particle duality.

No statistics, no acausality. This is the only main philosophical feature that wave mechanics and matrix mechanics had in common. Their principal stakes were elsewhere, but both shared a definite dislike for statistical considerations and deliberately eluded the language of probabilities. Although Schrödinger initially supported the statistical Bohr-Kramers-Slater theory, its defeat must have affected him, too, for, just like Heisenberg, he consistently used the term

[35] Schrödinger to Wien, 18 June 1926, WWM.

[36] Erwin Schrödinger, 'Der stetige Übergang von der Mikro- zur Makromechanik', *NW* 14 (1926): 664–66 (emphasis added).

'intensities' instead of 'transition probabilities'. Moreover, he hoped to explain quantum transitions through a causal and continuous process: in a linear combination of vibrational modes, some coefficients would grow, while others would decrease in time, thus accounting for the gradual transition from one vibrational mode to another.

Reactions to wave mechanics. The rivalry between the two approaches has sometimes led commentators to assume the authors of matrix mechanics accepted Schrödinger's theory only reluctantly, after it found a very enthusiastic general reception among physicists. A distinction between happiness and quickness can provide a more accurate perspective. The captains of matrix mechanics were among the first to abandon the sinking vessel and to start using the new methods of wave mechanics, although in ways that often transcended the boundaries of Schrödinger's original intent.[37] Pauli was the quickest: he learned of the new achievement from Sommerfeld, and in April 1926, simultaneously with Schrödinger, developed the proof of the 'mathematical equivalence' of the two theories. Born was happiest: he easily and enthusiastically converted to wave ontology in his papers of summer 1926. Heisenberg was the unhappiest, but even he used wave functions in his June 1926 paper. Only Dirac was slow, first turning to Schrödinger's methods in August 1926.

Their reaction to the philosophy of wave mechanics was certainly much more critical, but even here some of Schrödinger's accomplishments could not be resisted. *Anschaulichkeit* had to be rehabilitated, at least partially. Much of matrix mechanics' former radical opposition to visualisation of atomic processes quietly disappeared from its authors' subsequent publications in the course of 1926. Besides wave mechanics, another visual concept also contributed to this change of heart: the proposal of the spinning electron gained quick acceptance, despite the initially sceptical reception by Pauli, Heisenberg and Bohr. At the end of the day, Euclidean geometry did not fail within the atom and visual pictures of microscopic processes proved, once again, their usefulness. *Unanschaulichkeit* retained some territory: quantum transitions, or mysterious jumps, avoided visualisation despite Schrödinger's initial hopes. But it became increasingly hard to insist on it as a grand philosophical principle, although Heisenberg (with some assistance from Bohr) continued his desperate struggle against visualisation until the spring of 1927 and his own paper on the indeterminacy principle. A better strategy was to hide the philosophical defeat by shifting the public debate to other issues of controversy.

The wave ontology appealed to at least some of the matrix people. Born, who had liked Einstein's idea of matter waves even earlier, subscribed to it

[37] Kozhevnikov and Novik, 'Analysis' (ref. 18).

enthusiastically. Bohr was also quite sympathetic and Pauli did not particularly object. Heisenberg was as unhappy about waves as just about all other physical ideas of wave mechanics. He wanted to deprive the wave function of its physical meaning as a wave and reduce it to a mere mathematical tool. Dirac also preferred particles to waves and the treatment of the wave function ψ as an abstract mathematical symbol.

The entire group united in opposition to Schrödinger's continuity claim. Born's only disagreement with Schrödinger was to insist that wave mechanics 'permits description not only of the stationary states, but also of quantum jumps'.[38] Pauli wrote to Schrödinger in May 1926: 'I have generally the strongest doubt in the feasibility of a consistent wholly continuous field theory of the de Broglie waves. One must probably still introduce into the description of quantum phenomena essentially discontinuous elements as well'.[39] The stated goal of Heisenberg's two papers of summer 1926 was to prove the essential discontinuity of atomic phenomena, even when described by the Schrödinger function. And famously, the dispute between Bohr and Schrödinger during the latter's visit to Copenhagen in September 1926 centred on their main disagreement on discontinuous quantum jumps.

Quantum Mechanics (Fall 1926)

While appropriating Schrödinger's wave mechanics, Born, Pauli, Heisenberg, Dirac and Jordan did not feel bound by his original interpretations but applied the theory quite liberally to new kinds of problems, thereby changing the meaning of its basic concepts. By generalising the approach to treat the multi-electron problem, Heisenberg and Dirac transformed ψ into a wave function in multi-dimensional space, which eroded its initial visual interpretation as a wave in ordinary space. By applying the method to the problem of scattering, Born, Pauli and Dirac changed ψ into a guiding field for particles and into a probability distribution, once again depriving it of its original physical meaning. By the end of 1926, Dirac and Jordan unified all these new accomplishments into a general scheme under the name of transformation theory and declared the (non-relativistic) quantum formalism completed. Their decisive synthesis brought about further shifts in philosophical positions.[40]

[38] Quoted in Beller, 'Born's Probabilistic Interpretation' (ref. 21), on 567. *Cf. also* Beller, 'Schrödinger's Dialogue' (ref. 21).

[39] Pauli to Schrödinger, 24 May 1926, in PWB, p. 326. (English translation from Hendry, *Creation* (ref. 12), on 86).

Limited Anschaulichkeit. The common perception that Schrödinger lost his philosophical struggle overlooks the major fact that he had basically won the battle for *Anschaulichkeit*. Objections to ordinary geometry, the usual ideas of space and time, and to visual pictures with either waves or particles disappeared. Born used all these notions in his papers on scattering in wave mechanics. Pauli made a further concession and a reversal of his earlier cherished beliefs when he rehabilitated the notion of the 'position of the electron within the atom', the probability of which was now determined by the wave function. Without an open admission of failure, the initial programmatic claim of matrix mechanics was dropped and disappeared from the discourse. However, the restoration of *Anschaulichkeit* did not become absolute: probabilistic arguments imposed restrictions on it. The theory permitted calculation only of the probabilities of electrons' positions and of the still-*unanschauliche* quantum jumps.

Symmetry between continuous and discontinuous representations. In Copenhagen in September 1926, while Bohr and Schrödinger conducted their very intense, principled and stubborn disputes about continuity and discontinuity in atoms, Dirac quietly worked on a paper that would render this entire polemic obsolete. Unlike the rest of the group, Dirac did not label his ideas 'philosophical', but his reformulation of the basic principles of quantum mechanics affected others' philosophical reasoning. Dirac developed a mathematical formalism in which both continuous and discontinuous quantum variables could be used in a relatively symmetrical fashion. His theory allowed transformations from one set of variables to another, thus putting them on an equal footing.[41] The continuity-discontinuity dilemma turned into a choice determined by simple mathematical convenience regarding which particular variables could work better for calculating one or another problem in atomic physics. It no longer made much sense to treat it as a matter of philosophical gravity.

Duality. Following Born's reinterpretation of the wave function as a guiding field for particles, both wave and particle visualisations of microscopic events began to be used, frequently and often interchangeably, in quantum mechanics. Some physicists preferred one over the other but the discipline as a whole demonstrated a rather promiscuous use of both corpuscular and wave pictures (partly justified by the transformation theory, although Dirac personally always gravitated toward particles). A physicist could use one or both of these visualisations as intuitively helpful pictures, but pushing the matter too far by asking for disciplined

[40] P.A.M. Dirac, 'The Physical Interpretation of the Quantum Dynamics', *Proceedings of the Royal Society A* 113 (1927): 621–41; Pascual Jordan, 'Über eine neue Begründung der Quantemmechanik', *ZP* 40 (1927), 809–38.

[41] Dirac, 'Physical Interpretation' (ref. 40).

usage or clear choice between them looked increasingly pedantic and old-fashioned. We may call such widespread carelessness and libertarian use of either wave or particle language with inconsistent switches from one to the other 'duality' to distinguish it from rarer occurrences of 'dualism', or serious statements about the ontological reality of wave-particle chimeras.[42]

Causality and statistics. With the erosion of earlier philosophical principles, a new, statistical idea was on the rise in the fall of 1926 through the contributions of Born, Pauli, Dirac and Jordan. In the corpuscular representation, the wave function determined probabilities of electrons' states and transitions. In August 1926, on the eve of his Copenhagen visit, Schrödinger explained in a letter to Wilhelm Wien his standing on the interpretational issues. Schrödinger rejected *a limine* 'Bohr's standpoint, that a space-time description is impossible', but showed somewhat more understanding for Born's emerging statistical picture:

> Today I no longer like to assume with Born that an individual process of this kind is "absolutely random", i.e., completely undetermined. I no longer believe today that this conception (which I championed so enthusiastically four years ago) accomplishes much. From an offprint of Born's last work in the *Zeitschr.f.Phys.* I know more or less how he thinks of things: the *waves* must be strictly causally determined through field laws; the *wave functions*, on the other hand, have only the meaning of probabilities for the actual motions of light or material particles. I believe that Born overlooks that — provided one could have this view worked out completely — it would depend on the taste of the observer which he now wishes to regard as real, the particle or the guiding field. There is certainly no criterion for reality if one does not want to say: the *real* is only the complex of sense impressions, all the rest are only pictures.[43]

Schrödinger was thus prepared for a compromise, on positivistic terms, between the wave and the corpuscular, the causal and the statistical, interpretations of the theory. If one were inclined to accept waves as the ultimate reality, the fundamental laws of the theory would be causal. If fundamentality of particles were assumed, their laws of motion would be probabilistic. Schrödinger preferred the former option, but was willing to put up with those who gravitated toward the

[42] Alexei Kojevnikov, 'Einstein's Fluctuation Formula and the Wave-Particle Duality', in *Einstein Studies*, vol. 10, *Einstein Studies in Russia*, ed. Yuri Balashov and Vladimir Vizgin (Boston: Birkhäuser, 2002), 181–228.

[43] Schrödinger to Wien, 25 August 1926, WWM. I am thankful to Cathryn Carson for a copy of the original text. The English translation is partially borrowed from Walter J. Moore, *Schrödinger: Life and Thought* (Cambridge: Cambridge University Press, 1989), 225–26.

latter. Born's position at the time, as expressed in his July 1926 paper on probabilistic scattering, seemed compatible. He personally liked the corpuscular and acausal picture rather than the one with waves and causality, but regarded this still as a matter of philosophical preference, not a scientific conclusion: 'I myself am inclined to renounce determinism in the world of atoms. But that is a philosophical question for which physical arguments alone are not decisive'.[44]

Philosophies of Compromise (1927)

In the fall of 1926, three centres could compete for leadership in the new quantum mechanics. In Copenhagen, Bohr was still rather silent in public (and had not published much at all since the failure of the Bohr-Kramers-Slater theory). But he hired Heisenberg as a lecturer, who kept on publishing important papers, attracting new visitors to the institute, and on the philosophical front still defended the remains of the matrix mechanics agenda (*Unanschaulichkeit* and discontinuity). Schrödinger promoted wave mechanics and the ideas of wave ontology and continuity in Zurich. In Göttingen, where the whole thing started, Born was determined to maintain momentum despite the damaging loss of Heisenberg to Copenhagen and, together with Jordan, was developing the probabilistic version of quantum mechanics. The following year, new philosophies appeared which drew upon the earlier ideas in more complex and mixed ways.

Born's move toward acausality. Approximately once a year Bohr invited a distinguished visitor to his Copenhagen institute. Extending such an invitation to Schrödinger indicated Bohr's interest in an agreement, cooperation and a possible deal, rather than a quarrel.[45] Indeed, during their week-long non-stop Copenhagen discussions, Bohr did not push hard on *Unanschaulichkeit* and was sympathetic to the wave mechanics in general and the wave ontology in particular. In return, he wanted Schrödinger to retreat on the continuity claim and accept the fundamental discreteness of atomic phenomena. A compromise along these lines would have included a fusion of the wave mechanics with discontinuous quantum states and jumps of Bohr's earlier atomic theory:

> A few weeks ago we had a visit by Schrödinger, which gave rise to much discussion regarding the physical reality of the postulates of atomic theory. I suppose you know that the wonderful results Schrödinger has arrived at has led to the

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[44] Max Born, 'Zur Quantenmechanik der Stossvorgänge', *ZP* 37 (1926): 863–67. For a more detailed analysis, see Beller, 'Born's Probabilistic Interpretation', (ref. 21), and Nancy Greenspan, *The End of the Certain World: The Life and Science of Max Born* (New York: Basic Books, 2005), 139.
[45] Bohr to Schrödinger, 11 September 1926; Schrödinger to Bohr, 21 September 1926, NBA.

> suggestion, taken up with great enthusiasm from various sides, that the ideas of discontinuity which underlie the interpretation hitherto given of the phenomena might be unnecessary. This appears, however, to be a misunderstanding, as it would seem that Schrödinger's results so far can only be given a physical application when interpreted in the sense of the usual postulates. Indeed they offer a most welcome supplement to the matrix mechanics in allowing to characterize the stationary states separately.[46]

Schrödinger, however, refused to accept discontinuity as stubbornly as Bohr insisted upon it. As we saw above, he preferred a compromise with Born rather than Bohr.

Born, for his part, resolutely declined Schrödinger's advances. As professorial wrangling often goes, he encouraged his *Privatdozent* Jordan to launch an open attack in print on the philosophy of wave mechanics. Schrödinger tried to smooth out the relationship and complained in a private letter to Born, which the latter ridiculed in his private circle. Early in 1927 Born and Jordan publicly proclaimed acausality as the most important philosophical lesson of quantum mechanics. Relying on the new formalism of the transformation theory, they explicitly criticised Schrödinger's wave ontology. On the other hand, their philosophy had room for Copenhagen's favourite discontinuity, thus making possible a compromise with Bohr.[47]

Schrödinger's move toward wave-particle dualism. In the fall of 1926, Schrödinger was named the second choice (after Sommerfeld, but before Born) in the search to fill the most prestigious chair of theoretical physics at the University of Berlin. After Sommerfeld declined as anticipated, Schrödinger accepted the offer and moved to Berlin (his former position in Zurich would subsequently become Pauli's). A win on prestige, however, eventually turned into an institutional disadvantage for Schrödinger. In subsequent years he worked in relative isolation, usually with only a couple of associates in Berlin, while much larger and more active research communities of younger students and postdoctoral visitors grew around Göttingen and Copenhagen (and also later around Heisenberg in Leipzig). In philosophical terms, Schrödinger moved toward an open critique of the statistical interpretation after Born had rejected a compromise: 'Personally I no longer regard this [statistical] interpretation as a finally satisfactory one, even if it proves useful in practice. To me it seems to mean a renunciation, much too

[46] Bohr to Kronig, 28 October 1926, RKZ; *Cf. also* Beller, Schrödinger's Dialogue' (ref. 21).

[47] Pascual Jordan, 'Kausalität und Statistik in der modernen Physik', *NW* 15 (1927): 105–10; Max Born, 'Quantenmechanik und Statistik', *NW* 15 (1927): 238–42; *See also* Beller, 'Born's Probabilistic Interpretation' (ref. 21), 572–73.

fundamental in principle, of all attempt to understand the individual process'. Eventually, he would also retreat from a strong wave ontology and, together with his *Privatdozent* Fritz London, embrace wave-particle dualism. London's lectures on wave mechanics in Berlin opened with a programmatic statement on the dual (wave and particle) nature of quantum objects.[48]

Heisenberg's move to indeterminacy. Born's acausality met with mixed reactions in Copenhagen. Heisenberg welcomed statistics as an argument against Schrödinger's philosophy, but both he and Bohr preferred to view it as a part of 'formalism' rather than the philosophy of quantum mechanics, which both intended to develop on their own. The resulting Göttingen-Copenhagen alliance, if it can be called that, formed out of convenience. On the basis of the shared 'formalism' of quantum mechanics, its major spokesmen advanced de facto diverging interpretational claims, but did not criticise each other's views in public, maintaining at least a posture of good cooperation.

Heisenberg, still Bohr's subordinate, refused to wait patiently. In his famous paper of March 1927, he argued that the statistical formalism led to a fundamental philosophical consequence: the unavoidable uncertainty in the simultaneous measurement of a particle's position and velocity. Although not quite so radical as the *Unanschaulichkeit* claim of the earlier matrix mechanics, it imposed a fundamental restriction on the visualisability of classical theories. In a letter to Ralph Kronig, Heisenberg summarised the combination of philosophical themes of his work as follows: 'I have recently done a paper about the visualizable content [*anschaulichen Inhalt*] of the (certainly discontinuous) quantum mechanics, based on the now completed scheme, which also presents my (or all of us here's) answer to the question: light quanta or waves. You will see it in the *Zeitschrift*!'[49]

Bohr's move toward complementarity. Bohr considered Heisenberg's uncertainty paper premature and they argued intensely over the manuscript. It appears that Heisenberg sent it out for publication without Bohr's approval, in a breach of existing institutional norms. An unwritten but strict rule in German universities at the time, and also in Copenhagen, required students, employees and visiting fellows to submit research papers to journals only with the permission of their professor and director of the institute where the work had been done. No matter

[48] Erwin Schrödinger, 'Der Energieimpulssatz der Materiewellen', *AP* 82 (1927): 265–72, on 272; cited following the translation in Erwin Schrödinger, *Collected Papers on Wave Mechanics* (Providence, RI: American Mathematical Society, 2003), 135–36; Fritz London, 'Quantenmechanik, insbesondere Anwendungen auf die Mehrkörperproblem u.d. Chemie', unpublished lectures at Berlin University, 1928–29, Fritz London Papers, Duke University Archive, Box 4.

[49] Werner Heisenberg, 'Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik', *ZP* 43 (1927): 172–98; Heisenberg to Kronig, 8 April 1927, RKZ.

how justifiably famous, Heisenberg was not yet a professor, even if he was about to become one. Once his anticipated professorship at a German university materialised, he left Copenhagen for Leipzig. It took a couple of additional years to heal, but not completely, his somewhat strained relationship with Bohr.[50]

In terms of scientific papers, Bohr wrote and published almost nothing during the two years when quantum mechanics was being created. Perhaps still recovering from the fiasco of the Bohr-Kramers-Slater proposal, he occupied himself primarily with administrative matters: the expansion of his institute with the help of a Rockefeller grant and arrangements for an increasing number of foreign visitors. Now that both the construction of a new institute building and the formal edifice of the new theory was complete, he felt the urge to develop his interpretation of quantum mechanics. Bohr's writing proceeded, as usual, slowly and required a helper with whom he could collaborate on discussing the manuscript and dictation. Always struggling to arrive at definitive formulations and almost never fully satisfied, Bohr often went through multiple revisions and proofs while dictating his papers. With the help of Oskar Klein, he completed the manuscript by the end of 1927.

Bohr's interpretation is complex and difficult to understand, in part because it draws on everybody else's, as if trying to ensure all important contributors would find something in it they personally cherished: Bohr's own favourite discontinuity, Schrödinger's wave packets, Heisenberg's *(Un)anschaulichkeit* and indeterminacy, and Born's acausality. According to Bohr, there is a fundamental discontinuous, somewhat mystical, individuality (*Individualität*) at work in all microscopic processes and our imperfectly human means of comprehending it. When trying to make sense of atoms, one cannot help but alternate between visual space-time and causal-logical descriptions of events. Both derive from classical physics and our macroscopic experiences and are therefore not entirely suitable for describing the strange microscopic world. But experimental settings, insofar as they involve macroscopic instruments, make it essential that we use such classical languages. An unavoidable and uncontrollable disturbance of the microscopic object in the process of observation imposes limits on their applicability, however. An experimental set-up designed to investigate and determine the space-time picture of microscopic phenomena makes impossible their causal representation and vice versa. In attempting to combine them too literally in quantum physics, as classical physics was able to, one becomes mired in inevitable

[50] 'I was so unhappy last winter, how everything became estranged and how ungrateful I seemed towards you … I hope you can forgive everything that I have done wrong'. Heisenberg to Bohr, 21 August 1927, NBA. *See also* Werner Heisenberg, *Liebe Eltern! Briefe aus kritischer Zeit 1918 bis 1945* (Munich: Langen Müller, 2003), 121–22.

contradictions: it is thus necessary to renounce the possibility of their simultaneous unlimited application within the quantum domain. Each representation separately is also insufficient for understanding the full range of possible experience with atoms, but every imaginable experiment can be accounted for in terms of one or other description. Though based on conflicting sets of notions, these representations should be taken not as mutually exclusive, but complementary — only their combined, alternating use can produce the fullest possible account of the microscopic world.[51]

Discussion

The preceding analysis reveals the intricate ways in which Weimar culture and its ideological values influenced the discourse of physicists during the creation of quantum mechanics. As Paul Forman has described in his analysis, the experience of general social crisis after the war affected scientists' mentality and inspired their talk about 'crisis in science'.[52] The latter notion often implied not merely the economic difficulties of the profession, but also crises in the conceptual foundations of existing knowledge. Scientists became much more willing, in comparison with relatively normal and stable times, to revise or entirely abandon fundamental principles and commitments of their respective disciplines. In the case discussed here, such culturally amplified criticisms were directed not only at basic concepts of classical physics, but even at some key assumptions of the quantum theory of the atom, which had only been around for a decade but was about to become labelled as, characteristically, the 'old' quantum theory.

Cultural concepts also guided the direction of scientists' criticisms and their search for new principles. Had, for example, a larger share of the debate about quantum phenomena taken place in Great Britain, the question of whether electrons have free will would have acquired a major prominence in the new theory. In Central Europe, the discussion centred instead around issues identified by Forman as carrying highly controversial, value-laden meanings within Weimar culture — *(Un)Anschaulichkeit* and *(a)causality* — both of which played leading roles in physicists' thinking and in their attempts to define the new principles of quantum theory. *Individualität* figured less prominently, but did make an appearance in Bohr's complementarity interpretation, essentially standing in for the indivisibility of quanta. The other two foci of the interpretational

[51] Niels Bohr, 'The Quantum Postulate and the Recent Development of Atomic Theory', *Atti del Congresso Internazionale dei Fisici*, vol. 2 (Bologna: Nicola Zanichelli, 1928), 565–88; For a critical analysis of its assumptions, see Heilbron, 'Earliest Missionaries' (ref. 20), esp. 199–200.

[52] Forman, 'Weimar Culture' (ref. 3).

controversy — continuity/discontinuity and wave/particle ontology — belonged to the general tradition of philosophising about physics. Within such a culturally framed debate, as the above analysis shows, quantum physicists found various, often competing and incompatible, ways to apply and translate these general concepts into the language and problems of their specific field.

The way they did this, and their philosophical discourse, appears to be simultaneously high-principled — they were utterly serious in making strongly worded philosophical statements; relatively undisciplined — their conceptions only 'more or less defined by other conceptions'; and opportunistic — the proclaimed principles kept changing too often. Having followed the twists and turns of their discussions, one can hardly avoid the impression that physicists acted as if compelled to hurry up in declaring general philosophical conclusions, which often happened to be premature, because the theory itself was still *in statu nascendi*. It appears proposing a philosophical interpretation was an invaluable act in itself, apart from the choice of a particular philosophy or the probable time for it to hold. Such a behavioural pattern, too, calls for a cultural interpretation.

Some background aspects of the phenomenon, at its most basic and obvious level, are not uncommon, but recognisable as typical and natural for a specific cultural group, the German or German-speaking academics, or *Gelehrte*. The culture of Germanic academe upheld the strong ideal of a scientific genius required to partially double as a philosopher. Culturally, a truly great scientist was expected not only to make discoveries in a special field of research, but to go into it in such depth as to contribute to a general philosophical outlook, and to such conclusions that would be meaningful to all members of educated culture, transcending narrow professionalism and disciplinary boundaries. To this widely shared belief we owe the abundance of printed talks and Habilitation speeches addressed to general academic audiences, in which German scholars discussed broader cultural meanings of their special field of study.[53] This genre of writing has provided some of the most valuable sources for Forman's work on Weimar culture and quantum acausality. For the purposes of current discussion, we can take it for granted as a well-established and entrenched ritual, which in the case of quantum mechanics, however, produced an atypical outcome.

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[53] Hermann von Helmholtz often served as the role model and a typical example of such a combination; see Emil Warburg *et al.*, eds., *Helmholtz als Physiker, Physiologe und Philosoph* (Karlsruhe: Müllersche Hofbuchhandlung, 1922); Lorenz Krüger, ed., *Universalgenie Helmholtz. Rückblick nach 100 Jahren* (Berlin: Akademie-Verlag, 1994). For a discussion of the 'physicist as philosopher' phenomenon, see Cathryn Carson, *Heisenberg in the Atomic Age: Science and the Public Sphere* (Cambridge: Cambridge University Press, 2010).

The very scope of the debate was already unusual — it would be hard to point out another scientific development in which the existing genre of philosophising produced an intellectual fight of such intensity and inconsistency of positions among such a number of prominent participants. The sheer volume of polemical writings and philosophical commentary accompanying the creation of quantum mechanics can be compared, perhaps, with only a case from another culture — the controversy provoked by the first publication of Darwin's *Origins*.[54] Though wider than usual, the circle of those who participated in the interpretational polemics around quantum mechanics was still restricted. It included several recognised leaders, as well as a few unavoidable marginal authors and outsiders to the field, but characteristically not the mainstream contributors to its technical development, the almost 200 post-docs, assistants and PhD students who authored the majority of publications during the first two years of quantum mechanics. More than a decade ago I had an opportunity to meet in Göttingen with one of the last living members of that cohort, Friedrich Hund, and in the course of conversation inquired in passing about Hund's own position in the interpretational controversy. He surprised me at the time by replying straightforwardly that it was not his business, but then added, somewhat more expectedly, 'but, of course, Bohr was right'.[55] As a young assistant in Göttingen and subsequently a postdoctoral fellow in Copenhagen in 1925–27, Hund occupied himself with calculations of molecular spectra using quantum mechanics, but was not entitled to contribute to the public debate about its interpretation.

Besides its strong interest in philosophising, the academic culture that produced quantum mechanics was also extremely sensitive to questions of hierarchy, with both these concerns closely linked. After all, contributing to the generally important philosophical outlook was considered the attribute of a truly great scholar, not necessarily of an aspiring or rank and file researcher. In this respect it is somewhat unusual to find among the entitled participants not only ordinary professors and *Geheimräte*, but also Pauli, Heisenberg and Jordan — all extremely important, but still junior contributors to quantum mechanics. Taking a closer look, however, one can see the precariousness of their participation. Pauli was involved mainly in the informal then unpublished exchange of philosophical ideas, via private correspondence. Jordan essentially entered the public debate on behalf of his professor, Max Born. And even the recognised pioneer, Heisenberg, before he became a professor himself, had a hard time insisting on his right to publish an interpretation that would become known as the uncertainty principle. He did this despite Bohr's reservations only by violating the existing strict, if unwritten, subordination rules governing publication procedures.

[54] I am thankful to Simon Schaffer for this observation.

[55] I am grateful to Klaus Hentschel for the invitation to take part in his interview with Hund.

Participation in the philosophical discourse was thus a mark of prestige, privilege and status — recognition of not merely the social but also the intellectual hierarchy and a person's crucial contribution to the field. 'Perhaps it was also a battle over who did the whole thing first', admitted Heisenberg many years later.[56] In my view, the genre of philosophising did indeed provide physicists with a vehicle for making claims over the entire theory, but the claims were about property rather than priority. Competing philosophical interpretations did not reorder the chronology of individual contributions to the emerging field, but they reassigned the relative importance of those contributions for 'the whole thing'. Nobody questioned Heisenberg's credit as the author of quantum mechanics' first proposal, but he was deeply concerned about the decrease in its perceived value during the months when Schrödinger's interpretation rose in popularity. Similarly, nobody tried or could deprive Schrödinger of his authorship of the theory's central equation, but, depending on the interpretation, his contribution could be presented primarily as 'mathematical' (= technical) rather than 'philosophical' (= fundamental). And Bohr, by offering the last, if not final word on the developing interpretation secured his public reputation as the leader of the new theory, despite the fact that he did not publish on it during its development in 1925–27. In contrast, Schrödinger's failure to establish the prevailing philosophical interpretation signified his loss of control over the field.

The emphasis on what each participant considered his personal major contribution to quantum ideas may explain many of the consistencies and inconsistencies in their philosophical pronouncements. After having invented wave mechanics, Schrödinger abandoned his earlier flirtation with acausality in favour of the (causal) philosophy of continuity and *Anschaulichkeit*. Having reinterpreted the wave function probabilistically, Born and Pauli reversed their pronouncements about acausality and statistics to the affirmative position. Bohr persistently emphasised the fundamentality of discontinuity in quantum phenomena, obviously linked to the postulate of discrete states in his original model of the hydrogen atom of 1913. Einstein had expressed scepticism about quantum mechanics early on, even before it turned acausal, largely because it did not offer an answer to the crucial question — for him, in view of his earlier contributions to quantum physics — on the wave- or particle-like structure of light.[57]

One can imagine a different situation: a major scientific accomplishment belonging, more or less unquestionably, to one distinguished scientist. The ritual of philosophising would be performed in this case, too, as the privilege and duty

[56] Heisenberg, interview by T.S. Kuhn, 1963, AHQP, quoted in Beller, 'Conceptual and Anecdotal History' (ref. 22), on 556.
[57] Kojevnikov, 'Einstein's Fluctuation Formula' (ref. 42).

of a great scholar, but the leader's right to furnish his theory with a general interpretation would also likely have remained unchallenged. The creation of quantum mechanics, in contrast, was a real group effort, although not a team effort. No other great scientific innovation of the period, including relativity theory, had so many crucial and temporarily closely linked contributions from different authors, each with his own agenda and aspirations, and thus so many potential leaders at once. The existing genre of philosophising required quantum physicists to translate the meaning of their scientific accomplishment into the language of cultural and ideological values of the time. At the same time, it also offered a culturally approved and respectable form of public discourse within which they could implicitly, and therefore without losing face, debate their rival claims for the entire theory, which inspired them to develop several competing and incompatible translations. The intensity of scientists' philosophical disagreements corresponded to the unusually high level of intra-disciplinary competition; the latter started long before the theory was in any sense completed, as did the former. New and crucial contributions continued coming; even the most basic assumptions of quantum mechanics were still in flux, as well as the relationships between individual authors. Thus also rhetorical strategies kept changing, resulting in opportunistic shifts in announced philosophical principles between 1925 and 1927.

Contemporaries overwhelmingly perceived Bohr as the ultimate winner in the interpretational debate over the opposition from Einstein and Schrödinger. Philosophers who analyse the dispute today often find it hard to explain from a logical point of view why the Copenhagen philosophy had to be preferred to the arguments of its critics. From the criterion of better adaptation to the cultural values of the time, Schrödinger's *Anschaulichkeit* argument too does not seem to be much weaker than his opponents' acausality claim. How does one then account for the apparent victory of the Copenhagen interpretation in the 1920s? It appears to me that the debate constituted only a tip of the iceberg, albeit the most visible one, of the ongoing intra-disciplinary rivalry. Printed philosophical words by themselves could provide public justifications and rationalisations for the outcome, but not necessarily decide the competition over the new theory.

The latter depended more, I suggest, on mainstream contributors — younger students mentioned by Ehrenfest in his letter quoted in the beginning of this paper, postdoctoral visitors and assistants. These fellows, such as Friedrich Hund, as a rule did not participate directly in the philosophical polemics, but published the majority of papers and calculations, cited others' works and together constituted the decisive reference group. These mainstream contributors to the new theory were not aloof from philosophical arguments, but also influenced by professional opportunities, available problems to solve, financial considerations and the institutional authority of their professors. Their movements between places and the collective body of work submitted by them for publication from the Copenhagen and Göttingen institutes defined the perception of where the leaders of the new field were. On occasions, in particular during the mid-1920s, postdoctoral culture and its burgeoning activity could effectively dominate entire research institutions, acquiring an intellectual momentum of its own, rather than following professors' and directors' agendas. This 'postdoctoral revolution' in science and its impact on the development and character of quantum mechanics ultimately requires a separate analysis of its own.[58]

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[58] Alexei Kojevnikov, '"Knabenphysik": The Birth of Quantum Mechanics from a Postdoctoral Perspective', presented at the American Physical Society April Meeting, 11–15 April 2008, St. Louis, MO.