CHAPTER 34

ORTHODOXY AND HETERODOXY IN THE POST-WAR ERA

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34.1 Introduction

The period after World War II witnessed a revival of interest in the interpretation and foundations of quantum mechanics. Albert Einstein and Erwin Schrödinger re-emerged from their wartime hiatus to resume their criticisms of the theory. In 1948, in a special issue of *Dialectica*, Einstein set out what was perhaps the clearest expression of his philosophical objections to quantum mechanics and the concept of reality that he felt must underpin a complete theoretical description. Einstein was afforded the opportunity to again present his views in the volume for the Library of Living Philosophers the following year (Einstein, 1948, 1949). In 1952 Schrödinger wrote to Niels Bohr that he had 'decided to take a firm stand against' the 'current views in quantum mechanics', particularly 'Born's probability interpretation', which he had 'disliked from the first moment on' (Schrödinger to Bohr, 3 June 1952, BSC, 32.3, AHQP). Other members of the old guard joined the chorus of dissenting voices. After having abandoned his attempt to develop his deterministic pilot-wave theory some twenty-five years earlier, a rejuvenated Louis de Broglie now declared, 'I am convinced that the whole question must be reopened' (de Broglie, 1953, p. 135).

This revival of opposition was further strengthened by events in the Soviet Union. The beginning of the Cold War saw the intensification of the ideological campaign against the enemies of dialectical materialism, following Andrei Zhdanov's speech on 24 June 1947. The subsequent discussions over quantum mechanics at the 1947 Meeting of the Academy of Sciences precipitated a critical attack on the 'Copenhagen school' from a number of Soviet physicists, notably Dimitri Blokhintsev (1952, 1953) and I. P. Terletsky (1952). A new generation of quantum dissidents also emerged in the

The Oxford Handbook of the History of Quantum Interpretation. Olival Freire, Oxford University Press. © Oxford University Press 2022.
DOI: 10.1093/oso/9780198844495.003.0034

early years of the Cold War, intent on pursuing alternative interpretations. David Bohm's ground-breaking paper on hidden variables in 1952 sparked a revival of interest in the subject, particularly among Marxist physicists both in Brazil and France (Bohm, 1952; Bohm and Viger, 1954; Bohm, Schiller, and Tiomno, 1955; Bohm and Schiller, 1955). At the same time the Hungarian physicist Lajos Jánossy proposed a non-linear modification of the Schrödinger equation (Jánnosy, 1952). In 1957, Hugh Everett III submitted his doctoral thesis on the 'relative-state' formulation of quantum mechanics, which would later be dubbed the 'many-worlds interpretation' (Everett, 1957).

In response, the founders of quantum mechanics launched a spirited counter-offensive. Werner Heisenberg, Wolfgang Pauli, Max Born, and Léon Rosenfeld were among the leading physicists to defend the 'orthodox view' against what they saw as fundamental misunderstandings and 'philosophical prejudices' of their critics (Heisenberg, 1955, 1958; Pauli, 1953; Born, 1953, [1953] 1956a, [1953] 1956b; Rosenfeld, [1953] 1979b, [1957] 1979a). It was in this context of debate that the myth of the 'Copenhagen interpretation' was born. When Heisenberg coined the term in 1955, there was in fact no widely shared interpretation of quantum mechanics. The various attempts to defend the 'Copenhagen interpretation' took the form of a series of retrospective reconstructions that often went beyond anything we can find in the writings of the late 1920s or 30s. Various interpretational commitments were appropriated, assembled, reinterpreted, and, in some cases, even revised. As Paul Feyerabend astutely observed, defenders of the Copenhagen viewpoint were often able 'to take care of objections by development rather than reformulation', thereby creating 'the impression that the correct answer has been there all the time and that it was overlooked by the critic' (Feyerabend, [1962] 2015b, p. 104). While historical narratives over the foundations of quantum mechanics have largely revolved around notions of orthodoxy and heterodoxy, little attention has been paid to the *diachronic* history of orthodoxy.

This chapter focuses on the post-war responses of physicists such as Heisenberg, Pauli, von Neumann, Born, Dirac, and Jordan, in an attempt to cast new light on the notion of orthodoxy that has structured much of our historical thinking about the foundations of quantum mechanics. As John Henderson explains, orthodoxy has a 'dynamical character' insofar as it 'is never made, fixed, or closed', but is continually transformed in an antagonistic relationship 'with its silent collaborator and public antagonist, heresy'. It is only in response to heresy, or what might be better termed 'protoheresy', that orthodoxies are framed and articulated. 'Heretical terms and concerns, if not heretical ideas, lay at the heart of orthodoxy' (Henderson, 1998, p. 39). Unlike religious orthodoxies, however, the Copenhagen orthodoxy was never officially codified, nor was it explicitly associated with a set of doctrines by a recognized authority. Henderson's remarks point to a more nuanced understanding of the dialectical history of orthodoxy and heterodoxy. It was largely in response to the challenges presented by Einstein, Bohm, and a new generation of physicists that many defenders of quantum mechanics framed their own distinctive positions. To this extent

I shall argue that the post-war orthodoxy was a dialectical response to the new challenges it faced in the early 1950s.

Yet this dialectic did not lead at any stage to a uniform 'orthodox' position. It was therefore never an orthodoxy in the true sense of the word. Far from creating a unitary Copenhagen interpretation, the post-war debates had the effect of dramatically expanding the range of interpretations that bore the label 'orthodox' or 'Copenhagen'. As early as 1961 Paul Feyerabend noted that while many physicists 'profess to follow either Heisenberg or Niels Bohr' or 'call themselves adherents of the Copenhagen point of view', it is difficult, if not impossible, to find 'some common element in their beliefs'. For this reason, Feyerabend went on to claim, 'there is no such thing as the "Copenhagen interpretation"' (Feyerabend, [1961] 2015a, pp. 74–5). This was evident in the wide range of 'orthodox views' on the measurement problem, the ontological or epistemological meaning of the formalism, and Bohr's doctrine of classical concepts. While Bohr's idea of complementarity is still commonly seen to have formed the central plank in a unified and widely shared 'orthodox view' of quantum mechanics, which emerged in the late 1920s, extensive historical scholarship over the past thirty years has challenged, if not seriously undermined, the notion that any such consensus among the founders of quantum mechanics ever existed. In some cases, these differences amounted to fundamentally contradictory positions (Howard, 2004; Camilleri, 2009).

It might be argued that whatever their differences on the finer shades of interpretation, the defenders of the 'Copenhagen interpretation' were united in the conviction that quantum mechanics was a complete theory, or at least as complete as it could possibly be. However, even this claim does not stand up to close historical scrutiny. While the commitment to completeness has traditionally served as a convenient way to demarcate orthodoxy from heterodoxy, such a clear-cut distinction obscures the subtle but important differences in the ways that physicists such as Bohm, Einstein, Heisenberg, Born, Pauli, Dirac, Jordan, and von Neumann understood completeness in the post-war era. As I shall argue, by the mid-1950s the battle lines between orthodoxy and heterodoxy would become increasingly blurred.

This raises an important question for the historiography of quantum physics. To what extent do the range of disparate views and their transformation over time threaten to dissolve the very notion of 'the orthodoxy' in quantum mechanics? In addressing this question, I suggest that focusing attention on the *satisfaction* that physicists expressed towards the standard view of quantum mechanics (and conversely, the dissatisfaction others expressed), rather than any particular interpretation or defence of it, offers a far more promising way to understand the battle lines that were drawn after the war. However, satisfaction was not an all or nothing affair. While the debates have often been presented as a conflict between two diametrically opposed viewpoints, epitomized in the epic clashes between Bohr and Einstein, in reality we find a spectrum of attitudes ranging from a deep conviction that the new theory represented the final word, to cautious optimism, to tentative scepticism, to uneasiness and outright rejection. Orthodoxy and heterodoxy, on this view, constitute two ends of a continuous spectrum.

Yet, even this fails to capture the subtle dynamics of orthodoxy and heterodoxy. Some physicists did privately express concerns about quantum mechanics in the 1930s, but were not prepared to make these public. Given the variety of views that circulated, it might be the case that we would be better served by speaking of ortho*praxis*, rather than ortho*doxy*. Framing things in this way suggests that what we have traditionally understood as the 'orthodoxy' is not to be found in a specific set of interpretive commitments or even the completeness of quantum mechanics. Instead, it is best understood as a *resistance* on the part of physicists to any attempt to pursue alternative formulations of quantum mechanics. This resistance in some cases extended to an outright denial of the very possibility of alternative views, but in less extreme cases, it was expressed as mere indifference to foundational questions. In some instances, physicists were content to adopt a pragmatic stance, though they privately nurtured the belief that quantum mechanics was only a provisional theory.

34.2 Responses to Bohm: Pauli, Heisenberg, von Neumann

Bohm's papers on hidden variables in 1952 marked a significant turning point in the history of debates over the interpretation of quantum mechanics. Bohm showed, contrary to all expectations, that it was indeed possible to reformulate the theory of quantum mechanics as a deterministic theory, in which particles like electrons moved in continuous trajectories in space. Moreover, such a theory was in complete agreement with the empirical predictions of standard quantum mechanics. This is something that von Neumann, Heisenberg, and Pauli had all deemed impossible in the 1930s. Indeed, all three had devised arguments in support of this view. While Bohm's papers sparked a revival of interest in hidden variables in the 1950s, notably from Jean-Pierre Vigier and de Broglie, they were received with 'a conspiracy of silence'. Max Dresden recalled that Bohm's papers were regarded as 'juvenile deviationism', not worth wasting one's time over.[1] At the Colston symposium in Bristol in 1957, Fritz Bopp expressed a view shared by many physicists: 'Bohm's theory cannot be refuted', but at the same time, 'we don't believe in it' (Körner, 1957, p. 51).

Yet, some physicists did respond to Bohm. While the appearance of his hidden variables theory did not shake their faith in the standard quantum mechanics, it did force several leading physicists to rethink their basic philosophical commitments. Prior to the appearance of Bohm's papers, it had generally been believed that hidden variables theories were impossible. The most famous argument for this conclusion

[1] According to David Peat, after Max Dresden presented a seminar on Bohm's work in Princeton, Oppenheimer stood up and declared, 'If we cannot disprove Bohm, then we must agree to ignore him' (Peat, 1997, p. 133).

was of course von Neumann's impossibility proof of 1932. But Pauli and Heisenberg had also defended this view, basing their conclusions on somewhat different arguments in the 1930s (Bacciagaluppi and Crull, 2009). In a lecture delivered to the Philosophical Society in Zurich in November 1934, Pauli declared that 'no supplementation of the assertions of quantum mechanics by other assertions in the sense of determinacy is possible' without departing from the statistical predictions of quantum mechanics (Pauli, [1936] 1994a, p. 102). Yet, in the early 1950s, Pauli was forced to revise this view. After initially dismissing Bohm's work, on closer inspection he was forced to admit 'the consistency of the causal interpretation' (Bohm to Phillips, December 1951, in Talbot, 2017, p. 143). Writing to Bohm on receiving a revised manuscript of his paper, Pauli stated: 'I do not see any longer the possibility of any logical contradiction as long as your results agree completely with those of the usual wave mechanics and as long as no means is given to measure the values of your hidden parameters' (Pauli to Bohm, 3 December 1951, in von Meyenn, 1996, pp. 436–441).

However, Pauli now raised new objections. While Bohm had managed to circumvent von Neumann's proof, his theory no longer treated position and momentum on an equal footing. In the standard formulation of quantum mechanics, the wave function in position-space is treated on a par with the wave function in momentum-space, but in Bohm's theory, this was not the case. According to Bohm's theory, any attempt to measure the particle's momentum would invariably affect the measured value. It therefore could not be treated as an observable of the theory in the same sense as position. Pauli described this '*artificial asymmetry*' between position and momentum in Bohm's theory as a form of 'artificial metaphysics'. To this end, Pauli argued, 'the interpretation of quantum mechanics based on the idea of complementarity' was 'the only admissible one' (Pauli, 1953, p. 42).

A full account of the objections levelled at Bohm's theory and its general reception among physicists in the 1950s would take us beyond the scope of this chapter. However, it is worth noting that Heisenberg took a similar approach in denouncing Bohm's hidden variables theory, both in his contribution to the *Festschrift* for Bohr's seventieth birthday and his 1955–56 Gifford lectures published as *Physics and Philosophy*. While conceding that 'Bohm's interpretation cannot be refuted by experiment', he dismissed the incorporation of particle trajectories into quantum mechanics as 'a superfluous ideological superstructure', which in his view, had only served to obscure the true nature of the theory. But it was the lack of symmetry that Heisenberg highlighted as the most serious weakness of Bohm's interpretation:

> Bohm's language destroys the symmetry between position and velocity which is implicit in quantum theory; for the measurements of position Bohm adopts the usual interpretation; for measurements of velocity or momentum, he rejects it. Since the symmetry properties always constitute the most essential features of a theory; it is difficult to see what could be gained by omitting them in the corresponding language. Therefore, one cannot consider Bohm's counterproposal to the Copenhagen interpretation as an improvement. (Heisenberg, 1958, p. 118)

Some twenty years earlier Heisenberg had argued that 'a deterministic completion of quantum mechanics is *impossible*' (von Meyenn, 1985, p. 410).[2] He now argued that any such a reformulation of the theory would *not* constitute an *improvement.* Here Heisenberg stressed that all attempts thus far to formulate alternative interpretations of quantum mechanics 'have found themselves compelled to sacrifice the symmetry properties of quantum theory'. This, for Heisenberg, was evidently too high a price to be paid for the retention of the space-time causal description of the electron's motion. To this end, Heisenberg concluded, 'the Copenhagen interpretation cannot be avoided *if these symmetry properties*—like Lorentz invariance in the theory of relativity—are held to be *a genuine feature of nature*; and every experiment yet performed supports this view' (Heisenberg, 1958, p. 128).

Whether the invariance under transformation of position to momentum can be regarded a physical symmetry in the same sense as Lorentz invariance is highly debatable (Myrvold, 2003, pp. 17–8). But from a historical point of view, this should be seen as a significant departure from the earlier defence of quantum mechanics in the 1930s. In an ironic twist, Heisenberg now appealed to an *aesthetic-realist* argument. While Heisenberg conceded that Bohm's theory could successfully reproduce all the empirical predictions of standard quantum mechanics, it failed to exhibit the all-important symmetry properties, which, for Heisenberg, constituted 'a genuine feature of nature'. Von Weizsäcker recalled that Bohm's papers were discussed in a seminar course organized by Heisenberg in the winter term 1953/54. While the 'course strengthened our conviction that all these attempts were false, we could not hide the fact that the deepest reason for our conviction was a quasi-"aesthetic" one. Quantum mechanics surpassed all competitors by its simple beauty that characterizes a complete theory' (von Weizsäcker, 1985, p. 321).

This argument must be seen in historical context. As the physicist Louis Michel later recalled, 'there was an irresistible ascent of the role of symmetries in the fifties ... Most of us then shared the enthusiasm that Heisenberg had at that time' (Michel, 1989, p. 377). In his Nobel lecture in December 1957, Chen Ying Yang declared, 'it [is] scarcely possible to overemphasize the role of symmetry principles in quantum mechanics'. When confronted with 'the elegance and beautiful perfection of the mathematical reasoning involved' together with its 'complex and far-reaching consequences', one could not fail to develop 'a deep sense of respect for the power of symmetry laws' (Yang, 1958, p. 565). As Arianna Borrelli has argued, the aesthetic appeal of symmetry principles was, for many physicists, a sign that 'symmetric theories have a better chance of reflecting inner principles of nature' (Borrelli, 2017, p. 22). These realist associations were evident in a letter Heisenberg wrote to his sister-in-law Edith Kuby in 1958, about the 'incredible degree of simplicity' of the symmetries that guided his unified theory of

[2] The argument was contained in an unpublished manuscript entitled 'Ist eine deterministische Ergänzung der Qauntenmechanik Möglich?', which Heisenberg sent to Pauli on 2 July 1935 (von Meyenn, 1985, pp. 409–18). The original manuscript is reproduced in the Archive for the History of Quantum Physics (microfilm 45, section 11).

elementary particles. 'Not even Plato could have believed them so beautiful. For these interrelationships cannot be invented. They have been there since the creation of the world' (E. Heisenberg, 1984, p. 144). The mathematical structure of quantum mechanics, Heisenberg later declared, 'must be part of reality itself, not just our thoughts about reality' (Heisenberg, 1971, p. 68). These were hardly the utterances of a committed positivist.

We can also discern a shift in von Neumann's thinking during this time, though he adopted a more pragmatic stance. In a letter to Pauli in October 1952 Bohm reported, 'von Neumann has agreed that my interpretation is logically consistent and leads to all results of the usual interpretation' (von Meyenn, 1996, p. 392). Indeed Bohm's sources informed him that von Neumann had even thought it quite 'elegant' (Bohm to Yevick, 16 February 1952, in Talbot, 2017, p. 247). This was quite a concession from the man who twenty years earlier had 'proved' that quantum mechanics was 'in compelling logical contradiction with causality' (von Neumann [1955] 1961b, p. 327). Indeed in two important papers that appeared in 1954 and 1955, von Neumann made no mention of his proof. Instead he now claimed that the 'best description one can give today' is that we 'do not have complete determination, and that the state of the system does not determine at all what it will be immediately afterwards or later'. But, he now acknowledged, this 'may not be the ultimate one (the ultimate one may even revert to the causal form, although most physicists don't think this is likely)' (von Neumann, [1954] 1961a, p. 486). While the 'prevalent taste' up until now been 'in favour of one of the two interpretations, namely the statistical one', he pointed out that 'there have been in the last few years some interesting attempts to revive the other [causal] interpretation' (von Neumann, [1955] 1961b, p. 497). In 1932, von Neumann had argued that 'quantum mechanics would have to be objectively false, in order that another description of the elementary processes than the statistical one be possible' (von Neumann, 1955, p. 325).[3] By 1955, he saw quantum mechanics as 'an example where alternative interpretations of the same theory are possible' (von Neumann, [1955] 1961b, p. 496; See also Stöltzner, 1999, p. 257).

This raises the intriguing question of why the English translation of von Neumann's *Mathematische Grundlagen der Quantenmechanik*, published in 1955 some three years *after* von Neumann had come to learn of Bohm's theory, included the famous proof he had in formulated in the original German edition in 1932. The answer becomes clear once we realize that the translation was actually completed in October 1949, before Bohm had begun working on hidden variables theory. In a letter to the president of Dover Publications on 3 October 1949, von Neumann explained that he had already

[3] There has been much recent debate about how exactly von Neumann understood the significance of his theorem for the question of hidden variables. Jeffrey Bub and Dennis Dieks have defended the proof against the claim that it is "silly" (Bub, 2010, 2011; Dieks, 2017), while David Mermin and Rüdiger Schack have argued that it contains a major oversight (Mermin and Schack, 2018). See the chapters in this volume on 'Hidden Variables' (Bub, Chapter 39) and on the development of the statistical interpretation.

completed a revised translation, having deemed it necessary 'to rewrite Dr Beyer's translation'. The task had taken him about six months in all, and according to his letter, the task had been completed in May 1949 (Neumann to Cirker, 3 October 1949, in Rédei, 2005, pp. 91–2). Owing to problems with copyrights (which were vested by the United States during the war) and finding adequate mathematical types, Dover gave up on the book due to lengthy delays. It would be six years before Princeton University Press would publish the English translation.

We may surmise that by the early 1950s, von Neumann's views had shifted. Eugene Wigner later recalled that von Neumann had always given him the impression that his belief in 'the inadequacy of hidden variables theories' was *not* in fact based on the reasoning in his celebrated proof (Wigner, 1970, p. 1009). The 'true reason for his conviction of the inadequacy of the theories of hidden variables' was that 'all schemes of hidden parameters which either von Neumann himself, or anyone else whom he knew, could think of, . . . had some feature which made it unattractive, in fact unreasonable' (Wigner, 1971, pp. 1097–8). This seems to have been a widely shared view among physicists in the 1930s. While references to the proof were commonplace, the proof itself was seldom examined in any detail.

For von Neumann, the pressing question now was not completeness but theory choice. The 'reason for preferring one version of quantum theory' over the other, he argued, is which interpretation gives 'better heuristic guidance in extending the theory into those areas which are not yet properly explained'. While acknowledging that 'physicists certainly had definite *subjective preferences* for one description or the other', in the end, von Neumann felt the judgment of the scientific community would depend not on philosophical arguments, but on 'which succeeds in pointing the way to explaining wider areas with greater [explanatory] power' (Neumann, [1955] 1961b, pp. 497–8). Here it is worth quoting von Neumann in full:

> while there appears to be a serious philosophical controversy . . . it is quite likely that the controversy will be settled in quite an unphilosophical way. The decision is likely to be opportunistic in the end. The theory that lends itself better to formalistic extension towards valid new theories will overcome the other, no matter what our preference up to that point might have been. It must be emphasized that this is not a question of accepting the correct theory and rejecting the false one. It is a matter of accepting that theory which shows greater formal adaptability for a correct extension. This is a formalistic, aesthetic criterion, with a highly opportunistic flavour.
> (Neumann, [1955] 1961b, p. 498)

In this remarkable passage, von Neumann argued that heuristic considerations, not matters of philosophical principle, would eventually decide between competing interpretations of quantum mechanics. Bohm's correspondence from the time suggests that while von Neumann was convinced of the logical consistency of his theory, he was sceptical about whether it could effectively deal with spin (Bohm to Phillips, early 1952, in Talbot, 2017, p. 147). It is worth noting that 'in spite of all its successes', von

Neumann felt that standard quantum mechanics had not yet led to a satisfactory theory of quantum electrodynamics, nor to a quantum theory of elementary particles. 'About these', he said in 1955, 'we know a great deal less than about the original quantum mechanics, and we are here in the midst of grave difficulties' (von Neumann, [1955] 1961b, p. 498). Here von Neumann left the door ajar for determinism, but most physicists remained sceptical of the heuristic value of Bohm's theory.[4]

34.3 Is Quantum Mechanics Final? Born and Dirac

If Bohm's papers persuaded some physicists that that a deterministic completion of quantum mechanics was at least *possible*, it was not a solution that appealed to the older generation of quantum dissidents. Neither Einstein nor Schrödinger responded with much enthusiasm to Bohm's work. Einstein dismissed it as 'too cheap', while Schrödinger was not much impressed either (Einstein to Born, 12 May 1952, in Born, 1971, p. 192).[5] While it is true Einstein did briefly toy with the idea of hidden variables, and gave his public endorsement to de Broglie's search for pilot-wave theory at the 1927 Solvay conference, it was not a path he found promising and certainly not one he followed himself in his later years. This shows that the claim that quantum mechanics was an 'incomplete theory' was understood in very different ways—both by its critics and apologists.

After the war, Einstein attempted to clarify the sense in which he understood quantum mechanics to be an incomplete theory of individual process. A 'more complete theory' could not be achieved simply by carrying out a *completion* of the existing theory of quantum mechanics. Responding to the suggestion that it might be possible to devise a hidden variables theory, Einstein told one correspondent, 'I do not think that one can arrive at a description of the individual system through a simple completion of the present statistical quantum theory'. He was adamant, 'it is not possible to get rid of the statistical character of the present quantum theory by merely adding something to the latter' (Einstein to Kupperman, 14 November 1953, in Fine, 1993, p. 269). As he explained in a letter in 1954:

[4] At a conference, *New Research Techniques in Physics*, in Rio de Janeiro and São Paulo, on 15–29 July 1952, I. I. Rabi also remarked: 'I do not see how the causal interpretation gives us any line to work on other than the use of the concepts of quantum theory' (Freire Jr, 2015, p. 40).

[5] Einstein presented a detailed criticism of Bohm's theory in his contribution to the *Born Festschrift* (Einstein, 1953). In an exchange of letters between Einstein and Schrödinger in January/February 1953, this criticism was discussed. Schrödinger also contributed a criticism of his own in this correspondence, which Einstein did not find compelling (AHQP, 37, 5–12, 13, 14 and 15). In a letter to Miriam Yevick, Bohm related his disappointment that Schrödinger had objected to Bohm's theory, on the grounds that the transformation theory was the real core of quantum mechanics (Bohm to Yevick, 16 February 1952, in Talbot, 2017, p. 247).

> The present quantum theory is in a certain sense a magnificent, self-contained system that, at least in my opinion, *cannot be made into an individual-theory by supplementing it*, e.g. any more, e.g., than Newtonian gravitational theory can be made into general relativity by supplementation. Somehow one must start from scratch, hard though that obviously is.
>
> (Einstein to Hosemann, 9 August 1954, in Fine, 1993, pp. 269–70)

Einstein's position here was actually close to the one that von Neumann, Pauli, and Heisenberg had defended in the 1930s. Quantum mechanics was deemed a 'self-contained' or 'closed theory', which was no longer susceptible to modification.[6] What was urgently needed, in Einstein's view, was not so much a 'reinterpretation' or a 'completion' of the existing theory, as the construction of an entirely new one. This is where he parted company with Pauli and Heisenberg. Here Einstein was prepared to entertain the possibility that one might have to modify the concepts of space and time in the construction of a new theory, though he admitted, 'I have not the slightest idea what kind of elementary concepts could be used in such a theory' (Einstein to Bohm, 28 October 1954, in Fine, 1993, p. 270).

Writing to Michele Besso in 1952, Einstein conceded that quantum mechanics is 'the *most complete possible theory* compatible with experience, as long as one bases the description on the concepts of the material point and potential energy as fundamental concepts' (quoted in Stachel, 1986, p. 375). Of course, for Bohr, the hope that it might be possible to replace 'the concepts of classical physics by new conceptual forms' rested on a fundamental misunderstanding (Bohr, [1929] 1987, p. 16). Bohr remained convinced, on philosophical grounds, that 'the language of Newton and Maxwell will remain the language of physics for all time' (Bohr, 1931, p. 692). In this sense, quantum mechanics was as complete as it would ever be.

Yet, this was a view of completeness that few of Bohr's contemporaries shared. The question of whether quantum mechanics would ultimately be superseded by some deeper ontological theory was not one that many physicists felt could be answered categorically, or with any degree of certainty. While Max Born dismissed Schrödinger's

[6] Heisenberg developed his notion of a 'closed theory' in later years (Heisenberg, [1948] 1974). As Alisa Bokulich explains, for Heisenberg, 'a closed theory is a tightly knit system of axioms, definitions, and laws that provides a perfectly accurate and final description of a certain limited domain of phenomena' (Bokulich, 2006, p. 91). At a 'Discussion on Determinism and Indeterminism' at the 1965 International Colloquium *Science and Synthesis*, Heisenberg attempted to clarify the sense in which he took quantum mechanics to be complete. 'Let us first speak about the old Newtonian mechanics. Is Newtonian mechanics complete, is it a closed scheme or is it not? I would say—and this may seem to you paradoxical—*it is a complete theory*, and it is absolutely impossible to improve it, in the following sense. If you can describe parts of nature with those concepts which are applied in Newtonian mechanics—namely coordinates, velocities, masses, and so on—then the equations of Newton are exact equations and every attempt to improve these equations is simply nonsense. But of course there are other parts of nature in which these concepts do not apply—this is already so in relativity, was already so in Maxwell's theory, where we had the concept of a field, and is certainly true in quantum mechanics, and so on. And in the same sense, too, I feel that quantum mechanics is complete' (Maheu *et al.*, 1971, pp. 144–45).

suggestion to develop a fully consistent wave theory as 'impracticable', he was careful to qualify his stance. '*I do not want to create the impression that I believe the present interpretation of quantum theory to be final*' (Born, [1953] 1956b, p. 131). In a paper expanding on his remarks at a discussion in December 1952, devoted to Schrödinger's recent papers on quantum jumps, Born took the opportunity to again clarify his position:

> I am far from saying that the present interpretation is perfect and final. I welcome Schrodinger's attack against the complacency of many physicists who are accepting the current interpretation because it works, without worrying about the soundness of the foundations. Yet I do not think Schrödinger has made a positive contribution to the philosophical problems. (Born, [1953] 1956a, p. 149)

In his Waynflete Lectures in Oxford in 1949, Born had made much the same point:

> It would be silly and arrogant to deny any possibility of a return to determinism. For *no physical theory is that final*; new experiences may force us to alternatives and even revisions . . . *I expect that our present theory will be profoundly modified* . . . But I should never expect that these difficulties could be solved by a return to classical concepts. I expect just the opposite, that we shall have to sacrifice some current ideas and use still more abstract methods. (Born, 1964, p. 109)

Far from prohibiting any possible change to the theory, Born left this an open question. Indeed, as early as 1929, Born had conceded in a letter to Einstein: 'the possible future acceptance or rejection of determinism *cannot be logically justified*. For there can always be an interpretation which lies one layer deeper than the one we know' (Born to Einstein, 13 January 1929, in Born, 1971, p. 103).[7] Born did however insist that 'if a future theory should be deterministic, it *cannot* be a modification of the present one, but must be essentially different'. In saying as much, Born was actually closer to Einstein than Bohr. Just how one could rebuild quantum theory anew, 'without

[7] This remark was actually a response to Einstein's criticism of the view Born had expressed in his lecture 'Über den Sinn der physikalischen Theorien', which he had presented at the public session of the *Gesellschaft der Wissenschaften zu Göttingen* on 10 November 1928 (Born, 1929). There Born had addressed 'the question of whether in the future, through extension or refinement', quantum mechanics 'might not be made deterministic again'. In answering this question, Born argued: 'it can be shown *in a mathematically exact way* that the established formalism of quantum mechanics allows for no such completion. If thus one wants to retain the hope that determinism will return someday, then one must consider the present theory to be contentually *false*; specific statements of this theory would have to be refuted experimentally. Therefore, in order to convert the adherents of the statistical theory, the determinist should not protest but rather test' (I have used the English translation in Crull and Bacciagaluppi, 2017, p. 223). In the letter to Einstein, Born wrote that he and Jordan were 'very grateful for your criticism . . . You are, of course, right that an assertion about the possible future acceptance or rejection of determinism cannot be logically justified' and 'of course we should not claim anything for which we have no rigorous proof' (Born to Einstein, 13 January 1929, in Born, 1971, p. 103).

sacrificing a whole treasure of well-established results', Born was happy to 'leave to the determinists to worry about' (Born, 1964, p. 109).

The issue that divided Born and Einstein in the 1940s and 1950s was thus not whether quantum mechanics was complete or not, but whether this mattered. As Born put it, in a letter to Einstein in 1949, 'I am inclined to make use of the formalism, and even to "believe" it in a certain sense, until something decidedly "better" turns up' (Born to Einstein, 9 May 1948, in Born, 1971, p. 175). Recognition of this point raises further questions about what in fact was the dominant view. Even if it was the view of most physicists that quantum mechanics in its current form was perfectly *satisfactory*, this did not necessarily imply a belief in its *finality*. Most physicists were content to use quantum mechanics, without concerning themselves with such questions. They simply did not share Einstein's *anxieties* about quantum mechanics. Yet, such a lack of anxiety should not be confused with a belief in completeness.

The debates over quantum mechanics brought to light questions about the future of physics. As Schrödinger put it, any attempt to draw philosophical conclusions 'from a "supposedly final" physical theory' such as quantum mechanics, as Bohr is wont to do, 'is highly suspect', for the simple reason that 'it is in the nature of any physical theory *not* to be final' (Schrödinger to Bertotti, 24 January 1960, in Bertotti, 1985, p. 85). To judge from the writings after the war, a physicist's attitude to such questions often reflected a mixture of homespun philosophy and idiosyncratic views about the nature of scientific progress. In a lecture entitled 'Phenomenon and Physical Reality', presented at the International Congress of Philosophers in Zurich in 1954, Pauli attempted to clarify the sense in which he understood quantum mechanics to be 'final'.

> The question is never: will the present theory remain as it is or not? It is always *in what* direction will it change? The answer to these invariably controversial questions can never be more than conjecture, even after all the circumstances have been weighed, among which the mathematical and logical structure of the known laws plays at least as great a part as empirical results. (Pauli, [1957] 1994b, p. 134)

Here we find none of the earlier rhetoric of 'proof'. Nevertheless, Pauli remained adamant that whatever surprises the future of physics held, Bohr's principle of complementarity would not be eliminated. In a letter to Born, Pauli wrote: 'I am certain that the statistical character of the ψ-function, and thus of the laws of nature, will determine the style of the laws for at least some centuries', though Born was more equivocal (Born, 1953a, p. 150). Even here Pauli did not categorically rule out the possibility of a new kind of physics at some point in the very distant future. In discussions on this question that took place at the Colston conference in 1957, Fritz Bopp remarked, 'what we have done today was predicting the possible development of physics—we were not doing physics but metaphysics' (Körner, 1957, p. 51).

Some physicists, however, did hazard a guess as to the direction that physics might take in the future. The problems of relativistic quantum electrodynamics, to which von Neumann alluded, were generally not regarded as bearing on the completeness of quantum mechanics. But some physicists did take this view. In June 1936, Dirac wrote to Bohr arguing that 'the beauty and self-consistency of the present scheme of quantum mechanics' did *not* preclude the possibility 'of a still more beautiful scheme, in which, perhaps, the conservation laws play an entirely different role' (Dirac to Bohr, 9 June 1936, BSC, 18, AHQP). Several years later, Dirac made clear the techniques of renormalization developed in the 1940s had not altered his views on the finality of quantum mechanics:

> It seems clear that the present quantum mechanics is not in its final form. Some further changes will be needed, just about as drastic as the changes made in passing from Bohr's orbit theory to quantum mechanics... It might very well be that the new quantum mechanics will have determinism in the way Einstein wanted... I think it is very likely, or at any rate quite possible, that in the long run Einstein will turn out to be correct, even though for the time being physicists have to accept the Bohr probability interpretation. (Dirac, 1982, pp. 85–6)

This was part of Dirac's more general views concerning the nature of scientific progress. In a lecture at the Canadian mathematical congress in 1949, Dirac asserted that the basic structure of quantum mechanics was 'almost certain to change with future development'. As he explained, it is 'a general feature in the progress of science that however good any theory may be, we must always be prepared to have it superseded later on by a still better theory' (Dirac, 1951, p. 11). In sharp contrast to the views expressed by Heisenberg and Pauli, in 1963 Dirac would claim: 'I think one can make a safe guess that [the] uncertainty relations in their present form will *not* survive in the physics of the future' (Dirac, 1963, p. 49). On most accounts, these views would qualify Dirac as an *opponent* of the Copenhagen orthodoxy. Yet, historians and philosophers have been reluctant, for reasons that are not altogether clear, to locate Dirac in the heterodox camp.

34.4 Philosophical Anxieties over Quantum Mechanics: Jordan and Wigner

In some cases, we can discern signs of discontent over quantum mechanics in the founding fathers after the war. Perhaps the most striking example of this can be seen in a little known paper presented for a symposium on the philosophical foundations of quantum theory in 1949 by Pascual Jordan. Jordan took the opportunity to reflect more deeply on the problem of measurement in quantum mechanics. In his earlier book *Anschauliche Quantentheorie*, published in 1936, Jordan had given a fairly standard account of the 'orthodox view' of measurement, in arguing that 'the *act of observation* is what first *creates* the definiteness' in an observed quantity. But he offered no clues as to what exactly occurred during this mysterious 'act of observation' (Jordan, 1936, p. 308). This has generally been considered Jordan's last word on the matter. Yet in his 1949 paper, Jordan admitted, 'there remain some questions about the process of observation itself—questions for which we do not get unambiguous answers because orthodox quantum mechanics treats the concept of "measurement" as a fundamental one which ought not to be analysed'. Here Jordan stressed that, contrary to the impression von Neumann had left, the act of observation 'must *not* be interpreted as any mental process, but as a purely physical one' (Jordan, 1949, pp. 269–70).

The measurement problem was the subject of vigorous debate among physicists in the 1950s and 60s. In a report on de Broglie's book *La Théorie de la Mesure en Mécanique Ondulatoire* written in 1957, Léon Rosenfeld took the opportunity to respond to de Broglie's charge of subjectivism, which he saw as typical of the misguided efforts of a number of physicists in recent years in attacking 'what they believed to be the "orthodox" theory of measurement'. In reality, physicists had simply taken 'a distorted and largely irrelevant rendering of Bohr's argument by v. Neumann'. Here Rosenfeld lamented that von Neumann's work, 'though excellent in other respects, has contributed by its unfortunate presentation of the question of measurement in quantum theory to create unnecessary confusion and raise spurious problems'. Here Rosenfeld bemoaned, 'there is not a single textbook of quantum mechanics in any language in which the principles of this fundamental discipline are adequately treated, with proper consideration of the role of measurements to define the use of classical concepts in the quantal description' (LRP, Box 4 Epistemology, correspondance générale, NBA). The failure of most textbooks on quantum mechanics to deal adequately with the foundational questions was, in Rosenfeld's view, partly to blame for persistent misunderstandings, in particular the suggestion that the consciousness of the observer might play a crucial role in the collapse of the wave function.

While most physicists felt these were issues that had been dealt with adequately in the 1930s, Jordan argued that the measurement problem had *not yet* found a satisfactory resolution, either in Bohr's philosophical writings or in von Neumann's formal treatment. Jordan's anxieties over the measurement problem preceded the wave of criticisms that appeared in the early 1950s. Prior to this point, one is hard pressed to find an orthodox physicist acknowledging that measurement posed a serious problem for quantum mechanics. After completing a thorough analysis of the process of observation involving the absorption and emission of photons by atoms and in experiments concerning the polarization of photons in quantum theory, Jordan could not see how one could avoid the assumption that 'a new axiom or a new physical supposition—not already contained in the Schrödinger equation—is involved':

> Therefore I conclude . . . that the notion of 'decision', 'quantum jump' or some other concept *not contained in the Schrödinger equation* is indeed necessary and unavoidable. It is then apparent that the situation—though it is clear in certain respects—does not allow a complete and final analysis; there remain open certain questions . . . It seems to me that entirely new conceptions are necessary . . . perhaps

> the real problem is to synthesize the two fundamental notions of quantum mechanics [waves and probabilities] and unite quantum mechanics still more intimately with thermodynamics. Unable to do so myself, I should like to emphasize the urgency of further thought upon these questions. (Jordan, 1949, pp. 275, 277)

One might read these remarks as consistent with later attempts to modify the dynamics of the Schrödinger equation. In emphasizing 'the urgency of further thought' and the necessity of 'new conceptions', Jordan here seems to have verged dangerously close to what many would regard as outright heterodoxy. However, exactly in what sense Jordan saw the orthodox formulation of quantum mechanics as 'incomplete' is difficult to say. Jordan suggested that treating entropy as a fundamental quantum concept might serve as 'a point where in the future *some generalization of the present theory* might start' (Jordan, 1949, p. 278). But these ideas were never pursued in systematic fashion. Jordan was among the first 'orthodox' physicists to publicly admit that the measurement problem in quantum mechanics constituted a *genuine problem*—and one that in his view was in urgent need of solution.

Though a number of physicists attempted to develop a quantum theory of measurement based on thermodynamic considerations in the 1950s and 60s, no consensus on measurement was ever reached. In the early 1960s Eugene Wigner publicly defended what he took to be the 'orthodox view', according to which 'it was not possible to formulate the laws of quantum mechanics in a fully consistent way without reference to the consciousness' of the observer (Wigner, [1961] 1983a, p. 169; Wigner, [1963] 1983b). By the 1970s Wigner remained open to the possibility of a fundamental revision of quantum mechanics, and he became increasingly convinced that 'far more fundamental changes will be necessary' (Wigner to Shimony, 12 October 1977, in Freire Jr, 2015, p. 167). He encouraged other physicists to pursue a range of alternative solutions to the measurement problem in the 1960s and 70s, and in doing so, 'helped to legitimize heterodoxy on this subject' (Freire Jr, 2015, p. 167).

Wigner's growing interest in foundational questions in the decades after the war might be portrayed as a gradual conversion from orthodoxy to heterodoxy. However, Wigner claimed he had *always* been troubled by certain aspects of quantum mechanics, and had several discussions with von Neumann on these questions over the years. In an interview with Kuhn in 1963, he explained 'I presented many puzzles to Johnny [von Neumann], which are still not solved and which still bother me on the theory of measurement and interpretation'. While Wigner felt 'there is some mystery here not completely cleared up', he was reluctant to make his views public. As he explained, 'during Johnny's lifetime I somehow did not want to write any paper on this. I don't know why not. As a matter of fact, I did write one, but I felt—well, I don't know' (Interview with Kuhn, 3 December 1963, AHQP). We can only speculate as to why Wigner was reluctant to publish earlier. But this suggests that theoretical physics was a cultural practice with its own socially accepted norms and conventions. An appreciation of this point serves to further complicate the standard historical narrative of orthodoxy and heterodoxy.

34.5 Rethinking Orthodoxy and Heterodoxy

In what sense then can we speak of the 'orthodox view' of quantum mechanics? If Bohr, Born, Heisenberg, Jordan, Dirac, von Neumann, and Wigner offered such different, and in some cases conflicting, views on quantum mechanics, what entitles us to classify them as 'orthodox'? Scholars have typically attempted to answer this question by identifying a set of 'common commitments' that characterize the orthodox view. Yet such reconstructions are deeply problematic in failing to capture the wide variety of philosophical views held by physicists who professed to defend the Copenhagen or orthodox view (Jammer, 1974, p. 87). Both Dirac and Born expressed doubts over whether quantum mechanics was a complete or final theory. And both explicitly raised the possibility of a return to determinism at some time in the future. This suggests that what really divided Dirac and Born from Einstein and Schrödinger was not the issue of 'completeness', but rather that the former regarded the statistical formulation of quantum mechanics as a perfectly *satisfactory* theory, while the latter did not.

As Dirac put it, if we can find an interpretation of quantum mechanics 'that is satisfying to our philosophical ideas, we can count ourselves lucky. But if we cannot find such a way, *it is nothing to be really disturbed about*. We simply have to take into account that we are in a transitional stage'. In short, Dirac saw little reason to be 'disturbed' or 'bothered' with such philosophical problems, 'because they are difficulties that refer to the present stage in the development of our physical picture and are almost certain to change with future development' (Dirac, 1963, pp. 48–9). Here physicists tended to take a long-term historical view. Born too conceded it was *possible*, and even likely, that in time quantum mechanics would be superseded by a better theory. But he saw no reason to be dissatisfied with the current interpretation. By contrast, Schrödinger made no secret of the fact that he had always 'disliked the probability interpretation of wave mechanics'. In the absence of a viable alternative, he reluctantly conceded 'one had to give up opposing it and to *accept it* as an expedient interim solution' (Schrödinger, 1953, p. 20). This highlights the sense in which the 'acceptance of a theory' is by no means straightforward. Casting the attitudes of physicists in terms of simple binaries like 'acceptance' or 'rejection', as Robert Westman has argued, 'all too often masks interesting differences in the meaning of "acceptance"' (Westman, 1975, p. 165). Thus, while we might say that the vast majority of physicists 'accepted' quantum mechanics, insofar as they continued to work with the theory, this in fact tells us little about their views on completeness.

One might then argue that what really separates the 'orthodox' Born from the 'heterodox' Schrödinger was not whether or not they held quantum mechanics to be a complete theory, or even how they interpreted that theory in any deep philosophical sense, but the extent to which they saw quantum mechanics as a *satisfactory* theory. Born can be regarded as orthodox, not because of his adherence to a prescribed set of widely shared ontological or epistemological commitments, but because he was favourably disposed to the statistical formulation of quantum mechanics. On the other hand, Schrödinger's begrudging acceptance of quantum mechanics as a provisional expedient reflected his dissatisfaction. This makes the orthodox–heterodox divide less about a set of objective criteria and more about subjective and personal attitudes based on individual epistemic criteria for a physical theory. These judgments were often based on idiosyncratic views about the aim and structure of physical theory, and thus went beyond the specific question of how to interpret quantum mechanics.

But of course, satisfaction was not an all or nothing affair, and often could be expressed to varying degrees. To this extent, it is perhaps more helpful to see orthodoxy and heterodoxy, not as two polarized attitudes, but as two ends of a continuous spectrum. One could therefore 'accept' the theory either enthusiastically or reluctantly, with many shades of grey in between. In canvassing the possibility of a return to determinism, Dirac and Born both expressed views that might be regarded as heterodox, but at no stage they did they voice a deep sense of dissatisfaction with quantum mechanics. Jordan, on the other hand, did express concerns about the measurement problem. What emerges from a careful examination of the different attitudes of physicists to quantum mechanics is a range of nuanced positions, which are not adequately grasped in terms of the simple dichotomy of orthodoxy and heterodoxy. Some physicists did take the view that quantum mechanics was a complete and perfectly satisfactory theory. Others did not regard it as 'complete', but were not particularly troubled by this state of affairs.

But quite aside from what beliefs physicists may have held about quantum mechanics, there were those who kept their opinions to themselves. The distinction between what one was prepared to say publicly, or in print, and what one thought privately adds a further layer of complexity to standard accounts of the orthodoxy. In a rare moment of candour, Arnold Sommerfeld confessed that he found it difficult to resign himself to certain aspects of quantum mechanics in a letter to Carl Oseen in 1931. 'I am not very happy with "indeterminate [*unbestimmt*] physics"', he wrote, 'especially when young enthusiasts or formalists talk about it in the department for hours'. While Sommerfeld felt compelled to 'acknowledge the legitimacy of the whole way of looking at it', he allowed himself to wonder whether '*perhaps it can still be overcome by some "metaphysics"* (all physics is metaphysics according to Einstein)'. The ingenious thought experiments that Bohr had devised to demonstrate the indeterminacy in measuring a particle's position and momentum did not really strike Sommerfeld as getting to the heart of the matter. 'How inelegant, for example, the general theory of relativity would become, if one were to take into account the precision of measurement there too!' (Sommerfeld to Oseen, 22 February 1931, Eckert and Märker, 2004, p. 322).

There are intimations that other physicists harboured private reservations about other aspects of quantum mechanics. In his interview with Kuhn in 1963, I. I. Rabi recalled that when he arrived in Europe in 1927, Schrödinger's interpretational aspiration for a wave theory of matter was not taken very seriously. There were, as he recalled, simply 'no consequences of it that we could see that were useful'. Physicists accepted the statistical interpretation simply because it worked. Nevertheless, Rabi indicated that like Schrödinger, he had always regarded the probabilistic interpretation as a temporary expedient. Schrodinger 'was always unhappy about the whole thing and *I am, too*, to the very present day, in the sense that *I can't get myself to regard quantum theory as other than provisional* in some way' (Rabi, Interview with Kuhn, 8 December 1963, AHQP).[8] We find a similar view in fellow American physicist, Earle H. Kennard. In an interview in 1970, Robert Marshak recalled that there had been considerable friction in the 1930s between Kennard and Hans Bethe at Cornell, because 'Kennard did not believe in quantum mechanics'. While Kennard made a number of important 'original contributions to the new field of quantum mechanics, he never fully believed in quantum mechanics and used to constantly argue about it with Bethe' (Marshak, Interview with Wiener, 15 June 1970, AIP).[9]

Kennard's views have received little attention, in large part because his published contributions to quantum mechanics in the late 1920s give the impression that he was untroubled by interpretational issues. After spending his sabbatical in Göttingen in 1926, Kennard published a number of important papers over the next few years, building on the probabilistic interpretation. His work greatly extended the understanding of the dynamics of wave packets and he predicted what is now commonly known as the 'Kennard phase' (Kennard, 1927, 1928). Yet, as late as 1929, Kennard would claim that quantum mechanics 'cannot yet be considered as a coherent and completed theory' (Kennard, 1929, p. 78). As Joseph Rouse has argued: 'Scientists can hold heterodox beliefs about fundamental issues in their disciplines as long as their research can be taken into account and used by others' (Rouse, 2003, p. 110).

While silence on such matters has typically been taken as implying assent to Bohr's view, it is not altogether clear what physicists may have thought privately on this question. It is entirely possible that many physicists were happy to *use* quantum mechanics, without committing themselves either way on the question of whether the wave function was the most complete possible description of the state of a system. *Using* a theory does not entail accepting that theory as true or even complete. Anthony Leggett expressed the point beautifully, on the occasion of the Niels Bohr Centenary Symposium in October 1985:

> I start with an awful confession: If you were to watch me day by day, you would see me sitting at my desk solving Schrödinger's equation and calculating Green's functions and cross-sections exactly like my colleagues. But occasionally at night, when the full moon is bright, I do what in the physics community is the intellectual equivalent of turning into a werewolf: I question whether quantum mechanics is the complete and ultimate truth about the physical universe. (Leggett, 1986, p. 53)

[8] American Institute of Physics, Oral History Interviews. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4836

[9] American Institute of Physics, Oral History Interviews. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4760–1.

When Leggett uttered these words in 1985, the landscape of physics had changed appreciably. Fifty years earlier, such views were considered 'high treason' (Schrödinger to Einstein, 23 March 1936, AHQP, 37). 'If there were people in opposition', Alfred Landé later remarked, 'they didn't make their opposition public' (Interview with Kuhn, 8 March 1962, AHQP).[10] But given the views we have presented from such physicists as Jordan, Wigner, Kennard, Rabi, Sommerfeld, Born, and Dirac, one wonders how many other 'werewolves' there might have been who were not prepared to make such a public confession. Perhaps others too entertained such night thoughts. Doubts about quantum mechanics in the early years might well have been more prevalent than we tend to think.

This brings us finally to the distinction between belief and action. Most accounts of the orthodoxy have focused on commitments, beliefs, or doctrines. But it may well be that *practice* is a far more relevant historical category. As Philip Pearle would put it, 'social deviance' in quantum mechanics comes in two forms: 'Closet deviance' is 'the *belief* that standard quantum theory', in spite of its enormous success, 'has conceptual flaws. Outright deviance is the temerity to try and *do* something about it' (Pearle, 2009, pp. 257–8). Few physicists who harboured reservations about quantum mechanics were prepared to go on the attack in public, or pursue alternative lines of research. While Schrödinger, Einstein, von Laue, and Planck remained critical voices in the 1930s, their criticisms were more symbolic gestures of defiance and critical analyses of the existing theory, rather than concerted efforts to develop new research programmes. It was only in the 1950s that new interpretations began to appear, and only in the late 1970s and 80s that these formed the basis of ongoing programmes of research. Reflecting on this history, perhaps we should say that action rather than belief was the true mark of the quantum dissident.

Abbreviations

BSC	Bohr Scientific Correspondence
AHQP	Archive for History of Quantum Physics
LRP	Léon Rosenfeld Papers
NBA	Niels Bohr Archive, Copenhagen
AIP	American Institute of Physics.

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