

Review for *Contemporary Physics*

Quantum Dialogue

Mara Beller, *Quantum Dialogue: the Making of a Revolution*. University of Chicago Press, 1999. Pp. xv + 365. US\$ ???. ISBN: 0 226 04181 6.

Review by: Jeremy Butterfield, All Souls College, Oxford OX1 4AL, England

This a very impressive book: a ‘must-read’ for readers of this journal interested in the history of quantum physics. Since Beller is a distinguished historian of the emergence of quantum physics, it is no surprise that this book—which analyses the ferment of 1925-1927 and the establishment over the next ten years of the orthodox “Copenhagen” interpretation of quantum theory—should be scholarly; and in most cases convincing in its specific claims.

But Beller achieves much more than this. She rebuts widespread views in three diverse fields (which I will discuss in order): the history of quantum theory, its interpretation, and the historiography of science. I hesitate to call these views ‘orthodoxies’: what seems an orthodoxy to an outsider or a novice is often a strawman, or sophomoric, to the expert. But Beller’s targets are certainly widespread. And, by my lights at least, her rebuttals are for the most part convincing.

Within history of physics, let me pick out one target (of some three to six, depending on how one counts). Namely, the idea that the two rival formulations of the new quantum theory—matrix mechanics and Schrödinger’s wave mechanics—had little impact on one another, despite Schrödinger’s proof of their equivalence, and the transformation theory of Dirac, Jordan and von Neumann. Beller argues that on the contrary, there was intense interaction between the various parties, with significant consequences for the development of their views. I shall pick out three such consequences (treated in Chapters 2-3, 4-5, and 6 respectively).

First, the matrix mechanics school (based in Copenhagen and Göttingen) were impressed, even jealous, of the success of wave mechanics, especially its mathematical tractability and Schrödinger’s derivation of the hydrogen spectrum. Indeed, this was one of the main reasons why they retreated from their early prohibition on all use of spatiotemporal concepts within the atom.

Second, the engagement with wave mechanics was also a major influence on Heisenberg’s views and in particular on his 1927 uncertainty paper. Here we find another of Beller’s targets: the idea that Heisenberg’s work in 1925-1927 was motivated by operationalist philosophy (i.e. the view that physical concepts and statements should be wholly definable in terms of observational or experimental concepts and statements). Beller argues that in fact Heisenberg was equally motivated by wanting an intuitive (*anschaulich*) interpretation of the quantum formalism, especially his own matrix mechanics; and she traces how the conflict between these motivations shows up in the papers (e.g. in the interpretation of uncertainty in terms of disturbance). Furthermore, Heisenberg’s situation

illustrates another of Beller's general themes—the sheer intellectual chaos of fundamental physics during those years. She spells out how most of the other “founding fathers” of quantum theory repeatedly changed their minds on both general ideas and countless details, were opportunistic in their research strategies—and were often confused.

Third, Bohr's doctrine of complementarity (first announced at Lake Como in 1927) was much more influenced by wave mechanics than is usually allowed. In particular, Bohr was concerned to reconcile his idea of stationary states with wave mechanics' idea that almost all states are not stationary, but rather superpositions of such.

In the interpretation of quantum mechanics, Beller's principal target is “Copenhagen”. She not only denies that it was the only coherent interpretation of the theory (and associated experiments, both real-life and *gedanken*)—as its advocates claimed; and that it was the best interpretation, or the best one at the time, having won in the heated debates e.g. vs. Einstein. She in effect denies that there *is* a coherent Copenhagen interpretation: she sees it as just a none-too-unified *melange* of vague claims. This view is very controversial; indeed it has already been controverted in the philosophy of physics literature. But here I shall pick out just one of these ‘vague claims’, apart from complementarity itself; (which is notoriously vague—and Beller has no trouble finding inconsistencies in Bohr's formulations).

This is the idea that the indivisibility of the quantum of action makes for an ineliminable, and uncontrollable, disturbance in measurement. After the Einstein-Podolsky-Rosen argument of 1935, which struck Bohr like a “bolt from the blue” (p. 145), Bohr held that this ‘disturbance’ could somehow be non-mechanical—“an influence on the very conditions that define the possible types of predictions regarding the future behaviour of the system” (p. 147), as Bohr puts it in his Reply to EPR. That phrase is famous: but are they weasel words? (And in any case, what do they mean?!) Beller maintains that Bohr's Reply can only work if given an operationalist reading (Chapter 7).

The second half of the book, weaving together history and interpretation, describes how this *melange* of views became orthodoxy. Here there is a considerable amount of debunking of Bohr (as some of Beller's Chapter titles hint, e.g. ‘The Rhetoric of Finality and Inevitability’, ‘Hero Worship’). Some readers will find this overdone; it is certainly hard-hitting—and for readers who like to have heroes, sad. But Beller has marshalled plenty of evidence for her case. One example among many: John Slater reported that before arriving in Copenhagen to work with Bohr, he had thought that ‘although Bohr's papers looked like hand-waving, they were just covering up all the mathematics and careful thought that had gone on underneath. The thing I convinced myself of after a month was that there was nothing underneath’ (p. 259).

In the historiography and philosophy of science, Beller's principal target is what she sees as an undue emphasis on the scientific community and a consensus within it: an emphasis common to diverse approaches to science studies, from Kuhn's “internalist” picture of normal science punctuated by revolutions, to various “social constructivist” approaches. Beller proposes instead (Chapters 1, 14, 15) that scientific work is much more interactive, or in her jargon ‘dialogic’ and ‘addressive’, than it is usually taken to be—at least by historians, if not by practising scientists. Indeed she begins the book

by quoting Heisenberg's remark that 'science is rooted in conversations', and gives a wealth of similar quotes by various figures. She also sees her proposed 'dialogism' as a unifying thread running through all her book's projects. For example, only by recalling how a scientific paper arises from—and is a way of continuing—such conversations, can we reliably discern in it the various, sometimes mutually contradictory, claims that the author is addressing to various other protagonists. I doubt that this applies to scientific papers in general; but it rings true about a period of such intellectual chaos as quantum theory in the 1920s and 1930s.

To sum up: this is an excellent, though controversial, book. Future work on the early history of quantum theory, and especially on the history of the Copenhagen interpretation, starts here.<sup>1</sup>

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<sup>1</sup>Thanks to H. de Regt, F. Muller and J. Uffink for comments.