Interpreting the Quantum World

This is a book about the interpretation of quantum mechanics, in particular about how to resolve the measurement problem introduced by the orthodox interpretation of the theory.

The heart of the book is a new result that shows how to construct all possible 'no collapse' interpretations, subject to certain natural constraints and the limitations imposed by the hidden variable theorems. From this perspective one sees precisely where things have gone awry and what the options are. Various interpretations, including Bohm's causal interpretation, Bohr's complementarity interpretation, and the modal interpretation are shown to be special cases of this result, for different choices of a 'preferred' observable. A feature of the book is a novel treatment of the main hidden variable theorems, and an extended critique of contemporary 'decoherence' theories of measurement. The discussion is self-contained and organized so that the technical portions may be skipped without losing the argument.

This book will be of interest to advanced undergraduates and graduate students in philosophy of science, physics, and mathematics with an interest in foundational problems in quantum physics. General readers with some technical sophistication will also find the book of value.

JEFFREY BUB received his PhD in mathematical physics from London University in 1966, where he studied physics with David Bohm at Birkbeck College and philosophy of science with Karl Popper and Imre Lakatos at the London School of Economics. With Bohm he published a 'hidden variable' dynamical reduction theory of quantum mechanics – the first theory to propose an explicit dynamics for the reduction or 'collapse' of the quantum state on measurement as a solution to the measurement problem. His early book, *The Interpretation of Quantum Mechanics*, was influential in developing the concept of a 'quantum logic,' and his numerous publications on the measurement problem and interpretative issues have helped shape the debate on the conceptual foundations of quantum mechanics. After working with Herbert Feigl at the Minnesota Center for Philosophy of Science, he taught physics and philosophy at Yale and subsequently at the University of Western Ontario. He has held visiting positions at Tel Aviv University, Princeton, and the University of California, and is currently Professor of Philosophy at the University of Maryland. *Interpreting the Quantum World* was joint winner of the 1998 Lakatos Award.

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For David Bohm

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Preface to the revised edition

The appearance of a revised edition presents me with the opportunity to make some corrections and improvements suggested by readers.

The main improvement is a much simplified and strengthened proof of the central theorem of the book in chapter 4. The original proof involved a 'weak separability' assumption (introduced to avoid a dimensionality restriction) that required several preliminary definitions and considerably complicated the formulation of the theorem. Sheldon Goldstein pointed out (private communication) that the proof goes through without this assumption.

What the theorem says can now be formulated quite simply. We know from the 'no go' hidden variable theorems that we cannot generate the probabilities defined by a quantum state, for ranges of values of the observables of a quantum mechanical system, from a measure function on a probability space of elements representing all possible assignments of values to these observables, if the value assignments are required to satisfy certain constraints. What this means is that, if we accept the constraints as reasonable and require that *all* observables are assigned values, we cannot interpret the quantum probabilities as measures of ignorance of the actual unknown values of these observables. In fact, the 'no go' theorems show that there are no consistent value assignments at all to certain well-chosen finite sets of observables, quite apart from the question of generating the quantum probabilities as measures over possible value assignments.[a] We also know that if we consider any quantum state ψ and any single observable R, the probabilities defined by ψ for ranges of values of R *can* be represented in this way, essentially because the Hilbert space subspaces associated with the ranges of values of a single observable generate a Boolean algebra (or Boolean lattice). So the possibility of consistent value assignments, or the representation of quantum probabilities as measures over such value assignments, must fail somewhere between considering a single observable and all observables.

[a] Note that the 'irreducibility of quantum probabilities in this sense arises from certain structural features of Hilbert space, brought out for the first time by the 'no go' theorems. It does not follow from earlier considerations, such as Heisenberg's uncertainty principle, which refers only to a reciprocal relationship between the statistical distributions of certain observables for a given quantum state, and says nothing about hypothetical value assignments to observables.

The question at issue is this: beginning with an arbitrary quantum state ψ and the Boolean lattice generated by a single observable R, how large a set of observables can we add to R before things go wrong? More precisely, what is the maximal lattice extension $\mathscr{D}(\psi, R)$ of this Boolean lattice, generated by the subspaces associated with ranges of values of observables, on which we can represent the probabilities defined by ψ, for the ranges of values of $\mathscr{R}$ and these additional observables, in terms of a measure over 2-valued homomorphisms on $\mathscr{D}(\psi, R)$?[b] The theorem provides an answer to this question on the further assumption that $\mathscr{D}(\psi, R)$ is invariant under automorphisms of the lattice $\mathscr{L}$ of all subspaces of Hilbert space that preserve the ray representing the state ψ and the 'preferred observable' R.

This analysis, in terms of the lattice structure of finite-dimensional Hilbert spaces, has been generalized by Rob Clifton and co-workers to cover continuous observables and mixed states in the general framework of C^*-algebras. See Clifton (1999), Clifton and Zimba (1998), and Clifton and Halvorson (1999).

It turns out that the sublattice $\mathscr{D}(\psi, R) \subset \mathscr{L}$ is unique. In fact, it is a generalization of the orthodox sublattice obtained by taking all the subspaces asigned probability 1 or 0 by ψ as representing 'definite' or 'determinate' properties of the system in the state ψ, and all other properties as indeterminate (so that the propositions asserting that the system possesses these properties in the state ψ are neither true nor false or, as the physicist would say, are 'meaningless' in the state ψ). From the standpoint of the theorem, the orthodox sublattice is obtained by choosing R as the unit observable I, but this choice leads to the measurement problem as I show in sections 4.1 and 5.1. Other choices for R can be associated with various non-orthodox 'no collapse' interpretations, for example Bohmian mechanics and the modal interpretation.

The choice of some 'preferred observable' R other than I requires introducing a dynamics for the evolution of *actual values* of observables associated with the 'determinate sublattice' $\mathscr{D}(\psi,R)$, as this sublattice evolves over time with the unitary evolution of ψ as a solution to Schrödinger's equation of motion. Of course, this dynamics for actual values will have to mesh with the Schrödinger dynamics tracked by ψ. I sketch such a dynamics in chapter 5. It turns out to be a stochastic dynamics that reduces to the deterministic dynamics Bohm introduced for the actual values of position in configuration space in his 1952 hidden variable theory, if we take R as continuous position in configuration space. This question has now been investigated in full generality by Bacciagaluppi and Dickson (1999). Michael Dickson has suggested (private communication) that my analysis of 'environmental modelling' and decoherence in 5.4, insofar as it depends on the details of this stochastic dynamics, might require some fine-tuning. I leave it to future readers to develop a more sophisticated analysis based on the general framework developed by Bacciagaluppi and Dickson.

[b] A 2-valued homomorphism is a map that assigns 1's and 0's to the elements of the sublattice in a structure-preserving way, and so defines an assignment of values to the determinate observables associated with the sublattice.

The uniqueness theorem can be understood 'neutrally,' as a way of relating a variety of 'no collapse' interpretations. But I see the significance of the theorem as much more radical. Quantum mechanics arose as a non-commutative matrix mechanics. As Bohr and Heisenberg saw it, quantum mechanics is a 'rational generalization' of classical mechanics, incorporating the quantum of action and the correspondence principle. What Heisenberg did was to ask how the kinematics of classical mechanics could be modifed so as to yield Bohr's frequency condition for the radiation emitted by an atom, when an electron jumps between orbits, rather than the classical frequency condition. Guided by the correspondence principle, he arrived at a theory of motion in which the representatives of certain dynamical variables do not commute. Shortly afterwards, Schrödinger developed wave mechanics from the wave–particle duality idea of de Broglie and proved the equivalence of the two theories.

Initially, physicists took the wave theory as a new way of modelling the microworld and regarded Heisenberg's non-commutative mechanics as a formally equivalent version of wave mechanics without any special foundational significance. But it soon became clear that the wave version of quantum mechanics did not automatically resolve the conceptual problems implicit in the non-commutative theory. Replacing a commutative algebra of dynamical variables with a non-commutative algebra is equivalent to replacing the representation of dynamical properties by the subsets of a set with the representation of these properties by the subspaces of a vector space, that is, it is equivalent to the replacement of a Boolean algebra for the representation of properties by a non-Boolean algebra of a certain sort. In fact, the salient structural feature of the transition from classical to quantum mechanics, as von Neumann saw (see the discussion in the Coda), is the replacement of a set-theoretic or Boolean structure for modelling the properties of a mechanical system with a projective geometry, rather than wave–particle duality. This structural change introduces a new element, the *angle* between subspaces representing properties, that is not present in a set-theoretic representation. The angles are related to probabilities – in fact, by Gleasons's theorem, to the only way probabilities can be defined on a non-Boolean structure of this sort. In the light of the 'no go' theorems for the representation of these probabilities as measures over possible value assignments on the non-commutative algebra of observables, the fundamental question of interpretation for quantum mechanics is how to understand these probabilities.

Now, the transition from classical mechanics to general relativity can be understood as involving the discovery that geometry is not only empirical but *dynamical*. That is, we now realize that geometry is not *a priori*. It makes sense to ask: what is the geometry of the world? As it turns out, the geometry of our universe is not a fixed Euclidean geometry, but rather a non-Euclidean geometry that changes dynamically as the distribution of mass in the universe changes. The significance of the uniqueness theorem, as I see it, is that just as the transition from classical mechanics to relativistic mechanics involves the discovery that geometry is dynamical, so the transition from classical mechanics to quantum mechanics involves the discovery that possibility is dynamical: the possibility structure of our universe is not a fixed, Boolean structure, as we supposed classically, but is in fact a non-Boolean structure that changes dynamically. The unitary Schrödinger evolution of the quantum state in time tracks the evolution of this possibility structure as a changing sublattice $\mathscr{D}(\psi, R)$ in the lattice of all subspaces of Hilbert space. So the Schrödinger time-dependent equation characterizes the temporal evolution of what is *possible*, not what is *actual* at time t.

At any particular time t, what is actually the case is selected as a sub-structure of the 'determinate sublattice' $\mathscr{D}(\psi, R)$ at time t by a 2-valued homomorphism on $\mathscr{D}(\psi, R)$, just as what is actually the case in a classical world is selected by a 2-valued homomorphism on the fixed Boolean lattice of possibilities. What is actually the case at time t in a quantum world must change over time in a way that meshes with the evolving possibility structure. So, in a classical world, change is described by the evolution over time of what is actual, where what is actually the case at time t is selected by a 2-valued homomorphism – the classical state – as a temporally evolving substructure against the background of a *fixed* Boolean lattice of possibilities. In a quantum world, what is actually the case at time t is selected by a 2-valued homomorphism as a temporally evolving substructure on a *changing* background of possibilities. So in a quantum world there is a dual dynamics: the Schrödinger dynamics for the evolution of possibility, and a dynamics for how what is actually the case changes with time, which turns out to be a generalization of Bohmian dynamics. From this perspective, we can understand the phenomena of interference and entanglement – essentially quantum phenomena – as arising from the way in which what is actually the case at t changes from t to t' in such a way as to mesh with the change in possibility structure from t to t'.

Apart from the new proof of the uniqueness theorem and minor corrections, I have revised the discussion in 5.3 on non-ideal measurements. Guido Bacciagaluppi pointed out (in a review for *Nuncius*) that I had defined positive operator valued measures in a non-standard way. The argument goes through nonetheless, and the new formulation avoids reference to POVs. Bacciagaluppi also pointed out a flaw in the argument I attributed to Rob Clifton in the Coda, about how Lorentz invariance might be preserved in Bohmian modal interpretations, and I have accordingly dropped that argument.

Preface

It was Michael Whiteman,[1] applied mathematician and mystic at the University of Cape Town, who first introduced me to the Einstein–Podolsky–Rosen 'paradox' and other mysteries of quantum mechanics. That was in 1962, and I was hooked. The following year I went to London on a Jan Smuts Memorial Scholarship (a princely sum of £500 for two years, renewable for a third), with the idea of studying philosophy of physics under Popper at the London School of Economics. Popper was visiting the US at the time and, apart from attending Lakatos' fascinating evening seminar on mathematical discovery,[2] I soon withdrew from the program. The sudden immersion in courses on the history of philosophy was too much of a culture shock after four years as a mathematics and physics undergradate.

Whiteman had suggested that I look up his friend G.J. Whitrow[3] if things didn't work out at the LSE. Whitrow's advice was clear: if I was interested in foundational problems of quantum mechanics, the choice was between Bohm in London or Rosenfeld in Copenhagen. Since a move to Copenhagen seemed too daunting a prospect at the time, I telephoned Bohm at Birkbeck College. He agreed to a meeting, with the understanding that he wasn't accepting any new graduate students. Eventually, on the strength of a paper I had written on the Einstein–Podolsky–Rosen argument (with some embarrassingly critical comments on Bohm's 'hidden variable' theory), I persuaded him to change his mind.

The theoretical physics group at Birkbeck consisted of Bohm, Hiley, and about half a dozen graduate students. Bohm was interested in understanding algebraic topology as a process-based rather than object-based formal language for physics, and we worked through the section on polyhedral complexes in Hodge's (1952) book on harmonic integrals in his graduate seminar. I recall being mystified. On the days when he came into Birkbeck, Bohm would arrive in the morning, begin a discussion on a topic he'd been thinking about, and continue until late afternoon. We'd all troop down to the cafeteria for lunch, or sometimes (mercifully, since the food was pretty awful) to one of the restaurants on Charlotte Street nearby. There were lots of gems in those discussions

[1] Author of *Philosophy of Space and Time* (1967), and *The Mystical Life* (1961).
[2] The material was later published as a four part article in *The British Journal for the Philosophy of Science* **14**, 1963–4, and in an expanded version in Lakatos (1976).
[3] Author of *The Natural Philosophy of Time* (1961).

but invariably, after we had brainstormed with Hiley over a particularly intriguing or puzzling idea, Bohm had thought of some entirely new way of looking at the matter by the next session.

After a while, it began to seem increasingly unlikely to me that I'd ever manage to work on anything long enough to launch a PhD dissertation. I started to spend more time away from Birkbeck doing my own reading, and I also began to visit the LSE regularly, where I sat in on Popper's seminar after he returned. It was during this period that I stumbled on a paper by Margenau (1963a) on the measurement problem. I read all the references in the bibliography I could lay my hands on, including London and Bauer's *La Théorie de l'Observation en Mécanique Quantique* (1939), which I managed to obtain from the publisher in Paris and laboriously translated into English.

When I surfaced again at Birkbeck, Bohm asked me to give a seminar presentation to the group on what I had been doing. He seemed genuinely interested and suggested I take a look at papers by Wiener and Siegel (Wiener and Siegel, 1953, 1955; Siegel and Wiener, 1956) as a way of resolving the measurement problem. Bohm's thought was that one should be able to exploit the Wiener–Siegel 'differential space' approach to quantum mechanics to construct an explicit nonlinear dynamical 'collapse' theory for quantum measurement processes.[4]

After that, I knew I had my PhD topic. We eventually published the theory as 'A Proposed Solution of the Measurement Problem in Quantum Mechanics by a Hidden Variable Theory' (Bohm and Bub, 1966a), together with a critique of the Jauch and Piron 'no go' theorem for hidden variables (Bohm and Bub, 1966b). Both articles appeared in the same issue of *Reviews of Modern Physics* as Bell's seminal critique of 'no go' theorems (Bell, 1966). As I recall, the underlying ideas of the theory were Bohm's, but he left it to me to work out the details. Bohm was never very interested in mathematical rigour – he simply 'saw' that things would work out in a certain way, and his physical intuition was always on the right track (although it was sometimes a frustrating business to map out all the twists and turns in his thinking).

I was Bohm's graduate student from 1963 to 1965. After I left Birkbeck we corresponded regularly for a few years. Bohm continued to work on the 'collapse' theory during the late 1960s – I have a lengthy unpublished manuscript of his dating from that time – but eventually we both lost interest.[5] I discovered quantum logic which, for a while, I thought was the answer to all the conceptual puzzles of quantum mechanics. Bohm found my fascination with quantum logic incomprehensible, and our correspondence languished.

[4] Curiously, he did not bring up his own 1952 hidden variable theory in this connection, and I don't recall him discussing the theory while I was a student at Birkbeck.

[5] The manuscript ('On the Role of Hidden Variables in the Fundamental Structure of Physics') has now been published in *Foundations of Physics* **26**, 719–86 (1996). The Bohm–Bub theory has recently been resurrected by Ron Folman (1994, 1995), who has been looking for experimental confirmation in a possible deviation from the quantum mechanically predicted experimental distribution for the decay time of massive particles, specifically the tau lepton, in the very short decay time region. See also OPAL Collaboration (1996). For an account of some earlier experimental tests of the theory, see Belinfante (1973), chapter 4.

My first and most important intellectual debt is to Bohm, and this book is dedicated to his memory.

After I left Birkbeck, I had a one-year post-doctoral position in the Chemistry Department at the University of Minnesota. Alden Mead, a physical chemist in the department, visited Birkbeck on a sabbatical during my last year there and offered me a position as his assistant. I was supposed to work on fundamental length theories, but I don't think much became of that. Ford Hall, the home of the Minnesota Center for the Philosophy of Science, beckoned from just across the mall.

The Center was an exciting place. Herbert Feigl and Grover Maxwell were there permanently, and there were many visitors. Hilary Putnam passed through and gave a talk on quantum logic that profoundly influenced my thinking on the interpretation of quantum mechanics. I was captivated by von Neumann's (1939) notion of a non-Boolean logic for quantum systems, and the idea that what is conceptually puzzling about quantum mechanics relative to classical mechanics is that the properties of quantum systems 'fit together' in a non-Boolean way, and this is what we ought to try and understand.

At the Center I met Bill Demopoulos. We were both intrigued by quantum logic – after a brief joint flirtation with Whitehead's process philosophy it was like a breath of fresh air. Working through the Kochen and Specker papers (1965, 1967) together was the beginning of a long collaboration and friendship. The ideas on quantum logic in my book *The Interpretation of Quantum Mechanics* (1974) reflect this collaboration. I still think the essential difference between classical and quantum mechanics is captured by the insight that going from classical to quantum mechanics involves the transition from a Boolean to a non-Boolean possibility structure for the properties of a physical system (see Demopoulos, 1976).

Bohm's ideas on hidden variables, and von Neumann's concept of a quantum logic understood as a possibility structure for events, have always been the two main influences on my approach to the conceptual problems of quantum mechanics. In a sense, this book reconciles these two opposing themes, from the perspective of a 'modal' interpretation in the sense of van Fraassen (1973, 1974, 1981, 1991), although the implementation of this notion is very different from van Fraassen's. But my more immediate intellectual debt is to Rob Clifton, with whom I have enjoyed a lively and extremely productive email correspondence and collaboration for the past two years or so. Much of the book is an extended discussion of our joint paper on a uniqueness theorem for 'no collapse' interpretations of quantum mechanics (Bub and Clifton, 1996).

I had constructed a class of 'no collapse' interpretations (Bub, 1992a, b, 1993a, 1994b, 1996), which I presented variously as versions of the modal interpretation or as Bohmian interpretations (in the sense of Bohm's 1952 hidden variable theory). The basic idea came out of my analysis of Bohr's reply to the Einstein–Podolsky–Rosen argument (Bub, 1989, 1990). Clifton was working on modal interpretations that exploit the biorthogonal decomposition theorem and proved a result justifying the common framework of the Kochen and Dieks formulations as unique, subject to certain constraints (Clifton, 1995b). After I received a draft of this theorem, I proved a uniqueness theorem for the class of 'no collapse' interpretations I had constructed (Bub, 1994a, 1995c). Later, Clifton saw the possibility of replacing the assumptions in my original uniqueness theorem with fewer and more natural assumptions, which eventually led to our joint theorem. My account of the motivation for the theorem, and the significance of the theorem for the modal interpretation, Bohmian mechanics, and Bohr's complementarity interpretation draws on the analysis in our joint paper. Needless to say, I bear sole responsibility for any foolishness in this exposition.

Like many others working on problems in the foundations of quantum mechanics, I have found enlightenment and inspiration in the writings of John Bell and David Mermin, and over the years I have benefited from discussions on the interpretation problem and the measurement problem with Roger Cooke, Bas van Fraassen, R.I.G. Hughes, Allen Stairs, Itamar Pitowsky, Michael Redhead, Harvey Brown, Jeremy Butterfield and, more recently, David MacCallum, David Albert, Andrew Elby, Jeff Barrett, Ron Folman, Pekka Lahti, Bradley Monton, Michael Dickson, Guido Bacciagaluppi and Jitendra Subramanyam. Rob Clifton, Bradley Monton, David MacCallum, and Jo Clegg read the manuscript in various drafts, and I have incorporated many of their suggestions for improvements in both style and content.

Finally, I owe a special debt to my wife, Robin Shuster – muse extraordinaire and Socratic midwife to many of the ideas presented here. I doubt that I would have completed the book without her constant encouragement and support.