Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony. James T. Cushing. 333 pp. University of Chicago Press, Chicago, 1994. Price: $65.00 (cloth) ISBN 0-226-13202-1, $27.00 (paper) ISBN 0-226-13204-8 (Reviewed by Tim Maudlin.)

What if it turned out that the famously bizarre, unsettling, and obscure elements of quantum theory, including the strange indeterminacy of properties (complementarity), the central role of measurement and observation, the "shifty split" between system and apparatus or between the quantum and classical world, and even indeterminism itself could all be avoided in a theory with the same empirical import as standard quantum mechanics? And what if the theory which eliminated all these problematic elements had been available to the founders of the Copenhagen interpretation? Surely they would have jumped at the chance to recover a clear, determinate picture of the world in which the observer is just another physical system governed by the same laws as all others—wouldn't they?

It is more and more an open secret that such a theory does exist—and has since 1927. Originated as the "pilot wave" theory by Louis de Broglie, recovered and improved by David Bohm, thrust back into view by the work of John Bell, the so-called "causal" or "ontological" interpretation of the quantum formalism allows one to maintain that a single set of perfectly determinate laws governs the evolution of everything in the universe, without any dichotomy between subject and object, observer and observed, potential and actual, or conscious and insensate. And despite determined efforts to ignore the theory in the hopes that it will go away, Bohmian mechanics seems to be gaining a growing following among those interested in the foundations of physics.

Upon first hearing of Bohm's theory, one's first reaction is that it must somehow be impossible. But there are the equations. One's second reaction is that there must be some serious interpretive problem (more serious than those surrounding the Copenhagen interpretation). But there isn't. And the third reaction is to ask how the Copenhagen interpretation came to be orthodoxy with such an alternative interpretation available. Cushing's book is the first historical work directed precisely at this question, and should be of interest to anyone interested in the foundations of physics.

The story of the causal interpretation invites many different questions, some purely physical, some historical, and some methodological. The first question, of course, is what the interpretation is. Cushing provides a detailed, if compact, presentation. The second question is what grounds we could have, now, to prefer Copenhagen over Bohm or vice versa. Here Cushing is mostly content to argue for parity: no presently available evidence rules out either interpretation, and neither holds a decisive edge in internal coherence and consistency. Bohmians there are who would argue that the causal interpretation is vastly superior in point of clarity and conceptual rigor, but for Cushing's purposes parity is enough to motivate the next query. For if Bohm's theory is at least as good as the prevailing Copenhagen interpretation, why isn't it taught, debated, and discussed in physics classes? Why aren't students who are struggling to understand the quantum theory given the causal interpretation to ponder? How, in short, did the Copenhagen interpretation acquire and maintain hegemony as a way of understanding the quantum formalism?

Cushing's answer is in his title: historical contingency. Cultural factors, factors which could well have been different, were at work in the formative years of the quantum theory. And although those factors did not account for the quantum formalism itself, which was produced as a response to objective empirical difficulties, they did decisively influence the way the formalism was understood: "I argue that 'internal' factors were most important for the emergence of the formalism of quantum mechanics, 'external' ones for the nature of the interpretation that was accepted" (p. 110). The external grounds include some that are purely sociological (the Copenhagen group was highly unified while its opposing camp tended to be fragmented), and some that are simply errors, such as mistaking consistency proofs for uniqueness proofs or failing to appreciate the conditions under which the so-called "no hidden variables" theorems of von Neumann were proven. And once the Copenhagen view gained ascendancy, inertia set in. Rival interpretations that made no new testable predictions were ignored since, after all, they don't really give us any new experiments to perform. The practicing scientist sticks to the prevalent interpretation until there is new *empirical* evidence that shows it to be untenable: one doesn't pause to reflect that had a rival interpretation (such as the causal interpretation) been initially adopted, it would be Copenhagen that would be dismissed as idle speculation.

Cushing is impressed, and concerned, that our fundamental world picture should be just a matter of chance. The average physicist is trained to think that the world is at base indeterministic, that there is something special about events that count as observations, perhaps even that the universe would not exist if someone were not here to experience it. Yet had things gone just a little differently, had the causal interpretation won its initial battles rather than losing them, we would be taught that the world is deterministic, that observers are physical systems just like the observed. Indeed, he is so impressed that he traces out a complete counterfactual scenario: if Bell's theorem had been proven in the 1930's, perhaps Einstein would have embraced the pilot wave theory rather than rejecting it, and with Einstein's imprimatur—who knows? As it was, Einstein himself played with something like Bohm's theory but rejected it on the grounds of nonlocality. What if he had known that no local theory can recover the predictions of quantum mechanics?

Such history-of-science fiction is amusing and stimulating, but shouldn't be taken too seriously. We don't know what Einstein would have said in those circumstances, nor how the broader physics community would have responded. Einstein's opposition to the Copenhagen interpretation was in fact well known, and he had powerful arguments on his side (the Einstein–Podolsky–Rosen argument), but all that didn't stop Bohr and company. And ultimately, it doesn't really matter how history might have gone. What is important is what *we* ought to think, how *we* should regard the causal interpretation, given what we now know.

Despite the historical contingency that Cushing focuses on, the founders of the quantum theory were not demonstrably irrational. They had an interpretation, albeit a problematic one, and all Einstein had was a program and wishful thinking. And Einstein was not irrational to reject the pilot wave theory as the correct solution: it was clearly nonlocal, and Einstein had no grounds to believe nonlocality to be unavoidable. Bohm's 1952 theory certainly got an unforgivably rocky reception, which Cushing amply demonstrates, but even a sympathetic theorist such as Bell was concerned about its manifest nonlocality. Indeed, it was just this concern that led him to his famous inequalities, and to his realization that nonlocality is a price that any theory must pay to retain the quantum predictions.

Bell's theorem changes everything. If the world is nonlocal, then it can be no objection that a theory is. There can be no doubt that Bohm's theory deserves at least as much respect and attention as the Copenhagen view. It is distressing that the theory is still so little known and appreciated. But through all adversity, the causal interpretation has survived and is now even prospering a bit. Cushing's work, as well as that of dozens of physicists and philosophers, is steadily raising it from obscurity.

Finally, what general lessons are there to learn from this episode? If our basic picture of the world can be so heavily influenced by historical contingency, what about the rest of our beliefs? Are there analogs to Bohm's theory everywhere, viable rival pictures of the world that lie undiscovered or, worse, discovered but neglected?

Cushing ends his book with some reflections on this broader question, the underdetermination of theories by evidence, and the act of faith that confronts us when two fundamentally different interpretations of the same formalism exist. But, fortunately, this situation seems to be extremely rare. Most disputes in science, such as that between the advocates of WIMPs and the partisans of MACHOs, can eventually be settled by observation. Many theorists may bet on the wrong horse, but in the end we know which horse won. The interpretive difficulties surrounding the quantum formalism are nearly unique in this respect. They can't be settled experimentally, and the relevant arguments are extremely subtle. As an example, even Cushing himself does not get Bell's theorem quite right: he repeatedly characterizes it as offering a choice between determinism and locality, when no such choice exists. Not even an indeterministic local theory can reproduce the predictions of quantum mechanics.

We have gotten into this difficult situation regarding the interpretation of the quantum formalism because the problems are hard and the arguments elusive. Anyone who wants to understand where we are, and particularly how we got here, should read this book.

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The Collected Works of Eugene Paul Wigner. Part A: Scientific Papers. Volume V: Nuclear Energy. Edited by Alvin M. Weinberg and Alfred M. Perry. 808 pp. Springer-Verlag, New York, 1992. Price: $129.00 ISBN 0-387-55343-6. (Reviewed by Lawrence Ruby.)

On January 1, 1995, pneumonia took the life, at age 92, or Eugene P. Wigner,[1] a pioneer of the nuclear age who has aptly been termed "the father of nuclear engineering in the U.S." Volume V is concerned with his contributions to this field and is an essential resource for historians of the nuclear age. After an introductory survey of Wigner's accomplishments by his longtime friend and collaborator, Alvin M. Weinberg, the book consists of a memoir by Wigner on his experiences in the Manhattan Project up to the time of the operation of the first Hanford pile. The memoir is followed by some of Wigner's major papers, annotated by Weinberg, on production reactors, breeders, shielding, radiation effects in graphite, resonance capture, temperature effects on reactivity, and reactor kinetics, to mention only a few. Volume V also contains some papers by Wigner on the sociological problems of nuclear energy and concludes with the reproduction of some of his many patents in the field of nuclear engineering.

In the years following his immigration to the U.S. in 1930, Wigner felt strongly about the threat to Western democracies from the rise of German militarism. When the discovery of fission was announced in 1939, scientists in many countries saw the possibility of a nuclear chain reaction which could release immense energy and have important military applications. At Princeton, Wigner and Breit calculated the critical mass of an explosive based on a fast-neutron reaction in uranium-235, and similar calculations were made by Fermi and by scientists in other countries. The story has been told many times about how Szilard and Wigner convinced Einstein to write a letter to President Roosevelt, alerting the government to the military potential of the fission process and urging a U.S. research program to counteract a likely effort on the part of Germany to make a nuclear explosive. U.S.-born scientists were also aware of the potentialities of fission, but as Weinberg observed, "...at the very beginning in the United States the political momentum was generated mostly by the refugee scientists—Szilard, Wigner, Teller, and, possibly with a lesser sense of urgency, Fermi."

In early 1942, U.S. work on a nuclear explosive was concentrated at the Metallurgical Laboratory of the University of Chicago, and Wigner was selected to head the theoretical group. By then, he had already studied many of the problems involved in the design of a chain-reacting system dedicated to the production of plutonium-239. In Chicago, Wigner's group designed a water-cooled graphite-moderated production pile. By the time the DuPont Company was assigned the responsibility for the construction of such a pile, Wigner's group was able to turn over to them a completely engineered design, dated January 9, 1943, for a 500 MW reactor which would produce 500 g of plutonium-239 per day. The reactor, as eventually constructed by DuPont, was virtually identical to the design supplied by Wigner's group. Wigner's memoir makes much of the agonies experienced by the scientists in working with the industrial giant DuPont during the construction phase. The core of the reactor was a cylinder of graphite of octagonal cross section, with its axis horizontal, containing aluminum process tubes for uranium ingots sealed in aluminum cans. The core was surrounded on three sides by a rectangular graphite block and then by an outer radiation shield. In the original design, there were to have been 1695 process tubes, holding 200 tons of uranium metal. As constructed, there were 1500 tubes in the core, but DuPont provided 504 extra tubes in the unused corner regions of the octagon. Only this additional capacity allowed the reactor to operate at its design power, since it was discovered, on initial testing, that a voracious neutron poison, xenon-135, was building up as one of the fission products in the uranium. It is remarkable that Wigner's engineering design for such a giant structure (40 feet to the top of the shielding) was done mainly by physicists accustomed only to table-top experiments, and that it involved a scale-up of 10^8 in power over Fermi's original pile, which did not attain its first criticality until Wigner's design study was nearly completed. The production reactor was supposed to operate for only 100 days,