A multifaceted scrutiny of the foundations of quantum theory

Open Questions in Quantum Physics

Edited by G. Tarozzi and A. A. van der Merwe

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Reviewed by Daniel M. Greenberger

There has been in the last few years an enormous resurgence of interest in the foundations of quantum theory. This interest has been sparked partly by the development of several beautiful experimental tests of the issues raised by the Einstein–Podolsky–Rosen paradox as well as other experimental techniques for probing quantum phenomena and for verifying the results of many of the standard textbook *gedanken* experiments. (See, for example, PHYSICS TODAY, April 1985, page 38.) Part of this interest is due to an abiding discontent with the foundations of the subject.

Other theories whose conceptual frameworks have produced uneasiness have generally foundered in the attempt to encompass new experimental results. But quantum theory has in fact passed all experimental tests to date with flying colors. Experimentally it is very healthy. In fact it has won a series of totally unearned victories, pushing its domain of applicability far beyond anything that could be anticipated in the 1920s.

Developed to deal with atomic phenomena controlled by electromagnetic interactions, it has proven itself in the domains both of strong and of weak interactions, whose energy ranges are vastly different. It can explain the dynamics of stars and the structure of ordinary matter. It still has the power to predict amazing new phenomena, and easily incorporates unexpected new effects such as the quantum Hall effect. Furthermore there appears to be no real evidence pointing to its possible limitations.

The problem with quantum theory is that its foundations are both very complicated and very suspect. Saying that a state of a system is a vector in a Hilbert space has no simple relationship to a classical description of the same system. It is only when one compares predictions worked out by its own peculiar rules that one can pass, via the correspondence principle, to the classical limit to compare results. And while the classical results rest on strong intuitive principles such as causality and locality, one sees evidence in the standard interpretation of quantum theory that neither of these is to be taken very literally.

Many of the founding fathers of the subject, Einstein in particular, thought that causality was a necessary precondition for doing physics at all. This was the basis for his refusal to accept quantum theory, and allowed him via the EPR paradox[1] to pinpoint exactly where the breakdown in causality lies.

The present volume consists of papers from a 1983 workshop on the subject at the University of Bari. Most of the papers are by people who would like to replace quantum mechanics by a "more realistic" theory, meaning one where particles have trajectories and obey causal and, they hope, local laws.

The major candidate for such a theory is the nonlinear, stochastic theory advocated by David Bohm, Louis DeBroglie, Jean-Pierre Vigier and others, which is represented by several papers here. In theories of this class the particles move along classical trajectories and quantum effects are introduced by a "quantum potential." However, this potential itself possesses some very unpleasant properties—for example, its influence does not diminish with distance. (Einstein was also unhappy with this.) It has the advantage over conventional quantum theory that one can analyze much more cleanly what goes wrong with Newtonian physics, as the quantum-potential theory is much closer to it in formulation. Also, because the particles have trajectories, the theory would seem to be more amenable to direct connection to gravity than is conventional quantum theory, although I don't think anyone has tried this yet. Nevertheless all physicists should be aware that they are wrong if they tell their students that there is "no viable alternative formulation of quantum theory." (However, one potent criticism of all nonlinear, realistic theories is that all the weird quantum results they try to reinterpret follow directly from the superposition principle for amplitudes. All experiments have a simple, clean interpretation in terms of it, and to dispose of it or hide it is like throwing out the baby with the bath water. The proponents of alternative theories should be much more conscious of this fact.)

The book also contains many papers that discuss various aspects of the EPR experiment. Some have good, concise summaries of various points. A number try to reconcile Alain Aspect's experiments[2] with locality and causality, which they can do only by assuming peculiar properties for photon detectors. (The usual assumption, that the detector can absorb but not enhance the original signal, is called "no enhancement," and it should be experimentally testable.) A property of many of these explanations is that those photons that are not detected have different properties from, and are not representative of, those that are detected. To the charge that their assumptions appear strange to believers of the conventional theory, they can reply, logically enough: "What could be stranger than the breakdown of causality? Nothing we do is *that* strange." Representative names here are Franco Selleri, Emilio Santos and Trevor Marshall.

In the opening paper of the conference, the philosopher Karl Popper proposes an experiment he considers crucial, but which in fact proves nothing at all. He does not calculate anything but makes several assertions that a simple calculation shows to be untrue. He states without discussion that Werner Heisenberg and Niels Bohr would predict a result for his experiment that would contradict quantum theory, an assertion that seems ridiculous. Popper's paper is violently polemical, seeking to create the (false) impression that the standard interpretation is all but dead. His presentation is both misleading and disappointing.

Popper fares much better in another

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feisty chapter at the end of the book, summarizing his "evolutionary" approach to the development of science. According to Popper science develops by exploring in many directions at once, with those that work leading to further progress, and those that don't being discarded. This picture of the rise of science is very similar to the idea of evolutionary development by natural selection. I have long felt that all explanations of scientific development were very unsatisfactory and didn't conform to what scientists actually do. Popper's picture, however, is quite satisfying, and it seems to me much more natural than hypothetico-deductive schemes or research programs, or even Thomas Kuhn's theory of scientific revolutions, which also has some deep truths. But Popper's theory, so far as I know, has not had a comprehensive presentation (at least not in English), and this represents a good introduction to it. One objection is that Popper seems to believe in an ultimate "truth," even though he is aware that we cannot attain it. But evolutionary theories do not recognize truth, only successful adaptation. As environments change, so does "success."

Open Questions in Quantum Physics also has a section with good experimental summaries on neutron interferometry, by Helmut Rauch, and quantum optics, by Leonard Mandel. All in all, it is a very mixed bag, with some nice review articles, some interesting new work and some material that seems plain wrong. However, it shows the directions of some new thought on quantum theory, and this and the last chapter by Popper are reason enough for a library to have it. I would not recommend it to individuals unless they have deep experience, abiding interest and clear, independent judgment in the subject.

References

1. A. Einstein, B. Podolski, N. Rosen, Phys. Rev. **47**, 777 (1935). This paper, Bohr's answer and many other seminal papers are reproduced in *Quantum Theory of Measurement*, J. A. Wheeler, W. Zurek, eds., Princeton U.P., Princeton, N.J. (1983).
2. A. Aspect, P. Grangier, G. Roger, Phys. Rev. Lett. **49**, 91 (1982).

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The Physics of the Quark–Gluon Plasma

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Heat up ordinary matter to a temperature of a few thousand kelvins and you obtain a plasma in which charged ions and electrons move around and interact through the electromagnetic field. Heat it up further, to 10^{12} K, and you obtain a quark–gluon plasma, in which quarks and gluons are no longer confined to the insides of nucleons.

The laws of nature describing the behavior of ordinary plasmas have been thoroughly studied and are well documented in the literature. But for quark–gluon plasmas we have so far no experimental information. We believe that the theory describing quark–gluon plasmas is quantum chromodynamics. Due to the nonlinear self-interactions between the gluons, this is an exceedingly complex theory (much more complex than quantum electrodynamics, where there are no self-interactions among photons). The only known way of performing first-principles calculations in QCD is by using numerical Monte Carlo methods on lattices rather than on continuous space–time. These require much computer time and are limited to a very narrow set of problems. In practice, one has to develop models and approximations based on physical intuition.

Berndt Müller's book is a compact treatment of our present understanding of quark–gluon plasmas. This is an understanding not yet confirmed by experiment. With the notable exception of quark–gluon plasmas in the early universe, Müller covers most relevant topics. The presentation is very clear but also very condensed. The reader will profit considerably from consulting original articles, references to which are conscientiously given.

The only hope for experimentally creating quark–gluon plasmas is by colliding large nuclei at as high energies as possible. The first experiments aimed at the observation of quark–gluon plasmas at relativistic energies will be carried out starting at the end of 1986. Although it is quite possible that plasma will be clearly seen only in ultrarelativistic colliders, which do not yet exist, the fixed-target experiments currently planned have attracted the interest of hundreds of experimenters. For them a crucial question is what is a good signal of, and a good method for diagnosing, the presence of a quark–gluon plasma. Müller discusses these only very briefly—in part because there is no single definite signal, and the experimental proof of the existence of quark–gluon plasma will be based on a combination of many different pieces of evidence.

At present Müller's book is useful as a reference volume. If you want to remind yourself of an idea, find the book and have a look. The field is developing very rapidly, however, and the decay time of the usefulness of the book will only be a few years. In