SHOULD QUANTUM PHYSICS GO UNQUESTIONED?

When is a question foolish? Herman Feshbach and Victor F. Weisskopf (October, page 9) suggest that where quantum mechanics seems incomplete, those questions whose answers are left out of a quantum mechanical description are foolish. For example, given a particle with a well-defined location, to ask what its momentum is is to ask a foolish question.

In taking this attitude, they adopt a widely held interpretation of quantum mechanics, one that holds that "the wavefunction ψ of a given state incorporates all there is to know about the system in that state." But the discussion that follows this remark does more to muddy the waters than to clarify the peculiarities of quantum mechanics, and their conclusion, though precisely right about the fantastic empirical success of quantum mechanics, falls far short of showing that there is nothing odd about it.

It is indeed difficult to put clearly and simply what is odd about quantum mechanics, and many attempts to do so are simply mistaken. Still, it can be done. The oddities in fact have nothing to do with whether the theory works as an "instrument" in the laboratory. This is (at least for the present) not in doubt, and so for the most part working physicists may well be excused from serious concern about the theory: Getting on with their empirical and theoretical work does not require that they settle what interpretation the theory should receive. But the very empirical success of the theory invites the question, What is the relation between what the theory says about the entities it deals with and the real attributes of those entities? This is the question that has evoked so many strange answers, none of which are quite as satisfactory as the classical physicist's straightforward response to the same question: that what her theory says is a complete characterization of all the physical attributes of the entities.

Feshbach and Weisskopf consider three main examples: radioactive decay, the Schrödinger's cat paradox and the Einstein–Podolsky–Rosen thought experiment. Let's consider the last of these, which involves a correlation between outcomes of spatially separated spin measurements. As Bell's inequality shows, what's odd about this correlation is that it is too strong to be explained by appeal to a simple realistic model in which the correlated particles have definite spin values that give rise to the measurement outcomes. Rather, the first measurement's outcome, though separated from the second measurement by a space-like interval, causes (in some sense of "cause") a change in the probability distribution for the second measurement. This connection, happily, cannot be manipulated to send messages (for example, messages that would serve to synchronize clocks). But nevertheless it is there, both in quantum mechanics and in the experimental evidence testing Bell's inequality.

The point of the EPR thought experiment was to argue for the incompleteness (not the empirical inaccuracy) of quantum mechanics; the assumptions were that a correlation between the outcomes of measurements had to have some causal basis, and that the causal signals involved had to travel at or below the speed of light. As Bell's theorem shows, these two quite natural assumptions are at odds with quantum mechanics. Either the correlation is just there, a bare correlation with no causal basis, or the correlation is the result of superluminal "causal" signals. Neither view is a comfortable one, yet one or the other must be right.

From a broader point of view, we can think of physics, and of science in general, as a process of seeking out and systematically capturing the basic correlations (regularities) in the world. There is a long tradition that demands that correlations be explained (when they are not mere coincidences) by invoking some sort of causal connection between the correlated events. And we have been very successful in finding such connections. There is another long and

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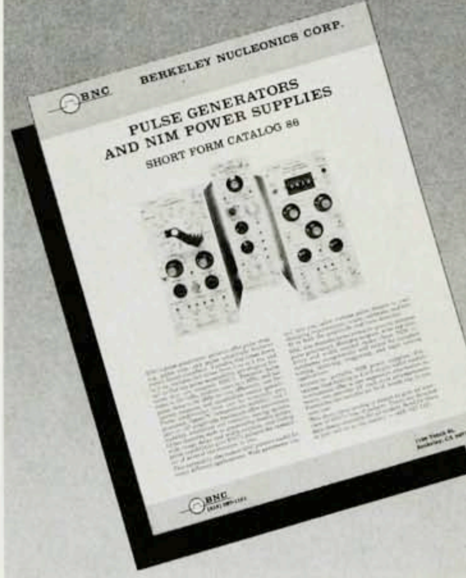

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successful tradition that has rejected influence at a distance, demanding that effects correlated with distant causes be explained as the result of intermediary, local causes (in modern physics, we say that the four forces are mediated by the exchange of particles). But in the experiments testing Bell's inequality, we have come to a parting of the ways. These two traditions are no longer compatible, and staying within one will require us to reject the other.

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I hope the Reference Frame column by Herman Feshbach and Victor Weisskopf helps put an end to some of the mystical exaggerations in the popular (and, according to the authors, sometimes not so popular) literature on the conceptual foundations of quantum theory. They remark incidentally that classical physics does not allow detailed predictability when one is dealing with chaotic systems, and that for such systems it is more sensible to work with distributions of positions and velocities than with individual trajectories. In this connection I wish to call attention to some little-known work of Max Born, since it bears not only on this point but also on the question of "reduction of the wavefunction" and whether, as often claimed, microscopic events "change abruptly and discontinuously."

Born considered simple examples of classical systems in which a sensitivity to initial conditions makes long-term predictability impossible because of the impossibility of specifying initial conditions with infinite precision. He called such systems "indeterminable." (His examples do not exhibit chaos in the sense of exponential sensitivity, on average, to initial conditions.) Born argued[1] that "it is misleading to compare quantum mechanics with deterministically formulated classical mechanics; instead, one should first reformulate the classical theory, even for a single particle, in an indeterministic, statistical manner. Then some of the distinctions between the two theories disappear, others emerge with great clarity. Amongst the first is the feature of quantum mechanics, that each measurement interrupts the automatic flow of events, and introduces new initial conditions (so-called 'reduction of probability'); this is true just as well for statistically formulated classical theory."

Born takes the point of view that the "reduction of the wavefunction" is not some discontinuous change undergone *by the system* but rather a change in what we *know* about the state of the system as a result of a measurement. The situation is then much like that in classical probability theory, and does not require that events "change abruptly and discontinuously."

Feshbach and Weisskopf remind us that the state vector evolves deterministically in time. It might be worth noting that the time evolution of state vectors in quantum mechanics appears to be not only deterministic but even more orderly than the time evolution described by Newton's second law, in the sense that there is no known quantum system whose wavefunction evolves chaotically in time. Here again Born's suggestion that quantum theory be compared with statistically formulated classical theory is useful. Consider two classical distribution functions, ρ_1 and ρ_2, in phase space. It may be shown under some mild restrictions that the scalar product $\int d^Nq\, d^Np\, \rho_1\rho_2$ is invariant in time, just as the scalar product $\langle\psi_1|\psi_2\rangle$ of two wavefunctions in the quantum description is invariant. This invariance means that the classical distribution function will not exhibit exponential sensitivity to initial conditions (chaos), and by analogy there may be no quantum chaos in this sense.

Reference

1. M. Born, Dan. Mat. Fys. Medd. **30**(7) (1955). See also M. Born, D. J. Hooten, Z. Phys. **142**, 201 (1955).

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Herman Feshbach and Victor F. Weisskopf's views concerning the foundations of quantum mechanics correspond exactly to the general attitude among physicists: There are no problems in quantum theory; it is only that some people make problems by asking the wrong questions. "Ask a foolish question and you will get a foolish answer," write Feshbach and Weisskopf. And which questions are foolish? Those that cannot be answered by using quantum mechanics in the normal way.

This is a description of the positivistic philosophy that prevails among physicists. We know how to solve practical problems concerning experiments, and Bohr has forbidden us to ask other questions. Euan Squires mentions in his book *The Mystery of the Quantum World* (Adam Hilger, Bristol, UK, 1986) a

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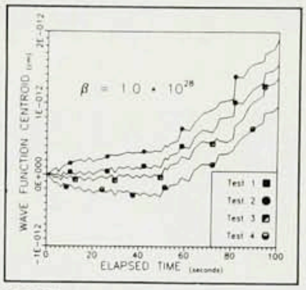
SCIENTIFIC GRAPHICS



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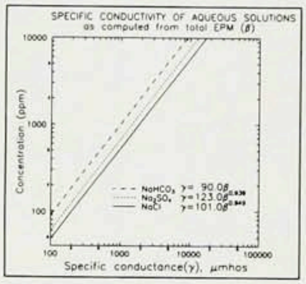

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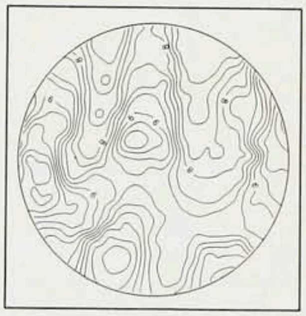

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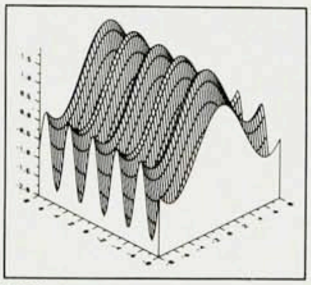

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Christmas carol from the 1950s describing this attitude:

At Bohr's feet I lay me down
For I have no theories of my own
His principles perplex my mind,
But he is oh so very kind.
Correspondence is my cry, I don't
know why, I don't know why.

Nowadays some nonphysicists and even some physicists have begun to ask "foolish questions." They state, for example, that quantum mechanics is generally considered to be a statistical theory concerning atomic events. Its verification, therefore, always presupposes a big sample of similar events, that is, a sample of identical systems prepared in the same way. Some people have begun to ask, What about individual events? Quantum mechanics seems to be unable to describe individual events in an unambiguous way. Should we not call quantum mechanics an *incomplete* description of reality? What are the philosophical consequences of this incompleteness?

According to Feshbach and Weisskopf, only questions concerning probability distributions are correct questions in quantum mechanical cases, not questions concerning individual events. Einstein, however, insisted that this failure in the quantum mechanical description of phenomena shows that quantum mechanics is not a complete description of reality. Pauli also emphasized this point, not as demonstrating the incompleteness of quantum mechanics but as characteristic of the new features of quantum reality as compared with the traditional conception of reality. Pauli said that in individual events we meet the *irrationality of reality*: Rational theories are not able to describe individual atomic events unambiguously.

It is characteristic of Western science in general that this feature of "irrationality" cannot be accepted. Pauli spoke of the "repression of the irrational" as a characteristic of Western thought. In quantum mechanics, hidden-variable theories are an expression of this repression: One tries to amplify quantum mechanics in such a way that it becomes possible, in principle, to describe individual events (for example, to speak of individual orbits). Physicists, however, have not found this way to be attractive, not even Einstein himself. Another attempt is the so-called ensemble interpretation, which associates the state function with a sample of similar systems, instead of an individual system as in the "orthodox interpretation." The ensemble interpretation, however, is unable to describe individual events at all. It "solves" the problem by refusing to speak of unaccountable individual events: an example of the "repression of the irrational" in its most explicit form.

It is usual to dispel the problems of individual events by stating that we always have a large number of atomic events in practical experiments. Granted; nevertheless each bubble chamber picture concerns an individual event. That a vast number of statistics is needed in each investigation is a proof of the statistical nature of laws at the level of elementary-particle reactions. From the point of view of philosophy it is not foolish at all to emphasize this feature of the atomic world as an expression of an essential change in the conception of reality.

The term "irrationality," which Pauli uses in this connection, is very good because it emphasizes the depth of this change. The new features of reality can also be described in other ways. One way is the concept of complementarity, which Bohr liked so much: The properties of the atomic objects partly depend on our method of investigation. The objects of the atomic world cannot be characterized with the aid of invariant properties; their properties are partly *created* in the measurement: Before the measurement a particle has no spin state in general.

These new features are very interesting from the point of view of philosophy and presuppose radical changes in our conception of reality and of the nature of human knowledge. Questions that physicists do not usually ask are important if we wish to understand the philosophical consequences of quantum theory.

That these questions have remained largely unanswered is an example of the dangerous splitting of our culture into isolated branches. Physicists are too apt to say that philosophers are asking foolish questions, while philosophers may have the opinion that physicists do not understand the nature of important questions at all, being interested only in technical details.

The reason why physicists wish to avoid certain questions is perhaps their fear that philosophy may easily lead from quantum theory to nonscientific speculation and mysticism. This is something that Pauli understood more clearly than most of his colleagues. He saw, however, that the "irrationality of reality" is a necessary consequence of quantum mechanics, and that this makes the borderline between science and questions of belief unclear.

Pauli was especially interested in the unconscious functioning of our psyche and in the concept of archetypes in C. G. Jung's depth psychology. In Pauli's unpublished correspondence he pointed out that physics and psychology must be considered as complementary sciences that only together can produce a reliable picture of reality. The "irrationality of reality" is, according to Pauli, created by the unconscious functioning of our psyche. The picture of reality that we can form is always just a picture in our consciousness. The unconscious processes associated with our perceptions and our thinking and theory formation form a "veil" that prevents us from seeing the "reality itself." For human knowledge, reality remains, in principle, "veiled," to use a very illustrative term of Bernard d'Espagnat's.

Thus, "foolish questions" can open up new vistas that may be extremely important for our way of thinking—and perhaps also for the future development of physics. Consequently I have found it necessary to criticize the writing of two very famous colleagues. I daresay that Pauli would have been seriously worried about the antiphilosophical attitude that is now common among physicists and that Feshbach and Weisskopf describe.

Perhaps two examples of this lack of interest in philosophy will illustrate what I mean:

▷ Attempts to avoid the "paradoxes" of the traditional interpretation of quantum mechanics are an example of the "repression of the irrational" that Pauli often emphasized. Only now, 60 years after the creation of the Copenhagen interpretation, are people beginning to realize that these "paradoxes" presuppose a new view concerning the role of the psyche in the world.

▷ The purely materialistic conception of reality finds an expression also in the cosmological theories based on the Big Bang hypothesis. A purely rationalistic description of the "first beginnings" is based on a one-sided conception of reality. The idea of a "wavefunction of the universe" in particular totally neglects the deep problems concerning human knowledge that were the origin and will remain the essence of quantum theory.

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The Reference Frame column "Ask a Foolish Question . . ." by Herman Feshbach and Victor F. Weisskopf is an interesting defense of quantum

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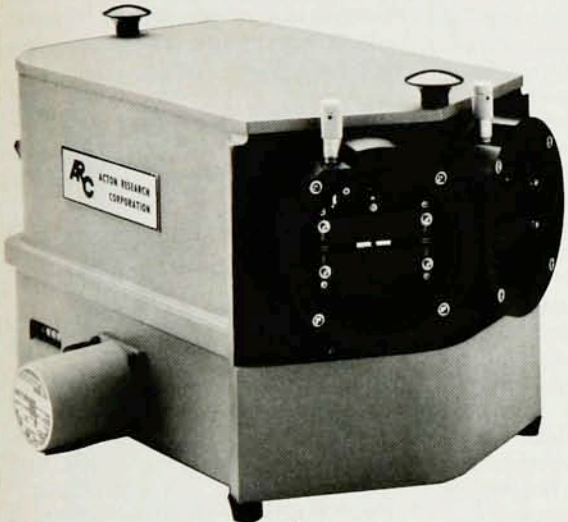

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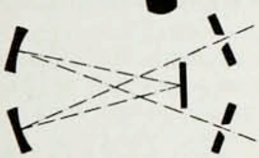

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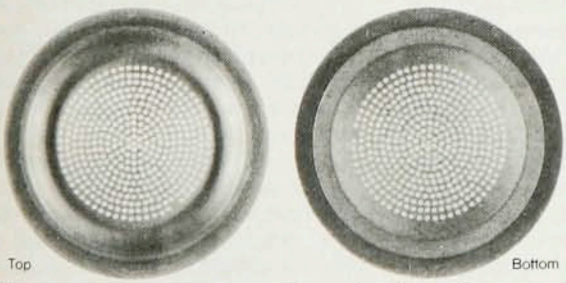

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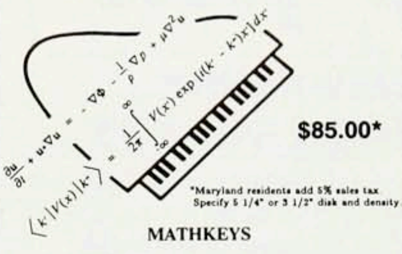

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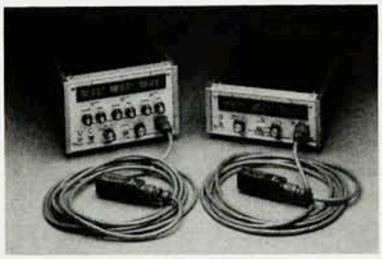

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theory against those who would describe it as indeterminate or acausal. However, certain conclusions should not pass without comment.

The authors argue against looking beyond quantum theory for answers by saying that there is no evidence that it suffers from any serious paradoxes that might necessitate a rejection of it in favor of any new theory. They state that the Schrödinger's cat paradox and the Einstein–Podolsky–Rosen paradox arise only because "foolish" questions have been asked. But the issues raised by these two paradoxes (and most questions for which quantum theory provides no answer) are far more profound than indicated in their column. Rather than being peripheral to quantum theory, these questions strike at its heart—the role of measurement and the nature of reality. (I do not wish to go into this complex question except to refer the reader to the excellent book by John S. Bell[1] that was reviewed on page 89 of the same issue of PHYSICS TODAY.)

Although we like to think that theories are rejected because of a contradiction with the evidence, such explanations are usually *post facto* constructions, designed to provide us with an impression of systematic scientific progress. (After all, to actually disprove a theory like quantum theory is almost impossible. It would amount to showing that there exist experimental observables that cannot be explained by any reasonable modifications to the Hamiltonian. Given the difficulty and indirect nature of obtaining most information on microscopic systems, such a task is almost insurmountable.) In actual practice, theories fall by the wayside because they do not provide satisfactory answers to a few specific questions that the scientific community has deemed to be interesting and important at that time. Which questions are "interesting" or "important" and what constitutes a "satisfactory" answer are matters of controversy during the period of transition in which rival theories compete for acceptance; and the resolution of this debate determines which theory becomes the standard one for all science practitioners in the future. The debate dies once the proponents of a particular position establish themselves as the dominant group. This process is not entirely objective.[2]

It is a good thing that, by and large, we believe in the rightness of the currently accepted theory. Science could not progress if everyone constantly kept reexamining its foundations. But to dismiss the questions quantum theory cannot answer as foolish is a mistake. It may well be that future historians will quote them as the interesting and important ones that led to the replacement of quantum theory by the new super quantum theory!

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2. T. S. Kuhn, *The Structure of Scientific Revolutions*, U. of Chicago P., Chicago (1970). P. K. Feyerabend, *Against Method*, Verso P. (1982).

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Are there foolish questions in quantum physics? Many answer yes, such as Herman Feshbach and Victor F. Weisskopf. We wish to present a different view.

All paradigms of physics start with the same perceptions and raise questions about concepts such as system, preparation, observable, measurement act, state, value of an observable and equation of motion. They differ only in the mathematical representations of the concepts, and the relations between such representations.

Each question has a crisp and precise answer. For example, a measurement act yields a precise numerical answer. In classical mechanics, the answer is determinate because it coincides with the result that would be obtained from an identical measurement act. In quantum physics, this answer is indeterminate because, in general, it differs from the result that would be obtained from an identical measurement act. It is predictable in terms of a probability. Again, as a function of time, classical mechanics and quantum mechanics satisfy the principle of causality.

Of course, in applications in which information may be incomplete or mathematically complicated, statistics is superimposed on each paradigm. Thus a statistical theory is formulated, such as statistical quantum mechanics or statistical quantum thermodynamics.

Feshbach and Weisskopf object to efforts to explicate and disseminate knowledge about the probabilistic character of quantum mechanics. They claim in particular that such writings exaggerate the role of chance in modern physics, and that the concept of probability, when it does occur in the solution to a quantal problem, merely reflects the inappropriateness, or foolishness, of the problem itself.

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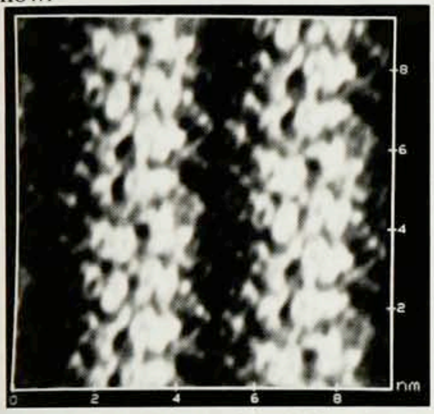

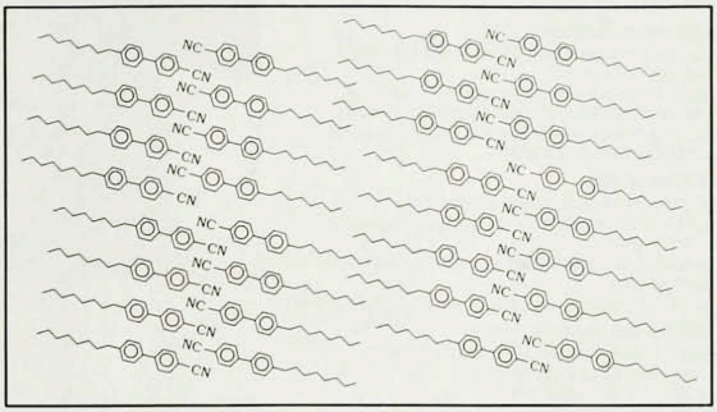

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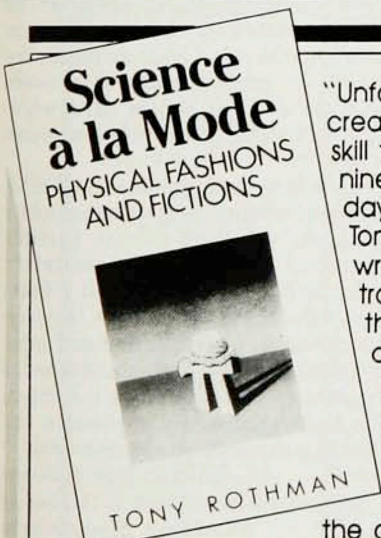

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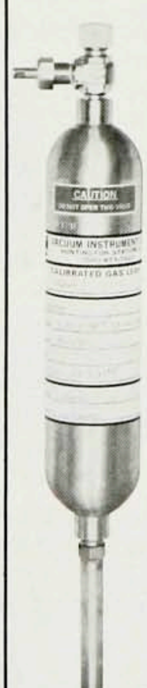

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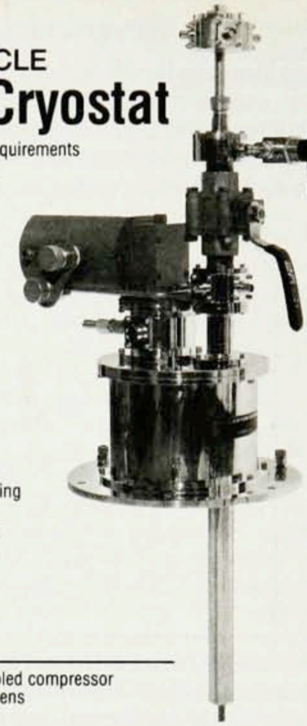

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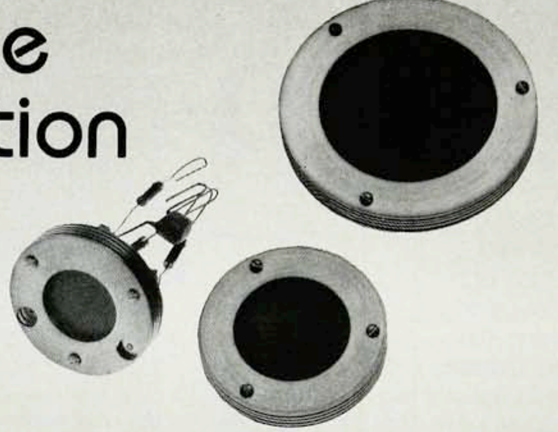

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Though no short Reference Frame column, and certainly not this letter, can possibly deal seriously with the numerous profound issues that were raised, we feel compelled at least to venture a few dissenting comments.

To say that quantum theory gives exact answers to appropriate questions and probabilistic answers to foolish ones is to say that all dynamical questions are foolish. However, quantum theory causally predicts nothing but future probability distributions and is otherwise quite indeterministic. This celebrated indeterminism is not ameliorated by noting that structural features like ground state energy (or any other eigenvalue) are not statistics.

To invoke the existence of deterministic chaos in classical mechanics is appropriate to underscore one need for statistical classical mechanics, but does nothing to erase the deep distinction between the reducible (information theoretic) statistical probabilities and the irreducible probabilities of quantum physics.

To characterize the allegory of Schrödinger's cat as foolishness is unwarranted because this allegory is a provocative literary device for explaining the quantum world view. Experimental, theoretical and philosophical investigations inspired by the arguments of Albert Einstein, Boris Podolsky and Nathan Rosen are surely no more foolish than were thoughts of light waves back when the establishment favored Newtonian corpuscles.

To declare that simultaneous measurement of any pair of canonically conjugate variables is not possible because of Heisenberg's uncertainty principle is a *non sequitur*. It reflects the not uncommon confusion between the concepts of preparation and measurement. It is the simultaneous preparation of determinate incompatibles that is quantally impossible, not their simultaneous measurement.

ELIAS P. GYFTOPOULOS
Massachusetts Institute of Technology
Cambridge, Massachusetts
JAMES L. PARK
Washington State University
11/88 *Pullman, Washington*

Herman Feshbach and Victor F. Weisskopf seem to imply that physicists who take seriously the Einstein–Podolsky–Rosen paradox are "asking a foolish question" because they do not quite understand the uncertainty principle of quantum mechanics. We think, rather, that this principle would be violated if the EPR state of two separate spin-½ fermions with a total spin of precisely 0 really existed.

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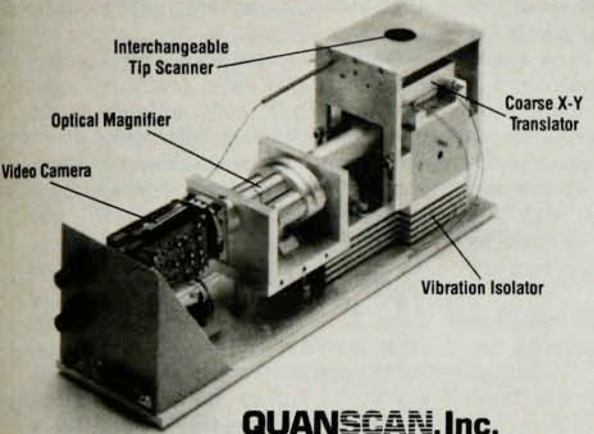

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Ignoring the difficulties that have preoccupied the minds of Einstein, Schrödinger and others since the problem was proposed 50 years ago, Feshbach and Weisskopf guide the reader's intuition along lines that suggest a classical picture. They write that the two fermions are emitted in the singlet spin-0 state and that "obviously the two spins are oriented opposite to each other." As they say, that statement implies that we could predict the spin state of a particle A (note, in any direction that we chose) just from measuring a particle B far away from A. In turn, that would mean that an apparatus could emit A and B in "eigenstates" along any direction that we might choose simply by turning the knobs of passive devices located at a distance. The statement is in contradiction with the uncertainty principle just as much as is the question "In what direction are the spins opposite?"

That contradiction with quantum mechanics cannot be accepted because, as Feshbach and Weisskopf say, quantum mechanics has been well established.

Using the method of Bell inequalities, David Mermin[1] exposed the EPR difficulty in detailed steps and amusing style. We prefer the method of discussing the properties of eigenstates,[2,3] but the contradiction between quantum mechanics and the singlet state (for separate particles) is clear with both methods.

In discussing the contradiction, we concluded[3] that the theory does not force us to assume the existence of the controversial state, because of the special allowance of quantum mechanics that in certain circumstances a "collapse" transition can occur. The situation of two fermions initially interacting in the singlet state and then becoming free suggests the collapse into a different state without violating the uncertainty principle. The notion of a "total spin" is meaningless here. The experiments show 100% correlation, as if the two free fermions were indeed in the singlet state, but that is not enough to prove its existence. More work is needed.

Feshbach and Weisskopf do not discuss the key point of separation (there is no difficulty, for example, about the singlet state of the interacting electrons of helium). However, Weisskopf has previously[4] emphasized the "nonseparability" of the two-particle state "because the quantum state extends from one proton to the other even when the protons have separated," quoting the canonical case of the interference of a single particle incident on two slits. That is correct for the double-slit experiment. But in the EPR experiments the wavepackets are much smaller than the distance that separates them. (Actually, the short range of their interaction would prove separability independently of the extent of the wave packets.)

Some physicists propose the existence of actions at a distance that would link the particles at any distance, but we happen to know that Feshbach and Weisskopf consider that idea unacceptable, and we fully agree.

Given the separation, the theory of relativity also rules out the singlet state. Such a state implies an exchange of information without delay, with amusing consequences. Suppose that at time zero particles A and B are emitted by a source in the singlet state. Their spins have no privileged direction, because a spin-0 system has none. Thus if A is measured first, at time T, it will have spin component plus or minus randomly, determined by the roulette action of the measuring device rather than at the source, so that the information would only be created at T. If no message can go from A to B, it should be legal for us, without looking at the result of the measurement on A, to bet on that result with other players after we (but not they) look at the result of the later measurement on B. We would always win, but anyone would say that either there was an instantaneous message from A to B or the measurement on A was not random, in which case there could be no singlet state. The second choice is clearly preferable.

The EPR problem concerns such fundamental elements of physics that its solution is bound to be very important, despite its appearance of being a "foolish" resistance to the 60-year-old "new ideas" of quantum mechanics. When it was proposed that short-lived neutral kaons could be "regenerated" by the passage of a long-lived neutral-kaon beam through matter without scattering, every theorist consulted except Abraham Pais, but including Weisskopf, dismissed the idea as "foolish." Yet the experiment of Wilson Powell and colleagues at Berkeley proved them wrong, and this interesting line of research led James Cronin, Val Fitch and their colleagues to discover *CP* nonconservation.

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ORESTE PICCIONI
WERNER MEHLHOP
BRIAN WRIGHT
University of California, San Diego
12/88

Herman Feshbach and Victor F. Weisskopf seem to follow the textbook writer's maxim: "If my book cannot answer it, the question is foolish." But those of us who entered physics to satisfy our curiosity about nature continue to ask until we have answers.

I do not challenge the claims regarding the successes and range of validity of quantum theory. But I do challenge the implied notion that quantum mechanics explains all and it is a waste of time to search for something more complete and descriptive of nature. I cannot stop asking why charge exists but atoms are neutral, why electrons are so light compared with nucleons, why energy is proportional to frequency, why the velocity of light cannot be exceeded, why the Lorentz transformation holds or why time only advances. All of these questions relate to observed reality, and I was under the impression when I made my career decisions that physics existed to explain reality.

Defenders of an intellectually bankrupt theory inevitably fall back on the word "language." Mathematics is taught and used because it is more precise than any language, but transcends all languages. The question of whether a theoretical particle can or cannot have precise values of position and momentum depends on the mathematical meaning given to the particle. This very point is in fact one of the failures of quantum mechanics. According to the universally used Copenhagen interpretation, one must think of two entities, the wave and the corpuscle. After pretending through all the mathematical equations that the wave is the particle, we jump ship to the corpuscle before comparison with experiment. The probability relation between the two cannot be avoided, ever, if we are to be consistent. Perhaps if quantum theorists would decide just what a particle is, the popular press would stop playing games with the concepts.

There are many who regard quantum mechanics only as a steppingstone to the real truth of nature, and nature is surely never inconsistent. Future historians will no doubt marvel at our mastery of technology and empirical science, but regard us as hopelessly infantile in the realm of understanding. For the future of humanity on Earth, let us hope that not everyone stops asking foolish questions.

JAMES I. BERG
Granville, Ohio
10/88

FESHBACH AND WEISSKOPF REPLY: Most of the letters are based on a misunderstanding of what we meant by "an inappropriate question." It seems that the term "inappropriate" was inappropriate. What we meant was expressed in our column as follows: "Observations are formulated in the language of classical physics. . . . But classical physics concepts are not always appropriate for the description of atomic situations."

Any question that an experimental setup is supposed to answer is necessarily formulated in terms of a combination of classical concepts. If this combination is not appropriate to the quantum mechanical situation, quantum mechanics can only give probabilistic answers. This is due not to an intrinsic indeterminacy of the quantum world but to the limited applicability of classical concepts to microscopic objects. We definitely did not mean that such "inappropriate" questions should not be asked. They are justifiably asked in many important applications of quantum mechanics.

Certainly quantum mechanics is odd in many respects. The fact that classical concepts are not appropriate to atomic reality is odd in itself. Although the EPR experiment is based on rather elementary quantum relations, it surely contains odd features such as the nature of the correlation between measurements at two distinct locations, as David Mermin pointed out in his article in PHYSICS TODAY (April 1985, page 38). There is much more to say about quantum mechanics than we were able to cover in a short comment.

HERMAN FESHBACH
VICTOR F. WEISSKOPF
Massachusetts Institute of Technology
Cambridge, Massachusetts
1/89

SSC: Don't Name This One for the Gipper

I would like to express my worries regarding the politicization of the so-called Ronald Reagan Center for High Energy Physics.

What has happened to the space program over the last decade should be taken as a serious lesson by the high-energy physics community. After space scientists worked within political channels and with the military establishment, increased resources for basic space research seemed assured. Instead we have seen NASA slowly change from a high-quality, largely scientific agency that sent men to the Moon and uncovered the secrets of the solar system into an unreliable arm of the military. Low-cost, high-yield scientific missions meant to follow the incredibly successful planetary flybys of the 1970s have been canceled, mothballed or postponed in favor of incredibly expensive and questionable missions chosen by the politicians and the military—"Star Wars" tests, the space station and so on.

There are already three signals that suggest that the high-energy physics community will be taken down the same political primrose path. The first is the well-known interest by the Pentagon in particle beams for the SDI program. The second was the choice of the home state of the President-elect, Speaker of the House and chairman of the Senate Finance Committee over Illinois as the site of the SSC. The third is the naming of the SSC the Ronald Reagan Center for High Energy Physics.

This is not a question of Democrat versus Republican or increased versus decreased military spending. This is a question of how far scientists will allow the politicization and militarization of pure science. Let us not forget that Fermilab was named after a great physicist, engineered by great physicists and has produced great basic physics. Enough SSC deals have been cut. Let us have the moral courage to tell the politicians that the SSC will be called the Richard Feynman Center for High Energy Physics and run by scientists for science's sake, or it will not be built at all.

MARK GROSS
California State University
Long Beach, California
1/89

Who Perceived the Perovskite?

In the *Physics News in 1987* article by Raymond Jeanloz (PHYSICS TODAY, January 1988, page S-45), it is claimed that "it was also in 1987 that perovskite-structured silicates were *found* to make up the bulk of our planet" (my italics), with reference given to work by Jeanloz and Elise Knittle.[1] In my view, this statement grossly distorts the facts. If the synthesis (or "discovery," as Jeanloz terms it) of high-T_c superconducting oxides began the "year of perovskite" in the physics community, the "year of silicate perovskites" began in 1974 and 1975 when I synthesized (Mg,Fe)SiO_3, (Mg,Fe,Al)(Al,Si)O_3 and $CaSiO_3$ per-

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