Thus the resolution of Maxwell's measurement problem is simply to abolish from quantum mechanics all projection postulates—including Maxwell's version (a)—which attempt to formalize the measurement act by describing it in terms of untenable assignments of quantum states to individual systems. This suggestion to renounce the notion of state reduction, first made long ago by Margenau,[5] has the merit of conforming fully to the actual practice of quantum physics, wherein the projection postulate is seldom used and never required. Moreover, abandonment of the reduction idea for its inutility offers in addition the parsimonious bonus of invalidating much profuse philosophizing predicated upon imagined but nonexistent quantal inconsistencies.

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[1] N. Maxwell, Am. J. Phys. **40**, 1431 (1972).

[2] Discussions of this point appear in many places; the original analysis is probably that given by J. von Neumann in *Mathematische Grundlagen der Quantenmechanik* (Springer, Berlin, 1932), Eng. trans. by R. T. Beyer (Princeton U. P., Princeton, NJ, 1955), p. 437.

[3] J. Park, Am. J. Phys. **36**, 211 (1968).

[4] W. Band and J. Park, Found. Phys. **1**, 133 (1970).

[5] H. Margenau, Phys. Rev. **49**, 240 (1936); Phil. Sci. **4**, 352 (1937).

The Problem of Measurement—Real or Imaginary?

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As Band and Park correctly point out, the main thesis of my paper is that if the problem of measurement is to be resolved, a new, fully objective version of quantum mechanics (QM) needs to be developed which does not incorporate the notion of measurement in its basic postulates. Band and Park claim that my argument in support of this thesis is based on certain premises that "are without physical basis." I am, however, quite unable to accept their criticisms.

In the first place they argue that it is an "unwarranted extrapolation" to claim that "if we designate the systems M as measuring instruments, QM predicts that after each S interacts with each M, the measuring instruments have some definite state m_i." But this is simply the condition for the measuring instruments to measure the observable A. Only if each M ends up in one or other of n distinguishable physical states (e.g., a pointer in one of n possible positions) will the M's function as measuring instruments at all. Thus, the assumption that each M measures the observable A, together with QM, clearly does entitle us to conclude that each M ends up in one of n distinguishable physical states.

Secondly, Band and Park argue that after S and M have interacted, M is in a *mixture* and not, as I maintain, in a *pure state*. Now it is of course true that if we consider only measurements made on M, then M may be held to be in a mixture (and likewise for S). If, however, we consider measurements made on the joint systems $S+M$ (the case that I consider), then we cannot in general regard M and S as being in mixtures, for then we will not be able to predict correlations that exist between observables belonging to S and M. According to orthodox QM, if S and M are initially in pure states, then the joint system $S+M$ persists in a pure state, and S and M cannot, properly speaking, be said to have independent quantum states at all. D'Espagnat, who has discussed this kind of case, suggests that we should call the states of M and S after these systems have interacted *improper* mixtures, and carefully distinguish this from *proper* mixtures.[1]

None of this affects the *inconsistency* problem faced by orthodox QM in the slightest. This can be seen quite simply as follows. As we have already seen, the assumption that each M functions as a measuring instrument implies that after S and M have interacted each M is in one or other of n distinct physical states. But this conflicts with orthodox QM. A basic tenet of orthodox QM is that if an ensemble of systems is in a pure state, then the associated state vector gives the most complete description possible of each individual system.[2] Now the state vector to be associated with $S+M$ will at best predict that if a *further measurement* is performed on $S+M$, then each M will be found to be in one or other of the n distinct physical states. The thesis that this constitutes the most complete description possible of $S+M$ rules out the possibility that each individual M actually is in one or other of the n possible "measuring" states before the further measurement is made. In other words, a quantum mechanical treatment of the interaction of S and M rules out the possibility that each M can function as a measuring instrument unless a *further* measurement is made.

I conclude that the inconsistency problem that I draw attention to in my paper really does confront orthodox QM and is not just a figment of my imagination. The problem that I consider—namely, how the systems $S+M$ can evolve from a pure state to the appropriate kind of proper mixture and at the same time not violate the time-dependent Schrödinger equation—has been articulated and discussed all too often in the literature.[3]

Band and Park conclude by remarking that "the resolution of Maxwell's measurement problem is simply to abolish from quantum mechanics all projection postulates—including Maxwell's version (a)—which attempt to formalize the measurement act by describing it in terms of untenable assignments of quantum states to individual systems." I have five comments to make here.

(1) From Band's and Park's remark, it looks as if we have but one proposal here. But in fact two quite distinct proposals are involved. First, there is the proposal, made by Margenau,[4] that we should reject a specific postulate of QM, namely the so-called "projection postulate." Second, there is the quite different proposal, made by Einstein,[5] that we should reject the orthodox interpretation of the quantum mechanical notion of state. According to this second Einsteinian proposal, the Copenhagen thesis, upheld in particular by Bohr,[2] that the state vector gives a complete description of the individual system, is to be rejected. Instead it is held that the state vector applies to an *ensemble* of systems and does not completely describe the individual system.

It is perhaps worth noting that both Margenau and Einstein were well aware that they were making quite different proposals. Thus Margenau explicitly criticised Einstein for making his very radical proposal.[6] And Einstein in turn remarked, "I do not think that Margenau's defense of the 'orthodox' ['orthodox' refers to the thesis that the ψ-function characterizes the individual system *exhaustively*] quantum position hits the essential (aspects)."[7]

(2) Band and Park claim that the projection postulate could only be at best a rather useless appendage to QM which is "never required." This is, I think, a little unfair. A genuine motivation for introducing the projection postulate does exist, namely to develop a version of QM which can satisfactorily explain the measuring interaction without calling on some part of classical physics. It is surely highly undesirable that quantum mechanics should rely in an essential way on classical physics for a treatment of the measuring interaction. Thus, upholding the projection postulate was not entirely without point, contrary to what Band and Park suggest.

(3) However, Band and Park are surely quite right in maintaining with Margenau that the projection postulate needs to be rejected. But they are wrong in holding that this suffices to remove the measurement-inconsistency problem. For the basic thesis that generates the inconsistency problem is not the projection postulate at all; rather, it is the orthodox thesis that the state vector gives a complete description of the individual system. As long as we retain this orthodox thesis, QM forces us to say that if an ensemble of systems S and measuring instruments M are initially in pure states, then each $S+M$ will persist in a pure state, and the individual measuring instruments cannot be in different physical states (i.e., pointers in different positions). And this means that QM "predicts" that no measurements are possible. Once we grant that measurements *can* be made and that the individual systems $S+M$ have different physical states, then we are *obliged* to say that the ensemble of systems $S+M$ is not in a pure state. And at once we get the contradiction. This argument—essentially the argument of my paper—nowhere presupposes anything like the projection postulate. It rests exclusively on the fundamental Copenhagen thesis that

QM is *complete*. Einstein was right when he said that Margenau's position did not hit the essential aspect of the matter!

(4) The inconsistency problem can, however, be avoided if we abandon the orthodox interpretation of QM altogether and adopt Einstein's statistical interpretation of QM.[8] For in this case the fact that the ensemble of systems $S+M$ is in a pure state does not at all preclude the individual M's from being in different physical measuring states (e.g., pointers in different positions).

(5) This kind of statistical interpretation of QM is, however, unsatisfactory in other respects. Its chief failing is that QM, given this interpretation, must presuppose some part of classical physics if any experimental predictions are to be forthcoming. For once we grant that the quantum mechanical notion of state applies only to an ensemble of systems and not to the individual system, it is clear that QM cannot of itself make any predictions about an individual system at all and, hence, strictly cannot make any experimental predictions at all. Once we accept the statistical viewpoint, it becomes entirely *consistent* to say that each individual M has measured A of S (e.g., that each individual pointer is in one of n possible positions) even though the ensemble of systems $S+M$ is in a pure state. But QM, given the statistical interpretation, cannot of itself *predict*, in the individual case, that M has measured A of S. Such a prediction only becomes possible with the addition of *classical* physics, which ensures that each M has the appropriate definite physical state (e.g., pointer in one of n possible positions). Thus QM, given Einstein's statistical interpretation, needs to presuppose classical physics for a treatment of the individual measuring system and, thus, for any specific experimental predictions to be forthcoming. This surely is an unsatisfactory state of affairs.[9]

Of course we might attempt to eliminate classical physics by adding a postulate to QM which asserts, roughly, that individual macro systems at all times actually possess appropriate macro properties and are consequently in definite physical states. But in order to make such a postulate precise, we would need to make precise the distinction between micro and macro which would, in effect, enable us to formulate QM in an entirely objective fashion as a theory about ensembles of micro systems with each individual micro system interacting with an individual macro system, the notion of "measurement" having disappeared from the basic postulates of the theory. Thus even Einstein's version of QM can only resolve the fundamental measurement problem—the problem, that is, of eliminating the need to call in classical physics for a treatment of the measuring process—if this version of QM is reformulated as a fully objective theory, the notion of "measurement" or of "observable" having been eliminated from the postulates.

My conclusion then is this: Band's and Park's specific criticisms of my argument are groundless. And their claim that Margenau's 1936 position resolves the inconsistency inherent in orthodox QM is incorrect. It is, however, true that if we adopt Einstein's position, a very radical departure from orthodox QM, then the inconsistency problem can be resolved. But even Einstein's position does not resolve the underlying measurement problem—the problem, that is, of how one can have a purely quantum mechanical treatment of the measuring process without bringing in classical physics at all. In order to resolve this problem we need to eliminate the notion of "measurement" from the postulates of QM altogether. For as long as QM is a theory which makes purely *conditional* predictions about what will happen *if* we make measurements, QM will not itself be able to make (unconditional) predictions without calling on classical physics for a treatment of the measuring process. Thus, even taking into account Einstein's viewpoint, the main thesis of my paper still stands.

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[1] B. d'Espagnat, *Conceptual Foundations of Quantum Mechanics* (Benjamin, Menlo Park, NY, 1971), pp. 80–87.

[2] It was just this *completeness* thesis which Bohr maintained in opposition to Einstein in connection with the Einstein, Rosen, Podolsky paradox; see N. Bohr, Phys. Rev. **48**, 696 (1935).

[3] See, for example, Ref. 1.

[4] H. Margenau, Phys. Rev. **49**, 240 (1936).

[5] Einstein gave a first rough exposition of his proposal at the 5th Solvay conference; see *Electrons et Photons*, Institute International de Physique Solvay, Rapports et Discussions du Cinquième Conseil de Physique (Gauthier-Villars, Paris, 1928), pp. 253–256. Subsequently he gave a clear account of his viewpoint in "Physics and Reality," J. Franklin Institute **221**, 349 (1936). For an account of the evolution and reception of Einstein's viewpoint see L. E. Ballentine, Am. J. Phys. **40**, 1763 (1972). It is interesting

to note that Popper gave a clear exposition and defence of both Einstein's and Margenau's position in his *Logik der Forschung* (1935) [Eng. trans. *The Logic of Scientific Discovery* (Hutchins, London, 1959), pp. 215–235].

[6] H. Margenau, "Einstein's Conception of Reality" *Albert Einstein: Philosopher–Scientist,* edited by P. A. Schilpp (Harper and Row, New York, 1959), p. 265.

[7] A. Einstein, "Remarks to the Essays Appearing in this Collective Volume," in *Albert Einstein: Philosopher–Scientist,* edited by P. A. Schilpp (Harper and Row, New York, 1959), p. 681.

[8] See L. E. Ballentine, Rev. Mod. Phys. **42,** 358 (1970).

[9] For a fuller discussion of this point see N. Maxwell, "Does the Minimal Statistical Interpretation of Quantum Mechanics Resolve the Measurement Problem?" (forthcoming).

On Wave Propagation in Snow

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In a recent Note in this Journal, Truman[1] has described the observation of wave propagation in the surface of snowfields. This phenomenon is commonly observed here by snowshoers travelling across meadows and lakes and occasionally in heavily wooded areas. The propagating wave is indeed visible if the light is good but is usually first brought to the attention because of the sound associated with it.

Interestingly, the earliest report of which we are aware[2] relates to snow conditions quite different from those noted in Ref. 1. The temperatures encountered by Cherry–Garrard and his party varied from the −40's to the −60's (Fahrenheit). The largest wave described in Ref. 2 had a vertical amplitude of a foot and propagated for an estimated three minutes.

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[1] J. C. Truman, Am. J. Phys. **41,** 282 (1973).

[2] A. Cherry-Garrard, *The Worst Journey in the World, Antarctic 1910–1913* (Chatto and Windus, London, 1965), pp. 291–292, 478.

Classical and Modern Physics by Kenneth Ford

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Charlotte Ward's excellent review[1] of Ford's *Classical and Modern Physics, Vol. I* in the March 1973 issue of this Journal suggests that textbook committees will have to wait for Vols. II and III to appear. Readers of the book reviews should be advised that Vol. II covering thermodynamics and electromagnetism, is available now at $8.00, and both Vols. I and II are available in a combined edition at $14.50. The third volume (on quantum mechanics and relativity) will be published in January 1974.

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[1] C. Ward, Am. J. Phys. **41,** 450 (1973).