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Toward a Microrealistic Version of Quantum Mechanics. Part II[1]

Nicholas Maxwell[2]

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In this paper, possible objections to the propensity microrealistic version of quantum mechanics proposed in Part I are answered. This version of quantum mechanics is compared with the statistical, particle, microrealistic viewpoint, and a crucial experiment is proposed designed to distinguish between these two microrealistic versions of quantum mechanics.

8. PROBLEMS OF PROPENSITY MICROREALISM

If the propensity microrealistic version of quantum mechanics (QM) outlined in Part I is to be a serious candidate for consideration, three important problems need to be solved.

(1) What does it mean to attribute quantum observables momentum$_o$, energy$_o$, and angular momentum$_o$ to 'appropriate' localizations?

(2) Can the thesis that stochastic transitions are to be associated only with *localizations*—and hence in a sense only with position and time measurements—be reconciled with the facts of quantum mechanical measurements as they emerge in the laboratory?

(3) How does the propensity viewpoint provide a realistic model for a system consisting of *two* (or more) particles, in which case the associated state vector ϕ is a function of six-dimensional configuration space and not of three-dimensional physical space?

Let us consider these three problems in turn.

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[2] Department of History and Philosophy of Science, University College, London, England.

(1) Let us begin with a consideration of momentum$_o$. Let us suppose a micro system with mass m and in a force-free region of space is, at time t_1, entirely localized within a reasonably small region δV, so that $|\phi_{t_1}|^2$ is zero everywhere outside δV. In this case if the system is subsequently localized at time t_2 so that the distance s between the region δV and the second localization is well defined, then we may attribute a value of momentum$_o$ to what we may regard as the two localizations, momentum$_o$ having the value $ms/(t_2 - t_1)$. Of course in attributing a value of momentum$_o$ to any two localizations, there will always be some degree of uncertainty in such an attribution, due to the 'spread' of the localizations involved. However, given any system confined to some region δV at time t_1, we can always, at least *in principle*, make the uncertainty in momentum$_o$ as small as we please by making the interval $t_2 - t_1$ sufficiently long.[1] In this way, given a system in any state, the notion of a precise, ideal determination of momentum$_o$ is that which would arise if $ms/(t_2 - t_1)$ were to reach the limiting value of precision. The fact that this value strictly cannot arise in nature does not really matter. The same kind of situation arises in connection with Newton's definition of 'natural' motion, embodied in his first law. What matters is that we can in principle approach the limiting case as closely as we please.

Suppose now we have an ensemble E of systems, each of which is in the pure state ϕ. And suppose *QM* itself predicts that some sequence of experimental preparations and localizations performed on the systems will yield results precisely like those that would have been yielded by means of the ideal momentum determination specified above. In this case we can hold that the experimental procedures constitute a measurement of momentum$_o$.

Other observables that can be defined in terms of position, time, and mass, such as energy$_o$, can be explicated in an analogous manner. Angular momentum$_o$ can also be brought into this general schema. Relative to an orientation **r**, a spin-$n/2$ particle has $2n + 1$ possible 'orientations.' Let us suppose that the state of a spin-$n/2$ particle is such that QM predicts that if the particle is localized in one or another of $2n + 1$ distinct spatial regions, then to each possible position a particular value of angular momentum, relative to **r**, can be assigned. In this case we can regard each possible localization as having a particular value of angular momentum$_o$ relative to **r**. To specify the value of angular momentum$_v$ of a spin $n/2$ system at any instant, we specify, relative to some **r**, the probability p_{2k+1} that the system would be localized in the $(2k + 1)$th position ($k = 1,..., n$) if the appropriate localizations were to occur. Angular momentum$_o$ is thus a feature of a particular kind of localization, relative to an orientation **r**. Angular momentum$_v$, on the other hand, is a feature of a microsystem as such—since once we know what the $2n + 1$ numbers $(p_1, ..., p_{2n+1})$ are for some microsystem relative to one specific orientation $\mathbf{r}_1$, we can readily calculate what they are for any other orientation $\mathbf{r}_2$.

This completes my answer to question 1. We can in effect see the propensity viewpoint sketched here as reinterpreting the generalized version of Born's 1926 postulate which gave a probabilistic interpretation to Schrödinger's wave mechanics. According to the generalized Born postulate, QM makes probabilistic predictions about the outcome of performing various kinds of *measurements* on microsystems. According to the propensity viewpoint advocated here, QM makes predictions about the outcome of localizations. The generalized version of Born's postulate can be reformulated like this:

I. Given an ensemble of systems each in the state ψ, and each undergoing localizations to which a value of the observable A can be attributed, then the probability that the value of A is a_k is given by $|\langle\phi_k, \psi\rangle|^2$, where $\{\phi_i\}$ is a complete, orthonormal set of eigenvectors of the Hermitian operator $\hat{A}$ corresponding to the observable A.

Let us now turn to the question of whether QM interpreted as making predictions exclusively about localizations can be reconciled with ordinary laboratory facts concerning measurement.

The microrealistic propensity version of QM advocated here is committed to the following thesis:

II. Given any (composite) system S, there exists a physical condition C, specifiable purely in terms of the quantum mechanical description of the physical state of S (i.e., in terms of values of quantum variables actually possessed by S), which is such that a localization occurs in S if and only if C obtains.

In the absence of a precise specification of C, we clearly need some indication as to when localizations occur, if the propensity version of QM advocated here is to make contact at all with experimental results. We can, I suggest, at least say this:

III. For a localization to occur in a system S it is necessary that S should have considerable complexity—i.e., be made up of a number of interacting fundamental particles. A *sufficient* condition for a microsystem to be localized is for the system to be *detected* by some macro measuring instrument, such as a geiger counter, scintillator, bubble chamber, or photographic emulsion.

The hope is, of course, that a precise articulation of postulate II will render postulate III unnecessary. However, in order to assess whether this hope is worthy of serious attention, we need first to consider whether the propensity version of QM plus postulate III succeeds in capturing, in an unproblematic fashion, the full predictive power of orthodox QM. If not, then of course specifying precise conditions for localizations to occur will hardly be of much help.

In order to see how the propensity version of QM copes with ordinary experimental situations, it is essential to keep in mind the distinction, implicit in much of the discussion so far, between *preparation*, on the one hand, and *measurement* on the other hand.[2] A preparation is some procedure which ensures that if a system is found in such and such a spatial region, then it will have such and such a quantum mechanical state ϕ. Landé[3] has called such a device a *filter*. Examples of filters are: a screen with a slit in it; a Polaroid film which only allows light of a specific polarization to pass through; a momentum selector which only allows systems of a specific momentum to pass through.

Now given a filter which prepares systems in some state ϕ, and given a system directed toward the filter in a different state ψ, we can in general calculate the probability that the system will pass through the filter, its state changing discontinuously from ψ to ϕ. But does not this discontinuous change of state involve a reduction of the wave packet? And if a specific filter prepares systems in a definite eigenstate of *momentum*, let us say, no localization being associated with this process, will we not need to postulate a *momentum*-type reduction of the wave packet, thus making nonsense of the idea that wave packet reductions are to be associated only with *localizations*?

Let us look in turn at how the two versions of QM that I have considered here—the frequency view and the propensity view—tackle this problem.

According to the frequency view there is *no* reduction of the wave packet, if this is thought of as some special kind of physical process. According to the frequency view, the situation is like this. We have initially an ensemble E of particles, to which we assign the state ψ. At a later time t (when each *individual* particle has, we may suppose, either passed through or failed to pass through the filter), the state vector to be associated with E is, let us suppose, ψ_t. We can, however, regard E as being made up of two subensembles, namely the ensemble E_1 of systems that passed through the filter and the ensemble E_2 of systems that failed to pass through the filter. To E_1 we associate the state vector ϕ_1 and to E_2 we can associate some state vector ϕ_2. The discontinuous transition of state from ψ to ϕ_1 for the particles that pass through the filter is then, according to this position, merely the outcome of changing ensemble E to E_1. The particles which have passed through the filter are in other words in state ϕ_1 if we take ensemble E_1, and in state ψ_t if we take ensemble E. There is here no *physical* reduction of the wave packet at all.

One snag does, however, arise in connection with this viewpoint. We suppose that the initial state ψ is a *pure* state, and so, too, the final state ψ_t. In this case we are *not* entitled to regard the final state of E as consisting of a mixture, there being two subensembles E_1 and E_2 with distinct states ϕ_1 and

ϕ_2. In order to claim that the ensemble E_1 has state vector ϕ_1 associated with it we need to say, it seems, that the state of E is not the pure state ψ_t, but rather the appropriate mixture $w_1\phi_1 + w_2\phi_2$. How then are we to explain the transition of the state of the ensemble E from pure state to mixed state, a transition which must it seems violate the time-dependent Schrödinger equation? Do we not have here a special kind of *physical* process which corresponds in a way to the old reduction of the wave packet?

The frequency viewpoint can, however, overcome this difficulty, since this viewpoint is not in fact committed to the thesis that the state of the ensemble E somehow mysteriously evolves from a pure state to a mixture. Rather one can say this. The ensemble E is at time t in the pure state ψ_t, and not in the mixed state $w_1\phi_1 + w_2\phi_2$. However, the experimental setup is so designed that no measurement can be performed on the particles which would reveal that the state of E is ψ_t rather than the mixture. In other words, the experimental setup is so designed that all measurements performed on systems belonging to E_1 will be in accordance with the strictly false assumption that the state of E_1 is ϕ_1. According to the frequency viewpoint, then, there are, strictly speaking, no ensembles with pure states. Rather, in preparing a pure state we so arrange our experimental setups that the (false) assumption that the given ensemble is in a pure state cannot give any false predictions. A preparation device is precisely a device which physically excludes the possibility of measurements which would reveal that the relevant ensemble is *not* in the desired pure state.

The frequency view is thus able to avoid postulating a physical discontinuous change of state to be associated with the action of a filter.

Let us now turn to the propensity viewpoint. And let us consider how the propensity view can rescue the idea that wave packet reductions are to be associated only with *localizations*.

Let us consider as before a filter which lets pass only those systems that are in an eigenstate corresponding to some observable other than position—such as momentum or angular momentum. An individual system is, we may suppose, initially in a pure state ψ. There is, let us suppose, a probability of 1/2 that the system will pass through the filter, acquiring the state ϕ_1, and a probability of 1/2 that it will fail to pass through the filter, thus acquiring the state ϕ_2. Does not the transition $\psi \to \phi_1$ (or $\psi \to \phi_2$) here inevitably involve a *physical* change, since according to the propensity view the state vector specifies the physical state of the individual system? No *localization* will, however, be involved here. We are thus, it seems, committed to the view that wave packet reductions are *not* restricted to localizations, which in effect refutes the propensity view we have been considering.

This is, however, not the case. For, according to the propensity view, if no localization occurs, then the state of each individual system must at time t

be the pure state ψ_t. In other words, we cannot regard the individual system as having either passed through the filter, having state ϕ_1, or as having failed to pass through the filter, with state ϕ_2. The actual physical state of the individual system is rather a superposition of these two states. There is a probability of 1/2 that the system will be localized on one side of the filter, and a probability of 1/2 that it will be localized on the other side. But until a localization occurs, we cannot regard individual systems as being either on one side or the other side of the filter. Rather, we must regard each individual system as being on both sides of the filter.

Thus the propensity viewpoint is able to retain the thesis that wave packet reduction is to be associated only with localization. Filters which prepare systems in eigenstates of momentum, etc., in no way undermine this thesis. For, of course, the propensity viewpoint predicts that a system which is actually localized (e.g., detected) on one side or the other of the filter will have behaved precisely *as if* its state were the pure state ϕ_1 or ϕ_2, even though in fact the system will have been in a superposition of these two states.

My claim is, then, that the propensity view sketched here, according to which QM in the first instance makes predictions about localizations—or about the two 'observables' *position* and *time*—recaptures in an entirely consistent manner the full predictive power of orthodox QM. The possibility of formulating QM entirely in terms of predictions concerning position and time is, incidentally, affirmed by Feynman and Hibbs, who write: "... all measurements of quantum-mechanical systems could be made to reduce eventually to position and time measurements... . Because of this possibility a theory formulated in terms of position measurements is complete enough in principle to describe all phenomena."[4]

There is one final problem that confronts the propensity version of QM advocated here, namely the problem of how the state vector ϕ for two (or more) microsystems can be interpreted as specifying the physical state of these systems when ϕ is a function of six-dimensional configuration space and not of three-dimensional physical space. There is however a perfectly simple, straightforward solution to this problem. In the case of a system consisting of two microentities, we can regard the position probability density function as assigning, to any *two* volume elements dV_1 and dV_2 of the volume V available to the entities, a definite probability $P_{dV_1 dV_2}$ that one entity will be localized in dV_1, the other entity being localized in dV_2—should the conditions for localization be realized. Thus, although the state vector is a function of six-dimensional configuration space, the quantum variable position$_v$ (i.e., position probability density) can be regarded as specifying the value of a physically real propensity in three-dimensional physical space. Clearly this can be extended to other quantum variables such as momentum$_v$, and also to systems consisting of more than two microentities. There is thus no problem about interpreting quantum variables position$_v$, momentum$_v$, angular momentum$_v$, and so on as attributing real physical propensities to microsystems, and thus specifying the actual physical state of microsystems, when two or more microsystems are involved. One point is, however, worthy of mention. In the case of two (or more) microsystems which are 'quantum entangled,' in Park's[5] memorable phrase, having no separate quantum states, then the position probability density function (position$_v$) attributes a propensity to the *composite* system, and cannot be interpreted as attributing distinct propensities to the two microsystems. In other words, according to the propensity version of QM, microentities that suffer quantum entanglement lose their individual identities in a quite straightforward physical sense, since essential physical properties can be attributed only to the *composite* system, and not separately to the individual microentities. It is only when localizations occur that members of a composite, quantum-entangled system can recapture their individual physical identities.

This concludes my review of how the conceptual problems which appear to lie in the way of developing a microrealistic propensity version of QM are to be overcome. One *technical* problem does of course need to be solved before a fully fledged microrealistic theory can be formulated: namely the problem of specifying precise, necessary and sufficient conditions for localizations to occur purely in terms of the physical states of the microsystems involved. There do not appear to be, however, any insuperable conceptual difficulties in the way of solving this problem. If the main arguments of the paper so far are more or less correct, then this technical problem clearly becomes an extremely important problem to try to solve. The main purpose of this paper is just to emphasize the importance of this much neglected problem, since its solution would provide us with that almost unheard of thing, a microrealistic version of QM.

9. COMPARISON OF THE FREQUENCY AND PROPENSITY APPROACHES TO MICROREALISM

We have before us two quite different approaches to the problem of developing a microrealistic version of QM. How are we to assess the respective merits of these two approaches ? In this section I wish to suggest that the propensity approach has perhaps certain advantages over the frequency approach—though I would of course wish to see *both* approaches pursued.

The great advantage of the propensity approach over the frequency approach is that only the former offers the hope of leading to a theory which enables us to understand fundamentally probabilistic processes in terms of the physical features of the individual physical systems involved. The frequency approach cannot offer this hope. The simple decision to adopt the frequency interpretation of probability carries with it the consequence that the basic notion of quantum mechanical *state*, having probabilistic implications, cannot be applied to the *individual* physical system, but only to the ensemble (or rather, this notion can only be applied to the individual system with respect to some specific ensemble). Consequently, the theory cannot enable us to understand why systems evolve in the way in which they do, on the individual level.

This difference can be brought out quite strikingly by contrasting the explanations that the two theories can give for typical 'quantum' phenomena.

Consider, for example, interference phenomena, such as that produced by the two-slit experiment, performed, let us say, with electrons, there being at any one time no more than *one* electron passing through the two-slitted screen. If we accept the frequency viewpoint, then it remains utterly mysterious why the electrons build up, on a statistical basis, an interference pattern on the detecting screen when each individual electron goes through either one slit or the other, and there is apparently no force in each individual case which influences the electron 'to take into account' the existence of the slit through which it did not pass. How is it possible for there to be, statistically, an interference effect if each individual electron goes only through one slit or the other, there being no possible physical reason why the existence of the other slit should effect the subsequent flight of the electron ? We ask this question, but no answer can be forthcoming if we accept the frequency view, since the *explanation* for the interference effect does not exist on the level of the individual physical processes occurring, but only on the level of the *ensemble* of systems. If we accept the propensity view, however, there is a very natural explanation for the interference phenomena in terms of the *individual* physical processes that occur. Each individual electron passes through *both* slits, in the sense that the propensity of the electron to become localized acquires values greater than zero in both slits simultaneously. The propensity of the electron to become localized evolves in a wavelike manner, interferes with itself, and then determines, in a stochastic fashion, where the electron will be localized on the detecting screen. Thus it is only to be expected that if this physical process is repeated a great number of times an interference pattern composed of distinct localizations will gradually be built up on the detecting screen.

Some defenders of the frequency approach have attempted to explain the outcome of the two-slit experiment in terms of Duane's quantum rule for the exchange of momentum. Landé,[3] for example, claims that Ehrenfest and Epstein succeeded in resolving the problem in terms of Duane's rule, and both Ballentine[6] and Popper[7] quote Landé with approval on this point. Ehrenfest and Epstein themselves, however, do not at all claim to have solved the problem in the way suggested. They conclude their second paper with the words: "It is, therefore, clear that the phenomena of the Fresnel diffraction cannot be explained by purely corpuscular considerations. It is necessary to attribute to the light quanta properties of phase and coherence similar to those of the waves of the classical theory."[8] (It should be remembered that Duane, Ehrenfest, and Epstein originally conducted the discussion in terms of X-ray diffraction rather than matter diffraction.)

The Duane, Ehrenfest, and Epstein approach to the problem does not then, it seems, succeed. It should be noted, however, that even if the technical problem had been solved successfully, this would not have helped the frequency viewpoint very much. For as long as the basic frequency viewpoint is retained there can strictly speaking be no explanation of physical processes on the individual level. On an intuitive level, the Duane approach looks promising: but this is because the Duane approach makes a kind of implicit appeal to the *propensity* idea. The basic Duane rule asserts that an object periodic in space, such as a crystal, can only take on momentum in discrete amounts. The hope presumably is that this rule may enable us to understand why electrons or photons, although particles, nevertheless interact with crystals in the probabilistic way in which they do, so as to build up apparent interference effects. The explanation arises from the physical structure of the individual crystal. There is in other words an implicit appeal to the *propensity* idea: An individual crystal has a certain propensity—which determines, stochastically, how it interacts with an individual particle—and this propensity is directly related to the physical structure of the crystal.

But clearly all this breaks with the basic frequency standpoint of interpreting probability in terms of the frequency idea. The appeal which is made to the Duane explanation for apparent interference effects thus badly backfires. Not only does the Duane explanation not work: Furthermore, in appealing to this explanation, champions of the frequency view reveal that they are secretly hankering after a *propensity* view.

This hankering for a propensity view becomes of course quite explicit with Popper's *particle* propensity interpretation of QM. The trouble, however, with Popper's view—apart from the fact that it appeals to the inadequate Duane treatment of interference—is that propensities are attributed to setups involving *macro*objects, such as screens and crystals, and not to microentities themselves. Thus a Popperian propensity version of QM cannot be microrealistic. Such a version of QM cannot provide us with an explanation for fundamentally probabilistic physical processes couched solely in terms of the physical features of the *individual microsystems* involved.

Interference phenomena are not the only quantum effects that can be understood rather naturally in terms of individual physical processes if the propensity view is accepted, but which remain utterly mysterious if the frequency-particle view is accepted. The fact that quantum statistics treats similar particles as strictly identical or indiscernible, having no distinct individuality, can also be understood rather naturally in terms of the propensity view. As far as the frequency-particle view is concerned, this lack of separate identity of similar particles must be highly mysterious, for, according to this view, any fundamental particle has its own unique trajectory, distinct from all other trajectories. It ought, then, to be possible to distinguish similar particles in terms of their quite distinct *trajectories*. If we accept the propensity view, however, the lack of individuality of similar particles becomes readily understandable. Particles that have become quantum-entangled do not have distinct trajectories. The fundamental physical properties of microentities, namely propensities, cannot be attributed separately to quantum-entangled systems: Only to the whole composite system can such propensities be attributed. Thus in a quite straightforward physical sense two similar quantum-entangled fundamental particles lose their separate identity and become one indivisible entity. Two *localizations* have a separate identity; but two quantum-entangled fundamental particles do not. (According to the propensity view, a quantum-entangled system becomes disentangled when localizations occur within the system.)

We may perhaps go even further than this, and hold that a realistically interpreted quantum statistics which treats similar particles as indiscernible is actually committed to the thesis that particles *do not have* differentiated trajectories. In other words, it may be true to say that quantum statistics cannot consistently be realistically interpreted in accordance with the frequency-particle viewpoint.

Both viewpoints face problems. The propensity viewpoint faces the problem of specifying precise, necessary and sufficient conditions for localizations to occur in any system S in terms of the *physical states* of S. Only when this is done can a precise, fully realistic propensity version of QM be formulated which can dispense with relying on postulate III of the previous section for making experimental predictions. As I have already emphasized, however, there do not appear to be any insuperable conceptual difficulties in the way of solving this technical problem. The frequency-particle view faces, however, a number of problems wholly in addition to the basic disadvantage that we have been considering of being quite unable to explain typical quantum effects in terms of individual physical processes. Briefly, let me indicate what some of these additional problems are.

1. We have seen that the frequency view is committed to the idea that QM in the first instance makes probabilistic predictions concerning the *classical states* of individual systems, which are simply detected by the

appropriate measurement procedures. This may work for position and momentum, but it is not clear how it works for other quantum observables such as angular momentum or spin. We cannot, for example, just say that each particle does actually twirl like a classical top at some specific orientation, spin measurements simply detecting this orientation. Nor can we say that as far as spin is concerned, QM merely makes predictions about the outcome of performing spin measurements—for this abandons microrealism. It may be that spin, and all other relevant observables, can be incorporated into the frequency view, perhaps along lines similar to those advocated here in connection with the propensity view: But until this is done, the frequency viewpoint cannot be held to capture the full predictive content of orthodox QM in a microrealistic fashion.

2. Gardner[9] has pointed out that the version of the particle view advocated by Popper (and also presumably be Ballentine) suffers from the major disadvantage that we cannot say that the values of position, momentum, etc. which a particle actually *possesses* just before measurement bear any relation to the *measured* values of these 'observables.' This point clearly strikes a lethal blow at Popper's and Ballentine's viewpoint, since the values of position, momentum, etc. which particles actually possess become wholly undetectable, wholly metaphysical, and irrelevant to experimental physics. The microrealistic program is utterly sabotaged. Fine[10] has, however, recently suggested a way in which Gardner's objection may perhaps be overcome.

3. If we are to take seriously the view that microsystems are straightforward *particles* possessing a precise position and momentum at each instant, then we need to hold presumably that microentities pursue reasonably smooth trajectories. A particle which could leap in a discontinuous fashion from point to point in space could hardly be said to be a particle at all. This means, however, that the frequency view needs to add an extra postulate to QM which asserts that *individual* systems evolve with continuous trajectories. There are, however, a number of objections to such a postulate. First, it is not clear what precisely such a postulate should assert. Second, any such additional postulate must it seems have a somewhat ad hoc character. Third, in view of what various anti-hidden-variable theories have succeeded in establishing, we may rather doubt that the continuous trajectory thesis is compatible with QM.

4. There is an additional reason for claiming that the frequency view needs an additional continuous-trajectory postulate—a reason which may be brought out by considering a rehash of Schrödinger's 'cat paradox.' Consider an ensemble E of boxes, in each box there being a cat, a radioactive atom, and a device which ensures that the cat dies if the atom decays. Let us suppose a

pure state ψ can be associated with the ensemble. After a time t we can calculate the fraction p_t of cats that will have died. And, according to the frequency view, strictly this is *all* that we can calclate. QM, on this interpretation, tells us nothing about the evolution of the *individual* system, except that each system evolves so as to comply with the statistical predictions of QM. This means that the frequency view in no way prohibits an individual cat being dead at one moment and then being alive at a later moment. Only if we add to QM some postulate concerning the *continuous evolution of the classical states of individual systems* can we exclude such possibilities—thus recapturing classical physics from QM. In other words, it is essential that some kind of continuous-trajectory postulate be added to the frequency version of QM for reasons entirely in addition to those discussed in 3 above.

5. If the frequency viewpoint is to provide us with a fully microrealistic version of QM, then not only must the notion of quantum observable be fully microrealistic, but also the crucial notion of quantum state. The frequency notion of quantum state appears to make, however, an inevitable if implicit reference to *macroscopic* objects or processes. According to the frequency view, and unlike the propensity view, the notion of quantum state does not in any sense specify the *physical* state of the individual microsystem. Rather, the notion of quantum state is applicable to the *ensemble* (or is applicable to an individual system only insofar as it is considered to be a member of such and such an ensemble). The crucial question then becomes: How do we specify such and such an ensemble ? If we can do this in purely *micro* terms, then we can give a purely microexplication of the crucial notion of quantum mechanical state. But this does not appear to be possible. For what we should have to do is specify the conditions for an individual particle S to be a member of an ensemble E in terms of the physical properties actually possessed by S, that is, in terms of the *classical state* of S. And the very basis of the frequency viewpoint is to divorce completely the idea of quantum mechanical state from the idea of the classical state of individual microparticles. In order to explicate the notion of quantum state we need, then, it seems, to refer to some *preparation procedure* which prepares systems in the given state (as we in effect saw in Section 8). Any such preparation procedure will inevitably involve *macroscopic* objects or processes, or such things as stable magnetic fields which can only be understood as arising from stable macroscopic magnets. In other words, if we accept the frequency viewpoint, implicit in the very idea of quantum mechanical state, there must be a covert reference to macroscopic entities. And this in turn means that the frequency view cannot in the end be said to be a fully microrealistic theory.

In answer to this charge it might be argued that the notion of quantum mechanical state need not refer to *specific* macroscopic preparation procedures, but rather may simply refer to *any* procedure which produces an ensemble of particles with the appropriate statistical properties. But this would have the very serious consequence that QM would become entirely unfalsifiable. For whenever we refuted experimentally a quantum mechanical prediction we should be obliged to conclude that we did not have the proper ensemble in the first place. It is only if we have more or less precise specifications as to what constitutes an ensemble E with quantum state ϕ associated with it that we can be in a position to *test* the predictions of QM.

There appears to be then a basic *conceptual* block to the frequency viewpoint leading to a fully microrealistic theory. It should be noted that the propensity view does not suffer from the same problem, since according to this view the state vector ϕ in effect specifies the actual microphysical state of the individual microsystem. There is thus no implicit reference to *macroscopic* objects in the propensity notion of quantum mechanical state.

I conclude that at the present time the propensity view appears to be more promising than the frequency view, in that there do not appear to be lethal conceptual objections to the propensity view leading to a microrealistic version of QM, whereas the same cannot be said of the frequency view. In addition, the propensity view provides an understanding of quantum phenomena in terms of individual microphysical processes, whereas the frequency view cannot hope to provide this.

10. THE POSSIBILITY OF AN EXPERIMENTAL TEST

We do not need to leave the matter here, however, for it appears to be possible, at least in principle, to decide between the propensity and frequency versions of QM on *experimental grounds.*

A basic difference between the two versions of QM is that the propensity view is committed to the existence of wave packet reduction, or localization, as a real physical process which does not occur in accordance with the time-dependent Schrödinger equation, whereas the frequency view denies the existence of any such physical process. Consequently, the two versions of QM will in certain circumstances make slightly different experimental predictions. It seems that it might be possible to capitalize on this difference for the case of radioactive decay.[11]

Consider the case of a nucleus that decays by emitting an α-particle. If we accept the propensity view, then we shall be obliged to hold that the wave packet associated with each α-particle (more strictly, the position probability density of each α-particle) slowly leaks out of the nucleus until the α-particle is actually localized outside the nucleus, and the nucleus has decayed. Suppose we now surround the α-particle-emitting nucleus with a continuously functioning α-particle detector. In this case at each instant that the detector fails to localize the α-particle outside the nucleus, the α-particle in effect becomes localized *inside* the nucleus, and there is a 'reduction of the wave packet' of the α-particle.

Thus, if we accept the propensity version of QM, which postulates localizations as real physical occurrences, we will be led to expect that in general the presence or absence of continuously operating α-particle detectors surrounding α-particle-emitting nuclei should make a substantial difference to the rate of decay of the nuclei. If, however, we accept the *frequency* version of QM, the presence or absence of α-particle detectors surrounding the decaying nuclei should make no difference to the rate of decay at all—since according to the frequency view measurement does not produce a *physical* wave packet reduction at all. Thus the two versions of QM will in general make different predictions for the case of the rate of decay of nuclei surrounded by a continuously operating α-particle detector.

For one important case, however, this argument breaks down. If the rate of decay happens to be exponential, then the propensity theory, like the frequency theory, predicts that the decay rate is unaffected by the presence or absence of detectors.

In order to see this, let us suppose we have a sample of α-particle-emitting nuclei that decay exponentially in the *absence* of α-particle detectors. We prepare the nuclei to be in their initial state at time t_0, so that in each case the state vector to be associated with each α-particle is zero everywhere outside the nucleus. As long as no measurements are performed on emitted α-particles, then the probability P_t that a nucleus has not decayed at time t is given by $P_t = \exp[-A(t - t_0)]$, where A is some constant.

Suppose now we make n successive instantaneous measurements for emitted α-particles at times $t_1, t_2, \ldots, t_n$. According to the propensity version of QM, performing a measurement for an emitted α-particle has the effect of localizing the α-particle inside the nucleus, if no α-particle is detected. The wave packet of the α-particle is 'reduced.' Thus, according to the propensity version of QM, each time a measurement is performed and no α-particle is detected, the decaying nucleus returns to its initial state. We are assuming, however, that between successive instantaneous measurements, no localizations occur. Thus between the rth and the $(r + 1)$th measurement, the rate of decay is exponential. In other words, if we let P_r be the probability that the nucleus has not decayed at the rth measurement, given that it has not decayed at the $(r - 1)$th measurement, we have

$$P_r = \exp[-A(t_r - t_{r-1})] \qquad (1)$$

But

$$P_{t_n} = P_1 P_2 \cdots P_n \tag{2}$$

Hence from (1) and (2) we have

$$P_{t_n} = \exp[-A(t_1 - t_0)] \exp[-A(t_2 - t_1)] \cdots \exp[-A(t_n - t_{n-1})] \tag{3}$$

i.e.,

$$P_{t_n} = \exp[-A(t_n - t_0)] \tag{4}$$

But (4) is precisely the exponential rate of decay that, we are assuming, QM predicts in the case where no measurements at all are performed. Thus, for the special case of exponential decay, the propensity version of QM, like the frequency version, predicts that decay rate is unaffected by the presence or absence of α-particle detectors.

However, it also emerges from the above argument that the propensity version of QM will *only* predict that the decay rate is unaffected by detectors for this special case of exponential decay. For it is only in the case of exponential decay that we can derive (4) from (3). Thus the above argument shows that if QM predicts a nonexponential rate of decay in the case where no localizations occur, then the propensity version of QM will predict that the presence or absence of α-particle detectors *will* affect the rate of decay. In the case of a nonexponential rate of decay we thus have the possibility of discriminating, on experimental grounds, between the two versions of QM.

Unfortunately, QM predicts that radioactive decay does proceed very nearly at an exponential rate. But not quite. Khalfin[12] has shown that all quasistationary states that have a lowest energy in their spectrum must eventually decay more slowly than exponentially. Winter[13] has discussed the nonexponential rate of decay of quasistationary states at times either short or long in comparison with the half-lifetime. Winter has shown that the decay rate at sufficiently long times decreases like an inverse power of time. And Winter remarks that there are no objections in principle to detecting these nonexponential rates of decay, although the experimental difficulties may well be formidable.

Thus it would appear to be at least in principle possible to decide between the frequency and propensity versions of QM on experimental grounds. If a certain type of nucleus is found experimentally to decay nonexponentially even in the presence of continuously operating α-particle detectors, then this in itself should suffice to refute the propensity version of QM. If, on the other hand, QM predicts a nonexponential rate of decay, and in the presence of α-particle detectors the decay rate is found to be exponential, then this refutes the frequency version of QM.

11. THE IMPORTANCE OF THE PROPENSITY IDEA FOR PHYSICS

Quite apart from the conclusions reached above, there is one rather general conclusion that I would like finally to emphasize. It is this. As long as we stick to the frequency interpretation of probability, and as long as probability enters in a fundamental way in our theories—so that these theories are indeterministic, and do not presuppose more basic deterministic theories—then we are debarred from understanding physical processes in terms of the physical characteristics of microsystems themselves—whether these be particles or fields. Only by developing theories in terms of the *propensity* idea advocated here can we hope to understand fundamentally probabilistic phenomena in terms of individual physical characteristics of microsystems themselves. This simple consideration presents, I believe, a powerful case for introducing the propensity idea into physics, whatever may be the success or failure of the particular attempt made here to do this.

REFERENCES

1. E. C. Kemble, *The Fundamental Principles of Quantum Mechanics*, McGraw-Hill, New York (1937), pp. 58–71.
2. H. Margenau, *Phil. Sci.* **25**, 25 (1968).
3. A. Landé, *New Foundations of Quantum Mechanics*, Cambridge University Press, Cambridge (1965).
4. R. P. Feynman and A. R. Hibbs, *Quantum Mechanics and Path Integrals*, McGraw-Hill, New York (1965), p. 96.
5. J. L. Park, *Am. J. Phys.* **36**, 211 (1968).
6. L. E. Ballentine, *Rev. Mod. Phys.* **42**, 358 (1970).
7. K. Popper, in *Quantum Mechanics and Reality*, M. Bunge, ed., Springer, Berlin (1967).
8. P. Ehrenfest and P. Epstein, *Proc. Nat. Acad.* **13**, 407 (1927).
9. M. Gardner, *Br. J. Phil. Sci.* **23**, 13 (1972).
10. A. Fine, *Br. J. Phil. Sci.* **24**, 1 (1973).
11. N. Maxwell, *Phys. Lett.* **43A**, 29 (1973).
12. L. A. Khalfin, *Sov. Phys. J.* **6**, 1054 (1953); *Sov. Phys.—Doklady* **5**, 515 (1960).
13. R. G. Winter, *Phys. Rev.* **123**, 1503 (1961).