

$$U = \int \cdots \int \sum_{k>j>4} Q(k, j) dv_5 \cdots dv_n.$$

Now the coefficient of $P_{(12)(34)}$ in the part of (5) involving W explicitly is $-\Delta_{(12)(34)}W = -\Delta_{(12)}\Delta_{(34)}W$. Hence it is seen that the effect of the terms involving $k>j>4$ is covered simply by displacing the origin for the energy by an amount U , which we suppose done. (This is not quite the appropriate displacement in the case of higher order permutations. For instance, in the case of $P_{(12)(34)(56)}$ one would wish to absorb in W only terms corresponding to $k>j>6$. However, except in the case of very high order permutations, which presumably involve very small integrals, the difference in the number of terms included is negligible in view of the fact that the number n of atoms is very large.)

It remains to be shown that we can disregard terms of the form

$$Q(3, 1), \quad Q(4, 1), \quad Q(3, 2), \quad Q(4, 2) \quad (35)$$

$$\text{and } Q(k, j) \text{ where } k>4 \text{ and } j=1, 2, 3, 4. \quad (36)$$

The effect of each individual term in (35)–(36) is small in absolute magnitude compared with that of $Q(1, 2)$ or $Q(3, 4)$ and presumably differs from the latter by a factor of the order δ , where δ is defined as in (1). Namely, there are $2n-2$ factors in (3) which are large in regions where $Q(1, 2)$ or $Q(3, 4)$ are important, as is seen on substituting (6) in (3), specialized to $P^{(i)} = P_{(12)(34)}$ and remembering that each ψ function is large only on its own atom. On the other hand, only $2n-3$ factors are large where any term of the type (35) or (36) is important. Furthermore, if each atom is coupled only to its neighbors, the terms (36) outnumber $Q(1, 2)$, $Q(3, 4)$ only by a factor $2z$. Since z , the number of neighbors, is small (6, 8, or 12) for the various cubic arrangements, we are justified in neglecting (35), (36) if the interatomic distance is suffi-

ciently large. From the foregoing considerations, it appears that the error due to dropping (35), (36) is of the order $2z\delta$ when measured relative to unity. This fact at first sight seems rather disquieting, as the errors we estimate due to other causes are of the order $2z\delta^2$ (cf., for instance, Eq. (23)). Thus the approximations (10) and (11) would appear to be the dominant causes of error, causing trouble unless δ is exceedingly small. However, one fortunately finds that the order of the error can be reduced to $2z\delta^2$ if in computing $V_{(12)}$, for instance, one takes the potential in the integrand of (3) to be not just $K(1, 2) + \sum W_i + U + C$ but rather that

$$K(1, 2) + \sum W_i + U + C \int \cdots \int \sum_{j>2} [K(j, 1) + K(j, 2)] |\psi_3|^2 \cdots |\psi_n|^2 dv_3 \cdots dv_n \quad (37)$$

inclusive of contributions from the time exposure charge clouds of atoms other than 1, 2. Such a procedure is essentially the analog of the Hartree method. The error resulting from (35) may seem to still be of order δ rather than δ^2 , but as a matter of fact is entirely inconsequential. Indeed for a given permutation $P_{(12)(34)}$ it is altogether unlikely that 3 or 4 be a neighbor of 1 or 2, meaning that for the great bulk of permutations, terms of the type (35) are nonexistent so long as we are including only coupling between adjacent atoms. Hence the approximations (10) and (11) are no more serious sources of error than the other causes which we consider.

It is interesting to note that no internuclear terms e^2/R^{jk} contribute to (37), as their contribution to C just cancels their effect on the rest of (37). As already stated, the terms $\sum W_i$ and U also have offsets in C .

In the preceding discussion we have considered for simplicity only fourth order permutations, but the argument can be readily extended to others.

Quantum-Mechanical Description

HENRY MARGENAU, *Sloane Physics Laboratory, Yale University*

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It is pointed out that, by removing a single quantum-mechanical postulate commonly accepted, several conceptual difficulties in quantum-mechanical description can be eliminated.

THE discussion of a recent paper by Einstein, Podolski and Rosen¹ has brought to light an interesting divergence of opinions as to the meaning of reality. To the writer it seems, however, that the issue at stake is more than the merit or demerit of any particular conception of reality, and that a very fundamental point of

quantum-mechanical axiology is involved. For Einstein, Podolski and Rosen have shown, by employing correctly the usual quantum-mechanical postulates, that the state of system 1 which, by hypothesis, is *isolated* from system 2, depends on the type of measurement performed on system 2. This, if true, is a most awkward *physical* situation, aside from any monstrous philosophical consequences it may have. We wish to show that, by the removal of a single postulate

¹ A. Einstein, B. Podolski and N. Rosen, *Phys. Rev.* **47**, 777 (1935); E. C. Kemble, *ibid.* **47**, 973 (1935); A. E. Ruark, *ibid.* **48**, 466 (1935); N. Bohr, *ibid.* **48**, 696 (1935).

commonly accepted, the real difficulty inherent in Einstein-Podolski-Rosen's conclusion disappears.

I

The postulate in question may be stated thus: When a (single) measurement is performed on a physical system, then immediately after the measurement the state of the system is known with certainty. That is to say, if the system is in a state φ , the measurement causes the state function to transform itself into ψ , where ψ is an eigenstate of the operator belonging to the measured observable. This assumption is clearly involved in Einstein-Podolski-Rosen's interpretation of the reduction of the wave packet, and if it be denied that in general a measurement produces an eigenstate, their conclusion fails, and the dilemma disappears. We shall refer to the postulate stated as I.

II

I is not self-evident, or *a priori* true; for there is hardly more justification for supposing that a single measurement determines completely the state *after*, than to suppose it to determine completely the state of the system *before* the act of measurement. The latter is usually denied; to admit the former supposition would introduce a peculiar asymmetry into quantum-mechanical description. But there are stronger arguments against I.

III

I is definitely contradictory to another, in a sense more fundamental, postulate, namely, Schrödinger's time equation. To supply substance to the following simple but formal consideration the reader may think of the system as a particle, of the state function as referring to a pure case with respect to momentum, of M as a coordinate measurement, and therefore of ψ as a δ -function. I implies that, under the act of measurement, $\varphi \rightarrow \psi$, or, to use customary terminology, the operator M converts φ into ψ :

$$M\varphi = \psi. \tag{1}$$

We observe that in this equation M cannot be a *unique* operator such as those with which we are usually confronted in quantum mechanics, for ψ depends on the outcome of the measurement

and cannot be predicted. That is to say, while we know, in our example, that ψ is a δ -function, the position of its peak is not fixed. ((1) must not, of course, be mistaken for a characteristic value equation, since φ and ψ are different functions.)

Eq. (1) is sometimes interpreted differently. It is supposed that M selects from an assemblage of systems φ a subassemblage ψ and that it is not the total assemblage φ which undergoes a transition from a state to an eigenstate.² But there are difficulties in such a view, of which two may be mentioned.

1. If φ truly referred to a larger collection of physical systems than ψ , the two functions would have to be constructed in different configuration spaces, and it would be difficult to give mathematical meaning to (1).
2. It is certainly proper to make a measurement on a single system. In that case the above interpretation, which involves the splitting off of a subassemblage, is meaningless.

We thus conclude that (1), if it has significance at all, must imply the actual transformation of the state function for a single system into an eigenstate function for the same single system.

On the other hand, the measurement is certainly a physical operation, describable in the ordinary manner as an interaction between physical systems. As such it is subject to Schrödinger's equation. Let the "natural" Hamiltonian operator (the form valid for the isolated state) of the system be H_0 , and let H_M represent the interaction with the measuring device (in our example coupling terms with the radiation field), so that $H = H_0 + H_M$. Then

$$H\varphi = (\hbar/2\pi i)(\partial\varphi/\partial t).$$

Thus if the measurement takes place in a small time Δt , we have

$$\Delta\varphi = (2\pi i/\hbar)\Delta t H\varphi.$$

Since $\varphi + \Delta\varphi$ is the function into which the measurement converts φ , and which we previously called ψ ,

$$\{1 + (2\pi i/\hbar)\Delta t(H_0 + H_M)\}\varphi = \psi. \tag{2}$$

This relation is of the same form as (1). But it is unlikely to make ψ a δ -function even if the correct form for H_M were known. The essential contradiction, however, arises from the fact that,

² I owe this criticism to Professor Kemble.

if we put

$$M = 1 + (2\pi i/h)\Delta t(H_0 + H_M)$$

as (1) and (2) require, the left-hand side of this equation is an unspecifiable, nonunique operator, while the right-hand side is unique.³ Since both (1) and (2) cannot be correct, we abandon (1).

IV

The assumption that a single measurement fixes a state either before or after the act of observation is untenable from the point of view of any reasonable probability theory. In contrast with classical physics, quantum mechanics defines its states in terms of functions, φ , in configuration space in such a way that there exists a unique (aside from arbitrary phases) correspondence between φ and a sequence of probability amplitudes. More explicitly, φ , and hence the state which it represents, is entirely equivalent to a set of numbers a_i associated with a certain operator P . This set of numbers defines a *probability distribution*, whose *elements* are the observations on the observable p which belongs to the operator P , and whose *properties* are the eigenvalues of P .⁴ A quantum-mechanical state is therefore synonymous with a probability distribution. A probability distribution, however, cannot be fixed by a single measurement, but requires a very large number of observations. This is true even if the distribution consists of zero's for all properties but one (quantum-mechanical pure state).

To illustrate: In throwing a die, the probability distribution, which corresponds to the quantum-mechanical definition of a state, is the sequence of numbers $\frac{1}{6} \cdots \frac{1}{6}$ for the properties 1...6. If a 5 turns up in a single throw, I can conclude that I have thrown a 5, but cannot infer the distribution $\frac{1}{6} \cdots \frac{1}{6}$. This can be done only after a large number of throws. The fact that I have thrown a 5, while it has converted my ignorance with respect to the outcome of this observation

³ This situation is of course known. Von Neumann (*Mathematische Grundlagen der Quantenmechanik*) deals with it most thoroughly. We find it difficult to accept his explanation for reasons to be discussed elsewhere.

⁴ For terminology see V. Mises, *Wahrscheinlichkeitsrechnung* (Deuticke, 1931). The present point of view has previously been presented in greater detail by the author in *The Monist* 42, 161 (1932).

into certain knowledge, has not changed the distribution of the probability aggregate from which I started.

V

The removal of I renders impossible the *preparation* of states in the manner advocated by Dirac and others. But the situation is perhaps not altogether unsatisfactory on that account, for it is still possible to *ascertain* a state, namely, by performing a large number of observations. A typical instance of a *complete* set of simultaneous observations which does define a state is the photographic record of a spectrum, where the intensities represent at once the distribution of probabilities among the various pure energy states. It must be admitted, however, that the relation between abstract states and experience is a source of difficulties which weaken the proposal here made and which must be subjected to closer scrutiny.

VI

While the previous arguments seem to show that I is in several respects undesirable and conflicting with other axioms, one may also convince himself that it is *unnecessary*. For no significant quantum-mechanical calculation requires I. Space does not permit an elaboration of this assertion here.⁵

VII

Returning to the argument of Einstein-Podolski-Rosen, the solution of their problem in the light of the present considerations is clear. If a great number of measurements of A had been performed on the state of system (1) after the interaction, *all* the coefficients ψ_n in Eq. (7) of Einstein-Podolski-Rosen's paper would have been determined. Conversely, if many measurements had been made on B , all the φ_s in their Eq. (8) would be known. A difficulty can arise only if the expansion of the right of (7), with the experimentally determined ψ_n 's, yielded a different function that does the expansion in (8) with experimentally determined φ_s 's. There is no indication that this will be the case.

⁵ The author hopes to return to the entire problem in a paper to be published in *J. Phil. Sci.*