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SYMPOSIUM ON QUANTUM MECHANICS*

ON THE PROCESS OF MEASUREMENT IN QUANTUM MECHANICS

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It is the purpose of this note to comment on some important problems which have been already vividly discussed by several authors. Besides the well known former discussions of Schrödinger and J. v. Neumann I should like to mention here especially H. Margenau's article, "Critical Points in Modern Physical Theory," (3) which strongly influenced my present discussion.

Quantum mechanics gives a very clear and unique answer to the question as to which possible results we may expect when we measure a certain observable, represented by an operator with certain eigen-values. We get an equally clear answer if we ask how great the probability of one of the possible results will be, provided a definite "*state*" or wave function is given. But there remain some questions about the process of observation itself—questions for which we do not get unambiguous answers because orthodox quantum mechanics treats the concept of "measurement" as a fundamental one which ought not to be analysed. It is not so clear, however, whether this attitude can be maintained without exceptions or restrictions.

Let us take a very simple case, mentioned also in Margenau's article. A beam of light goes through a Nicoll, giving us linear polarised light. A single photon may be isolated out of this polarised beam of light; and this single photon may strike another Nicoll. The process is to be described in the following way. We have to construct a wave function for this single photon. The wave function is a Maxwellian wave, split up by the second Nicoll into two waves, which we call φ and ψ.

* The following questions were sent to each of the invited contributors of this symposium: 1a) What is a physical system, and what is meant by the state of a physical system in classical physics and in quantum mechanics? 1b) What philosophical clarification in the concept of a state has resulted from quantum theory? 2a) What is the status of particles in quantum mechanics? 2b) What philosophical changes of the particle concept, and in the concept of mechanism, have resulted from the quantum theory? 3a) In what ways has "causal explanation" been modified by modern quantum theory? 3b) How do these changes affect the philosophic problem of determinism?

The symposium was conducted by Prof. Henry Margenau, with the assistance of the editor. Further papers will appear in the next issue by E. Kemble and W. H. Werkmeister. Additional contributions on the subject are welcome.

This splitting is the first step in the process of measurement of a certain observable, but it is *not* identical with a measurement itself, as has been emphasized by Margenau. The second Nicoll can be used to let the photon make a *decision*—to be reflected by the Nicoll or to go through it—and this shows us how the *first step* in each process of measurement takes place: if we measure momentum by applying a diffraction grating, or a spin component by a Stern-Gerlach experiment, we always make application of an apparatus dissolving a given wave function into new wave functions $\varphi_1, \varphi_2, \varphi_3, \cdots$ (orthogonal to each other) which are the eigenfunctions of the operator corresponding to the observable we intend to measure. Practically in each case these different waves $\varphi_1, \varphi_2, \varphi_3, \cdots$ must cover different parts of space: then they allow a reduction of the intended observation to an observation of position—as discussed by Margenau for the example of measurement of momentum by a diffraction grating.

But the Nicoll alone does not suffice to perform the measurement. Let us consider this point further, though it has been already treated by Margenau: The two wave functions $\varphi = \varphi_1; \psi = \varphi_2$ are still able to *interfere* with each other; and this means that the "decision" has not yet taken place. Therefore we let the wave φ fall upon a photographic plate. (We assume here an idealised photographic plate: *Each* photon hitting it will be absorbed, and a single photon will with certainty activate a certain silver grain. In a sense this photographic plate makes the decision. Any further interference between φ and ψ is now excluded; therefore no contradiction can occur if we make the assertion that now only two distinct possibilities are left: Absorption of one photon or none, with probabilities given by the intensities of φ and ψ.

I have tried to outline here a definite view about the process of measurement or observation. But it is characteristic of the difficulties of the problem that this view is strictly opposed to that of several prominent authors. According to my own idea it is necessary for each observation to make—*by a real physical process*—the waves φ, ψ *incoherent* with each other. Then, making use of v. Neumann's terminology, we get—instead of a "pure case," represented by a single wave function—a "mixture," which may be represented by the statistical matrix

$$U = U^+ = (U_{nm}). \tag{1}$$

As v. Neumann (4) has shown, we get the "expectation value" $\bar{A}$ of any operator A by the formula

$$\bar{A} = \sum_{nm} A_{nm} U_{mn} = Sp(AU). \tag{2}$$

But v. Newmann is inclined to believe that no physical process can convert a "pure case" into a "mixture"; indeed such a process would not be describable by a Schrödinger equation. He assumes this conversion to be only a mental process of the observer—if the observer forgets those relations between the wave functions φ and ψ, which make them able to interfere with each other, then in the mind of *this* observer the "pure case" is turned into a "mixture." We have here a case of what Margenau called the "subjective view"; and we have even an extreme radical case of this view—the state, represented by the wave function, expressing indeed the *actual* knowledge of the observer, and not his *potential* knowledge. Neumann's discussion surely has shown this idea to be consistent in itself, and probably it will not be possible today to show it to be incorrect. But I should like to try another way out of the difficulties. According to v. Neumann a linearly polarised photon would become a photon without polarisation (described not by a single electromagnetic wave as its wave function, but by an unpolarised beam) by the mental process that the observer forgets its state of polarisation. Perhaps such a conclusion cannot be avoided; but let us *try* to avoid it, and to give a more objective meaning to the notion of states (or wave functions).

Chr. v. Weizsäcker in a lecture proposed an idea closely related to v. Neumann's, and also opposite to my view sketched above. He believes that a specific process of *perception* (not to be described by concepts known today) is involved in the "decision" of the photon between its two possibilities; and this process cannot be identified with any purely physical process. To show the difference between his opinion and my own I should like briefly to recapitulate what I said above, in a more general form.

In more orthodox formulations of quantum mechanics one is accustomed to say that the process of *observation* (or *measurement*) makes the photon decide between the two possibilities—or makes any other observable take one of its different eigenvalues. But I think that what is here called "observation," must not be interpreted as any mental process, but as a purely physical one; we may better call it, following Margenau (3), the *preparation of a state*, chosen from those which correspond to a certain operator or observable. The essential point seems to me to be that this process must be a *macrophysical* one. Macrophysics by definition deals with objects or processes which allow an application of the traditional concept of reality. It is essential that we may think of a macrophysical object as existing independently of any process of observation. Certainly we *know* of the planet Pluto only because we possess astronomical observatories; but we believe Pluto to have existed already in the time of homo neandertalensis. This is what we call, in the German literature, "*Objektivierung*," to think of objects as existing independently of the processes of observation. Or to put it otherwise: It belongs to the definition of macrophysics that we are here never faced with the characteristic microphysical features of complementarity.

Now we have indeed in each case of microphysical observation and measurement a situation in which the microphysical object of observation makes a *track* of macrophysical dimensions. Usually this is made possible by an *avalanche process* set off by the microphysical object of observation. To induce this track (giving a macrophysical *record* of the microphysical decision), is—I think—in some cases *identical* with the decision itself.[1]

[1] On another occasion, I have expressed this point of view as follows: „Keineswegs handelt es sich darum, dass unser wahrnehmendes *Bewusstsein* als solches zum Mitsplieler der physikalischen Vorgänge wird. Sondern der Begriff der „Beobachtung," wie er hier gemeint ist, zielt darauf ab, dass ein makrophysikalisches *Dokument* geschaffen, eine makrophysikalische *Spur* erzeugt wird, welche das Beobachtungsergebnis in der objek-

This opinion is clearly opposite to v. Neumann's; and I believe that v. Weizsäcker regards his own opinion, mentioned above, as irreconcilable with mine. But perhaps one must not take the ideas of v. Neumann and v. Weizsäcker as precisely identical: v. Neumann has put forward a very clear and definite meaning, but v. Weizsäcker's thesis seems to me to be a little vague—probably on purpose. Perhaps it would be possible to reconcile my opinion with his, by saying that creation of a macrophysical track or document is a special case of what v. Weizsäcker calls "perception," something which perhaps may be conceived also to occur in other forms, unknown to us hitherto, and of a more "psychic" character. Only v. Neumann's position is clear enough to allow opposition and criticism; though I do not believe that I can disprove it, I hope to show another position to be possible also.

Let us first consider what might appear to be a difficulty. A silver grain in a photographic plate—or any other object suited to allow a macrophysical track to be produced by a microphysical decision—is nothing other than an accumulation of microphysical individuals. If we try to give a *complete* description of the silver grain, then we have to mention its atoms and their wave functions—and we are faced again with those difficulties which we tried to avoid by emphasising the macrophysical character of the silver grain.

This leads us to acknowledge that it is both possible and necessary to formulate a physical axiom not formulated hitherto. Above we held it to be part of the definition of macrophysics, to show no complications in the manner of complementarity, but to allow a complete "objectivation" of phenomena in space and time. But usually one defines macrophysics only by stating that it deals with great numbers of microphysical individuals—and this is another and a different definition. We need therefore a special axiom to express the empirical fact that these two definitions define the same thing—that really each large accumulation of microphysical individuals always shows a well defined state in space and time—that a stone never, unlike an electron, has indeterminate coordinates. One often vaguely believes this to be guaranteed already by Heisenberg's $\Delta p \cdot \Delta q > h$; but in fact this relation only provides a possibility and not a necessity for the validity of our axiom. Let us assume that, in our experiment involving the photon, the photographic plate be removed, but that we have an arrangement whereby a macrophysical stone will fall according to the decision of the photon. Then, if

tiven Wirklichkeit aktenkundig macht. Statt eines menschlichen Auges denken wir uns besser eine Photoplatte als „wahrnehmendes" Organ. Ein Lichtquant, welches ein Silberkorn in der Photoplatte entwickelbar gemacht, oder welches durch Ionisierung eines Moleküls die Bildung eines Nebeltröpfchens in der Wilsonkammer eingeleitet, oder welches endlich in einem Zellkern eine Mutation zustande gebracht oder ein Bakterium getötet hat—dies Lichtquant *hat sich lokalisiert*, hat einen bestimmten Ort angenommen, unter Verzicht auf seine komplementäre Erscheinungsmöglichkeit als räumlich ausgedehnter Wellenzug. Das Entscheidende ist also nicht ein Vorgang in unserem menschlichen Bewusstsein, sondern vielmehr das Zustandekommen eines makrophysikalischen Tatbestandes (als Spur und Dokument der Beobachtungstatsache „Lokalisierung des Lichtquants an bestimmten Ort"), welcher *seinerseits* (weil er ein makrophysikalischer Vorgang ist) die volle „*Objektivierung*" gestattet, d.h. als nunmehr etwas unabhängig und unbeeinflusst von weiteren Beobachtungsvorgängen Daseiendes gedacht werden darf."

we strictly assume v. Neumann's view, the stone comes to possess a wave function which makes it undecided whether it does fall or does not, and an observer has the opportunity to compel the stone to a decision by the mental process of forgetting that interference between the two wave functions of the falling stone would be possible. Schrödinger's famous cat is another illustration of this point. I think we can summarize the situation by saying that indeed a new feature—to be formulated by a new axiom—lies in the fact that such things do not happen; all formulations of quantum mechanics hitherto given do not suffice to exclude them. We are unable to make a clock with a hand which does not always point to a definite figure on the dial. This is a well known fact, but a fact of which present theory gives no sufficient account.

It seems possible to give a still more precise meaning to our new axiom. Let us look at a special case. The emission of an alpha particle by a nucleus (this nucleus may be assumed to be infinitely heavy and to be located at a definite point) is regulated by a spherical wave. Now it is doubtless possible that by some suitable arrangement we could cause interference between alpha emissions in widely different directions, as in the case of photon emission by an atom. But if we let this emission take place in a Wilson-chamber, we always get the picture of a Wilson track showing the particle to have taken a well defined direction. Why is that?

One will scarcely doubt that the thermal motion of the gas molecules must play a decisive role in this instance. I will not discuss here the application of v. Neumann's and v. Weizsäcker's ideas to this case. My own opinion is this. We have to see the cause of the phenomenon not in any "perception," nor any mental process, nor in the fact that drops of water are formed—for surely in the absence of water (though then any direct observation would be difficult) the particle would have a definite direction of emission and we would have tracks of ionisation in the gas. The decisive point seems to be that in consequence of the gas temperature all possibilities of interference between wave functions of different atoms are destroyed. For if we were to fill the chamber not with ordinary gas but with liquid helium at the temperature $T = O$, I do not see why interference of alpha emission over wide angles should not remain possible.

Returning again to our photon, we may say that the Nicoll itself would be able to make the two waves φ and ψ incoherent, provided the Nicoll had a sufficient degree of Brownian movement. Generally we can regard Brownian movement as that factor which is suited to create incoherence and to destroy every possibility of interference. If this idea is correct, then we see that thermodynamics is involved in quantum mechanical observation; and this is in harmony with a fact showing irreversibility to be connected with observation: We draw from an observation consequences about the probabilities of experiments to be made *afterwards*; we cannot reverse this relation.

But while thermodynamics is essential for the concept of observation and measurement, this concept itself seems to me to be indispensable in thermodynamics and in the notion of entropy. The relation of thermodynamics and quantum mechanics—especially thermodynamical statistics and quantum mechanics—has been the object of much discussion. Let us mention here only the first and the last stages of the subject.

1) Pauli (5) emphasised that even in quantum theory there remains the necessity of an "hypothesis of elementary disorder," which has to be acknowledged as an additional axiom besides the "pure" quantum mechanics as formulated by the Schrödinger equation. Our macrophysical axiom mentioned above stands in close connection with this axiom of elementary disorder, governing each thermodynamic system; indeed, we may also say each macrophysical system.

2) During the last years Born (1) and Green, in a series of papers, developed a fascinating account of thermodynamical statistics based upon quantum mechanics. Those results of their endeavour which are related intimately to our question here may be formulated in two theses:

A) Quantum mechanics in its full content implies irreversibility as a necessary consequence.

B) But "pure" or "restricted" quantum mechanics, which applies only the Schrödinger equation without the concepts of preparation of states, observation, measurement or "decision", would not do so.

Point A) has been emphasised by Born himself. Point B) requires some comment in order to show that it is really in accord with Born's statement and not in any contradiction with it. Born's exposition allows us to see with great clarity where the concept of "decision" comes to play its role: The notion of *transition probabilities* is used—they are given by his formula [23], (1) which is derived from [21]. This is exactly the point in which we are interested here: It was the whole purpose of our discussion to show the inadequacy of the statement that the intensities of the photon waves φ and ψ are probabilities (of transition or of decision—this is only a verbal difference), and to look for the physical process which makes these waves incoherent. Now we meet exactly the same problem: From Born's formula [21]:

$$J(\lambda,\lambda') = \frac{4\pi^2}{h^2}\left|\int_0^t V(t;\lambda,\lambda')e^{(2\pi i/h)(E-E')t}\,dt\right|^2$$

we proceed to his formula [23]:

$$J(\lambda,\lambda') = t\cdot\frac{2\pi}{h}\left|V(\lambda,\lambda')\right|^2\delta(E-E');$$

we can do so by a well known mathematical process, making use of the assumption that we have so many energy values E that they form practically a continuous spectrum. But the physical justification of this mathematical process lies in the same axiom which Pauli called the quantum theoretical form of the hypothesis of elementary disorder. Or to put it in other words: this mathematical process contains the supposition that the different waves involved here can be treated as *incoherent* ones.

Still another way of expressing the view presented here is the statement that quantum mechanics in its full content, *including* the concept of transition probabilities and their normal quantum mechanical application, is indeed a *sufficient* basis for formulating entropy and thermodynamics. Therefore our thesis B), formulated here in order to throw some light upon the questions discussed in this article, is indeed in full agreement with Born's thesis A). And naturally Born's treatment of the problem—establishing [23] as a consequence of [21]—is fully adequate and correct; furthermore Dirac's (2) famous investigation of the quantum mechanical justification of Einstein's laws of emission and absorption constitutes a simple special case of the more general problem treated by Born. We wish here only to point to the details of the logical background of this treatment.

To do so, let us look at the fact that we do not learn exactly from this treatment *which* of the oscillators of the radiation field gains the photon emitted by Dirac's radiating atom. We learn only that a photon is created having a certain energy $h\nu$; but the necessary assumption of infinitely many oscillators in the radiation field—necessary to allow the mathematical limiting process used for [23]—is applied in such a manner as to exclude any identification of the created photon with the quantum number of an exactly known oscillator of the radiation field. Therefore it is essential and characteristic for this treatment that it gives us a well defined wave function for the resulting atom, but not for the resulting state of the radiation field—here we must be content with such information as can be given by the approximation of a continuous manifold of oscillators. This restriction—which does not give me the impression of being a process in the mind of the observer—*makes it impossible that any interference between the two states after and before the quantum jump can occur;* and this again is a necessary condition for the concept of quantum jumps itself to be meaningful. The fact that here a new axiom or a new physical supposition—not already contained in the Schrödinger equation—is involved, is shown by the possibility of really different cases of interaction between an atom and the radiation field: If we desire to describe the selective reflection of light at the surface of Hg vapour (*Wood's* experiment), we meet with another and a totally different situation: The atom does *not* come to a decision between the two stationary states involved.

Summarizing our results, we gain, I believe, a rather consistent and clear picture of the problem of measurement and I hope one can accept it as no less convincing than v. Neumann's picture which it has been our aim to avoid, although one cannot say that there are absolutely convincing objections against it. Since v. Neumann has given a detailed account of his idea, we shall follow him, developing our own picture in a manner parallel to his.

Central to v. Neumann's more detailed discussion is Szilard's idea of a phenomenological thermodynamics which does not neglect fluctuations and Brownian movement. This idea, developed in two admirable papers (6), (7), stands rather isolated apart from the flow of modern physical ideas; but I am inclined to regard it as one of the greatest achievements of modern theoretical physics, and believe that we are still very far from being able to evaluate all its consequences. The present discussion of these issues is very incomplete, and I refer the reader to *Szilard's* own writings for more careful exposition.

It is generally thought that the concepts of thermodynamics are strictly macrophysical ones—that a single atom never has a temperature, and never contains a definite amount of entropy. But the tendency of *Szilard's* views is to acknowledge also a microphysical applicability of thermodynamics. Let us again think of our photon—it may now be in such a state as can be represented by an unpolarised beam: The two wave functions φ and ψ are incoherent and of the same intensity. According to *Szilard* this means that the photon possesses the entropy $k\ln 2$, where k is the Boltzmann constant. The justification of this statement has been discussed by *Sziland* and by v. Neumann so thoroughly that we may dispense with further comment here.

According to v. Neumann this entropy is the result of a mental process of the observer: By forgetting the coherence between φ and ψ (as we put it above), *he* creates the entropy $k\ln 2$. Therefore for another observer, who did not forget, the photon has the entropy 0; the notion of entropy becomes a relative one, different for different observers or different memories. In my opinion—assuming the conversion of a polarised photon into an unpolarised one to be a real physical process—this entropy too has an objective meaning, independently of the mental processes of any observer. Therefore we are forced to acknowledge a new, hitherto not recognised type of physical uncertainty.

Let us consider now an atom in a box of volume V. We know from quantum mechanics that the atom does not possess definite space coördinates—because its momentum has a well defined value. But independently of this fact there must exist another kind of deviation from classical concepts if our discussion above is not entirely false. The atom may be subject to Brownian movement. If we wish to observe whether it is contained in the part V_1 of the volume V, and if $V_1 = V/2$, then we have to make an experiment by which the entropy $k\ln 2$ is taken away from the atom. The process has been discussed in detail by Szilard and Neumann; we may for instance obtain a compression in which V is reduced to V_1, after the usual manner of thermodynamic processes; we have then—under strictly controlled conditions—a guarantee that the atom is located in V_1. This process is reversible; for by extending its Brownian movement over the whole of V, the atom again gains the entropy $k\ln 2$.

We are not forced to see here a novel physical phenomenon if we take the attitude of v. Neumann: According to him the entropy $k\ln 2$ is an equivalent of the fact that I (the observer) do not *know* whether the atom is in V_1 or in $V - V_1$. But assuming this amount of entropy to have physical reality, independent of the gaps of my or your knowledge and expressing the maximum of knowledge (of *any* observer) *compatible with the physical situation*, we are forced to regard this entropy as the description of a state which by a kind of "thermodynamic complementarity" renders the spatial coordinates of the atom undefined, independently of wave mechanical complementarity, which is another thing.

Perhaps this conclusion makes our argumentation rather suspicious; perhaps Neumann's view should indeed be preferred at this point. But to make the argument complete let us look once more at the problem of the polarised photon. Our assumption that the unpolarised photon is really different from each polarised one—even if an observer has forgotten the polarisation—is exactly analogous to the assumption that the atom in thermal motion may really be not in V_1 and not in $V - V_1$, but is thermodynamically distributed over the whole volume V. It also implies that this state of affairs is different from a state in which the atom is really in one of the Volumes V_1 and $V - V_1$, but that we have forgotten it.

Now such consequences cannot be avoided if we see the process of observation and measurement as we tried to do above. If we assume the exposed silver grain to be indeed in a state of well defined decision as to its developability, then we must conclude that it is not merely *voluntary* resignation on our part if we do not describe the silver grain in terms of wave functions of its single atoms. Doing so would entangle us in contradictions, as we have already seen above. Therefore the physical situation itself must contain guarantees that such contradictions cannot take place—and only a second type of complementarity can give this guarantee. There must exist in the silver grain a certain situation by which its description in terms of atomic wave functions is made impossible—only in this manner can the grain function as it does. This decisive point also plays its role very clearly in Margenau's discussion; for Margenau, in calling attention to "measurements annulling a system," treats the absorption of a photon as a process which is *not* to be analysed in terms of wave functions and Schrödinger equations of the photon and of the absorbing atoms. Therefore I conclude—and in this respect my view differs somewhat from Margenau's to which it corresponds closely in many points—that the notion of "decision," "quantum jump" or some other concept *not contained in the Schrödinger equation* is indeed necessary and unavoidable.

It is then apparent that the situation—though it is clear to a certain extent—does not allow a complete and final analysis; there remain open certain questions. For one cannot avoid the difficulties merely by describing the silver grain (or an analogous part of any observational instrument) as a "mixture" of the form (1); this would not help us much, for it cannot describe an increase of entropy any better than the Schrödinger equation of a "pure case." It seems to me that entirely new conceptions are necessary.

Surely the basic discussions here presented are superfluous in respect to all applications of quantum mechanics hitherto made. In all these cases it is possible to make a clear distinction between the formalism of quantum theory—Schrödinger equation and so on—and its "interpretation" in terms of transition probabilities. Perhaps this distinction (by *Heisenberg's* so-called "*Schnitt*") is an element of the theory which cannot be disregarded, and which in *all* occurring problems may take the same form. But I do not see why we should be convinced of that. This simple separation in the application of the notions of waves and of probabilities may be possible only in a restricted field of problems (not including, perhaps, biology; and perhaps not even including some phenomena of protein molecules); perhaps the real problem is to synthesize the two fundamental notions of quantum mechanics, and to unite quantum mechanics still more intimately with thermodynamics. Unable to do so myself, I should like to emphasize the urgency of further thought upon these questions.

The best description of this state of affairs which can be given by present theory is to be found in the mathematical process applied by Dirac and Born. Here we see a definite supposition operating besides the Schrödinger equation; a certain type of "complementary" assumed—but not the well known complementarity of Bohr and Heisenberg, which is based on definite wave functions; on the contrary we meet with a certain limitation of the applicability of wave functions (as was shown above, the resulting state of the radiation field does not possess an exactly defined wave function able to interfere with its wave function *before* the act of emission). This limitation, destroying interference, allows us to apply the concept of quantum jumps. So far as it goes this treatment is undoubtedly a correct and a logically consistent one; and naturally also all cases such as the selective reflection mentioned above can be dealt with by this method, in a suitable way. Nevertheless the impression remains strong that perhaps this is a point where in the future some generalisation of the present theory might start.

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