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The Theory of Measurement in Quantum Mechanics

GORDON REECE

Imperial College, London, S.W.7

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INTRODUCTION

The very impetus for the formulation of quantum mechanics in its modern (post-1926) form came from a reconsideration of the measuring process in the quantum context.

If there is one feature which distinguishes quantum phenomena from classical ones (we shall define our terms later, in Part I) it is the 'reduction of the wave-packet' that occurs upon observation. Where, when and how (or indeed whether) this reduction occurs will form the subject of our inquiry.

Our programme will be as follows; Part I will be devoted to establishing the definitions we shall require, in particular of what is meant by a measurement. We shall also examine the means whereby a measurement is effected, as such an examination is generally accepted to be inseparable from the definition of measurement. We shall then use these definitions to proceed to a more precise formulation of the 'measurement problem' than is normally given in treatises on quantum mechanics.

In Part II we shall outline the various attempts made within the statistical framework to give a coherent solution to the problem. Here, as in Part III the solutions will be given in chronological order, so that the overall presentation will be an historical one. We shall naturally follow each solution through to the present day, so that each Part will be, in this sense, complete in itself.

Part III will describe those solutions which have looked beyond the statistical viewpoint. To what extent these are solutions, and to what extent we can expect a solution to emerge along such lines will form the main point of our discussion.

We have adopted the common terminology of classifying the solutions of Part II as the 'subjectivistic' approach, while granting the attempts described in Part III the title of 'objectivistic'. We do not like this categorisation and adopt it 'without prejudice'.

Part IV is an *annotated* Bibliography. It includes, in addition to those works explicitly referred to in the body of the text (Parts I, II, III), all the relevant literature on the subject. We have felt this to be a better way of presenting a broad picture of the work in our field without overloading the main part of the text.

Though this was intended to be a mathematically-oriented treatment of the problem, it has been necessary for us to dwell from time to time on the purely epistemological aspects of the solutions that have been proposed. This may be unfortunate, but it is necessary as the only reason for rejecting most—or all—of the solutions or for accepting any as more than purely semantic evasions of the problem rather than as solutions of it must be epistemological.

* * *

PART I: THE PROBLEM

Si parva licet componere magnis
(If we may measure small things by great ones)

Virgil, *Georgics* (IV)

Definitions of Measurement

1. *The Classical Case*

It is commonly stated in the literature of quantum mechanics that the essential feature of classical physics is that measurements are in principle exact, and that the extent to which they are not exact represents a feature of the experiment under consideration that could be eliminated if we had the time or the patience. There is to our knowledge no authority for this statement that is based on the literature of classical mechanics itself. It is clear that the essentially 'classical' nature of classical physics emerged from the reconsideration of that discipline in the light of quantum experiments. There is a serious danger in defining classical physics in this way. There is for example nothing to prevent constant redefinition of what we mean by classical physics as new problems arise in quantum theory. Margenau (37, p. 40) has characterised this with a complaint that modern writers tend to equate 'classical' with 'bad'.

There is also an unfortunate tendency to confuse the words 'classical' and 'macroscopic'. This is particularly important in the present context, as we shall rely on an intuitive understanding of what is 'classical', whereas there is no theoretical reason why we should not treat all 'macroscopic' problems as quantum phenomena. It is for us to decide in each case whether it is of any importance that there is, for example, a non-zero but admittedly very small probability that we shall find ourselves in outer space tomorrow morning. It is difficult to see how anything but common-sense can save us from considerations of this kind.

We shall rely on the following definition: Classical physics is a well-known set of rules for solving problems in physics; it gives its answers in the form of exact numbers. This leaves aside the question of what relation we consider exists between the exact answers given by the rules of classical physics and the actual values (if any) of the quantities under consideration.

2. *The Quantum Case*

In the early literature there is very little attempt to define what is meant by the term 'measurement' in the quantum context. Heisenberg, Bohr (2, 4) and other early authors write of measurement as though there were no need to specify precisely the meaning of the term. It should be noted that they all describe particular examples of what they mean by a quantum measurement (Heisenberg's microscope in particular, which is referred to also by Bohr (4)). No writer before von Neumann was prepared to generalise. The impression one gains from reading the literature of the late '20s is that the full implications of the final departure from classical physics—which it should be emphasised took place in 1925, not in 1900—had not sunk in. Bohr and Heisenberg both appear to be relying on an intuitive understanding of the term 'measurement'; that this understanding was not intuitive nor even present is indicated by the care with which later writers (see below) defined their terminology.

Even von Neumann did not actually define what he meant by a measurement, though his treatment of the process was so detailed that it is unfair to demand what might have proved to be a facile definition. Instead of this we are given a breakdown of the nature of the process on the basis of the (unstated) assumption that it involves a macroscopic record of a microscopic event. This may be obvious but it should be recognised as essential to the process.

Shimony, writing in 1963 (90) said:

> 'The founders of quantum theory, particularly Heisenberg and Bohr, discussed with much subtlety the epistemological problem of relating atomic physics to human experience; but except for a few remarks, usually tentative and oblique, they do not consider the ontological problem of mind and reality.'

We feel this criticism to be justified. Without an understanding of what is meant by measurement and where it occurs (does the measurement consist of taking a reading or of registering it in the consciousness of the observer? —we ask the question in the confidence that we shall be able to provide the answer by drawing on later literature) we cannot expect to gain any meaningful insight into the process itself. Von Neumann may not have defined his terms directly but he examined the implications of his stated and unstated assumptions so carefully that we should be reluctant to criticise his approach on these grounds alone.

Writing in 1935, Schroedinger gave the first definition of what was meant by a 'measurement':

> 'The systematically induced interaction between two systems (object and measuring instrument) is called a measurement of the first system if a

directly observable variable of the second (position of a pointer) is always reproduced within certain limits of accuracy on immediate repetition of the process (on the same object, which may be presumed to have been subjected to no other influence in the meantime).'

(Translated from p. 824 of (16).)

This insistence on immediate repeatability is crucial. Though Schroedinger admits in the next line that this is 'not a perfect definition' it is perhaps relevant to observe that if it is enforced it precludes any measurement—if we are allowed to call it such—that destroys the property in question. There is no suggestion of a qualification such as 'in principle': Schroedinger is too honest for that. By 1935 it had been recognised that things that were possible only 'in principle' were potential sources of serious conflict between theory and practice. To put the restriction into practical terms: it is extremely difficult to see how a Stern-Gerlach experiment could ever be repeated. The errors involved in an approximate repetition are *not* errors of observation but may well be intrinsic 'unpredictables' governed by the theory itself.

Schroedinger's path can lead either of two ways. If unrepeatable experiments are to be excluded, then we must turn either to a reformulation of the probabilistic viewpoint, such as (see (41)) Schroedinger himself has since appeared to favour *or* to a theory which reduces all experiments (and hence all mechanics) to a structure based on such experiments. This latter course was indeed taken by Birkhoff and von Neumann, without explicit details of the reduction (18). The details are given by Jauch, writing much later (1968) (121), and thus drawing on the work done since 1936.

Jauch distinguishes (121) between two kinds of measurement. A measurement 'of the first kind' is one that can be repeated without the risk of obtaining no answer or of obtaining a different one. This is the logical corollary of Schroedinger's (16) definition. Jauch puts it thus: 'Suppose we repeated the measurement immediately after it has occurred . . . then we would with *certainty* observe the particle inside the volume of the counter.'

A measurement 'of the second kind' is one that changes the property measured so that repetition would necessarily give a (statistically) different answer. It is of course possible that by sheer chance the particle happens to have the same momentum again at the instant of the second measurement. That is why we have insisted on the 'statistical' nature of the difference.

Schroedinger did not accept the need for the second kind of measurement. Jauch asserts that the first kind is typical and restricts his discussion to this type. We are inclined to doubt whether this reduction is either generally possible or even theoretically permissible in the way stated. If there is any meaning in the distinction it must be that the first type of experiment is, on repetition, dispersion-free, and the second is not. Jauch (121) tells us (p. 114) that there are in fact no dispersion-free states in quantum mechanics. All *quantum* measurements are 'disturbing' and it is thus, of course, a *classical* feature of the classical measuring apparatus that Jauch is describing, not a property of the observed system itself.

The first explicit description of measurement and measuring apparatus, which drew its inspiration from the critique implied by Einstein *et al.* (15), was made by Elsasser (23):

> 'A measurement constitutes the limiting case in which inductive inference concerning the microscopic features tends towards certainty.'

'Certainty' here means, as is clear from the context, what we have defined above as 'classical' certainty. Elsasser pointedly contrasts the quantum situation with the 'classical' one where the 'microscopic features of a system are determined in function of its microscopic ones'.

But it was left to Margenau, in 1937, in the course of a beautiful paper (25) to make the criticism that we have suggested and to put the matter right. We quote:

> 'The usual way to theorize about measurements is to select, for no obvious reason at all, a specific type of experiment, analyze it very fully, and then generalize the *results* without inquiring very much whether these results fit the multitude of other measurements that might be considered. The experiments chosen are mostly imaginary ones, a feature which of course detracts in no way from the value of the example as long as the imaginary procedures are permitted by known physical laws. Heisenberg's experiment for determining the position or the momentum of a particle by means of a γ-ray microscope, and Bohr's slit experiment have perhaps borne the brunt of this procedure. Not as common but yet quite popular, particularly among mathematicians, is a mode of inquiry which starts with quantum mechanical theory, considers what ideally a measurement ought to be, and then manages to find one which fits the prescription. In studying (10) J. von Neumann's excellent book on the foundations of quantum mechanics we have had the impression that this method was being employed. What makes the method seemingly successful is the wealth of existing types of measurement; it insures that any prescription can be filled.'

And later, Margenau answers his implied question:

> 'A measurement is any physical operation by means of which the numerical value of a physical quantity can be determined.'

And:

> 'A measurement usually consists of an operation plus an observation, but this need not be the case.'

We disagree with this last statement, which seems to conflict with Margenau's own careful distinction later in the same paper between measurements and the *preparation of states.* His example of these is as follows:

> 'When electrons are made to pass into a magnetic field, a new state with respect to electron spin has been produced, but the spin has not been measured.'

We feel the contradiction to be obvious.

Kemble (24) in 1937 insisted, rather oddly, that the criterion for a satisfactory quantum method of measurement should be, for example, that 'it shall be in principle capable of yielding results of arbitrary precision independent of the limitations due to the Heisenberg uncertainty principle'. Though the key words are, of course, 'in principle' we take exception to the misuse of the word 'arbitrary' even if we accept what is implied in the statement, which we do not. This is, in any case, a very roundabout way of requiring that the measuring apparatus be essentially 'classical'.

The first attempt to lay down a clear definition of *where* and *when* a measurement took place (as opposed to defining its nature, dealt with by Margenau) was made by London & Bauer (26) in 1939:

> 'Mais un couplage, même avec un appareil de mesure, n'est pas encore une mesure. Celle-ci est achievée seulement lorsqu'on a *observé* la position de l'aiguille. C'est précisément cet enrichissement de connaissance, acquis en vertu de l'observation, qui donne à l'observateur de droit de choisir entre les différentes composantes du mélange prévues par la théorie ...'

Thus we see that it was only in response to the questions raised by Einstein *et al.* (15) that a really searching enquiry was made into the meaning of terms that had consciously or unconsciously been carried over from classical physics into quantum mechanics, and which had not in fact preserved their classical meaning.

The Measurement Problem

There is no equivalent in classical physics to the 'measurement problem' in quantum mechanics. The problem arises in the following way. A system S is in a state ϕ, happily evolving according to the time-dependent Schroedinger equation. We decide (leaving at least one major question—that of free will—aside for the moment) to measure an attribute (say position) of the system. Once we measure the attribute we can say that we have measured it and obtained the result x. We may not say that the system *was* in the state x before the measurement, merely that we have found it to be so. Thus, on the one hand, we can merely predict that at time t the system will have some non-zero probability of being in the state x, but on the other hand after the measurement has been made we have, within the limitations of experimental error, found it actually to be in the state x.

How, then, does this situation differ in principle from the following (classical) problem?—If I throw a die in the air I know that there is a probability of 1/6 that I shall obtain a six. The difference lies in the fact that if I were so minded I could rig up elaborate apparatus to calculate according to the laws of classical mechanics precisely how the die would land once I knew the orientation, velocity, spin and other relevant 'classical' variables. So, subject to the proviso that errors of experiment are (*equally*) allowable in classical (and quantal) calculations, I must admit that I was deluding myself in saying that all I knew was that the probability of throwing a six was 1/6. In this case, we say that we are not in possession of maximum information.

The essential difference between this situation and its quantum counterpart is that a knowledge of the probability of finding the system in state x constitutes according to the Copenhagen interpretation of quantum mechanics precisely the maximal information that we seek. Whether or not this is an acceptable situation, it is the view of the currently favoured interpretation of quantum mechanics. We shall deal with the objections to it, the alternatives, and what we consider to be the reasons for being satisfied with this essentially probabilistic view in Part III.

So, on the one hand there is always in classical statistical theory the implied possibility of improving our knowledge and going from the discontinuous probabilistic view (in our example 1/6 becomes instantaneously either 0 or 1 depending on whether we get a six or not) merely by working harder at the problem and as we gathered more and more information showing that the probability would converge either to 0 or to 1, while on the other hand we are faced with the (quantum) situation that we cannot in principle know more than the probability, which will rarely be either 0 or 1, before the state assumes *instantaneously* one or other of (say) two alternatives with certainty.

To anticipate some of the points that we shall raise in connection with the attempts to replace the probabilistic theory with a deterministic one, we must add that we consider that there are no truly classical situations of the kind described above. That is to say, there are no immediately obvious cases of maximal information in the classical case and therefore there is no reason to suppose that any could be realised in practice. To deal with a commonly quoted example, Schroedinger gives the case ('ein burlesker Fall') ((16), p. 812) of a cat in a torture chamber. The whole paradox hinges on the statement, made in apparent innocence, that it must needs be entirely obvious that the cat is either dead or alive, on examination, at any given time, and not, say 80% dead and 20% alive. 'Dead' and 'alive' are, of course, the two eigenstates of what we may call the cat's existence-function. The paradox occurs as a result of the ingenious transfer of the superposition principle from the quantum level of the murderous machine to the classical level of the cat's existence function. While we realise that we are running the danger of confusing errors of observation with errors inherent in the situation and dictated by the laws governing it, (they look the same, of course) we are prepared to maintain that it is far from obvious how to determine, for example, whether a cat is dead or alive. We do not even have a generally accepted medical definition of death. The proponent of the paradox may reply that he can construct another, more obvious, example. Let him, and we shall try to dispose of his assumptions in the same way.

It is our view that the paradox may well crumble once we accept that there are virtually no experiments the outcome of which can be predicted with absolute certainty, even in principle. We can get very close to certainty, perhaps, but that is not the same thing at all. In the circumstances, we feel that it is hardly surprising that when the conditions of a classical observation are subjected to the rigorous scrutiny of quantum analysis it transpires that certainty is unattainable except as a special case of probability.

The proof that quantum mechanics could not be reduced to a classical stochastic theory 'involving a probability distribution function of position and momentum' was given by Cohen (107).

If we are to be consistent, we must return to our example of the die. If the 'quantum situation' obtains here too, we must allow of the possibility that the odds would converge to a value other than 0 or 1. We should ourselves rate this much higher than a possibility.

Summary

As is clear from the foregoing consideration, the problem of measurement does not arise solely in connection with the simultaneous determination of incompatible variables, but arises with the careful examination of any single quantum measurement.

The problem is a serious one: so serious that extraordinary solutions may have to be considered, all the obvious ones having been tried and found wanting. No-one should be surprised if quantum physics has to follow the Hippocratic dictum:

> 'Extreme remedies are the most appropriate for extreme diseases. (*Aphorisms*, I. vi.)

* * *

PART II: THE SUBJECTIVISTS

> 'But to us, probability is the very guide of life.'
>
> Bishop Joseph Butler (1692–1752)

Chapter 1

We shall now embark on a description of the 'orthodox' approach to quantum mechanical measurement. By this we mean any approach to the theory of measurement that is based on an acceptance of the probabilistic nature of quantum phenomena. We shall leave the description of any criticism of these approaches that disputes the essentially statistical character of the theory until Part III, where these alternative formulations will be considered.

Von Neumann

The first coherent attempt to interpret what happened when measurements were made on quantum systems was that of von Neumann (10). Whatever may be said about von Neumann's work with the benefit of thirty-seven years' hindsight, his work is still unrivalled for its thoroughness and the perceptiveness of its approach. Gaps there may be, looseness of logical argumentation there is, but the fact remains that von Neumann's work is the fountainhead of all subsequent writing on the subject. Not a paper has been published on measurement that fails to give due acknowledgement to von Neumann's work, whether it is used as a supporting reference or as the butt of the writer's criticism. It is one of the true masterpieces of our time. The fact that it was written only six years after the Heisenberg-Schroedinger reformulation of quantum mechanics must rank as one of the miracles of modern mathematical physics.

That minor modifications have been made since 1932 to von Neumann's approach does not detract from its fundamental validity: assuming only that one accepts the 'probabilistic premiss' as either true or at least self-consistent. We should go so far as to say that von Neumann proved the only two results of unquestionable relevance to our problem: the impossibility of hidden variables within the system given certain assumptions and the irrelevance of the point at which the 'cut' is made between observer and observed. This latter has been picturesquely compared to the problem of whether one's spectacles are part of what one is looking with (one's eyes) or what one is looking at.† Von Neumann showed that it did not matter.

Von Neumann's Theory of Measurement

It would be impossible to describe in these pages the whole of von Neumann's theory: what we shall give must needs be a crude caricature of it.

To measure the quantity A in the system σ, we represent the quantity A by an Hermitian operator A in the Hilbert space associated with the system σ. The axioms of Hilbert space are well known and are given by von Neumann in Chapter II. The operator A will have (and here we must qualify our acceptance of von Neumann's analysis) a pure discrete spectrum of eigenvalues. Von Neumann asserts that these eigenvalues represent the values that the quantity A can take upon a measurement: he writes 'a measurement of A then has the consequence of changing each state $|\psi\rangle$ into one of the states $|\psi_1\rangle, |\psi_2\rangle, \ldots$ * which are connected, with the respective results of measurement $\lambda_1, \lambda_2, \lambda_3, \ldots$ (the eigenvalues). The 'kets'* are orthonormal.

The assertion quoted is the 'projection postulate'. Its necessity has been

† Due to Dr. G. Fay.

queried, as has its physical plausibility. These doubts are dealt with on (pp. 92–96).

The process of measurement is characterised as follows. If we have a measuring apparatus m (a system), the system to be measured and the measuring system interact. Let the state of m be given by the ket $|\psi_{(t)}\rangle$ in the Hilbert space $\mathscr{H}^m$ associated with m. The state of the combined system $\sigma + m$ is given by

$$\sum_n c_n|\phi_n\rangle \otimes |\psi_{(t)}\rangle$$

which is a vector in the product-space $\mathscr{H}^\sigma \otimes \mathscr{H}^m$. It has been discussed (55) whether either or both of the states may be mixtures, rather than pure.

Von Neumann defines two types of process by which a system evolves:

$$\text{(1)} \qquad U \rightarrow U' = \sum_{n=1}^{\infty} (U\phi_n, \phi_n) P_{[\phi_n]}$$

$$\text{(2)} \qquad U \rightarrow U_t = e^{-(2\pi i/h)tH}\, U e^{(2\pi i/h)tH} \qquad \text{(p. 351)}$$

H is here the energy operator (Hamiltonian), t the time. H is independent of t. (1) and (2) are the two essential processes for the time-evolution of a system: (2) is continuous, while (1) is essentially discontinuous. P is a 'projection operator'.

(1) is statistical, while (2) is 'causal'. The transformation (2) is clearly unitary, since

$$U_t = B \cup B^{-1}$$

where B is a unitary operator as the energy operator is a real one (this point should, we feel, be made in the context of a free-ranging discussion that will take in for example probabilities p such that $0 \leqslant p \leqslant 1$ is not necessarily satisfied—see references (29) and (117)).

As Krips (123) has put it:

> 'At this point von Neumann digresses into philosophical considerations, with a consequent increase in the controversial content of his theory. He claims that the measurement process can only (sic) be considered complete at a time t'' when the state of $\sigma + m$ is represented by a density operator
>
> $$W^{\sigma+m}_{(t'')} = \sum_n |c_n|^2 |\phi_n\rangle\langle\phi_n| \otimes |\psi_n\rangle\langle\psi_n|.\text{'}$$

Here, Krips complains, von Neumann introduces a number of *ad hoc* postulates in order to ensure that he 'manages to satisfy the requirement of objectivity by including in the measurement process a reduction of the wave-packet performed by the observer'.

We have indicated that there is another, purely mathematical, objection to von Neumann's theory. There is an element of question-begging at the point where he dismisses the problem of the continuous spectrum (p. 220, English edition) by specifying in advance the 'measurement accuracy'. This gap has not been filled to our knowledge.

Von Neumann's Results

On the strength of the above assumptions, and of others stated and unstated, von Neumann derives a number of results. One of the most significant of these is that the introduction of *hidden variables* (see Part III) cannot add to our ability to understand quantum systems.

Von Neumann puts it thus (pp. 304–5)

> 'no further repetition of successive measurements can bring order into this confusion. In the atom we are at the boundary of the physical world, where each measurement is an interference of the same order of magnitude as the object measured, and therefore affects it basically.'

In other words, without directly assuming the absence of hidden variables, one can demonstrate that they have no role to play in the system (quantum mechanics) which is therefore an essentially statistical one. Von Neumann's argument for this has been criticised, perhaps most recently by Rosenfeld (103), on the grounds that it is a circular one. In some way, his critics maintain, the absence of hidden variables is implicit in the structure of the theory as given by von Neumann. The logical basis for a general proof of the impossibility of hidden variables is thus absent. All that von Neumann showed, and this much is generally accepted, was that *within the system* nothing could be gained by searching for hidden variables. Even if one accepts this point of view, it means that before quantum mechanics could be placed on the same footing as classical mechanics we should have to modify the system itself. Attempts to do so are described in Part III.

Von Neumann's greatest single contribution to the theory was his theory of the 'psycho-physical parallelism'. We have already alluded to this (p. 89). On this point there is no dispute as to the accuracy or relevance of his work. The result states that it is immaterial where we place the 'cut' in a measuring system.

Let I be the system observed, II the measuring apparatus and III the observer. Von Neumann shows that the same final result is obtained whether the measurement be taken to occur between I and II or between II and III. In the one case, rule (2) (p. 90) must be applied to I, while rule (1) is used on the interaction between I and (II and III). In the second case, (2) is applied to I and II, and (1) is used on the interaction between I and III.

When we recall how striking is the role of the measuring operation, it is remarkable that there is such arbitrariness in our assignment of the point at which it occurs. It should not surprise us that this is true in the classical analogy we gave. There are here not two different processes of the types (1) and (2), merely one set of laws for all measurements.

If we write the result in von Neumann's own notation:

$$\mathrm{I} \mid \mathrm{II}+\mathrm{III}$$
$$\mathrm{I}+\mathrm{II} \mid \mathrm{III}$$

we can see precisely what is meant by the 'shifting of the cut'. That there is something really extraordinary afoot is clear from the fact that II is (according to most authorities, especially Bohr) a macroscopic device. Indeed Bohr maintained that measurement *presupposed* the existence of classical systems. Now let us imagine a Heisenberg microscope (II), with a system (I) under it and our eyes (III) glued to the output. If the projection postulate is to be valid in the ordinary sense, the system is thrown into an eigenstate when it is observed: i.e. either at the input to II or at the output from it. But not both—surely not?

This apparent contradiction lends weight to the criticisms (given below) of the projection postulate. If the psycho-physical parallelism proof is, as we believe, valid, and the projection postulate is also true, then what is the state of the system (light rays) that is travelling through II? The fact that we can apparently nominate arbitrarily the state in question seems to us to be a striking example of free will—to say the least.

As the light inside the telescope does not have any observable properties until it has emerged, it seems far more likely that we have asked a seemingly well-formed question which, like any question concerning the simultaneous measurement of two incompatible variables, is in fact excluded by the restricted structure of quantum mechanics. It is of course very easy to formulate such questions—there is a non-denumerable infinity of them.

Thus we see that von Neumann provided mathematical solutions to problems that had puzzled physicists. In solving these problems, he raised even more fundamental questions about the nature of reality (see (15)) and the validity of quantum mechanics itself. The effect of his work cannot easily be overestimated.

Margenau's Critique (1935/6)

As a direct result of the paper by Einstein *et al.*, and in an attempt to answer it, Margenau proposed (21) to drop the 'projection postulate' of von Neumann (that a measurement necessarily produces an eigenstate of the observed system). His assertion is that the EPR paradox relies on the implicit assumption that this postulate is true. He asserts that the paradox disappears when the postulate is removed, and—most important—that the postulate has no real justification. This last criticism is expressed as follows:

> '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' (p. 240).

An equally serious criticism is that the projection postulate is contradictory to the 'more fundamental' postulate of the Schroedinger equation, in the way we have described in Part I.

Elsasser's Comments (1937)

By the time Elsasser wrote, it was possible for him (23) to make the statement that 'in quantum mechanics each measurement produces *some* perturbation' without attribution: the Copenhagen interpretation had really captured the thinking of physicists. Elsasser also wrote that:

> 'It might appear, especially from von Neumann's theory, that a quantum statistical mechanics can be built up that maintains in each step a strict analogy to the corresponding classical concepts.'

We do not feel this criticism to be justified except in the very literal sense. It conjures up the image of a reader of von Neumann turning the pages eagerly in search of an explanation of what ψ was, in classical terms. One would have to be very naïve to do so today, but maybe Elsasser's warning was more appropriate in 1937.

Reichenbach (1944)

In common with others (below), whose theories we shall examine in more detail, Reichenbach (27) accepts the basis of von Neumann's argument. He gives, for example, the following definition of a quantum-mechanical measurement:

> 'A measurement of an entity u is a physical operation relative to which the ψ-function of the physical system is represented by one of the eigenfunctions of u, ...'

We could not ask for a clearer statement of acceptance of the projection postulate.

However in a footnote on p. 14, Reichenbach writes:

> 'J. von Neumann's proof (that no "hidden parameters" can exist) ... shows only that the assumption of hidden parameters is not compatible with a universal validity of quantum mechanics.'

Margenau's Renewed Onslaught (1958)

Clearly feeling that the points he had made in 1937 had not been taken, Margenau wrote (62) more than twenty years later that

> 'the trouble with the idea of measurement is its seeming clarity, its obviousness, its implicit claim to finality in any inquisitory discourse'.

Measurement 'stands at the critical junction between theory and experience'. 'Uncertainty implies no ban on measurements: it prescribes the structure of theories.' He repeats his insistence (p. 85) that measurement should do nothing but ascribe a number to a physical quantity. The uncertainty would thus come from 'state-preparation' (p. 86) not measurement.

Suessmann (1958)

Suessmann wrote (63) that the von Neumann proof for the non-existence of hidden variables was subject to the criticism that it assumed that the hidden variables were not disturbed by measurement. This is only one of the criticisms that can be made, and in our view not the most serious one. We shall deal with the problem of hidden variables in detail in Part III from the point of view of those writers who seek an acceptable alternative to the statistical formulation.

Suessmann pointed out that it did not fundamentally change the validity or otherwise of von Neumann's theory if we did not assume that the observed state was not pure but one of an ensemble described by a mixture, before observation.

Durand (1960) *and Fine* (1969)

A much more radical criticism of von Neumann was made by Durand (70): he castigates von Neumann's theory as 'in some respects in conflict with the actual procedures of measurement'. He offers a theory in which 'even if the value $b(a)$ of B corresponding to the eigenvalue a of A is found in the observation $\psi(t)$ is not in general the associated eigenstate $\psi_a(t)$. Fine (125) gives a critique of Durand's analysis as follows:

> 'Durand's scheme is to correlate distinguishable pure states of the object with distinct expected values of the apparatus observable.'

Fine proceeds to demonstrate—to our satisfaction—that this account is invalid.

Albertson (1963)

The first really constructive criticism of von Neumann's theory came from Albertson (85). However, this writer redefines the concept of measurement in a dubious way—which happens to coincide with the results he obtains. He derives an operator from which it is possible to determine $\langle R \rangle_{av}$ and the dispersion of R before the measurement interaction on the basis of the measurement. He then asserts:

> 'If one accepts the proposition that the function of measurement in quantum mechanics is to determine the average value and the dispersion of some physical quality. ...'

Quite simply, we do not. However, Albertson has provided us with more information than we had before. We prefer to follow Margenau's definition of measurement given on page 85. However, this paper does follow Margenau's prescription in that it explicitly rejects von Neumann's projection postulate. It may well be that it is necessary to modify our definition of 'measurement' in the light of this paper, but we are not convinced. The fact that Albertson rejects the projection postulate on the one hand and finishes up with an even more statistical type of result than we were led to expect by Margenau when he proposed the rejection *may* mean that this is the best we can get. That in itself would be an interesting result but it is implicitly asserted rather than proved by Albertson.

Margenau Again (1963)

In 1963 Margenau gave (89) a review of the state of the theory of measurement at that time, together with further pertinent comments of his own. He once again asserts (cf. p. 24) that the projection postulate is not necessary for the consistent formulation of quantum mechanical measurement theory. An interesting feature is the occurrence of negative joint probabilities for the measurement of non-commuting operators. We remark that these negative probabilities, for which Margenau offers no explanation, could be explained along the following lines.

If we know that the probability of a die landing with the six upwards is, on a frequency basis, 1/6, we may add a small lump of plasticine in an appropriate place, and then recalculate the odds, again by taking a large number of throws. The odds may well be, say, 1/12. There has then been a decrease in the probability of throwing a six.

Consider now the equation

$$p_1 + p_2 = p \tag{1}$$

This is merely a probability equation illustrating the addition property in the case where the events referred to in p_1 and p_2 are disjoint. It can however equally well be regarded as an arbitrary partition of p. If $p = 1/6$ and $p_1 = 1/2$, p_2 will be positive, as normally required. If however $p = 1/12$ and $p_1 = 1/6$ then p_2, which may be regarded as a correcting term, will be negative. But this does not alter the validity of the equation (1) as a probability equation.

This argument can be found in a similar form in M. S. Bartlett's paper: 'Negative Probability', *Proceedings of the Cambridge Philosophical Society* (1945), pp. 71–3.

The question remains: what is p_2 the probability of? Bartlett does not answer this question, and we cannot provide an answer either.

Shimony (1963)

In the course of a review of the 'Rôle of the observer in quantum theory', Shimony describes (90) von Neumann's as 'the most systematic theory of observation' and accepts the projection postulate without question. We point this out not so much to criticise Shimony's point of view as to emphasise the unquestioning thoroughness with which physicists had absorbed the postulate and the unawareness that they displayed of Margenau's perennial criticism.

Jauch (1964) (96)

Jauch concentrated his efforts on re-interpreting the measuring process in such a way as to eliminate von Neumann's 'ultimate observer' as he points out that the shifting of the cut in the observed/observing/observer system does not for all its elegance remove the problem of the essential difference between the two forms of transition. His theory is dealt with in detail (see pages 101–104).

Rosenfeld (1965) (103)

Rosenfeld, in common with Daneri *et al.* (80, 108) attacked von Neumann's approach to the disproof of the hidden variable hypothesis as 'circular'. He asserts that it is impossible to include the observer and retain objectivity.

Sneed (1966)

In a paper which deals exhaustively with the empirical basis for the projection postulate, Sneed (112) comes to the following conclusions:

As it is stated, von Neumann's argument for the projection postulate is not sufficient to warrant its conclusion. Additional premises are needed.

In the course of examining a suggested plausible way of filling out the argument by adding premises, he is led to scrutinise the notion of state of a physical system which is employed in von Neumann's argument, and the way of constructing the state of a physical system which allows the premise in question to be most readily justified is rejected as being at variance with the usual way of speaking of the state of physical systems. (Note that this approach was taken up by Jauch with more success (see p. 91 and pp. 101–104).)

Sneed attempts to 'give a precise formulation of a commonly suggested way of relating the state of a physical system to statistical statements which are given an objectivistic interpretation in terms of relative frequencies of results of measurements'.

The final conclusion is that 'if some more favorable way of construing von Neumann has not been overlooked, we might conclude either: (1) von Neumann's argument happens to be invalid although other empirical arguments for the projection postulate (e.g. from the Stern-Gerlach effect) might be valid; (2) the projection postulate is not to be taken as an empirical claim and von Neumann is at best confused in thinking he is offering an empirical argument for it; or (3) the projection postulate is false.' That much is unexceptionable. No one to our knowledge has—in spite of the repeated challenges thrown down—jumped in with a plausible justification of the projection postulate. Writers either attack it or assume it without question.

Summary

We have given a resume of what we consider to be the most significant comments on von Neumann's formulation of the problem: we have so far restricted ourselves to those writers who accept the probabilistic basis of quantum mechanics. We have also not yet commented in full on the work of those writers whose criticism purports to provide a viable alternative to von Neumann's work in those respects in which the writers consider it deficient. It is therefore not surprising that our judgement of the comments that are summarised above is generally harsh: thirty-seven years would be a long time to have to wait for a consistent alternative to a very plausible original. If it were not for the work of the writers summarised below, we should be able to say that von Neumann had had the last word. He himself never went into print after 1932—not even to the extent of revising the 1932 text—on the subject of measurement.

* *

Chapter 2

The Theory of London and Bauer

The first major attempt at a reformulation of the problem of measurement in quantum mechanics after the publication of von Neumann's book was made by London & Bauer in a slender volume (26) (Paris, 1939). It is perhaps unfortunate that this book was published where it was, when it was, as it has been out of print ever since. This has resulted in an unwarranted neglect, and it is only really since 1963 that writers have felt constrained to take into account London & Bauer's point of view.

We have already pointed out (p. 86) that London & Bauer were the first to establish a satisfactory—or at least consistent—definition of where a measurement took place. Taken together with Margenau's points (pp. 85–86) this meant that there was a complete theory of what constituted a measurement. We shall try to point out those respects in which their approach differs from that of von Neumann (above).

Firstly, we must say that their presentation is by any standards much more systematic and elegant than the labyrinthine account of von Neumann. Their proof of the non-existence of hidden variables (but see below, Bub, 1968) is very much shorter and to the point. They give a lovely demonstration of the impossibility of the presentation of a pure case by a mixture ((26), p. 31). Their presentation is not overladen with references to statistical thermodynamics, and there is no danger of a misunderstanding of the kind Elsasser mentioned (above, p. 24).

Secondly, they write in a spirit of confidence—exemplified by the following extract:

> 'Ce n'est donc pas une interaction mystérieuse entre l'appareil et l'objet qui produit pendant la mesure un nouveau ψ du système. C'est seulement la conscience d'un 'Moi' qui peut se séparer de la fonction $\Psi(x,y,z)$ ancienne et constituer en vertu de son observation une *nouvelle objectivité* en attribuant dorénavant a l'objet une nouvelle fonction $\psi(x) = \psi_k(x)$.'

It is this point which constitutes London & Bauer's most controversial—and of course entirely arbitrary and unprovable—contribution to the theory of measure. We feel that it simply states a fact, and are not in any way tempted towards the conclusions that Shimony (90) draws, He writes:

> 'In this passage London and Bauer seem to be stating some important ... proposition regarding the place of mind in nature.'

We should go so far as to claim that the word 'mind' has no place in a scientific discussion. London & Bauer do not use it, and we do not think that it was their intention to do so. It might be relevant to quote the following comment from the psycho-analyst Ernest Jones's autobiography (*Free Associations*, The Hogarth Press, London 1959, p. 155, footnote):

'I would here disclaim belief in any metaphysical entity named the "mind".'

London & Bauer are referring not to some mystical entity but to the observer's brain as a classical or quantum-mechanical entity for producing an interpretation of the numbers that Margenau insists on.

They give a neat analysis of the Stern-Gerlach apparatus, but only *after* having established general principles and as an illustration of them.

As an example of the elegance of London & Bauer's arguments we give their proof of the irreducibility of a pure case (translated from pp. 31–2).

'In order to do this we shall establish that a statistical matrix P obtained by mixing two statistical matrices Q and R

$$(1) \qquad P = aQ + bR \quad \text{with} \quad a + b = 1 \quad \text{and} \quad a \geqslant 0, b \geqslant 0$$

cannot be an elementary statistical matrix (such that $P = P^2$) unless $Q = R = P$. Let us form

$$\begin{aligned} P^2 &= a^2 Q^2 + b^2 R^2 + ab(QR + RQ) \\ &= a^2 Q^2 + b^2 R^2 + ab(Q^2 + R^2 - (Q - R)^2) \\ &= aQ^2 + bR^2 - ab(Q - R)^2 \end{aligned}$$

where we make use of the condition $a + b = 1$. Thus:

$$P - P^2 = a(Q - Q^2) + b(R - R^2) + ab(Q - R)^2$$

Let us now recall that the matrices $Q - Q^2$ and $R - R^2$ as also $(Q - R)^2$ are always [positive] semi-definite.† It follows therefore that these vanish if P is to be an elementary matrix ($P = P^2$). In particular we get $(Q - R)^2 = 0$ whence follows:

$$Q = R$$

for the square of an hermitian matrix is zero if and only if it is zero.... From $Q = R$ and from (1) we get $Q = R = P$'. The semi-definiteness of $(Q - Q^2)$ and $(R - R^2)$ is a simple corollary of the fact that the weights of the component pure states lie between 0 and 1. Lest there be any doubt, they have of course not assumed that Q and R are elementary.

The missing step 'where we make use of the condition $a + b = 1$' is as follows:

$$\begin{aligned} a^2 + ab &= a(a + b) = a.1 = a \\ b^2 + ab &= b(b + a) = b.1 = b \end{aligned}$$

so that $a^2 Q^2 + abQ^2 = (a^2 + ab) Q^2 = aQ^2$ etc.

† i.e. $(\xi, (Q - Q^2)\xi) \geqslant 0$ for all ξ.

It should be clear from the beautiful simplicity of their argument why we are drawn to London & Bauer's approach. London & Bauer are to von Neumann as Mozart is to Wagner: delicacy against splendour.

Bub (119) *on London and Bauer* (1968)

Bub writes of London & Bauer's proof of the impossibility of hidden variables that:

> 'This whole analysis is beautifully simple, perfectly correct, but quite irrelevant to the hidden variable approach. Given the initial values of the Hilbert space vector and the hidden variables, then within the framework of a hidden variable theory it is possible to predict whether the final value of the Hilbert space vector will be $|S_+\rangle$ or $|S_-\rangle$ if the reflexive structural process from which the system is abstracted is such as to represent a measurement of the observable S, ... The hidden variables do not decompose the quantum ensemble into sub-ensembles which consist of systems with definite spins in the classical sense, i.e. in which each system is associated with a unique spin vector defined simply as a function of the hidden variables.
>
> 'The aim of a hidden variable theory is rather to reinterpret the notion "measurement of an observable" in a non-classical, non-Copenhagen sense.'

On London & Bauer's behalf, we reply to Bub that his 'analysis is perfectly correct, but quite irrelevant to the' quantum-mechanical approach of London & Bauer. For a more detailed defence, see Jauch & Piron's summary of their revision of von Neumann's (and hence also of London & Bauer's) theory, in (87).

The Interim: 1939–1964

We have read and digested a great deal of literature stemming from the period between 1939 and 1964. We do not wish to waste our time or that of the reader on the many unsuccessful attempts made during that time to resolve the measurement problem within the probabilistic framework. Details of the literature are given in our Bibliography.

* *

Chapter 3

Gleason's Theorem

In 1957 Gleason published (54) the proof of the following fundamental theorem in the theory of measurement.

> 'Let p be a measure of the closed subspaces of a separable (real or complex) Hilbert space H of dimension at least three. There exists a positive semi-definite self-adjoint operator T of the trace class such that for all closed subspaces A of H
>
> $$p(A) = \mathrm{trace}\,(TP_A)$$
>
> where P_A is the orthogonal projection of H onto A.'

T is a linear operator satisfying the conditions

(i) $T^+ = T$

(ii) $T^2 \leqslant T$

(iii) trace $T = 1$

The semi-definiteness condition (ii) means $(\xi,(T - T^2)\xi) \geqslant 0$, all ξ

Jauch writes (96):

'The theorem of Gleason ... is useful to show that the representation of quantum mechanical states is less arbitrary than it is customarily assumed (or at least presented). It shows that if one tries to generalise states, one would have to do so in the sense of [states where for instance the probability distribution of an individual observable may depend on the exact physical conditions of the measuring device] ....

'The operator T is von Neumann's density operator. Every T which satisfies conditions (i)–(iii) has a discrete spectrum. Its spectral resolution has thus the form

$$W = \sum_n p_n P_n$$

where P_n is a projection operator with a one-dimensional range .... The eigenvalues p_n satisfy then

(i)′ $p_n{}^* = p_n$

(ii)′ $0 \leqslant p_n \leqslant 1$

(iii)′ $\sum p_n = 1$'

If $T^2 = T$ the state is pure. In all other cases it is called a mixture.

[(i)′ is strictly redundant as it is implied by (ii)′.]

Varadarajan (1962, 1965)

In 1962 Varadarajan published a paper (82) of great importance, which has clearly influenced other writers since.

However, we wish here to quote from his book (104) of 1965, with its neat summary of the significance of Gleason's theorem and its consequences.

'This theorem, due to Gleason and established by him in 1957, forms naturally the corner stone of the foundations of quantum theory. Its proof which is a mixture of spherical geometry, classical harmonic analysis on the sphere and some standard functional analysis ... As a consequence one can derive all the standard formulae, such as the expectation values of observables in states ... This is undoubtedly the culmination of the von Neumann programme of describing quantum theory through the ideas of geometry and it represents the point of departure for all modern quantum mechanical discussions. It may be emphasised that with the single exception of Gleason's theorem everything else in the discussion so far has remained substantially unchanged since von Neumann put them down in his book and articles.'

Jauch's Theory

The first major contribution to the literature on measurement as such since London & Bauer was made by Jauch in 1964 (96). Drawing on the theorem of Gleason, and on the considerable volume of work that had been done in the field of the algebra of closed subspaces of a Hilbert space starting with the classic paper of Birkoff & von Neumann in 1936 (18), he had the novel idea of re-examining the notion of state to see whether there might be a simple algebraic explanation of the 'strange duality that has haunted physics'.

Jauch asserts that his analysis dissolves the problem into a pseudo-problem. We shall present the essence of his argument here and then give our conclusions.

The definition of state which Jauch prefers is as follows:

> 'a state is the result of a series of physical manipulations on the system which constitute the preparation of the state'.

Let S be the system of observables of a physical system. It is a set of self-adjoint operators. Two states W_1 and W_2 are *equivalent* with respect to the system S if

$$\text{trace}\, A W_1 = \text{trace}\, A W_2$$

for all A in S. This is written $W_1 \sim W_2$. $\sim$ is clearly reflexive and symmetric, and its transitivity is almost as trivial. It is thus an equivalence relation. Now each state is designated as a 'microstate' and the equivalence class is called a 'macrostate'. The justification for this terminology follows.

If W_1 and W_2 are two different microstates, $W = \lambda_1 W_1 + \lambda_2 W_2$ with $\lambda_1 \geqslant 0$, $\lambda_2 \geqslant 0$; $\lambda_1 + \lambda_2 = 1$ is a microstate. The class of states equivalent to W is independent of the 'representatives' W_1 and W_2 in the equivalence classes of W_1 and W_2. Or

Theorem:

If: (i) $\left.\begin{aligned} W &= \lambda_1 W_1 + \lambda_2 W_2 \\ W' &= \lambda_1 W_1' + \lambda_2 W_2' \end{aligned}\right\} \left\{\begin{aligned} W_1 &\sim W_1' \\ W_2 &\sim W_2'' \end{aligned}\right.$ (ii)

then $W \sim W'$

If σ_P designates the subset of projections in S, for all $E \in \sigma_P$

$$\begin{aligned} \text{trace}\, EW &= \lambda_1 \operatorname{tr} EW_1 + \lambda_2 \operatorname{tr} EW_2 && \text{by (i)} \\ &= \lambda_1 \operatorname{tr} EW_1' + \lambda_2 \operatorname{tr} EW_2' && \text{by (ii)} \\ &= \operatorname{tr} E(\lambda_1 W_1' + \lambda_2 W_2') \\ &= \text{trace}\, EW' \end{aligned}$$

Then we can generalise to σ by using the spectral theorem.

As a result of this theorem 'it is possible to transfer the operation of mixture to the equivalence classes'. Jauch uses the notation $[W]$ for the class of microstates which are all equivalent to W. The theorem allows us to define a mixture of macrostates by the formula

$$[W] = \lambda_1[W_1] + \lambda_2[W_2] = [\lambda_1 W_1] + [\lambda_2 W_2]$$

And it follows that if $W_1 \sim W_2$ then any mixture $\lambda_1 W_1 + \lambda_2 W_2$ is in the equivalence class of $[W_1] = [W_2]$.

If we denote the microstate after the measurement of the observable projection E by W^E, we can formulate the question: is the class $[W^E]$ independent of the representative W? It is, if and only if we can transfer the change of the state under measurements to the classes. Thus

$$[W^E] = [W]^E$$

We now seek a necessary and sufficient condition for this to happen. The measurement of E changes the state to $W^E = EWE + E'WE'$ [for proof see Jauch (121), p. 166] where $E' = I - E$ (I is the universally true statement). So E' represents *not-E*. If E commutes with W, so will E'. Since $E^2 = E$, we have

$$\begin{aligned} EWE &= WE \\ E'WE' &= WE' \\ W' = EWE + E'WE' &= WE + WE' \\ &= W(E+E') \\ &= WI = W \end{aligned}$$

So

$$W' = W.$$

Now, if $W' = W$, $WE = W'E = EWE = EW' = EW$, so that W and E commute. This is therefore the necessary and sufficient condition we require.

And we have:

Theorem (Jauch): The necessary and sufficient condition such that an ideal measurement of a proposition represented by the projection operator E does not *disturb* a state W is that E commute with W.

It is perhaps strange that this fundamental theorem, so long assumed to be true was not in fact proved until Jauch proposed his theory. Even then it is 'proved' only in the sense that if one accepts Jauch's terminology, he has shown that this obliges one to accept that the theorem is true and this establishes the connection between Jauch's theory and the conventional one.

It also gives us good cause for believing that Jauch's macrostates are what we should be happy to accept as macrostates, and that they are in particular amenable to the classical treatment suggested by the theorem above.

In this section of his paper entitled 'The Union and Separation of Systems' Jauch assembles the mathematical apparatus necessary for the deduction that 'the reason for the occurrence of process (1) ... is merely a mathematical consequence of the reduction of a pure state to one of its component subspaces.' The process (1) is the same as von Neumann's process (1) (see p. 90).

This enables Jauch to assert that if the measuring device is now linked to a larger classical system it is in the same equivalence class as the state measured by the measuring device alone, the two states being therefore indistinguishable from one another. 'One of the most vexing problems of quantum mechanics dissolves into a pseudoproblem.'

Thus without stepping for one moment outside the probabilistic framework of quantum mechanics Jauch has apparently achieved the 'tour de passe-passe bien difficile à réaliser' with which London & Bauer had challenged physicists.

Daneri, Loinger and Prosperi

In 1962 Daneri *et al.* published a paper (80) outlining their own theory of measurement. It is surely one of the most 'difficult' papers ever published in the field of measurement. We treated it from the outset with circumspection as we have sought a simplification of the problem and we are suspicious of any attempt that leaves it more complicated mathematically while claiming a substantial resolution of the physical difficulties. Daneri *et al.* state that they are following the philosophy of Jordan & Ludwig (see (61)). Others to do so include Weidlich (118) and Green (60). The feature common to all these approaches is essentially that they wish to reduce the problem to a statistical-mechanical one. In this sense (and they appear to agree with this) they are all in fact 'objectivists' and belong rightly in Part III. Their work was endorsed by Rosenfeld (103).

An example of the sheer complexity of their approach is the quintuple summations involved in equations (12.5)–(12.7) of (80); e.g.:

$$\frac{(Mu_{k\nu}(t) - s_{k\nu}/S_k)^2}{s_{k\nu}^2/S_k^2} = \sum_{\mu\mu'\lambda\lambda'}{}' \sum_{j=1}^{s_{k\mu}} \sum_{j'=1}^{s_{k\mu'}} \sum_{l=1}^{s_{k\lambda}} \sum_{l'=1}^{s_{k\lambda'}} L^{(k\nu)*}_{\mu j,\mu' j'} L^{(k\nu)}_{\lambda l',\lambda' l'}$$
$$\cdot \sum_{uu'vv'} U^{(k)}_{\mu j, e_0 u} U^{(k)*}_{\mu' j', e_0 u'} U^{(k)*}_{\lambda l, e_0 v} U^{(k)}_{\lambda' l', e_0 v'} a_u a^*_{u'} a_v^* a_{v'} \qquad (12.7)\dagger$$

Recently, however, serious doubt has been cast on their claim to have succeeded in their aim by Bub (120), whose paper also contains an excellent summary of Daneri, Loinger & Prosperi's theory. We are satisfied by Bub's argument and leave the reader to judge, if he wishes, whether it in fact demolishes the theory of Daneri *et al.*

We quote below Daneri, Loinger & Prosperi's judgement of Jauch's theory. Their view of their own work is understandably less harsh. We quote, without comment: Their theory 'constitutes an indispensable completion and a natural crowning of the basic structure of present-day

† Actually careful examination of DLP's text reveals that the term $L^{(k\nu)}_{\lambda l',\lambda' l'}$ should read $L^{(k\nu)}_{\lambda l,\lambda' l'}$!

quantum mechanics'. So, naturally, they are 'firmly convinced that further progresses (sic) in this field of research will consist essentially in refinements of (their) approach'.

Critique

Until someone comes forward with a more plausible account of the measuring process than is described by Jauch, his theory must stand. It is elegant and attractive. It appears to deal with the strange problem of measurement in a simple and precise way. That is not to say that the usual mystique will not re-emerge in a new form: Jauch's theory is not universally accepted, but it has provoked his opponents into a rethinking of their views. It was possible for Jauch to write in 1967 (114) that 'When this idealized notion of 'classical' (commuting) observables is adopted as an additional requirement of a suitable measuring device, the solution of the dilemma is easy ... It is based on the theory of equivalence classes of states. A measurement determines not individual microstates but only equivalence classes of such which we might call macrostates. It is then easy to show that the two states cA and $c'A'$ and the mixture of A and A' with probabilities $|c|^2$ and $|c'|^2$ are always in the same equivalence class with respect to that specific measurement. There is thus no possibility to "collapse the state vector" ... contrary to the present doctrine of quantum mechanics.' This last assessment carries also the name of Wigner.

However, Daneri *et al.* (108) write in disparaging terms of Jauch's theory. 'This author *claims* to have dissolved, by means of a suitable analysis, the entire problem of the measuring process into a pseudo-problem. In *reality*, he has missed the point. As it has been *pointed out* by Rosenfeld (private communication ...) [but see (103)] 'he has *unfortunately* dismissed as unimportant the behaviour of the amplifier, which ... is just the key to the understanding of the reduction (of the wave packet). If one reads Jauch's text *carefully*, one notices the logical gap in his argument just at this point; in fact, he does not give any physical justification for considering the probability operator of the microsystem II (the microscopic part of the measuring device), which gives him the reduction.'

'In Jauch's opinion, any attempt, like ours, of finding in the macroscopic nature of the measuring instruments the reasons for the occurrence of a mixture at the end of the measuring process would be doomed to failure. He *claims* that this conclusion follows ... etc.'

They proceed to give a number of arguments against Jauch's theory, which with the possible exception of the quotation from Rosenfeld, do not amount to anything more than a restatement of the position as it was before Jauch published his paper. We take particular exception to the partly patronising, partly tendentious use of the words italicised (by us).

Most of the criticism that has been levelled at Jauch's work comes into the category of 'objectivistic' argument, and will be dealt with accordingly in Part III.

* * *

PART III: THE OBJECTIVISTS

'That power which erring men call Chance'
John Milton (*Comus*, line 587)

Chapter 1

Einstein, Podolsky and Rosen (1935)

A serious challenge to the 'Copenhagen interpretation' of quantum mechanics came from a paper written by Einstein *et al.* (15) in which they give a careful analysis of the measurement of two non-commuting operators, showing that

> 'Either (1) the quantum-mechanical description of reality given by the wave function is not complete or (2) when the operators corresponding to two physical quantities do not commute the two quantities cannot have simultaneous reality. Starting then with the assumption that the wave function does give a complete description of the physical reality, we arrived at the conclusion that two physical quantities, with non-commuting operators, can have simultaneous reality. Thus the negation of (1) leads to the negation of the only other alternative (2). We are thus forced to conclude that the quantum-mechanical description of physical reality given by wave functions is not complete.'

They immediately grant that one solution to this apparent dilemma is that 'our criterion of reality is not sufficiently restrictive'.

Subsequent writers have taken up the challenge thrown down in the last paragraph of this paper:

> 'While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible.'

Schroedinger (1935)

Schroedinger wrote a wide-ranging paper (16) which includes the well-known 'cat paradox' discussed in Part I. We translate from p. 824:

> 'The rejection of realism has logical consequences. A variable in general *has* no definite value before I measure it: measuring it then does not mean determining the value that the variable *has*.'

But, we insist, this is a semantic problem, not a physical one, because we are free to associate the measured value with an attribute of the observable which we shall call its 'value'. This procedure does not appear to lead to inconsistencies, but it does allow of the paradoxes of Schroedinger and Einstein. But this would suggest that it is partly as a result of the use of confusing terminology that these paradoxes arise. We have already suggested that the Schroedinger paradox would crumble under a careful re-examination of the nature of an apparently 'classical' observation. We further suggest that the paradoxes would never have seen the light of day had there not been an unwarranted transfer of the terminology of classical mechanics to the quantum realm without sufficient care.

At the same time as the work described in Part II, a parallel effort was being made to resolve the measurement problem by a much more fundamental attack. A succession of physicists drawing their inspiration from Einstein's stated doubts about the validity of the 'subjective' 'probabilistic' approach of the Copenhagen school set out to replace it by an 'objectivistic', 'deterministic', and 'causal' system of quantum mechanics.

It had been difficult enough to convince laymen of the essentially statistical nature of quantum phenomena, and hence of all phenomena. Now the physicists themselves began to have doubts. Granted, the theory appeared to be successful. But then so had the phlogiston theory, not to mention the archetype of causality—Newtonian classical dynamics—which had been refined into relativistic classical dynamics. (There is of course nothing non-classical about relativity except the date of its emergence.) Could it not be too easy to accept the finality of a theory, only to discover that there was evidence that the theory was incomplete? This situation had never failed to arise before. History seemed reluctant to use a full-stop. It had sprinkled physics with commas, semi-colons—even question-marks and exclamation marks—but never a full-stop before. As we have said, von Neumann showed that within the system of quantum mechanics there was no possible advantage in introducing 'hidden variables', as these could not add to our knowledge of the system. Von Neumann writes:

> 'It is therefore not, as is often assumed, a question of a re-interpretation of quantum mechanics,—the present system of quantum mechanics would have to be objectively false, in order that another description of the elementary processes than the statistical one be possible.'

This assertion has been challenged on the grounds that von Neumann did not allow of the widest possible choice of hidden variables. In response to this, Jauch and others have refined von Neumann's argument. The debate continues: the latest salvo was fired in 1968 (reference 122). We are reluctant to embark on a detailed discussion of the theories proposed, as they seem to have a higher mortality rate than do the counter-theories. Bohm, for example, wrote in 1957 (51) of his 1952 theory (40) that it was 'rather artificial in form, besides being subject to the criticism ... that it implies instantaneous interactions between distant particles, so that it is not consistent with the theory of relativity.' So he set out to look for 'a further new explanation (!) ... in terms of a deeper subquantum-mechanical level.'

De Broglie on Bohm

De Broglie had been a sympathetic reader of all the attempts to reformulate quantum mechanics on a deterministic basis. In (52) in 1957 he described

'... la théorie de la double solution que j'ai à nouveau développée depuis quelques années à la suite d'un travail de M. David Bohm ....'

In 1956 he had written, in (93) about Bohm's 1952 theory alluded to in the above extract that:

> 'He assumes that the ψ-wave is a physical reality (even the wave in configuration space !). I have already stated why such an hypothesis appeared absolutely untenable to me.
>
> 'Bohm's papers contain still other statements that strike us as dubious ... The modification ... he proposes as a remedy seems to me artificial.'

Then de Broglie pays tribute to Bohm's achievement in focusing attention 'on the possibility of an interpretation of Wave Mechanics different from the one that is now prevalent'.

That Bohm's theory appears to have been constructed artificially to satisfy a particular need is not, we must insist, a reason for rejecting it. Once again, we recall that 'Extreme remedies ...' The striking fact is that the potential supporters of Bohm's point of view, de Broglie among them, have not felt drawn to any of his proposed theories.

To the unsympathetic reader, Bohm's theories take on at times an almost bizarre appearance. Landé has put it rather well (102). He castigated Bohm for inventing *ad hoc* 'hydrodynamic forces' to explain simple facts in a complicated way. There is, of course, as Landé points out, nothing in principle to prevent the success of a 'sub-quantum' explanation—except its failure.

Thus we have the spectacle of respectable physicists grubbing around at the sub-quantum level for the philosopher's stone that will transmute uncertainty into deterministic certainty. The whole search for determinism carries the aura of alchemy. It has had about as much success as did its mediaeval precursors in discovering nuclear fission.

We do not propose to spend a great deal of time discussing the details of these theories, details of which are given in the Bibliography, under references (40), (44), (48), (50), (52), (68), (93), (95) etc.

Bohm versus Jauch

An interesting example of the way in which the exponents of 'objective' quantum theory have catalysed the orthodox workers into a careful reformulation of their theory is given in reference (122).

In 1963 Jauch & Piron published a paper (87) in which they modified von Neumann's proof of the impossibility of hidden variables to accommodate criticism that it had depended on the validity of quantum mechanics and was thus essentially a circular argument. This point was made with emphasis by Daneri *et al.* (80) and by Rosenfeld (103). Instead of assuming that quantum mechanics was valid, Jauch & Piron took the lattice of propositions, which is shown to be isomorphic to the lattice of subspaces of Hilbert space. These propositions are treated as yes–no experiments.

Bohm & Bub wrote a paper (106) rebutting the argument of Jauch & Piron, and asserting that the measurement problem could not be solved within the framework of the statistical interpretation.

In the correspondence referred to above (122), Jauch & Piron defend their position and Bohm & Bub are allowed the right of reply.

Jauch & Piron merely restate in non-technical language the position they took in 1964. Bohm & Bub use the analogy of non-Euclidean geometry: they say that since the deviation of elliptic geometry from Euclidean geometry may be so slight that it could not yet have been tested for our universe, we are not entitled to exclude the possibility that we live in a non-Euclidean world. However, we feel that the analogy breaks down at this point. Terrestrial experiments can be designed to discover the curvature of space (if any): but there are no concrete manifestations of divergence from a Euclidean structure. Bohm & Bub must maintain—if their approach is to withstand Landé's methodological criticism—that the non-Copenhagen (or whatever) nature of quantum mechanics manifests itself *in the measurment process*. But surely something strong enough to manifest itself in every observation ever made, in every interaction between quantum and classical systems, should be amenable to experimental deduction.

The analogy now takes the following course: if there were a freakish property of the physical world that did not correspond to the Euclidean prediction, we should certainly look for a non-Euclidean solution. (But where are Bohm's Brocken, Hohenhagen and Inselsberg?) So far, however, in ordinary Geometry, we have found *no* properties that could not be best accommodated by a tightening of the Euclidean axiomatic system. It would be a rash man who submitted himself to the discipline of non-Euclidean geometry merely because he found difficulty in proving the mid-point theorem. We can and should resist any temptation to join him in this masochistic exercise.

* *

Chapter 2

Causality: Why?

No one seems to have stopped and asked *why* we consider causality to be a good thing and probability to be an essentially bad one. After all, the only theory that has yet failed to yield a refinement is a probabilistic one, while all but the latest deterministic theories represent merely one stage in the process of refinement.

There is in principle no good reason for supposing that the basic laws of our world are not probabilistic. There is no essential merit, no moral superiority in a deterministic theory. There is, as evidenced by the constant refinement, no essential element of 'truth' in deterministic theories as compared with statistical ones.

Would we be so keen to find a causal, deterministic answer to our problem if we had not been brought up on Newtonian mechanics? Look back further, to the first stages of learning: we manipulate exact numbers and get exact answers. Is it surprising that we grow up to expect the quantities of adult sums to behave in the same way as their infant counterparts?

We repeat what we said at the beginning of Part I: that the 'classical' nature of classical physics emerged from the reconsideration of that discipline in the light of quantum theory.

Laplace's Demon

In this connection it is interesting to trace the history of 'Laplace's demon' in the literature of physics.

Laplace wrote in 1819 in his *Théorie Analytique des Probabilités*:

> 'Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situations of the being (*êtres*) who compose it—an intelligence sufficiently vast to submit these data to analysis—it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. The human mind offers, in the perfection which it has been able to give to astronomy, a feeble idea of this intelligence. Its discoveries in mechanics and geometry, added to that of universal gravity, have enabled it to comprehend in the same analytical expressions the past and future states of the system of the world. Applying the same method to some other objects of its knowledge it has succeeded in referring to general laws observed phenomena and in foreseeing those which given circumstances ought to produce. All these efforts in the search for truth tend to lead it back continually to the vast intelligence which we have just mentioned, but from which it will always remain infinitely removed. This tendency, peculiar to the human race, is that which renders it superior to animals; and their progress in this respect distinguishes nations and ages and constitutes their true glory.'

Mach, writing in 1883, said of this:

> 'Laplace even conceived a mind competent to foretell the progress of nature for all eternity, if but the masses, their positions, and initial velocities were given. In the eighteenth century, this joyful overestimation of the scope of the new physico-mechanical ideas is pardonable. Indeed it is a refreshing, noble, and elevating spectacle; and we can deeply sympathize with this expression of intellectual joy, so unique in history.
>
> 'But now, after a century has elapsed, after our judgement has grown more sober, the world-conception of the encyclopaedists appears to us as a mechanical mythology in contrast to the animistic of the old religions....
>
> 'Physical science does not pretend to be a complete view of the world ....' ((126), pp. 558–9).

But, we protest, Laplace conceived of an intelligence with those powers in order to *contrast* it with the actual powers of the human intellect. And he was writing in the nineteenth century, not the eighteenth.

But now let us see what happened to Mach's description, when it was re-interpreted for twentieth-century audiences by Andrade in 1956:

> 'Laplace ... put forward the view that an intelligence which should know the masses &c. ... would be able to ... foretell the whole future and trace back the whole past. This mathematical and mechanical mythology was at one time widely accepted in certain scientific circles: in fact a famous writer (footnote: Ernest Mach) of the end of the past century said that at that time a great majority of scientists were of this way of thinking.' ((49), p. 239)

Poor Mach.

Determinism is constantly set up like a nine-pin, simply to be knocked down anew. We shall resist the temptation to repeat the process and content ourselves with the observation that if no one really believed in determinism before the advent of quantum mechanics it seems a futile task to 'restore' the 'lost' determinism.

We conclude with the words of T. H. Huxley (*On the Physical Basis of Life*, 1868):

> 'And what is the dire necessity and "iron" law under which men groan? Truly most gratuitously invented bugbears. ... It is very convenient to indicate that all the conditions of belief have been fulfilled, by calling the statement that unsupported stones will fall to the ground, "a law of Nature". But when, as commonly happens, we change *will* into *must*, we introduce an idea of necessity which most assuredly does not lie in the observed facts, and has no warranty that I can discover elsewhere. For my part, I utterly repudiate and anathematise the intruder. Fact I know; the Law I know; but what is this Necessity, save an empty shadow of mine own mind's throwing?'

* * *

PART IV: AN ANNOTATED BIBLIOGRAPHY

This bibliography is intended to include all the major works on the theory of measurement, together with those other contributions that we have found helpful. As our intention is to give an historical picture of the evolution of the problem and the attempts to solve it, we have taken the unusual course of arranging the Bibliography in chronological order.

We have, however, arranged the publications of each year in alphabetical order. We have also provided an index of authors, so that this Bibliography can be used in the conventional way if so required.

1926

1. Born, M., *Zeitschrift fuer Physik*, **38**, 803. This paper is the source of the probabilistic interpretation of quantum mechanics.

1927

2. Heisenberg, W., *Zeitschrift fuer Physik*, **43**, 172. The uncertainty relations are introduced here.
3. von Neumann, J., *Goettinger Nachrichten*, p. 273. Derives a formula for the mean value of a physical quantity: $Qe^{-\beta H}$, where Q is the matrix operator of the quantity and H is the Hamiltonian of the system.

1928

4. Bohr, N., *Nature*, **121**, 580. Source of 'Bohr's interpretation of quantum mechanics': he introduces the idea of complementarity and emphasises the non-causal nature of quantum mechanics. 'Measurements in the Quantum Theory', pp. 582–4, §3, is a detailed examination of the 'Heisenberg microscope'.
5. Solvay Congress, *Reports on the 1927 Solvay Congress*, Paris, Gauthier-Villars, 1928. Pauli attacks de Broglie's point of view, p. 280.

1929

6. Dirac, P. A. M., *Proceedings of the Cambridge Philosophical Society*, **25**, 62. 'The Basis of Statistical Quantum Mechanics.'

1930

7. Heisenberg, W., *The Physical Principles of Quantum Theory*, University of Chicago Press, Chicago, Ill., 1930. Since reprinted by Dover Publications, N.Y. The famous 'Chicago Lectures' of Heisenberg.

1931

8. Bohr, N., *Atomtheorie und Naturbeschreibung—Atomic Theory and Description of Nature*, Cambridge University Press, 1934.

1932

9. Kolmogorov, A., *Annals of Mathematics*, **33**, 175. Shows that any projective geometry whose elements have a locally compact topology must be over the real, complex or the quaternion field.
10. von Neumann, J., *Mathematische Grundlagen der Quantenmechanik*, Berlin, Springer, Chapters IV–VI.
    —— *Mathematical Foundations of Quantum Mechanics* (tr. R. T. Beyer), Princeton University Press, 1955.

1933

11. Bohr, N., *Nature*, **131**, 421, 457.
12. Bohr, N. and Rosenfeld, L., *Danske Vidensk. Selskab.*
13. Kolmogorov, A., *Grundbegriffe der Wahrscheinlichkeitsrechnung*, Berlin. The first explicit statement of the axioms of classical probability theory.

1935

14. Bohr, N., *Physical Review*, **48**, 696.
15. Einstein, A., Podolsky, B. and Rosen, N., *Physical Review*, **47**, 777.
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17. Schroedinger, E., *Proceedings of the Cambridge Philosophical Society*, **31**, 555.

1936

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20. Lindsay, R. B. and Margenau, H., *Foundations of Physics*, John Wiley, New York. Chapter 2: 'The Problem of Causality'.

21. Margenau, N. H., *Physical Review*, **49**, 240.
22. Schroedinger, E., *Proceedings of the Cambridge Philosophical Society*, **32**, 446.

1937

23. Elsasser, W. M., *Physical Review*, **52**, 987.
24. Kemble, E. C., *The Fundamental Principles of Quantum Mechanics*, McGraw-Hill, New York. 'The Measurement of Dynamical Variables', pp. 381–47.
25. Margenau, H., *Philosophy of Science*, **4**, 337. Still worth reading.

1939

26. London, F. and Bauer, E., *La Théorie de l'Observation en Mécanique Quantique*, Hermann, Paris.

1944

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1945

29. Dirac, P. A. M., *Reviews of Modern Physics*, **17**, 195. Develops a formal probability for non-commuting observables to have numerical values. The first use of complex probabilities, but no interpretation given.
30. Powell, F. C., *Proceedings of the Cambridge Philosophical Society*, **41**, 57. Establishes the connection between observables and measurement.

1946

31. Groenwold, H. J., *Physics*, **12**, 405. Introduces the idea of 'entangled' state and 'infringed' state between initial and measured states, an idea which was never followed up. Asserts categorically that the observer is 'unsusceptible of a physical treatment'.

1947

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1948

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34. Bohr, N., *Dialectica*, **2**, 317.

1949

35. Bohr, N., discussion with Einstein, A., *Albert Einstein Philosopher-Scientist* (edited by P. A. Schilpp), Evanston, Illinois.
36. Born, M., *Natural Philosophy of Cause and Chance*, Oxford University Press.

1950

37. Margenau, H., *The Nature of Physical Reality*, McGraw-Hill, New York.

1951

38. Bohm, D., *Quantum Theory*, Prentice-Hall, New Jersey.
39. Feynman, R. P., 'The Concept of Probability in Quantum Mechanics' in the *Proceedings of the Second Berkeley Symposium in Mathematical Statistics*, Berkeley, California.

1952

40. Bohm, D., *Physical Review*, **85**, 166, 180. 'The development of the usual interpretation of the quantum theory seems to have been guided to a considerable extent by the principle of not postulating the existence of entities which cannot now be observed... (which is)... an extraphysical limitation on the... theories.... As an alternative... we wish to prevent (sic) another hypothesis... that the world as a whole is objectively real and that, as far as we now know, it can correctly be regarded as having a precisely describable and analyzable structure of unlimited complexity.'
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42. Wigner, E. P., *Zeitschrift fuer Physik*, **131**, 101. Suggests that the existence of a conservation law implies a restriction on the measurability of any quantity that does not commute with the conserved quantity.

1953

43. Ludwig, G., *Zeitschrift fuer Physik*, **135** 483. 'Der Messprozess.'
44. Wiener, N. and Siegel, A., *Physical Review*, **91**, 1551. The wave function is replaced by a mapping of points of differential space, 'closely related to Hilbert space but having an associated measure'. Probability amplitudes are replaced by probabilities or probability densities.

1954

45. Ludwig, G., *Die Grundlagen der Quantenmechanik*, Springer, Berlin. V. Der Messprozess: see (43). An exhaustive account, taking the view later expressed in detail by Daneri *et al.* wherein the apparatus is taken to be in a metastable state.

1955

46. Bastin, E. W. and Kilmister, C. W., *Proceedings of the Cambridge Philosophical Society*, **51**, 454. 'The Concept of Order' II: Measurements. Described by Bastin in Colston Proceedings (55).
47. Weizsaecker, C. F. von, *Die Naturwissenschaften*, **42**, 521. An excellent summary of the position of quantum logic as it stood at the time of writing.
48. Wiener, N. and Siegel, A., *Il Nuovo Cimento*, Suppl. **2**, Series X, 304.

1956

49. Andrade, E. N. da C., *An Approach to Modern Physics*, London.
50. Siegel, A. and Wiener, N., *Physical Review*, **101**, 429. 'Theory of Measurement in Differential-Space Quantum Theory.' A development of the authors' theory (see 44): here they concede that its serious drawback is that 'one must admit the "real existence" of the individual systems represented by points in differential space'. Criticises Bohm's theory (40) as in it 'the position (or functions thereof) is the only variable that can ever be measured on an individual system without in general simultaneously altering its value'.

1957

51. Bohm, D. and Aharonov, Y., *Physical Review*, **108**, 1070. Possible experimental tests for Einstein–Podolsky–Rosen paradox (15).
52. de Broglie, L., *La théorie de la mesure en mécanique quantique*, Gauthier-Villars, Paris. De Broglie here resuscitates his 1927 theory of the 'double solution', spurred on by the work of Bohm *et al.* Unexceptionable treatment of the orthodox theory, drawn largely from London & Bauer (26).
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1958

58. Bohm, D., *Causality and Chance in Modern Physics*, Routledge and Kegan Paul, London. (American edition published by van Nostrand, New Jersey, 1957.) This is Bohm's statement of faith.
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1959

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1960

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70. Durand, L., III, *Philosophy of Science*, **28**, 115. 'On the theory of measurement in quantum mechanical systems.' See (125).
71. Jauch, J. M., *Helvetica Physica Acta*, **33**, 711.
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1961

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75. Ludwig, G., in *Werner Heisenberg und die Physik unserer Zeit*, Vieweg, Braunschweig.
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78. Zierler, N., *Pacific Journal of Mathematics*, **11**, 1151. A new Axiomatisation.

1962

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82. Varadarajan, V. S., *Communications on Pure and Applied Mathematics*, **XV**, 189. 'Probability in Physics and a Theorem on Simultaneous Observability.' But see (111).
83. Wakita, H., *Progress of Theoretical Physics*, **27**, 139. Part II of (72). Concludes that the 'reduction of the wave packet' is unnecessary in theory but convenient in practice.
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1963

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1966

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107. Cohen, L., *Philosophy of Science*, **33**, 317. It is here proved that quantum mechanics cannot be formulated as a classical probability theory.
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1967

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